THE NEW ORLEANS MEDICAL AND SURGICAL JOURNAL,

DEVOTED TO MEDICINE AND THE COLLATERAL SCIENCES.

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"Summum bonum Medicinæ, sanitas."—GALEN.

NEW-ORLEANS CHARITY HOSPITAL.

VOL. II.—FOR 1845-46.

NEW-ORLEANS.

PUBLISHED BY S. WOODALL, 49, CAMP STREET.

1846.

THE

NEW-ORLEANS MEDICAL JOURNAL.

Vol. II.—No. 1.

JULY, 1845.

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ART. I.—*An Essay on the subject of Quarantine Laws; read before the Physico-Medical Society of New-Orleans, at the sitting of Feb. 15th*, 1845. By William P. Hort, M. D., of New-Orleans.

The subject of quarantine laws is presented to the society for consideration, for the following reasons:

1. The public press has on various occasions called for the establishment of quarantine laws, in this city.

2. Acts of the Legislature have been passed at different times, establishing such laws, which, however, proved to be unavailing, not on account of the laws being defective, but because public opinion was opposed to them, influenced and sustained by a large majority of the medical profession.

3. About a year ago, a respectable Medical Society, in this city, brought the subject before the Legislature.

4. A talented Professor of the Medical College has published a pamphlet in favor of quarantine laws, in which he has distinguished himself by the diligent researches that he has made, and the very able manner in which he has arrayed authorities in their favor.

5. The physicians in the towns situated on the Mississippi river, appear to be generally in favor of such laws.

6. It is a subject of great importance to a commercial city like New-Orleans. Quarantine laws injure alike the ship owner and the shipper; they are particularly onerous to passengers; they would destroy our trade with Mexico, Havana, the West India Islands and South America, from May to November; and during this period, it is almost the only trade carried on in New-Orleans.

Yet if it can be shown conclusively, that such laws are necessary, then should individual give way to the general welfare.

My attention was called to this subject twenty-two years ago, having been a student in the rival Colleges of New York and Pennsylvania. In the former, Dr. Hosack, the distinguished professor of the Theory and Practice of Medicine, contended that yellow fever was an imported disease, and contagious, and consequently that quarantine laws were ne-

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cessary. And in the latter school, the no less learned and talented professor Chapman maintained the very opposite doctrines.

Without any improper motive to influence me, I came to the conclusion that the weight of evidence was in favor of the Philadelphia school; yet so strong were the facts, and authorities, and reasoning, on both sides, that I determined to suspend my final decision until my personal experience and observation should convince me.

Quarantine laws had their origin during the crusades. The plague was supposed to have been brought from Palestine by the crusaders, and introduced into the European ports on the Mediterranean. From the derivation of the word, the system would appear to be of Italian origin, it being derived from quarantina, alluding to a detention of forty days.

The system is then comparatively of modern origin. The expediency of such laws in the countries whose shores are washed by the Mediterranean Sea will not now be disputed. My remarks will be confined to the questions of the contagiousness and importation of yellow fever, on which the expediency or necessity of quarantine laws in Louisiana alone depends. Of the origin of diseases called endemic, or those of local origin, and confined to a certain section of country, (as the plague in the Grecian camp, so beautifully described by Homer), and of epidemics, which travel from country to country, and from continent to continent, and from one hemisphere to the other hemisphere, apparently controlled by no fixed laws, we know nothing more, strictly speaking, than what was known in the time of Hippocrates. That eminent ancient physician was a close observer of nature; he had traveled much, and had no doubt witnessed many endemic and epidemic diseases; and yet in a chapter which he devotes to the subject, he declines to assign any natural cause, and humbly ascribes the whole work to the "το Θεον"—the divine spirit or agency, which we should call the God of Nature.

In fact no subject is involved in greater obscurity than the doctrine of miasm. Precisely as the endemic fever was produced in the Grecian camp by the fierce action of the sun's rays on the marshy ground between the Tanais and Scamander rivers, where the Grecian army was encamped, do *we* see disease engendered under the same circumstances, in many parts of our country; we can perceive the connection between the cause and the effect; we can often trace the most formidable diseases to local causes; we smell something that is extremely offensive and peculiar, which sometimes strikes us like electricity, producing nausea and even vomiting, and a sense of tension round the base of the brain; and we are sensible that the nervous system has sustained a severe shock by the vertigo, trembling, and loss of physical power, that we experience. But chemistry has as yet detected no noxious qualities in this offensive air; whatever miasm is, it has hitherto eluded the most delicate tests and experiments. Having premised these remarks, I shall proceed to the consideration of what is called the importation of yellow fever. In our classical studies we have read of nations contending for the honor of their country having given birth to some distinguished individual, but everywhere the paternity of yellow frever seems to be disclaimed. From the United States it is traced to the West India Islands, thence to the coast of Africa, and thence to Siam, where some are of opinion that it originated. But no doubt, should we prosecute our researches in Africa and Siam, we would find persons there who would tell us that it was imported from some other parts of the world, and so on, until we should find ourselves revolving in a circle *ad infinitum.*

If it is maintained that a disease originates in a certain country, and causes or circumstances are pointed out, which are supposed to produce it, then, wherever we find in other places and parts of the world the same causes or circumstances, we cannot see why the same disease should not be produced in the one, as well as in the other place. If such causes however do not exist, the disease cannot exist, unless the doctrine of contagion be admitted.

Infectious air from the hold of a ship, or from clothes or goods, or from a trunk, might destroy a few individuals exposed to its influence; but it could not go far; it would soon be diluted so as to become innocuous; or should it become modified in some way in an impure atmosphere, then it would no longer be the same disease. One fact is here introduced to illustrate this position. In 1817, a barge left this city with goods for a store keeper at Bayou Sara; during the passage up the river, and shortly after the arrival of the barge, every one of the crew and passengers died of yellow fever. The goods were landed and conveyed to the store; and the store keeper who opened the packages, although he was warned not to do so, sickened and died of yellow fever; but no other person in the neighborhood contracted the disease. The whole subject is then narrowed down to the question of contagion. It is asked; why do we hear nothing of the yellow fever having prevailed on this continent, and in the West India Islands, before they were discovered and inhabited by Europeans; why then would *we* ask, do we hear nothing of bilious and congestive and typhus fevers, and divers other diseases, unknown to this continent, anterior to that period. Will any one say that bilious and congestive and typhus fevers are imported? Yet there was a time when they were no more known on this continent than yellow fever.

Disease follows in the track of civilization, not carried by the people from one country to another, but developed by the great physical changes brought about by industry, and agricultural pursuits. The surface of the earth once sheltered from the suns rays by luxuriant vegetation is laid bare to the action of those rays; the surface of the earth is turned up by the plough, exhalation and evaporation follow; vegetable matter is decaying in large quantities, or large cities are built, and people become crowded together within a very limited space, and filth and offal accumulate. Then marshes are exposed, and great changes must be going on in the atmosphere near the surface of the earth; and is it at all strange, that under such circumstances, new diseases should be developed? But the yellow fever is said to have been imported from Siam; in other words, it is affirmed that the fever originated in Siam, and thence spread gradually to all parts of the world in both hemispheres, where it has since prevailed. What is there, however, in Siam, as far as we can see, to produce yellow fever, which we do not find in the corresponding latitude in the Western hemisphere? It is a genuine yellow fever region, including the greater part of the West India Islands, Vera Cruz, Chagres, Carthagena, and lying between the 10th and 20th degrees of North latitude. The valley of the Ganges, the centre of the cholera region, lies between 20 and 30 degrees North; and in the Western hemisphere, in the corresponding latitude, we find New-Orleans, Havana, and all the Mexican ports and West India Islands, not included in the first range. The most unhealthy part of the African coast, from the equator to 10 degrees North, corresponds to that portion of country in South America, situated between the mouths of the Amazon and Orinoco rivers, including Cayenne, Surinam and Guiana, a most decided yellow fever region. New-Orleans and Grand Cairo are precisely in the same latitude, and similarly situated; the one being on the bank of the Mississippi, and the other on the bank of the Nile, and both on alluvial soil, and in a hot climate. Both the Northern and Southern shores of the Mediterranean between the 30th and 40th degrees of North latitude, are included within the region of plague; and in the same latitude in this hemisphere, lies all our seaboard, and Southern cities from New-Orleans nearly to Sandy Hook. What is there in Siam, or on the African coast, calculated to produce malignant fever, that we do not find in the low marshy pestilential country lying between the Amazon and Orinoco rivers? What is there at Alexandria or Grand Cairo, or Smyrna to produce disease, as far as location, latitude, and heat and moisture are combined for such cause, that does not equally exist on the bank of the river, where the city of New-Orleans stands? In looking over the map of the world, we see in the countries North and South of the equator, but chiefly North as far as 49 degrees, three great regions of disease; first, of plague; secondly, of cholera; and thirdly, of yellow fever. If all the circumstances were precisely the same in these three regions, only one of these diseases would probably exist; but as there is some modification in the circumstances, there is also a variety in the character of the pestilence engendered.

There was a time when neither the African fever, nor that supposed by some to have been imported from Siam, had been heard of; but as the enterprise of Europeans, under the impulse of increasing civilization, carried them into all parts of the world, and induced them for commercial purposes to make settlements, in due process of time, the peculiar diseases of the climate became developed; and therefore we would lay it down as a general rule, that under the same or nearly the same circumstances, we shall find the same or very nearly the same diseases, without going to the trouble of importing them.

To every physician who has had an ample experience in the Southern States, many illustrations will recur, of the development of malignant disease. There have been repeated instances of families residing for years on a plantation, enjoying perfect health, yet in cutting down a belt of timber that intervened between the dwelling house and a mill pond or some marshy ground,—malignant fever has almost immediately occurred, carrying off half the family, and driving the other half away. But to illustrate it on a larger scale, we will advert to what has occurred in Italy. The Pontine marshes are about 18 miles from Rome in a North-east direction; formerly, when the population of Rome consisted of two millions, they were protected from the Pontine marshes by an immense forest, but since the timber has been cut down and the Campagna di Romagna completely laid open, the population of Rome has been reduced to about forty thousand, and whenever the wind blows from the North-east for four or five days, the most malignant fevers appear, from which no one is exempt. All this, however, may be admitted as to malignant fevers generally; but we have to prove that yellow fever is developed and generated in this country, or we must admit that it is an imported disease. In the town of Bloomington, in North Carolina, there is no intercourse with the yellow fever region from the end of May until November, and very little at any time, yet about the time the rice fields are drained, cases of yellow fever often occur amongst the unacclimated; and I have known entire crews of vessels from New York, or some other northern port, arriving at that season, to be cut off in seven or eight days. I have seen the yellow fever in Florida fifty miles from the Gulf, where it could not have been imported, where the black vomit was forcibly ejected five or six feet, and where it could be traced to a local cause on the plantation. In the lower part of the house-lot of one of my neighbors, a small pond was formed by the trampling of stock, into which a quantity of cotton seed had been washed by heavy rain. The negro cabins were on one side of this pond; some nearer to it than others; the dwelling house was distant about one hundred yards. At the end of August, 1825, the yellow fever appeared, attacking both blacks and whites, and no doubt the mortality would have been great, if the gentleman had not promptly moved all hands to the pine woods which were close at hand. I was sitting up one night with three of the white family, expecting one of them to die before the morning, when the night being very warm, the thermometer at 92 degrees, I opened a window in the direction of the pond to admit a little fresh air, and never shall I forget the pestilential blast that came from the pond. It produced nausea and vomiting, and the sensation of a band of iron round the head. Three days after, I had the yellow fever.

In the summer of 1826, some men died of yellow fever in Apalachicola Bay. They had come direct from Baltimore, bringing out a large stock of goods to establish a store in the interior of the country. At that time, there was no settlement at Apalachicola, and no intercourse with the West Indies, or any other place, from which the disease could have been imported.

There is such constant intercourse between this city, and the Mexican, West Indian, and South American ports, that there always will be a vessel here from one of those ports about the time that yellow fever may occur, and if the disease is imported one year, why not every year? for the yellow fever prevails annually at these foreign ports; there is constant intercourse with them during the summer, and, on an average, about the same number of strangers here who are unacclimated.

In 1839, the first case of yellow fever occurred in the mint; a young man of the name of Clark, a machinist, died of the disease about the end of July. That year, at the foot of Esplanade street, all the filth of the city below Canal street was thrown over the wharf into the river; so long as the river was high, it was immediately carried off by the rapid current; but about the 1st of July, the river began to fall, and very soon a most disagreeable smell was perceived whenever the wind blew from that direction. It forcibly reminded me of the pestilential odor from the pond in Florida, of which I have just spoken. On going to the end of the wharf, I found that the bank was exposed; and the vegetable remains, dead animals, and every kind of filth thrown on it, appeared to be a living mass of maggots. From Hospital street, two squares above, to Manderville street, three squares below, every unacclimated person residing on the front levee, without respect to age or sex, died. A puff of air coming from that spot produced nausea and vomiting, even in acclimated persons. In the mint, two officers and several of the workmen died, and not one unacclimated person escaped the disease.

It is remarked that the yellow fever generally makes its appearance near the bank of the river, and from this circumstance it is inferred that the disease is imported. If we recollect the immense batture that is forming in front of the city; that fresh alluvial deposits are made every year, and that when the river falls rapidly in summer, an immense surface, extending for miles, covered with vegetable and animals remain in a state of putrefaction, is exposed to the action of the sun, it will no longer appear surprising that the fever should originate on some part of the alluvial bank, and that unacclimated persons exposed to its influence, whether near the levee, or on board the shipping, should be the first to suffer. On the contrary, it is the very result we should predict from the facts of the case. A fatal yellow fever desolated the town of Woodville last year. It may be well to remark, that it is not one of the river towns, it is about twenty miles from the river near the Louisiana line. Some vague reports stated that this fever was imported from Natchez; there was, however, no yellow fever in Natchez last year, and in this city it was sporadic and not epidemic; for, of a population exceeding one hundred thousand, only 148 persons died of yellow fever, although there was no want of subjects. Happily for the cause of truth and medical science, this subject was effectually put to rest, by the report of Messrs. Valleti and Logan, two distinguished physicians of this city, who visited Woodville while the disease was raging. They conferred freely with all the physicians of the place, philosophically examined and weighed every circumstance, and came to the conclusion that the disease could not have been imported, and that it was of domestic origin. Proof of the local origin of yellow fever in Louisiana is most abundant; one ort wo cases, however, will serve for illustration. In 1817, there were three boats running on the river, but they seldom visited the city during the sickly season. One, however, ventured down that year; two gentlemen are now living who were on board that boat; the yellow fever broke out amongst them before they arrived at New.Orleans. In 1819, the yellow fever carried off a whole family at Bayou Sara; the disease was traced to a quantity of putrid bacon exposed in a cask in the back yard. Prior to this family being attacked with the disease and destroyed, no case of yellow fever had occurred there; nor did any other case occur, for the cause was immediately removed. In summing up the account of the yellow fever in 1793 in Philadelphia, Dr. Rush observes: "I shall resume this interesting discussion in another place, in which I shall mention a number of additional facts, not only in support of the domestic origin of bilious yellow fever, and of its not spreading by contagion and of course of its being impossible to import it." At page 43 of that account, he attributes the fever to damaged coffee, in the immediate neighborhood of which, he says the first case occurred, and where the pestilential odor was much complained of. The greater part of the persons in that neighborhood died, (145.) Is there not a remarkable similarity between this case, and the case of 1839 in this city?

Dr. Hutchinson, the inspector of sickly vessels, was directed by the Governor to ascertain the origin of the disease; he addressed letters to many of the physicians; and from their replies, he concurred in opinion with Dr. Rush, and concludes by saying, "that not having heard of any foreigners or sailors being affected, nor of its being found in any lodging houses, it was not an imported disease."

There was, however, a difference of opinion on the subject, as we see by the letter of Dr. Redman, President of the College of Physicians, but who, says Dr. Rush, failed to designate the place from which the disease was imported, at what time it was introduced, and in what manner, in answer to certain interrogations in the Governor's letter.

Dr. Rush enumerates many other places north and south of Philadelphia, where a similar disease prevailed that year, but says, that in none of those places was there any suspicion of the disease being imported, or conveyed by an intercourse with the city of Philadelphia.

In 1794, he is still of the same opinion, and says, there is not a vestige of proof of its importation, p. 217. He further says, the year 1795, furnishes several melancholy proofs of the American origin of yellow fever. All the physicians of New York and Norfolk agree in its having been generated in their respective cities that year. On one occasion the disease was ascribed to a vessel from St. Marks, and Dr. Rush observes: "I had attended nearly thirty persons, and upwards of a hundred had it, before the vessel arrived. It is very probable that this would be found to be the case in every instance where the yellow fever is supposed to have been imported from a foreign port to one situated within the yellow fever region in this country. In this city, I have every reason to believe that it has ever been the case; for many cases occur which are never reported; and many are carried to their long home, said to have died of malignant fever, the physician not wishing to assume the responsibility of announcing the existence of yellow fever; or never having previously seen it, he may not recognize the disease. In the mean time, while this is going on, a vessel may arrive from Vera Cruz or Havana; and some unacclimated person being on board, either of the crew or passengers, may take the fever and die, naturally enough of yellow fever, produced as has been already suggested by the pestilential atmosphere on the bank of the river. Surely such facts as these will not sustain the doctrine of importation.

In 1797, we find the College of Physicians at Philadelphia calling for a *more* rigid quarantine. Up to this time, then, by their own confession, there had been no quarantine law in operation that proved of much service. In 1798, we read, that "the disease was produced by the same causes which excited it in former years." We now come to the year 1799, and let us see what happened after a *more rigid quarantine* had been established. In July, says Dr. Rush, the city was alarmed by Dr. Griffiths, with an account of several cases of yellow fever in Penn street, near the water. The strictness with which the quarantine laws had been executed for a while rendered this account incredible with many people, and exposed the doctor to a good deal of obloquy. At length a vessel was discovered that had arrived from the West Indies, on the 14th May, one day before the quarantine law went into operation, from which vessel the disease was said to be derived. However, upon investigating the state of this vessel, it appeared that she had arrived with a healthy crew, and that no person had been sick on board during the voyage. That same year 1799, the Academy of Medicine pronounced the disease to be of domestic origin, and the College of Physicians asserted it to be imported, because it had been imported in former years; a kind of reasoning which amounts to nothing at all. Of sporadic cases which appeared in 1800, Dr. Rush observes, "no one of the above malignant cases could be traced to a ship, or to a direct or indirect intercourse with persons affected with the disease."

While Philadelphia was thus visited with a few sporadic cases of yellow fever, it was epidemic in New York, Providence, Norfolk and Baltimore. In the last named place, it was publicly declared by a committee of health to be of domestic origin. In 1801, a number of cases of yellow fever appeared at New Bedford, Portland, Norwich, New York, and in some parts of New Jersey, and of Pennsylvania. In none of the above places, says Dr. Rush, could the *least* proof be adduced of the disease being imported. In 1802, the fever in Philadelphia was referred to a vessel called the St. Domingo packet, but faithful and accurate inquiries proved that this vessel had been detained twenty-one days at the Lazaretto, and that not one of fourteen men who worked on board of her afterwards had been affected with sickness of any kind. The same year the yellow fever was epidemic at Baltimore and Wilmington. In the former place, it was admitted by their board of health to be of domestic origin, and in the latter place, the same fact was proved by Dr. Vaughan. In 1803, the yellow fever was epidemic in New York, Philadelphia and Alexandria. In the latter place, Dr. Dick informed the public that it was derived from domestic putrefaction.

Speaking of the yellow fever of 1805, Dr. Rush remarks, an attempt was made to impose a belief that two cases of fever, traced to local causes, had been taken by contagion from a ship at the Lazaretto which had lately arrived from the West Indies, but a careful investigation of this tale proved that neither of the two subjects of the fever had been on board that, or any other ship then under quarantine. I shall conclude this valuable testimony of Dr. Rush, by transcribing a letter received from the health officer at New York, dated 7th of Sept., 1805:

"I most sincerely deplore the unfortunate state of our city. What do people now say of the origin of the disease? You may state that not a single vessel on board of which a person has been sick with fever, or on board of which any person has died of any disease while in the West Indies, or on the voyage home, has ever gone up to the city. There is not a shadow of proof, or of suspicion that can attach to the health officer, or to infected vessels this season. J. B. RODGERS."

Here, again, it appears, that the most rigid quarantine was of no avail, and when Dr. Rodgers asks, "what do people now say of the origin of disease?" it expresses clearly his conviction,—1st that it was not imported, and was therefore of domestic origin; and 2ly, if imported, that the most rigid quarantine law availed nothing. I have now fairly and carefully traced Dr. Rush's opinions of yellow fever from 1793 to 1805, and find no variableness, or shadow of turning. From beginning to end, he assigns the disease to domestic causes and contends that it was never contagious. I think Dr. Rush was wrong in calling the disease a bilious yellow fever. There is no connection between the two diseases; they are essentially distinct. But as a patient observer of facts, with his strict adherence to truth, and his acknowledged abilities—his deliberate opinion on this subject after an experience of thirteen years, is certainly entitled to great weight. There is perhaps no better evidence on record.

Some time during the war excited by the French Revolution, a British Frigate was despatched from England to re-enforce the fleet in the West Indies. Before she had entered any port, the yellow fever broke out, rapidly reducing her crew. The Captain put into Antigua. A medical board held a consulation. They directed her to be well fumigated and painted. She went to sea, and a second time the fever appeared; again was she compelled to return to Antigua. She was then unloaded; every thing to her very ballast, was taken out of her; she was again fumigated and painted, reloaded and sent to sea. In a short time the yellow fever appeared for the third time in this fated frigate. Necessity again drove her into port. The medical board could give no further advice. A physician who resided there, of the name of Musgrave, told the Governor that as the medical board confessed that they could do no more, he would, if permitted, point out the cause of the fever. His proposition was assented to. He had every thing taken out of the frigate, and then asked the Governor if there were any persons condemned to death in the island. There were—these persons were ordered by the doctor to go into the hold of the ship and take off the inner planking next the kelson; at the same time he cautioned all other persons not to expose themselves by looking down the gangway. Three officers disregarded his advice. The condemned felons descended to perform this work; they ripped off the inner planking, but not one came alive out of the hold. The three officers who looked down the gangway and exposed themselves to the pestilential air, soon took the yellow fever and died. After this, not another case occurred on board during the time she was cruising in the West Indies. This fever could not have been imported, for it broke out at sea; but the result of Dr. Musgrave's experiment is conclusive proof that it was of domestic local origin.

I have already spoken of the yellow fever region. On this continent it extends at present to the 30th degree of north latitude; although it may occasionally pass beyond this limit. But the northern limit is receding, and not advancing; and this has been the case for a quarter of a century. Within this region, the intensity and type of the fever must depend on local circumstances. I have enquired of many persons from various countries who have resided in Havana, if the yellow fever there varies much in its symptoms. All have replied, not generally, but there are some exceptions. These exceptions, after strict enquiry, were, I am satisfied, only a difference in intensity owing to peculiarity of temperament and habits of life. But in this city, the type is sometimes inflammatory, sometimes congestive, and sometimes neuralgic. The fever, therefore, presents three distinct types, each one of which may vary in intensity in different cases.

If the disease were imported, it would only vary in intensity from the disease existing at the place from which it might be brought; but as it varies here not only in intensity but in types without a corresponding change of type at Havana and Vera Cruz, the inference is, that our yellow fever is of domestic origin, governed by local circumstances. And how much stronger does this argument become, if we admit that the yellow fever is, as some contend, a specific disease, and contagious.

Another strong proof of the domestic origin of yellow fever, is the exemption of creoles from that disease. This is a general rule—though I have heard of some exceptions. The belief, however, that the creoles enjoy an immunity from their being born here, is universal with all the elderly French creoles, and in fact with all the old inhabitants with whom I have conversed, and I am induced to believe that the cases of yellow fever, said to have occurred amongst creoles born here, were some other form of fever; but at any rate they cannot overthrow the general rule. Now if the fever is imported, why should they be more exempt from the attacks of yellow fever than citizens from adjoining parishes, and adjoining States? As bilious, and certain malignant diseases appeared in Philadelphia early in the sickly season prior to the developement of the yellow fever, so dysentery, diarrhœa and congestive fever appear in this city prior to a visitation of yellow fever. No body supposes the first mentioned diseases to be imported; and why should one malignant disease be supposed to be imported more than any other malignant disease, when it is apparent that both arise from the same causes slightly modified? The congestive fever, called by the creoles "*peste froid*" or cold plague, is a more malignant, formidable and fatal disease than the yellow fever, and if the one can be engendred in the country, why not the other?

If the yellow fever is imported, then according to a generally received rule, all other diseases should give way before it; and if quarantine laws could prevent its reaching us, then in all probability would other diseases, equally, if not more fatal, prevail; for like causes produce the same effects everywhere under the same circumstances; and here we are on an alluvial soil, with a dense population, of which a large portion is unacclimated; and surrounded by swamps; we have a most variable climate; combined heat and moisture; and every other element calculated to produce disease, which we see producing disease under the same circumstances in all parts of our Southern country, and in all parts of the world.

I have stated that diseases are developed in the progress of civilization, yet the more matured operations of civilization may control and modify diseases, and confine them within closer limits. The yellow fever that about half a century ago, prevailed almost every year in the Atlantic cities, even as far North and East as Providence, has now evidently receded; and New-Orleans once almost in the centre of the yellow fever region, is now just on the Northern limit. This great change has been brought about, not by quarantine laws, but by the energy of mind directed to internal improvements, and by judicious police regulations. I shall have occasion to allude to this subject again. What did quarantine laws accomplish; what could the concentrated power of a Russian Emperor effect in arresting the progress of that malignant and fatal disease called the Asiatic cholera? Many persons contended that it was a contagious and imported disease, and that its stern and silent march could be arrested by human legislation and effort. The valley of the Ganges, as has been before remarked, is the centre of the region of cholera. In Hindostan, however, it has set all laws and systems at defiance. It has driven the British troops from the plains to the mountains; and again another year from the mountains to the plains. In 1802 or 3, it left the valley of the Ganges, and carried death and destruction from the Himalayan mountains along the mountain ridges to the Southern extremity of Hindostan. Dr. M'Arthur informs us that there were official returns of more than one hundred thousand deaths that year, and the true number must have greatly exceeded this, from the difficulty that always exists in that country of procuring correct returns. In 1817, it traversed Hindostan, Cabool, Affghanistan, Persia, and reached the Russian frontier posts in Asia. It then retired to the valley of the Ganges, and remained there thirteen years, when it started anew, traversed Asia, and while the Russian troops were guarding every road and pass on the European frontier, forming a complete '*cordon militaire*,' the cholera broke out in Moscow, six hundred miles in their rear. It reached the latitude of sixty degrees North, swept rapidly over the continent of Europe, crossed the channel to Great Britain, and whilst we were felicitating ourselves that an ocean rolled between us and that dire pestilence, it appeared in Canada. The people of the Northern States were much alarmed; they adopted every measure that human ingenuity could suggest to prevent its ingress into the United States, but it passed at once from Quebec to New York, and finally reached this city. Was this an imported or contagious disease? Did any quarantine law or military power, or human effort avail to arrest its onward progress for one moment? It came,—passed on in its career of destruction, and disappeared like a hideous dream, exhibiting throughout, the utter impotence of human effort and human skill to arrest its march of death. Having spoken of what may be accomplished by judicious police regulations, your attention is called to what occurred in Philadelphia, when the cholera was in the United States. Generally it excited the utmost terror and alarm, and people were flying in every direction to escape its icy grasp. But the enlightened physicians of Philadelphia did every thing they could to calm the fears of the public. They assured their fellow citizens that the disease was not contagious; that fear of it created its greatest danger; that it might be greatly mitigated, by the most strict observance of cleanliness; by maintaining a calm state of the mind, banishing every thing like fear, and by judicious police regulations. They succeeded in all their noble views, and the consequence was that Philadelphia suffered less from the cholera, than any other city or place in proportion to its population. A more successful effort to arrest or greatly mitigate disease, was never more apparent than in this instance. It was an intellectual and moral triumph.

Dr. Lind informs us that many persons escaped the yellow fever in Pensacola in 1765, by retiring to the ships which lay in the harbor, (p. 169, diseases of warm climates.) If the yellow fever was imported into Pensacola by the ships which were lying in the harbor, how could the inhabitants find safety on board these ships? And yet this very case is cited in proof of the importation of the fever, and the doctrine of contagion. But Lind says, (pp. 36 and 124, diseases, &c.) They have very limited ideas of the history of yellow fever who suppose it to be peculiar to the

East or West Indies. It was admitted to have been generated in Cadiz after a hot and dry summer in 1764, and in Pensacola in 1765; and at page 123, the same author says, this disease, the yellow fever, appears in all the Southern ports of Europe after hot and dry weather. The opinions of Dr. Lind are deservedly considered of very high authority. Perhaps no physician has seen more of the yellow fever, or has enjoyed the same advantage of noticing that disease in all parts of the world where it prevails. He was for many years at the head of the medical department of the British Navy, and was, therefore, no doubt conversant with the opinions of all the surgeons and physicians in the fleets, on this subject.

The same author in the same work, states that two British ships of war were cruising on the Eastern shore of Africa. Being in want of wood and water, they repaired to a favorable spot for anchorage in the island of Madagascar. The captain of one ship, by the advice of the surgeon, sent the men detailed for that special duty on shore after sun rise, with orders to return before sun set. A large portion of them were attacked with highly inflammatory fever, which yielded promptly to bleeding and general antiplagistic remedies. The captain of the other vessel, also by the advice of his surgeon, sent the boats ashore after sun set, with orders to return before sun rise. In this ship, malignant yellow fever broke out, and was very fatal; yet although in a ship of war of fifty or sixty guns, probably crowded with stores; where under the deck the air cannot be pure, and the weather intensely warm, the fever was not communicated to a single person in the ship who had not been on shore. Dr. Johnson, in his work on the diseases of tropical climates, states that a British squadron was blockading Batavia, in the Island of Java. The vessels were lying off a small island, both high and dry, about two miles from shore. On the arrival of the sickly season, the admiral, fearful that the pestilential air from the land might reach his squadron and affect his crews, moved, contrary to the advice of every medical man on board the ships, to a low, swampy island, about six miles from the shore. It proved to be a grave yard. We now here read of a more malignant form of yellow fever. Scarcely one who went on shore even for a short time, escaped the fever, and all died, who slept one night on the island; but not a case was communicated in any one of the ships by the sick and dying, to those who had not been on the fatal island.

After such cases as the two foregoing, how can it be believed that the yellow fever is a specific and contagious disease? We have seen it of local origin in the island of Madagascar, where the cause only operated at night; and in the vicinity of the Island of Java, where it operated at all times; but in neither case was it communicated from person to person; exposure to the pestilential air was necessary to contract the disease. This establishes its character as a miasmetic endemic—a form of disease no more imported, or contagious, than congestive, bilious, and malignant intermitting fevers.

When my attention was first turned to this subject, more than twenty years ago, I was forcibly impressed with the fact, that the most strenuous advocates of contagion, and the importation of yellow fever, and quarantine laws, were physicians who, for the most part, had never seen a case of the disease, or whose experience was very limited; while on the other hand, the opinions of the medical men whose experience had been ample in tropical climates, was almost uniformly opposed to the doctrines of contagion and importation. And since that period, I have seen nothing to controvert the fact; on the contrary, it has been abundantly confirmed. In discussing the question of the importation of yellow fever, that of contagion had been occasionally introduced; it is, however, necessary to give that branch of the subject a full and distinct consideration.

During the summer of 1822, while pursuing my medical studies in the city of New York, I met with the following report in one of the reviews published in that city. It was the report of a French physician, who had been sent from Paris to visit all places where the yellow fever prevailed, to collect facts in order that the question of contagion or non-contagion might be decided. The yellow fever was raging in one of the French West India Islands, either Martinique or Guadaloupe. The hospitals were filled with French soldiers; the disease was extremely malignant. He introduced the blood of one of the worst subjects into his veins; at another time he inoculated himself with black vomit; he lay quite naked for four hours in bed between two soldiers who were dying of the fever; and his body was all the time wet with the clammy perspiration of death. Not satisfied with these experiments, he swallowed a wine glass full of black vomit. He did not contract the disease, though his system was considerably disturbed by such bold and singular experiments. After visiting a great many places, and collecting a vast amount of facts, and recording the opinions of practising physicians, he decided that there was not a shadow of proof of the contagiousness of yellow fever; and that the practising physicians universally disbelieved it.

When the yellow fever broke out in 1822, in the city of New York, I happened to be boarding within the infected district; and being a regular attendant at the hospital, I had an opportunity of seeing and hearing all that was going on. Whatever was the origin of that fever, which I shall not stop to discuss here, although I am persuaded that it was of local origin, it commenced in Rector street, a small street extending from Broadway alongside of Trinity Church-yard to the North River. Thence it gradually spread in different directions until it formed an infected district, bounded by a line extending from river to river, just at the commencement of the Park, where the Astor House now stands, and including within that line, all the city down to the Battery. Why it stopped there, no one could ever tell—but it is a fact favorable to the opinion of its domestic origin, and difficult to be accounted for on the principles of contagion and importation. All who had the disease, took it in the infected district. Not a single case could be found that originated out of that district, although every effort was made by Dr. Hosack and other physicians who believed it to be contagious, to find such a case. Dr. Townsend, who contended for the importation of the disease, notwithstanding that the vessel said to have brought it from Havana, had regularly performed quarantine at Staten Island, did not, however, believe it to be contagious. Amongst many cases cited to prove that it was not contagious, the doctor states that he attended a man who had taken the fever in the infected district; that this man had a large family; that they all occupied a cellar, which was crowded, badly ventilated, very dirty, and where, in short, every thing was combined to favor contagion; yet although the disease was very malignant, and the man died, no other occupant of the cellar took the fever.

Dr. Beck, (see appendix to Gregory, p. 653,) says: "During the prevalence of the fever, six persons lay sick with it at Greenwich, a village about a mile from the city. All of these had contracted the disease in the infected district: five of the six died there. In no instance did they communicate the disease to physicians, nurses, attendants, or friends. To give additional weight to this fact, it should be recollected that to this place had been transferred the seat of business; that in consequence of this, almost all the merchants of the city had retired to it. and that to accommodate them, hundreds of temporary buildings had been erected. From this influx of inhabitants, it may naturally be inferred that the village was crowded to excess; and from this circumstance, as well as the want of comfortable accommodations, every thing seemed favorable to the propagation of the disease. Still, although a number of very decided cases of the fever were carried to this place, lay sick, and died, it was not in a single instance communicated."

From the official returns of Dr. Bailey, the health officer of the port, it appears that seventy persons sick with the fever, were sent down to the Marine Hospital on Staten Island. Of this number, thirty-seven died, of whom eighteen had black vomit. These cases were regularly attended by the physicians and nurses of the establishment, not one of whom was affected by the disease; "nor has a single case, says the Dr., come to our knowledge of any person taking it who was engaged in transporting the sick from the city to the hospital."

Again, Dr. Beck says: "In addition to the cases already recorded, there were a large number of persons, who after having contracted the seeds of the disease in the city, had the disease developed in them after their removal into different and distant parts of the country. There occurred about thirty-six cases of this kind, and at the following places, viz: three at Newark, New Jersey; one at Harlaem; three in the city of Jersey; one at Tappan, New York; six at Bloomingdale; one at Albany; three at Middletown Point; four in different parts of New Jersey; one at Newtown, Long Island; one at New Canaan, Connecticut; two at Amboy, New Jersey; one at Hampstead, Long Island; one in Westchester; one at Bloomfield, New Jersey; one at Woodbridge, New Jersey; one at Saugatuck, Connecticut; two at Bushwick, Long Island; two at Elizabethtown, New Jersey; and one at Boston. Of this number, there were twenty-seven deaths, being three-fourths of the whole number, or seventy-five per cent., proving the disease to have been of a very decided and malignant character; and yet not in a single instance was the disease communicated. That one hunred persons sick of a disease highly malignant, and contagious, (as some contend,) located in different parts of the country, should not even in a solitary instance have communicated it to a second person, seems to us literally impossible; and we believe that were it not for the influence of pre-conceived opinions, and long cherished theories, facts of this sort would come home to the minds of men with a force perfectly irresistible." Dr. Beck continues: "There are some persons who have contended that yellow fever may be contagious in one kind of air, and not so in another. All the testimony adduced from the fever of 1822, is directly adverse to this position. If we suppose the cause of yellow fever to enter into chemical combination with the surrounding foul atmosphere, then it would no longer be the same disease. If, on the other hand, the air serves merely as a medium for transmitting the poison to a greater distance, then no reason can be assigned why, if you approach near enough to the sick body, contagion should not display itself as well in a pure, as in an impure atmosphere. It has been already shown, that not merely in the pure air of the country, but even in the most impure and unhealthy parts of our city, patients sick of the yellow fever in 1822, were uniformly approached with perfect impunity. The air, therefore, in the infected district must have been more venomous than the contagious poison itself coming off directly from diseased bodies—that is, poison diluted in atmospheric air, must have been more powerful than pure unmixed poison itself; a proposition absurd in itself.

We infer, then, as the air of the infected district was more deleterious than actual contact with the sick, the poison existing in the air must have been some other than effluvia from the bodies of the sick.

I shall not follow Dr. Beck any further, but will give Dr. Peinotto's opinion of his whole argument.

I have made free use of this admirable argument against the contagiousness of yellow fever, because it will serve as a model by which to analyze those that are supposed to be contagious. It recommends itself more strongly to the medical critic, because it is divested of all speculation. The question of the origin of yellow fever, is with logical propriety, left out altogether, the writer's aim having been simply to sift the question whether the epidemic of 1832 was, or was not contagious. To this object, so important every way to the interests of the community at large, and of mercantile cities in particular, the author restricts himself, and he has accomplished it in a manner which has forbidden all reply or contradiction."

Since the first time I saw a case of yellow fever in 1822, to the present time, I have not seen the least evidence to prove that yellow fever is contagious, but facts have been constantly accumulating to prove the reverse. In this city, my experience has been chiefly among the Irish and German emigrants. I have generally found them very much crowded, and particularly the Irish. In several instances where perhaps twenty or thirty persons occupied two or three small rooms, and all unacclimated, I have attended five or six with the yellow fever, while all the rest, although in constant contact with the sick, have escaped the disease. How could this happen under such circumstances so favorable to the propagation of the disease, if the fever were contagious? There may be two or three physicians in this city who believe that yellow fever is contagious; but in opposition to this opinion, the faculty may be said to be almost unanimous. Sporadic cases of yellow fever are utterly repugnant to the doctrine of contagion. Until within three years past, during which time the fever has been rather sporadic than epidemic, the yellow fever prevailed with great regularity as an epidemic every other year, but there were always sporadic cases in the intermediate years. If the disesase were contagious, why should there be sporadic cases one year and an epidemic the next? This would seem to imply that it might be contagious one year and not another; but this is absurd. The existence of sporadic cases is decisive of the question of contagion.

It has been stated that the quarantine laws have preserved Natchez from the yellow fever, as that disease has not appeared there since that system was adopted. If the disease is contagious, and can be carried by persons, and so transmitted to others, the quarantine laws, in this event, have been of no use whatever; for it is well known that every year persons from this city who wish to stop at Natchez, evade the quarantine laws by going a short distance above Natchez, and returning with the first boat that may be descending the river.

In this city the disease has been greatly modified during the last ten or fifeen years, and it is milder and more manageable than it formerly was; and with a rapidly increasing population, there are fewer persons in proportion attacked, and the mortality is diminished one half, if not more.—And this result does not arise from the absence of unacclimated persons in the summer season; there is always an abundance of such material for the fever to work upon. It is to be attributed to the improvements that have been made in the city; the paving of the streets and yards; the use of asphaltum; the erection of brick buildings in the place of old decayed wooden houses; the greater care that is taken to clean the streets and gutters, by letting the water run through the streets when the river is high; and removing all filth and offal. Much more might be done, and no doubt will in time be done, and we should promote the interests of the community far more by devoting our attention to this subject, than by establishing useless quarantine laws. For three years past, that is, 1842, 1843 and 1844, there has been scarsely any fever worth speaking of in this city. It is during this period that the quarantine laws have been established at Natchez, and therefore there has been no fair test of their utility. As steamboats are constantly ascending and descending the river during the summer season, as well as at other seasons of the year, whenever the yellow fever has prevailed, it should, if contagious, have been carried to all the towns on the river, and from them to all parts of the adjoining country. But yellow fever has prevailed in this city, while the congestive fever has existed at Natchez, Grand Gulf, Vicksburg, and other towns on the river. It is not pretended that yellow fever is more dangerous than the true congestive fever of the highest grade; in fact, yellow fever is the least dangerous and fatal of the two. And quarantine laws cannot affect the existence of that fever, for all admit that it is of local origin, and that it is not contagious. In fact, the doctrine of quarantine laws is losing ground everywhere, and perhaps in half a century, it will have become almost an obsolete idea. The Journal des Débats, published in Paris, while commenting on the existing quarantine laws of France, remarks as follows: "While England and America are conforming to the spirit of the age, and to the progress of human science, shall we proudly maintain our quarantine laws? The effect of this perseverance is, that we, who possess an admirable position on the three seas; we, whose soil appears the almost compulsory line of passage for so many nations—we find ourselves, in fact, further removed from Egypt and Constantinople than all the rest of Europe. We, who take so much care to avoid the plague, are avoided by travelers, as if we were pestiferous. Within the last few days our Ambassador at Constantinople, and our Chargé d'Affaires at Alexandria have been summoned to Paris, and patriotic as they are, they proceeded, one by the Austrian steamboat, via Trieste, and the other by the English boat, via Southampton. In consequence of the quarantine laws, the passage from Constantinople through Trieste, and from Alexandria via England, to Paris, gives the traveler a gain of seven days.

"The following is a curious contradiction; we have established a direct line of steamboats from Alexandria to Marseilles, in order to anticipate all other nations in our communications with Egypt, and by this mode we arrive at Marseilles from Alexandria in seven days, but we pass fifteen days in the Lazaretto on our arrival in France. Is that common sense? Our quarantine laws cause our Levant packet boats a loss of two millions. They deprive France of the transit of thousands of wealthy travelers returning from India to England, who would expend in France from five to six millions a year. It is estimated that quarantine laws enhance the price of primary material consumed by our manufacturers, such as cotton and wool, at least ten per cent.

"They cause an enormous prejudice to our commercial shipping interest; and they affect it not only in the Levant, but every where else. A few weeks since a ship laden with cotton arrived at Havre with about twenty passengers. She had touched at the West Indies, and had a case of yellow fever during the passage. The board of health at Havre prescribed twenty-five days' quarantine. The owner, who was present, ordered the captain to set out instantly for Southampton. There the passengers met with no difficulty, and embarked in the steamer for Havre, where they were without hesitation permitted to land. But as the cotton could not be disposed of advantageously at Southampton, the ship returned to Havre. The board of health however, recognised her, and insisted on the twenty-five days' quarantine, although the passengers and their baggage, which might have conveyed the yellow fever, as well as the ship itself, had arrived at Havre several days, previously."

This article from the *Journal des Débats*, gives us a fair illustration of the immense loss, the gross absurdities, and great inconveniencies which result from quarantine laws.

I have now endeavored to prove:

1st. That the yellow fever, like the other malignant diseases of the south, is of local origin.

2nd. That it is not an imported disease.

3rd. That it is not contagious.

4th. That civilization has developed diseases, which a higher grade of civilization, aided by changes of climate, may modify or abolish.

5th. That there is a yellow fever region, in any part of which, the fever may at any time originate.

6th. That on this continent, the yellow fever region has receded greatly.

7th. That the yellow fever has been abating in New Orleans in a ratio with the improvements going on in the city.

8th. That there is no occasion for quarantine laws. That experience has shewn them to be useless here; while they would be very expensive, highly injurious to our commercial interests, and onerous to passengers.

If we consider the history of yellow fever for one hundred and fifty years, we shall find that it prevailed once all along our Atlantic coast as far north as Portland in Maine; also, in the Southern parts of Europe; that its limits are now *twelve* degrees farther south; that this has been brought about by changes of climate, over which, man has no control; and also, by police regulations and the general improvements which have kept pace with, and resulted from the progress of civilization; and that this has been eminently the case in the city of New Orleans. This should encourage us to persevere in our efforts. Commissaries should be authorized and required to look into back yards, and have every thing offensive removed. Acts of the council should be passed to have the surface of the earth every where coated with something, to suppress the exhalations from the alluvial soil: such as round stones, or paving stones, or shells and sand, or bricks, or asphaltum. All low wet empty lots should be filled up. Offal deposited in the streets, and all filth should be promptly removed, and instead of being used to fill up lots, or thrown in the rear of the city, it should be thrown into the current of the river, where it would be carried off. Water should always be permitted to run through the streets when the river is high, and when it is too low, the water works, or additional works for that purpose, should be brought into play. The wharves should be particularly attended to, and as the river falls, persons should be prevented from throwing animal or vegetable matter underneath them, and all such matters so soon as discovered should be immediately removed. Carrying into effect these measures would scarcely cost a tithe of the expense of a quarantine establishment properly conducted. And how vastly superior, and conformable to the spirit of the age are such measures, to the dubious experiment of those laws.

They might lull us into fatal security, and divert our attention from the other more important considerations; in which event, even if yellow fever were prevented from entering the city, other diseases equally fatal, probably would prevail. But let the measures suggested be adopted, and pushed forward by enlightened and energetic action, and there is little reason to doubt that in the course of a few years New Orleans would be exempt from the yellow fever. It would be no longer within the region of that pestilence; and at the same time, the very measures that would bring about this great result, would also protect us from the other malignant fevers of the summer and fall, which are peculiar to our climate and position. There would then be no obstacle to the progress of this city. In less than half a century, she would so far have accomplished her high destiny, as to be the first city in the western hemisphere in wealth, population, commerce and general prosperity.

EXTRACT from the Proceedings of the Physico-Medical Society of New-Orleans.

At a regular meeting of the Physico-Medical Society of New-Orleans, held Saturday evening, 15th February, 1845, the article read by Dr. Hort, on the subject of Quarantine Laws, was on motion of Dr. Farrell, referred to a committee of five, to report at a subsequent meeting. The President appointed on said committee Drs. Farrell, Hort, Jones, Anson and Dowler.

At the regular meeting of the same Society, Saturday evening, May 10th, the following report was submitted by the committee and unanimously adopted:

The committee appointed to report on the expediency of Quarantine laws as a means of preventing the importation of yellow fever into this city, beg leave to state, that in considering this subject, they have felt the responsibility which appertains to questions affecting the health and prosperity of the city, and the lives of the inhabitants.

That while they admire and appreciate the ability with which several eminent Medical men have advocated the contagiousness of yellow fever, its importation from the Eastern, into the Western hemisphere, and the consequent necessity of the establishment of Quarantine laws, they nevertheless consider that the weight of testimony and of facts is immeasurably on the other side of the question; and which opinion is further confirmed by their own experience and observation.

That they can see no reason why the same local and general causes, under the same circumstances, or very nearly so, should not produce similar results in the production of malignant fevers, in both hemispheres of the world.

That where sufficient causes exist to engender disease in one place, it is useless to speculate on the question of its importation from some other place.

That in reviewing the history of the yellow fever for one hundred and fifty years past, the committee have come to the conclusion that it was developed, as were many other malignant diseases, before unknown, by the march of civilization urged forward by commercial enterprize.

That in this way, in the course of time, yellow fever became developed in both hemispheres, confined within nearly the same parallels of latitude, and forming distinct yellow fever regions, in addition to the regions of cholera and plague.

That in the gradual progress of civilization, measures have been adopted, and changes of climate have taken place, which have greatly diminished the yellow fever region in this hemisphere; and that its northern limit is now twelve degrees south of what it was a hundred years ago, in the time of Lind.

That this great result has been accomplished, not by quarantine laws, but by other judicious police regulations, together with great changes in the local features of countries; and those atmospherical changes, over which man has no control.

That quarantine laws, even should their existence be deemed necessary, are inadequate to the protection of a seaport of easy access; as Dr. Rush says, that a *still more rigid* quarantine called for in 1797, in Philadelphia, failed to accomplish the purpose desired. In 1805, the same fact is affirmed by Dr. Rogers, health officer at New York. In 1822, if imported, the system again failed at New York, (and in this city, it signally failed in 1820 or '21, when a rigid quarantine was established at the English turn.)

The committee are therefore of opinion, that quarantine laws are unnecessary and inexpedient for the protection of the city.

That even if they did prevent the importation of yellow fever, (admitting for one moment for argument sake, that the disease might be imported,) they could not at any rate prevent the existence of diseases equally fatal; such as the congestive fever, and the malignant types of intermitting and remitting fevers.

That facts seem clearly to prove, that the yellow fever has decreased in malignity, in a ration with the improvements of the city—as the draining of the land in the rear of the city; the paving of the streets; the filling up of empty lots; the use of asphaltum; permitting the river water to run through the streets, when the river is high; and the removal of filth and offal from the streets.

That instead of quarantine laws, the measures last alluded to, should be steadily persevered in, and carried by an enlightened policy, to a still greater extent; which would not only have a tendency to avert yellow fever, but all other malignant diseases, peculiar to our climate and position, at a particular season of the year.

The committee, in conclusion, sum up this report by declaring:

That they believe the yellow fever to be a disease of local or domestic origin, and that it is not an imported disease.

That it is never contagious.

That it may be made to yield to judicious police regulations.

That quarantine laws are very expensive to the community, and that they are not only unnecessary and inexpedient, but worse than useless. They therefore recommend;

1. That the commissaries in each ward, be required to look into back yards and lots; and be authorized to cause every thing offensive to be promptly removed.

2. That the different Councils of the city should exert themselves to the utmost in their official capacity, to have the surface of the earth covered over with something, to prevent the exhalations from the alluvial soil, on which the city is built; either round or paving stones, or bricks, or shells and sand, or asphaltum.

3. That the owners should be compelled by law to fill up all low swampy lots within the limits of the city.

4. That all offal deposited in the streets should be promptly removed; and if possible, before the heat of the day.

5. That whenever the river is high, the water should be allowed to run through the streets day and night; and that when it is too low, the water works, or if nenessary, additional works established for the purpose, should be brought into play.

6. That above all, particular attention should be paid by the city authorities, to the alluvial bank, particularly under the wharves of the Second Municipality, which is annually uncovered as the river falls, exposing an immense surface of fresh deposit, covered with every kind of decaying vegetable and animal matter, which daily accumulates, either carried there by eddy currents of the river, or thrown in by the inhabitants.

The committee deem this last consideration to be of the highest importance, as there is every reason to believe, that the bank of the river under the wharves, is more productive of disease in the summer, than all other causes in the city, combined.

7. That instead of depositing the filth and offal collected in the streets by th

scavengers, in empty lots or in the rear of the city, it is recommended to the city authorities to have all such filth and offal thrown into the current of the river.

They would also observe, that the measures just recommended, would not be attended with one-fourth of the expense of a quarantine establishment properly conducted; while should they be pushed forward with zeal and energy, the time might, and no doubt would, ere long, arrive, when New-Orleans would no longer be within the yellow fever region; and consequently exempt, not only from that pestilence, but from all the other fatal diseases of the summer and fall, peculiar to our climate and to our position. This accomplished—what would there remain to retard the growth and prosperity of our city? She would speedily accomplish her high destiny, and in less than a quarter of a century, become the most wealthy, prosperous, and populous cities in the Western Hemisphere.

J. FARRELL, Chairman.

On motion of Dr. Graham, Drs. Farrell and Hort were appointed a committee to have it printed.

It was further resolved, that a copy of the same be presented to the Councils of the city.

THOS. HUNT, M. D., Pres.

W. G. G. WILLSON, M. D., Sec'y.

ART. II—*A few remarks on Typhus Fever, commonly called Winter Fever. By* EDWARD MONTGOMERY, M. D., *of Middleton, Carrol County, Mississippi.*

Typhus, or Typhous Fever, is a disease which has existed in the world and occupied more or less of the attention of medical men from the days of Æsculapius to the present time; yet its ancient origin and almost universal prevalence have not procured for it a truly scientific mode of treatment; neither is its essential nature as yet properly delineated or understood, in a pathological and physiological sense, by a great majority of physicians of the present day. Typhus fever is acknowledged by all to be a very dangerous and distressing malady; it therefore is strictly incumbent on every medical practitioner to study its nature and causes, and diligently to observe every discovery in hygeine or therapeutics calculated for its prevention or cure. This is my apology for obtruding my name in a medical periodical, amongst the more gifted sons of Hippocrates; and if, by contributing my humble mite, I shall succeed in arousing the attention of a few of the medical fraternity to a careful investigation of this fever, I shall feel amply repaid for my slight labor, and the pages of your excellent journal shall not be occupied altogether in vain. My attention has been directed to this disease, at this time, from a case which I was called on to examine about three weeks ago; I believe it was a very well marked case of typhus fever, and was one of great severity, although the patient convalesced. I have attended several patients the past fall with typhus, and last summer I saw at least twenty cases of this continued fever, some of which, the physicians here would designate the typhoid fever. This latter, I believe to be identical with typhus, only milder in degree, modified or controlled by the age, habits or particular idiosyncracy of the patient. Indeed, the first few cases of typhus which I saw, in Mississippi, during the summer of 1843, I was rather doubtful as to the nature of the disease; being so much influenced by the warmer climate, and by the mode of life of the subject, the disease was rather masked, but on a careful analysis of the manner of accession, the symptoms, progress, duration, termination, &c., I soon recognised it to be identical with the typhus fever which I had so often sedulously observed in Great Britain and Ireland. I was fully convinced of this fact last summer, when I witnessed the disease in a more malignant type, entering a neighborhood and spreading, apparently by contagion, to the several neighbors, visitors, members of the family, and nurses. Indeed, I am of opinion, that all the cases which are here named "*winter fever*," are of the typhus or typhoid variety. I never have yet seen a case of synocha (the continued fever of Europe, from συνεχω, to continue) in Mississippi, but the typhus, I believe, to be a very common disease here; yet, I am sorry to say, it is frequently treated empirically, or as a different disease. Last summer and this winter it prevailed in this part of the State to a greater extent than common, which I think was probably owing to the same causes which produced so many cases of bilious, malignant, intermittent and remittent fevers, namely, the very rainy spring and unusually hot summer. There has been and still is a great diversity of opinion concerning typhus and typhoid fever; some physicians, and those of very high standing in the medical world, think that these diseases are entirely different: amongst whom are Chomel, Andral, Louis, Lombard, Gerhard, &c. On the other hand, a great many high authorities contend for the identity of these two diseases; on this side of the question we have Graves, Christison, Allison, Hall, Connolly, Cusack, &c. My humble opinion is with the latter named gentlemen; and I think no close observer, who has had much experience of the typhus fever as it occurs in the British Isles, can fail to recognise the typhoid of Chomel, Louis, &c., as the same disease; and I am equally as certain that the typhoid, nervous and winter fevers of Mississippi, are identical with the typhus of England, Scotland and Ireland; but as I said before, influenced by the difference in climate, habits, mode of life, regimen, &c., of the patients. I can easily understand how the resolute Irishmen, and the hardy Scot, inured to toil and difficulties, can bear up under the morbific virus for five or six weeks, and exhibit the high burning fever, the wild delirium and convulsions, and the well developed *petechiæ*, without any appreciable organic disease of the brain, mucous membrane or glands of the intestines; whilst the very same kind of fever will produce the small "rose spots" of the skin, slight muttering delirium, stupor and *dothinenteritis*, with organic disease of the glands of Brunner and Peyer, when it attacks the efféminate and lax-fibred Frenchman, or the sun-burnt inhabitant of the balmy South. I have seen numbers of cases of the true epidemic typhus where the petechiæ were absent; others with ulceration of the bowels; and hundreds with all the symptoms, considered by Chomel and others as pathognomic of typhoid fever; indeed, in Ireland, by way of indicating the degree or intensity of the fever, we say the patient is laboring under "*typhus gravior*" or "*mitior*," or the "typhus fever," or "nervous fever," as the case may be: meaning the same disease by all these four terms, only using them to explain the mildness or severity of the attack, in the same sense as we use the terms confluent, modified and district, when speaking of small pox. In epidemics of typhus fever, we will often see a patient violently affected with hot burning fever, dry, black furred tongue, violent delirium, &c., and his nurse or attendant will be seized with it and exhibit no delirium, a moist tongue, moist skin, in short show quite a different aspect, yet the disease will progress in the same manner as the former one, and if *post mortem* examinations were instituted in both, there could, most likely, be no pathological signs found in one, but would exist also in the other. Again, it is acknowledged by Andral, Chomel and Louis, that *dothinenteritis is not always present* in typhoid fever, and the last named great pathologist and physician now avows his belief in the contagiousness of typhoid fever, which was the great argument on the side of the opposite school. It is not to be wondered at, that French physiologists should contend for the non-infectious nature of typhoid fever, when a vast majority of them deny that typhus fever is contagious. I think it is true that the cases of typhus, characterized by a weak, quick compressible pulse, fever not very high, little thirst, slight stupor, prostration, &c., are not so contagious as the ones marked by more violent symptoms. The former, is the *adynamic* fever of Broussais; it usually lasts five or six weeks in cold countries, or three or four weeks in tropical climates. The severe form of typhus generally holds the patient three or four weeks in cold regions, and from eight to fifteen days in higher latitudes. That typhus fever is contagious, is well substantiated from the fact that nearly every nurse and physician attending on typhus patients will be certain to take the disease. Some persons, of course, are more obnoxious to it than others; the indefatigable pathologist and scientific physician, Dr. Christison, has had the disease eight times, and I believe in every instance he was affected whilst attending the fever wards in the Edinburgh Infirmary. Children are not susceptible to the contagion of typhus; but all temperaments from eight or ten years to old age, are liable to take the disease; the ages from fifteen to twenty-five years being most obnoxious to it. One attack does not cause immunity from the disease; we are liable to take it again and again; but after one attack the person is not so susceptible for two years, and it is rare for a person to have the disease twice within twelve months. Independent of contagion, the disease may be propagated by filthy, small, crowded, or ill-ventilated dwellings: the noxious effluvia arising from dirty narrow streets, jails and camps, are also great causes of typhus fever. It is also frequently caused by poor, unwholesome diet, and meagre clothing amongst the peasantry of the country, which is otherwise healthy and salubrious. Malaria and marsh miasmata are also said to be causes of typhus fever. Confinement, close study, and mental affliction are also reckoned amongst the causes of this distressing disease. When typhus fever can be propagated by so many different causes, it is reasonable to suppose that it is not so very often induced by contagion; indeed it is a consoling fact to the friends and visiters of the sick, that if they do not remain too long, or in too close contact with the patient, they will not be apt to be infected by the disease. Typhus prevails to the greatest extent in summer; epidemics of it take place at that season, the cases in winter being for the most part sporadic; but no season of the year is particularly exempt from it. Any thing like an epidemic typhus, which I have seen in Mississippi, has prevailed in the months of July and August, as in the last summer; the disease affected all ages from five or sixty years, all colors and constitutions were subject to it in a certain neighborhood, where it seized upon every family in the "creek bottom," and proved fatal in seven or eight instances. In describing the *symptoms* of typhus fever, I shall have recourse to my note case, and shall mark down none but what are really taken from nature—observed at the bedside of the patient; and I may here remark that there are a great many signs noted down which are common to other diseases; and that the symptoms vary in different individuals, but there are in *all cases* of typhus fever certain diagnostic marks, which unerringly point out the disease; of these essential symptoms I may mention the quick bounding pulse, for the first few days, and then the small, quick, and weak pulse, the stupor (from whence is the origin of the name τυφος, *stupor*) low muttering or violent delirium, peculiar smell of the patient, and stools always very fœtid, fuliginosity of mouth, sordes on the teeth, picking of bed clothes, *subsultus tendinum*, &c. The duration of the disease, its mode of termination, the regular succession of the peculiar symptoms, &c., also tend to render the diagnosis of this disease sufficiently plain to a practitioner of moderate experience and intelligence.

Typhus fever is most frequently ushered in by many premonitory symptoms, as morning sickness, alternate rigors and flushes of heat, apathy and listlessness, general muscular soreness and headache, the hair of the head appearing to the patient as if standing rigid on end, and so painful that it cannot be combed without the subject suffering; anorexia, paleness, troublesome dreams, &c. At other times the disease sets in rather suddenly; with rigors and chilliness, although the skin is hot, violent headache and excruciating pain of the back and loins, great nausea, paleness and anxiety of countenance, restlessness, very little sleep, and what there is, disturbed with frightful dreams, frequent sighing, eyes red and suffused, countenance pale and haggard, pulse at the accession, quick, bounding, full and compressible, for perhaps three days, when it becomes small, quick, weak and often intermittent. Skin generally hot and dry throughout the disease, although a little cooler in the morning when all the morbid chain is somewhat modified; but there are numerous cases where the skin is moist throughout the disease and these cases are generally very tedious in their progress. Tongue mostly dry, chapped and sore, and coated with a dark brown fur, sometimes it is moist and tolerably clean throughout, indeed it is curious how little dependence we can have on the appearance of the tongue in regard to our prognosis and diagnosis of this disease, for we often find it moist, soft and rather clean, and at the same time an extensive and alarming amount of irritation and disease going on in the intestinal canal; again, we may see the tongue dry, deeply furred, deeply indented and bleeding, so sore that the patient is unable to protrude it, or he puts it out to a small extent, spear-pointed and tremulous, and yet there may be no appreciable structural disease in the stomach, intestines, liver, lungs, brain or spinal marrow. There is frequently great thirst, but disinclination to drink much, from the sense of constriction, soreness and nausea of the stomach. There is early prostration, irritability of temper, and very fœtid stools; sometimes constipation, and sometimes diarrhœa. As the disease advances, say from the third to the fifteenth day, the pulse usually becomes quick, hard, weak and often intermittent; the nervous symptoms become more strongly marked, great stupor and indifference to every thing around, heavy dull aching sensation in the head, glassy appearance of the eyes, petechiæ, &c. From the seventh to the fifteenth day of the disease, in this climate, the symptoms will be at their maximum severity; in colder latitudes, as that of Ireland or Scotland, the disease appears to be at its height from the fifteenth to the twenty-first day from its accession. From these periods to the end of the disease, there is usually great prostration, low muttering or violent delirium, pulse intermittent, which may be chiefly owing to the softening of the left ventricle of the heart, sordes on the teeth, epistaxis, bloody stools, continual movement of the extremities, picking the nose, ears or bed clothes, coma and convulsions, *subsultus tendinum*, fœces and urine discharged unconsciously and involuntarily, the patient lies on his back and slides down on the bed, the extremities become cold, there is hiccough and sinking of the under jaw, the eyes are half shut and fixed, the breathing is laborious, irregular and sonorous, the patient appears bloodless and cadaverous, and death soon closes the awful and humiliating scene.

Should the disease be complicated at the accession, or during its progress, with bronchitis, pleurisy or pneumonia (which is very frequently the case) there will be dry, hacking cough, dyspnœa, sense of tightness, pain or constriction of the chest, the inspirations will be short, the cheeks polished and flushed, inability to breathe easy in certain positions, &c., together with the physical signs made manifest by the aid of the stethescope, an instrument which should never be dispensed with in the examinations of patients with typhus fever. The pathology of this disease is not so striking, or rather the *post mortem* appearances do not exhibit an amount of organic disease commensurate with the violence of the symptoms during life; thus we can generally discover no structural lesion in any of the parts of the body, except perhaps an enlarged spleen, softening of the left side of the heart, slight serous effusion and partial sanguineous injection of the membranes and substance of the brain; hyperæmia, congestion or ulceration of the mucous membrane of the intestines, and the glands of Brunner and Peyer. The liver is sometimes enlarged, soft and friable, the mesenterec glands swollen or enlarged, and if the respiratory organs have been implicated in the disease, we will see inflammation, congestion, adhesions, effusions, and suppuration in the particular parts affected in the thorax. It is a fact well worthy of observation and untiring research, that in a disease where the mental manifestations are so limited, abnormal, or so completely perverted, that pathologists are unable to discover lesions on the brain and nervous system, sufficient to explain and account for such wonderful mental phenomena which obtained during life. Indeed pathology throws very little light on the real nature of this malady, either in a physiological or a therupentical sense. Neither the *error loci* and spasm of the extreme vessels, Cullen's theory;—nor the irritation and *gastro enteritis* of *Broussais*, nor the doctrine of the humoral pathologists, can explain the cause of the very peculiar symptoms and manifestations of idiopathic fever. So it is with those cases of typhus (and they are numerous) in which we cannot discover any structural lesion sufficient to account for the violence and severity of the disease. It is true, in all cases, the blood is altered in its physical and chemical constitution; it is black colored, does not coagulate firmly, and is deficient in the salts and coloring matter. It is probable, then, that this abnormal state of the vital fluid, and the very certain *functional* disease of the nervous system, may be the only morbific agents which keep up such a series of distressing symptoms in those cases of typhus where necroscopic examination showed no organic disease. Indeed, I have no doubt but the diseased state of the blood, and that peculiar implication of the nervous system, functional derangement, are the principal pathological conditions which give character and force to the morbid chain of distressing symptoms which exist in typhus fever; the structural lesions which are observed as gastro-enteritis, enlagement of liver and spleen, softening of heart, intestinal ulceration, &c., being secondary, coming on after the disease had existed for some time in the system. In many cases, there is that peculiar state of the system termed fever existing for many days without any symptom or sign of local disease; but in typhus as in other febrile diseases, structural lesions may be produced during the progress of the malady. Some think that idiopathic fever is *electric*, and therefore independent of any organic disease whatever. There is no doubt, in my humble opinion, but the altered state of the blood, and that peculiar *functional derangement of the nervous system* are what constitute typhus fever, and whatever local lesion appears is the effect of that abnormal state of the system. The *prognosis* of this disease is rather uncertain and difficult; the corpulent and those in the prime and vigor of life, are more apt to succumb to the disease, than the spare and slender patient. The athlectic and plethoric are very apt to take on local congestions and hemorrhages during the progress of the disease. When the tongue is very dark colored, dry, deep brown, fur along the centre, and red on the edges, the prognosis is unfavorable. Again, the tongue may be moist and very slightly furred, and the patient give very little expression of suffering, and yet the case may be a very dangerous one. I have witnessed many such cases which proved fatal, contrary to the expectations of the physician and friends. Last summer nearly all the fatal cases were of this class; the tongue would be soft and moist, not furred, and but slightly coated, pulse full, voluminous and bounding until near the end of the disease, when it would become very irregular and intermittent, skin often moist and never very hot; little or no delirium, but a great degree of *sang froid*, an indifference; the patient often soliliquising in a mild tone of voice, would answer slowly but rationally when spoken to; not peevish or fretful, would say "he did not know how he felt," or "that he was tolerably well." Such cases I always look upon as extremely dangerous. When the moral manifestations are greatly disturbed at the commencement, and the eyes glaring and wild, the case should be regarded as very serious. When there is much muttering delirium, sunken cheeks and eyes, subsultus tendinum, involuntary discharges, and picking of the bed clothes, the case is almost certain to prove fatal. Indeed, the two last named symptoms are very constant and certain harbingers of death; but I had several cases last summer where the patient convalesced after having these frightful prognostics for three or four days. Epistaxis, and intestinal hæmorrhage are also of unfavorable augury, and more especially if accompanied by large-sized petechiæ. The prognosis is also unfavorable if there is great tenderness over the abdomen, and hiccough; or when there is an inability to protrude the tongue, and it is very tremulous. I believe the old are as likely to get over it as the young, and there is often more danger of a fatal result in the man or woman in the pr me of life, than at any other age. I well remember attending families in Ireland, where the father and mother, and from six to a dozen children were all down with typhus together, the old couple, aged, perhaps fifty or sixty years, would convalesce, the family would peradventure all recover, but one fine robust youth who would be cut off at twenty-four or twenty-six years of age. Some cases are ushered in with great violence, excruciating headache, and pain in back and loins, great general soreness, tossing and rolling in the bed, violent hot skin, thirst and noisy delirium; and yet such cases often terminate early and favorably. Where there is much complication with any of the diseases of the chest the case is much to be dreaded, but when the disease progresses regularly without very changeable pulse, or state of skin, without diarrhœa or hæmorrhage free from coma, or apparent idiocy, when there are lucid intervals, when the patient is quite rational, no moving of extremities, subsultus, drawing up of the knees, nor picking the bed clothes, the prognosis is favorable.

Treatment:—There has been a great diversity of opinion concerning the treatment of this disease, from the days of Hippocrates to the present time. At this hour, the medical world is divided and sub-divided in regard to the remedial management of typhus fever. Some contend that we can do nothing to shorten, or greatly ameliorate the violence of this disease. Others advise a kind of routine practice, or symptomatic treatment, not attempting to cut short the disease, but merely guarding against local congestions, adapting their remedies to suit the symptoms as they arise: regulating the bowels, diet, regimen, &c. Others again assert, that the disease can be arrested, cut short, or at least, rendered mild and innocuous in a great majority of cases. Of the last named class, I profess to be an humble disciple; I believe typhus fever can often be arrested in its first accession by a bold and judicious treatment; and I am well convinced, that in most cases, the violence of the disease can be very much mitigated, and even prevented from assuming a malignant character, by well directed energetic treatment during the first few days of the fever.

If called to a case early in the disease, and the patient be at all robust or plethoric, I open a vein and bleed freely, taking from twelve to twenty ounces of blood, according to circumstances. If the patient is not robust, is old, or it is too late in the disease, I apply the scarrificator and cups to the back of the neck, and along the spine as low down as the sixth or seventh *dorsal vertebra*, not abstracting much blood, but taking a little from six or seven different places along the spine. If venesection is employed, it is seldom necessary to repeat it, but if cups or leeches are used, it will probably be requisite to use them a second, or perhaps a third time. Simultaneous with the venesection, I administer the following efficient purgative, repeating it or not, according to circumstances :

℞ Hydrarg. Proto-chlor. grs. xv—xx.
Pulv. Antimonialis, grs. x—xv.
Pulv. Jalapæ, ℈ iss—℈ ii.
Pulv. Ipecac. grs. ii. M.

When this begins to operate, plenty of warm drinks, as thin gruel, barley water, mint tea, &c. should be freely administered. If the skin is very hot and the fever still high, the whole surface of the body should be sponged over with cold water ; this always should be done in the presence of the physician, as it might be carried too far, or applied to a patient who could not bear it without producing local congestions. To assist in mitigating the febrile action, and allaying the cerebral excitement, one or two teaspoonsful of the following mixture may be allowed every three hours :

℞ Infus. Digitalis Purp. ℥ ii ;
Infus. Rad. Rhei ℥ ii ;
Antim. Tart. grs. x ;
Tinct. Opii, ʒ ii. M.

As soon as the pulse is lowered and the nervous symptoms modified, this may be gradually omitted. If the thirst is urgent, cold drinks may be freely allowed, the common soda draughts will be a very pleasant beverage during the disease. Free ventilation and strict cleanliness must be attended to : the diet, of course, should be very light, such as gruel, pearl barley, rice, sago, tapioca, arrow root, and mush ; sugar, honey and molasses, should be prohibited, from the amount of combustible material which they supply to the system, thereby adding fuel to the flame. Let it be remembered, that I do not advise the venesection and strong purgatives to old or debilitated patients, nor to any, except during the first two or three days of the disease. A great many physicians object to such treatment in typhus, alleging, as their reason, that we should endeavor to husband the strength of the patient to enable him to bear up against the long continuance, and weakening effects of the disease ; but we should remember, that if the violent symptoms be allowed to pursue their course uncontrolled, the subject will be prostrated to a much greater degree than if the bleeding and catharsis had been resorted to ; and again, we are enabled to give stimulants and tonics much earlier after adopting the heroic method. During the further progress of the disease, say, from the eighth day, and in old and debilitated cases, from the accession of the disease, we are to watch and combat symptoms, by frequent ablutions with ice, cold water to the shaved head, and cold or tepid to the whole surface of the body, give the aqua acetatis ammoniæ frequently, regulate the bowels with frequent small doses of saline aperients. such as sulph. magnesiæ, phosphat. sodæ, sodæ et potass, tart., potass. bitart., &c. It will also be very well to give occasionally, during the progress of the case, the following, at bed time, to keep up the secretions, &c. ;

℞ Mass. pil. hydrarg. grs. v.—VII ;
Pulv. ipecac. grs. i—ii. M.

Or this :

℞ Hydrarg. proto—chlorid. grs. v.
Magnes. grs. xx.
Pulv. ipecac. grs. ii. M.

I believe the frequent exhibition of mercurials is very pernicious in this, as in most other diseases; the patient has a dry furred tongue, abdominal tenderness, and bad looking stools. To correct this, the routine practioner will pour in the calomel and blue pill, day and night, which will only aggravate the symptoms, exciting and keeping up gastric, intestinal, and hepatic irritation. In the accession of the disease. I advise the use of strong mercurial cathartics, because I believe that they then act as sedatives, overcoming and subduing the febrile excitement, modifying the nervous irritability, and changing the vitiated and abnomal secretion of the body. But afterwards there is nothing to be gained this way, and to give frequent small doses of mercurials is only to add to the existing excitement, by irritating the already too sensitive liver, and mucous membrane of the alimentary canal. Aperients should be used frequently, as the dislodgement of the foul and fœtid fœces, so common in this fever, is always attended with an amelioration of the patient. The saline aperients, I prefer, because they act agreeably, without nauseating or prostrating the patient, and because they may supply the chemical principles to the blood of which it is deprived during the progress of typhus fever. Notwithstanding high authorities are against me, I also feel convinced that this class of medicines is highly refrigerant, if administered with an eye to that effect. When I prescribe a refrigerant such as the tart. potass. and soda, sulph. magnesia, nit. potasæ, mur. ammoniæ, &c., I order it to be swallowed in powder, enveloped in a spoonful of mush; and direct plenty of cold water to be drunk immediately afterwards; the last two salts, are applicable in the advanced stages of the disease. If the bowels are very torpid, enemata and castor oil may be used, but on no account administer drastic cathartics in the advanced stages of the disease; for though it is all-important to prevent fœcal accumulations, yet irritating cathartics must not be given, as they would tend greatly to increase the erithism which is present in the gastro-enteric mucous membrane. Local hyperæmia must be guarded against by the prompt application of leeches or cups to the region which is suspected to be implicated. Bronchitis and pneumonia are particularly to be guarded against in this disease. If, therefore, any symptoms indicative of either, become manifest, the chest should be cupped freely, and the prescription given above, (the *digitalis antim. tart.* &c..) should be freely administered, until the inflammatory symptoms are subdued. If the patient is very watchful, or is much distressed with starting in sleep, and general uneasiness, an anodyne may be given as often as found necessary, such as the pulv. dov. grs. x—xv. morph. grs. ¼; liquor opii sedat. gutt. x—xv. &c., &c. I do not think that opiates are contra-indicated in this disease; of course there will be some cases where they cannot be given with impunity; and I would here emphatically remark, that *no one particular plan of treatment is applicable to every case of typhus fever.* Generally speaking, I think opiates are advantageous, and the refreshing, calm and happy slumber which they produce, must necessarily conduce to the speedy recovery of the patient. When there is great watchfulness, and restlessness, and any fear or doubts of the propriety of giving soporific anodynes, a blister applied to the nape of the neck will prepare the patient to bear them with impunity, and then they will be very certain to produce the desired effect. Tonics and stimulants have become very important remedies in the treatment of this disease within the last ten years. I remember when physicians condemned wine, brandy, quinine, &c., in typhus, and believe stimulants as pernicious in this disease as in scarletina or peritonitis : indeed, I know old practitioners at this day who would as soon administer verdigris, as quinine, in their treatment of typhus fever. I am sorry to say that a great many of our young physicians entertain the same false notions, and they are more inexcusable, not being prejudiced with any antiquated theories, nor being biassed with the teachings and prelections of any of the venerable contemporaries of Cullen. I trust a brighter day is now dawning on the medical world, the daily discoveries which are being made in chemistry, pathology, physiology and neurology will soon enable the studious and scientific physician to account for the origin of every sign and symptom in disease, and cause him to comprehend the *rationale* and *modus operandi* of every therapeutic agent prescribed. I have often startled the followers and advocates of Broussais, by proposing the use of quinine, brandy, &c., in the advanced stages of typhus fever. "What," say they, "would you advise tonics when the tongue is so dry, the pulse so quick, the head affected and the skin feverish ?" But the medical practitioner should be ashamed of his ignorance of physiology, and of his being so far behind the advance of science, who does not know and understand that the tongue may be dry and furred, the pulse quick, the eyes suffused, and the skin hot and dry ; and yet no inflammation existing, but very probably great prostration and debility. On the other hand, how often is the tongue moist and clean, and the skin soft and perspiring, and yet serious inflammation or ulceration present in the stomach or intestines.

There are many other and more certain pathognomonic signs and symptoms which will be readily observed by the scientific physician, that will clearly demonstrate the actual condition of the internal organs in disease. In the typhus fever of Mississippi, I believe we should generally commence our tonic remedies about the sixth or eighth day of the disease ; if the case will bear them early, the more favorable is the prognosis, and the more rapid is the recovery. When a case progresses for eight days without much change, and we are certain of no local congestion or inflammation existing, we should try some tonic ; if the nervous system is much deranged, and the mind greatly disturbed, it may be well to premise a blister to the nape of the neck. The tonic may be quinine, carbonate of ammonia, brandy, or wine, &c., according as the case may indicate ; commence with caution—if, after the third or fourth dose, the pulse becomes fuller, softer and slower, the skin softer and the mental phenomena calmed, we are certain of the tonic proving serviceable ; on the other hand, if it increases the uneasiness and watchfulness, the heat of skin, and quickness of the pulse, we should omit it for a day or two, and try again. Sometimes quinine or the carb. ammonia, and infus. cinchonæ will disagree, and aggravate the mental and corporeal irritation, whilst wine or brandy would act most advantageously ; the particular tonic must therefore be selected which seems to suit the indications of each case. The aqueous solution of quinine mixed with a small portion of lemon juice, an infusion of cinchona slightly acidulated with elixir vitriol, the carbonate of ammonia dissolved in *decoc. polygal. seneg.* Wine and brandy are each very excellent tonics in typhus fever. I believe it is best to commence them in small doses, given once every three hours; but there is really less risk in the early use of tonics in this disease than might be expected; if it is treated in the beginning as I have pointed out, the stimulating system may be commenced very soon, and that with perfect safety. One important criterion to judge of the admissibility of tonics, is the action of the heart as presented by a stethescopic examination; as soon as the sounds of the heart become weak, irregular or intermittent, tonics should be commenced and prescribed freely I will not soon forget a case I was called to, in consultation, about six weeks ago; the patient was a man of rather slender frame, and nervo-sanguineous temperament, about 25 years of age; he was in the fourteenth day of the disease, really weak, but so disturbed in his mind, and so much irritability and restlessness present, that his debility did not appear so conspicuous; his pulse was quick and wiry, the eyes suffused, the countenance haggard and anxious, sometimes wild-looking, skin dry, no sound sleep; when he would doze, there was muttering delirium, subsultus, and picking of the bedclothes; tongue dry and covered with a brown fur; forehead and temples rather warm; heart's action week, bowels regular. As he had been much the same way for a weak previously, and was gradually but unobservedly sinking, I suggested the application of a blister four inches long by two and a half inches broad, to the back of the neck, and the internal use of sol. sulph. quinine. The second dose appeared to sooth and allay the nervous irritability, his pulse became fuller, softer and slower, the skin and tongue moister, and a very calm sleep for an hour ensued. The quinine was kept up for the fourteen hours I remained, five grains given every three hours, and when I left, the subsultus, startling and soliloquising in sleep, picking of the bedclothes, &c., had very much subsided, and he convalesced rapidly. In a great many weak, nervous constitutions, good wine will be found a very appropriate tonic. and more especially if the subject of the disease has an antipathy to medicine. Brandy is very applicable to those persons who have been *bons vivants* when in health; it requires to be given in small doses, and very regularly and frequently, to keep up an equal degree of stimulous without any corresponding depression. If the bowels are rather constipated, it is a good plan to administer the quinine dissolved in senna tea, as recommended by some practitioners. I wish to impress it strongly on the minds of physicians, that we should commence the use of tonics and stimulants early in this disease; in fact as soon as the heart's action is weakened, and the least symptoms of corporeal prostration manifest. We must not keep waiting for the clean, moist tongue, the soft cool skin, and the lucid and composed mental faculties, before resorting to them; if we do, in a great many cases, death will relieve us of any further attentions on the patient. And be it remembered, that premising the application of a blister to the nape of the neck for five or six hours prior to the exhibition of tonics, will conduce very much to their salutary effects, and render the patient more tolerant of their action. I will never forget the case of a gentleman who is now a respectable physician in Ireland: he took a violent typhus fever in June, 1841, contracted when attending similar cases in the small, crowded, and filthy streets of the town where he resided. I was practising in the same town, and had been his fellow student for two sessions at college, I therefore felt the utmost solicitude for his recovery, and aided by the oldest and most talented physician in the place, attended to him very sedulously night and day. As I remarked, his case was extremely severe, and went on for two weeks with unmitigated violence; so much so, that we thought it would certainly prove fatal. At the fourteenth day, the old physician, who had previously objected to all tonics and stimulants, told me he would agree to a trial of any thing in that way I should suggest, as he thought all hope was gone, and it was immaterial what prescription we might give; indeed he manifested every unfavorable symptom; his high and violent delirium had given way to low muttering, approaching to *coma*, constant *subsultus tendinum*, picking the bed clothes, sliding down on his back in the bed, involuntary discharge of fœces, feeble and irregular pulse, cold extremities, &c. We had previously applied sinapisms to his extremities, and a blister to the back of the neck, had regulated the bowels by mild aperients and enemata, given the *spts. mendereri, spts. æther. nitrosi, et mist. camph.*—besides repeated doses of antimonials and ipecac, refrigerants, &c., without any apparent amelioration of symptoms. After the consultation and agreement to try stimulants, I commenced at seven or eight o'clock P. M., to give two teaspoonsful of the best cogniac brandy every two hours, increasing the dose to a tablespoonful. In twenty-four hours no person could have been more delighted than I was at seeing my friend and old college *chum* rescued, as it were, from the jaws of death. Immediately after the exhibition of the brandy and water, his nervous symptoms became moderated, his tongue moist, intellect more rational, extremities warm, pulse fuller and more regular; next day he used gruel with his brandy occasionally, and a very favorable convalescence ensued. About two weeks after this, I took the disease myself with great severity, and immediately after I got up, my brother, who attended, to me was violently seized with the same awful malady; to the judicious administration of tonics and stimulants, and the use of blisters and sinapisms, I attribute the successful issue of all the cases. Whenever the extremities appear to get colder than the rest of the body, or when there is much cerebral disturbance, *stupor*, or *coma*, large warm mustard cataplasms should be forthwith applied to the arms, and feet, and legs. When there is great mental optuseness, stupor or coma, the head should be shaved, and a large strong blister (emplastum lyttæ) applied from the *anterior fontanelle* along the course of the *sagital suture*, over the occiput and down for 3 or 4 inches the back of the neck. I have often seen typhus patients lying in a comatose state for ten or twelve hours with a large blister over their heads, and hot sinapisms to their extremities; these means will generally rouse them to consciousness, and if tonics are then given judiciously, a favorable result will be likely to take place. A mixture of *camphor* mucilage of gum arabic and spirits of nitre, will relieve *subsultus tendinum*, but that is more effectually done by tonics which, in this stage of the disease, fulfil many and more important indications. If severe diarrhœa, or intestinal hemorrhage be present, it should immediately be checked; small doses of *hydrarg. c. creta* and *pulv. dov.*, *magnesia* and *pulv. opii.*, enemata of starch and tinct. opii., or acet plumb. and opii., are the means to be used in arresting these distressing and debilitating complications. I would here repeat the charges with regard to cleanliness ; every time that the bowels act, the fœces should be carried off, and it would be well to keep constant fumigations in the house or chambers of the sick ; sulphuric acid poured on a plate of common salt or salt petre, will answer the purpose. Free ventilation, and perfect quietness are also to be observed. After the fever is subdued, or in popular phrase, after "*the cool*" is manifest, we should order small quantities of fresh beef tea, to be drunk occasionally, or if the patient is very weak, and the stomach unable to perform its functions, it may be administered *per anum*, with an enema apparatus. It will be well to scrape and clean the tongue, and detach the *sordes* from the teeth, occasionally washing the mouth with vinegar or strong cream of tartar tea. We should do every thing in our power to hasten complete convalescence by ordering the beef tea, a *slightly boiled* egg or two, every day, rice, Irish potatoes, gruel and arrow root with wine, tea or coffee with a thin slice of toasted light bread ; some porter may also be allowed occasionally. We thus assist the *vis medicatrix naturæ* to regain the deficiency and loss sustained by the humors and solids of the system ; and by a judicious course of diet, the constitution is fortified against those adynamic diseases, such as consumption, dropsy, diarrhœa, hepatic diseases, &c., which are so apt to come at the close of lingering typhus cases. On the other hand, we must be cautious, and not over-load the weakened and perverted digestive system ; we must forbid a *solid*, strong food, or any and every thing that is crude and indigestible, such as beef, bacon or ham, cheese, sausages, spiced meats, &c.

In conclusion, I would remark, that I do not claim any originality or superiority in regard to my views of the nature and treatment of typhus fever. I write this with the unobtrusive intention and conscientious desire of arousing the attention of my medical brethren of the Southwest to a careful examination of this prevalent disease. I believe it has often raged with more or less violence (assuming different aspects in different localities,) in all of the States in the Union, and I do know that it has prevailed in some parts of Mississippi, when it was treated as a different disease, or as some strange infectious plague, or contagious and malignant epidemic ; the physicians utterly at a loss to know what treatment to pursue. It may be asked what advantage is to be gained by knowing that those anomalous and severe maladies are cases of typhus fever, when it is acknowledged by most medical authorities that very little can be done to arrest or cure that obstinate affection? I answer, much is to be gained every way by a proper diagnosis of this disease ; for even, if we admit, with Dr. Pitcairn and his followers, "that we may modify the violence of a fever but cannot cure it," then, all our efforts should be used to check it, at least ; but if we see the disease raging in all its protean forms, and are at a loss to know what it is, we will feel very diffident in employing remedies, no matter how we may fancy we are pursuing a scientific treatment—" reposing on general principles." I think, therefore, that the "expectant method" is a very bad system of practice in typhus fever ; I believe that frequently the disease can be cut short at the commencement, and if not entirely dispelled, I know that much good will arise from a vigorous and energetic and *judicious* use of the various remedial measures I have enumerated. Of course, I claim not originality for the views I have expressed, I mark out no *one specific course* to be pursued in *all* cases; to use the language of an eloquent lecturer and scientific physician, "there is no mode of treatment *universally* applicable, and he who would treat this disease with wine and stimulants only, or he who contents himself with purgatives and diaphoretics, or he who limits his practice to the lancet and blisters—that man knows nothing of the pathology or physiology of fever." Let us then sedulously examine the disease in its various phases, and find out the cause, nature and seat of every sign and symptom; our remedies can then be directed with confidence and energy for the removal or alleviation of the malady, and typhus fever will cease to be called an intractable and unmanageable disease.

March 29th, 1845.

An Essay on the Spontaneous Gangrene which prevails in Mexico; read in French, before the Louisiana Medico-Chirurgical Society, on the 7th May, 1845, By Angelo Binaghi, M. D., della Universita di Pavia.

Among the different kinds of gangrene, such, for instance, as that which is the result of the highest degree of inflammation—hospital gangrene, that created by ergot, or by an external traumatic, physical or chemical cause, as well as that species which is the effect of the *nisus in reductionem* among the aged, spontaneous gangrene particularly invites our attention; not only on account of the interest which it excites in the observing physician, but also on account of the investigations to which it gives rise, and which may in some manner become the cause of important and useful discoveries in medicine: especially since the great progress of pathological anatomy, which has contributed so powerfully towards substituting, in lieu of erroneous principles, others more solid and more true, because based upon the material alterations, discovered by means of autopsy. We have a proof of this in the subject which is now to engage our attention:

Spontaneous gangrene, which is in Mexico the cause of as many amputations as all the other internal causes of disease united, appears to have chosen this Capital for its residence, as ophthalmia has chosen Egypt, the cholera, the East Indies, the pelagra, Lombardy, or the plica, Poland, &c. Mexico, once the Venice of America, is situated in the interior of the country, about eighty leagues from the Gulf of Mexico, and about fifty leagues from the Pacific Ocean. It is built upon a plain at an elevation of more than seven thousand feet above the level of the sea, at the 19th degree of north latitude. The temperature is constantly at about 50 to 60 degrees Fahrenheit; the soil is sterile, abounding in salts of soda, (*terrens tequesquitoso*,) and at the depth of two feet a brackish water is met with, whilst at about one league distance a thermal sulphurous spring, called *le Penol*, exists. This city offers a vast field for the observing physician, on account of the variety of diseases of the sanguine system which prevails; such as affections of the heart and its annexæ, aneurisms, congestions, abscesses by congestion, membranous irritations, and inflammations, &c. This town which is fatal to phthisical subjects when the secretions, especially from the cutaneous surface, are so deficient that baths are absolutely necessary for health; where man is more obnoxious than elsewhere to the abuse or free use of alcoholic liquors, here it is that spontaneous gangrene is most frequent and most fatal.

During the space of one year, (1837,) I saw three or four cases, and heard of others; a few remarks upon some of these will enable the reader to form some idea of their nature and symptoms.

The first case, was in the person of Sr. Pascua; after having had both lower extremities amputated by Dr. Villetti, at different periods; and I believe, also, the superior extremities, the patient then entered the hospital, where he died a few months afterwards. Dr. L. Jecker made the autopsy of this man, and preserved the abdominal aorta and iliac arteries, as morbid specimens. As to the other parts of the cadaver, I am not prepared to speak. On laying open longitudinally these arteries, the following peculiarities were presented: on the free surface of the internal tunics of said arteries, at unequal distances, were found a number of superficial ulcers, either round or oval, with smooth borders: from one to four lines in diameter; more numerous near the bifurgators; some were of a white, others, of a pale yellow, color; others resembling aphthæ; others gray and *mammillonated*; others again covered with flocculi, which were easily detached, and resembling in structure, fibrine. The capillary system of the internal coats of these arteries presented no other changes, or any degree of injection.

The second case was a poor man, aged forty-five years, who presented symptoms of marasmus, which usually characterize the last stages of all chronic diseases. He had a gangrenous spot as large as half a dollar on the dorsum of the right foot, analagous to an ordinary eschar, produced by caustic applications. I could not detect the pulsations of the arteria dorsalis pedis. The pulsations of the arteria crusalis, at the point where it emerges from beneath Poupart's ligament, were scarcely perceptible on pressure. In consultation, it was decided that amputation would be useless, because the disease had undoubtedly extended already as high as the iliacs. In a short time afterwards, the patient expired.

On examination, after death, the principal arterial trunks of the diseased limb were found completely obstructed; in fact, their calibres were obliterated. The corresponding iliacs presented the same morbid alterations as those which were specified in the first case.

The third case, was a man of the higher class, aged thirty-eight years, and whose *enbonpoint* precluded all idea of any constitutional affection; he complained of constant pain in the bottom of the left foot, corresponding with the first phalanx of the great toe; at this point, the skin was callous and presented no other signs of inflammation than the pain, which was not increased by pressure; he compared the painful sensation to that arising from corns; the temperature of the extremity was normal; the skin on this leg was rather drier than that on the other. The pulsations of the dorsal artery of the foot were visible; and when pressure was made upon it, it was found contracted, tense and isochronous with the other arteries of the body. The crural artery of the same side, was similarly affected, though less characteristic. Besides these peculiarities, the organism seemed healthy. About two months posterior to this time, amputation was performed above the knee; the patient recovered, perhaps to submit, at some subsequent period to another amputation. The limb was examined, and the same pathological changes were observed in the arteries of this as of the preceeding limbs.

The fourth was a subject, aged 32 years, of a robust constitution—and a sanguine temperament. This man usually enjoyed good health until about two months before I saw him. At this time, he complained of a constant pain at the sole of the left foot, without being able to account for it. Having given him a careful examination, I found he presented all the symptoms of the third case, except the seat of the callosity, which corresponded with the phalanx of the third toe. Finding the disease of recent origin, and the subject otherwise in good health, I determined to try to arrest the disease by internal medication, rather than amputate; reserving this as the last chance, in case I failed in my first effort to check it. For this purpose, I resorted to general bleeding, mercurial inunction, drastic and saline cathartics, the free use of diluents, full, warm, prolonged baths of two and three hours; hot fermentations to the affected limb, and low diet. The patient bore this active treatment quite well; the objective symptoms gradually disappeared, and at the end of four months, he was able to resume business. I did not, however, dismiss my patient, without advising him to restrict himself to a low and exclusively vegetable diet; to use repeated warm baths, and from time to time to resort to some of the purgative medicines first administered; I also opened an issue below the internal condyle of the femur, on the upper and posterior part of the tibia, which I advised to be kept discharging for life. In addition to this precautionary measure, I suggested the constant use of *gum-elastic* stockings.

Wishing to make some remarks upon the disease in question, let us first examine whether the name it bears, is the most appropriate. As to myself, I should answer in the negative; because, in my opinion, the *gangrene* termed *spontaneous*, is but the consequence of another disease—a mere symptom of the last stage of said disease; as a dropsy which supervenes upon chronic disease of the thoracic, or abdominal viscera; at all events, this name might only be appropriate in the last stage of the disease; when no means can arrest its progress, and rescue the patient from certain death. If the physician is unable to detect the primitive cause of the affection, he can, at least, by the aid of pathological anatomy, understand its true seat and the nature of the lesion. * * * *

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The autopsy of the three first cases, shows that the seat of the lesion is in the internal tunics of the arteries; a lesion, which causes the obliteration of the arterial tubes, and consequently, the gangrene.

Is this affection of the internal coats of the arteries, the cause, or the consequence, of an inflammation? I cannot believe it to be an acute inflammation, such as exists in a case of phlebitis, otherwise, we should have fever and general excitement of the heart and arteries. Besides, supposing it to be an acute inflammation, why does it not spread through the whole arterial system in a short time, instead of extending so slowly, as to require even several years for the final development of the gangrene? Why are the diseased points, or ulcers, on the inner surface of the arteries scattered over this tissue, leaving the spaces between them perfectly normal, instead of involving the whole free surface of the arterial tunic? Can it be called chronic inflammation, or that state of congestion arising from atony of the tissues, either pre-existing in subjects of a lymphatic temperament, or as a consequence of the alteration induced by a previous acute inflammation? I believe neither to be the case. The internal coats of the vessels, examined in the first case, were not at all injected; what then is the essence of the disease—the *causa proxima morbi?*

To come to the point at issue, I must compare this with some other kinds of gangrene; and then speak of the etiology of the disease.

The gangrene consequent upon a high degree of inflammation, is the effect of an excessive action of the capillary system of vessels, combined with a hypersthenic diathesis;—in a word, over action in a part, ends in the death of that part. Such is not the case in the affection under review.

Gangrena senilis, which arises from the obliteration and ossification of the arteries, produced by the diminution of the vital energy, and the prevalence of the inorganic principles, has no other connection with spontaneous gangrene, than the obliteration of the vessels. As to the gangrene, arising from the protracted use of the *secale cornutum*, it would be useful to know what alterations are produced in the arteries in this disease, because such knowledge might shed important light upon our subject; and this perhaps, would enable us to ascertain that gangrene from the ergot, takes place in the same way as that called spontaneous; that, in both the same alterations are found, and finally, that different causes may produce similar effects; that is, an affection of the internal tunics of the arteries, to which a name might be given more significant of the disease.

As to the causes of this malady, so common in the city of Mexico, to what are we to attribute it? Shall we ascribe it to the free use of the *pulque*, a drink which is the product of the fermentation of the juice drawn from the *agave Americana*; a plant which is extensively cultivated in some parts of Mexico, and which constitutes the principal drink in the Capitol? Such cannot be the cause, else this disease would prevail in other sections of the country, where this drink is generally used. To the constant use, and even abuse of *cayenne pepper?* this cannot be the cause, since this article is freely used as a condiment in all parts of the country, and by all classes of people. To the *tequesquitosa* quality of the soil, which consists essentially of the salts of soda? this is not at all probable, because this kind of soil abounds in many sections and countries where this malady is unknown. To what then shall the disease be ascribed?

Above it has been observed that the temperature in the city of Mexico, constantly stands at from fifty to sixty degrees (Farenheit), and the functions of the skin are here extremely languid; on which account, the inhabitants are forced to use almost daily, the warm bath, in order to preserve their health; when the physicians find it necessary, to order baths much more frequently and much more prolonged than in other parts of the world. Here, the functions of the skin, through which, in a healthy state, those heterogeneous principles should be thrown out of the organism, all more or less checked, or suspended, thus causing those elements now no longer fit for the purposes of nutrition, to be retained in the system, and which may probably be the existing cause of the disease of the arteries. In other words, I believe that the essence of this disease consists in an abnormal nutrition of the internal tunic of the arteries, produced by a *dyscrocia*, which proceeds from an imperfection of the functions of the skin, or from any other cause capable of altering the chemico-organic conditions of the fluids, abnormal nutrition, which ends in the complete obliteration of the arteries, and consequently gangrene is the result.

Moreover, I believe that this perversion of nutrition would take place on the internal tunic of the arteries, as *cacoderms* are sometimes developed on the surface of the body; and like these morbid productions, it does not occupy the entire surface of the arterial tunics, but is confined to spots here and there, leaving the intervening space perfectly normal. The first case, to which I have already alluded more than once, demonstrates that if in this affection, the physician confines himself to the amputation, without regard to the cause of the disease, with a view to arrest it by therapeutic means, it will continue to be reproduced, and extend to the arteries in the remaining stump, and finally would reach the heart, but for the fact that it invades arteries distributed to organs so necessary to life, that death results ere it reaches the seat of life.

The primitive cause, so productive of this disease in the city of Mexico, is, in my opinion, the same as that which keeps so low the temperature of the town, and which also, gives rise to the various maladies of the sanguine system, particularly the arterial, viz: the high elevation of the city above the level of the ocean. At such an elevation, the air is greatly rarified—hence the imperfect oxygenation of the blood, from the fact that, at each inspiration, a less quantity of oxygen is taken into the system than under ordinary circumstance.

The atmospheric pressure, at this elevation, is comparatively very light; from which it results that the *lateral* pressure of the fluids upon the vessels must prevail over the longitudinal, and must modify the peripheric functions, or the functions of the free extremities of the capillary system; consequently the secretions must be torpid, and the principles which should be elminated through the skin and other emunctories, be they excrementitious, or decarbonising, remain in the organism, and alter the *crasis* of the liquids, which give rise to the abnormal nutrition that manifests itself in the internal tunics of the arteries.

Why does this disease attack the arteries and not the veins? Why begin in the lower instead of the upper extremities? Why assail males rather than females? These are questions on which we may speculate *ad infinitum*.

I believe that an affection which might bear some analogy with that which excites spontaneous gangrene, in regard to the efficient cause, would be that species of aneurism which, according to Scarpa, is the result of the slow morbid degeneration of the internal tunic of the arteries; with these differences, however, that the latter interests, or involves the great aorta almost invariably; the former begins near or before the last divisions of the arterial trunks into the capillary system; the one is confined to a particular locality of the artery; the other, extends to all the arteries of a limb, and travels from the periphery towards the centre; the one attacks all the coats of the artery. causing their rupture ; in the other it is confined to the internal coat exclusively; in other words, the affection which gives rise to this species of aneurism, gradually penetrates through the different coats of the artery; whereas, the other spreads itself by continuity of tissue over the surface of the internal coat ; the one ends in rupture, the other, in an obliteration of the artery ; in one, the ligature, when practicable, may rescue the patient from death ; in the other, it is useless, because the disease will be reproduced above the ligature. The cases of aneurismatic diathesis, of which I have seen two, in the city of Mexico, might, in my opinion, occupy a place between these affections ; and might furnish an argument in favor of the analagous origin of the two diseases. In one of the cases with the aneurismatic diathesis, there were six distinct, obvious aneurisms, occupying different parts of the body. Here it was justly deemed useless to operate. Although the patient does not seek advice until the disease has made some progress, and with a view only to be relieved of the constant pain seated in the bottom of the foot, without suspecting the serious nature of the disease ; still, there are other symptoms which enable the physician to diagnosticate the affection ; such as a callous hardness at the seat of pain ; without heat, redness or tumefaction of the integument. The skin, in general, especially that covering the affected limb, is extremely dry, though the temperature is at the healthy standard. But the symptom which I would characterise as pathognomonic, to which the pain directs attention, is furnished by the *arteria dorsalis pedis ;* to prove this, I refer to case third. When the gangrene is declared, the pulsations in the diseased limb are only perceptible in the crural artery, and much less then, than in the normal state, as has already been mentioned in the third case. The progress of the disease is very slow ; sometimes two or three years elapse, before gangrene is manifested. The age at which the disease usually sets in, is between thirty and fifty years of life.

Having said that I regarded the affection as a lesion of nutrition, the indication which follows, is to correct it. To this effect, I employed as the principle therapeutic agent, *mercury*, in the form of ointment externally. As this medicine has been employed with success in scrofulous and dermoid affections, and from analogy, in syphilis ; besides, mercury eliminates from the system, through the salivary and other organs, the *debris* of the organism. Concerning this disease, with the advocates of the contra-stimulus, to be an inflammation of the arteries, the use of mercury would meet the indication according to their views ; since experience has taught us that this medicine is a powerful antiphlogistic, and such as to produce a salutary effect that could not be often obtained by other similar means ; in such as metro-peritonitis puerperalis, and all extensive phlegmonous inflammations threatening gangrene. In mercury the homœopathits would find the application of their principle—*similibus similia curantur.* Although I cannot agree with them in regard to minimum doses, above all in the treatment of organic diseases ; since in the case treated by me, I employed at least, two pounds of mercurial ointment. I would not be understood to state that this powerful medicine should be exclusively used in the treatment of spontaneous grangrene ; on the contrary, there are others, such as iodine and its preparations, may be used with signal advantage. Be it remembered, however, that although it be a disease slow in its progress, yet it is invariably serious in its consequences—and as such, should be opposed by prompt and efficacious treatment. I do not consider venesection as always indicated; although in particular instances, as in the one already detailed, it may be called for. The object of the warm bath, is to stimulate the function of the skin, and by prolonging the bath, to introduce into the system a certain amount of fluid. By the free use of saline and drastic purgatives, we increased the venal and intestinal secretions; and by this means, I was enabled to correct the morbid nutrition. It is easy to perceive the motives which led me to adopt precautionary measures with my patient before I dismissed him. To avoid a relapse, and to arrest the further progress of the disease, the patient may be advised to take up his residence in a warm climate, upon the sea coast. How many diseases of a serious nature here, by this means have been checked or removed. Even in cases of amputation, it is necessary to continue the use of such therapeutic means as have been recommended, else the disease will continue to extend, as in the case of poor Pasqua, until death put a stop to its progress. Experience has shown that after gangrene has developed itself, amputation is almost always useless, because the disease has, by this time, invaded the iliac árteriés; but if, however, this grave operation should be performed; it may be recommended to prevent union by the first intention, and thereby establish a salutary drain from the diseased limb.

This disease is so dangerous, as to require, at least, caution in pronouncing the prognosis, a prognosis which must be modified according to the progress of the affection, which, in general, is unfortunately far advanced; because the patient applies for assistance only after having suffered for a certain period from the disease. Oftentimes, he then refuses to submit to an energetic treatment and a severe diet; still less, to amputation, unwilling to believe that a bearable pain can be the sign of serious disease, and the forerunner of an evil which smoulders beneath the surface, sooner or later to develope itself in the form of a frightful gangrene.

An account of the Yellow Fever which prevailed in Woodville, Mississippi, in the year 1844. By Andrew R. Kilpatrick, M. D.

Yellow fever is the disease of the present era, which is the cause of much controversy in the medical world, as the plague was in the days of Sydenham, syphilis in the age of Hunter, and small-pox in the times of Jenner; and it is to be hoped, the great fermentation which is now in progress, will ultimate in fixing the nature and character of this disease beyond cavil. Men of the most lofty and gigantic intellects having labored on the subject, until wearied and vexed, they abandoned it in despair to future generations to meet with no better success. When such men as Rush, Hosack and Caldwell—Chisholm, Bancroft, Johnson and Chervin, with a host of other bright luminaries, have failed, how can we hope for success? But each has a part to perform in the erection and adornment of the temple of medical science, and those who cannot frame and construct, can at least assist at the quarries in preparing stones for the great edifice, and for some grand master, who may yet come forward to the noble work, and erect an imperishable monument to his genius. With these feelings, I enter upon the task, hoping that some facts which I shall record, may fall into the hands of some future philosopher, who may make such use of them as shall redound to the benefit of science and mankind.

The same path has been trodden before me by Drs. Logan, Valetti and Stone; and I can only follow and glean such items as they have left; and I have endeavored, as much as possible, to avoid a repetition of what has already been placed on record, except so far as was necessary to render clear and comprehensible what was rather incomplete and obscure in their reports. And when our three articles are examined, and all the important facts selected and arranged, still there are some very interesting data which are omitted, and which would assist much in a full comprehension of the whole case. The town of Woodville is situated in the State of Mississippi, lat 31° 7′, seven miles above the southern boundary of the State, fifteen miles in a direct line east from the Mississippi River, in an elevated, rolling section, surrounded by open fields, which have been in cultivation for forty years or more; except on the northern boundary, which is a broken woodland, chiefly of pine growth, varied with some narrow swamp lands, through which small streams run from town to be discharged into the Buffalo Creek. But as a full description of the soil and growth has been furnished by my predecessors, in the first volume of this journal, pp. 241, 530, I deem it unnecessary for me to describe it any further.

The village, which began to be settled about the year 1809, has uniformly been a healthy place; so much so, that many persons from more southern localities have come here to spend the summer seasons. It now presents a somewhat neater appearance than the generality of inland towns, with a sprightly, intelligent and moral community. There are few places where the buildings are compact, being for the most part separated by open grounds, parterres and garden plats, which present a pleasing and grateful variety to the beholder; although, I am sorry say, except in the court house square, there are few shade trees, the merchants mostly using awnings to protect themselves from the heat of the sun. If we were to adopt the custom of some Eastern nations, and each man plant a tree; or if parents were to plant the seed of some forest tree at the birth of each son, we should soon have a delightful, shady village. Owing to the unpaved condition of the streets, their declivities, and the hard rains, they repeatedly require work and repairs which is mostly done by ploughing and spading, digging down high places to fill up washes and gutters; and frequently too, dirt, trash, green bushes. and manure from stables, are thrown into them, and on vacant lots, which are sources of much annoyance to persons living near such deposites. This was often the case last year, as many of our citizens will easily recollect. The West Feliciana Rail Road, which had been in course of erection since 1835, was completed in the month of October, 1842, extending from the Mississippi River, at Bayou Sara, to the southern limits of this town. Before the completion of this rail road, the merchants of Woodville were in the habit of bringing their goods from Bayou Sara and Fort Adams on wagons, and the communication between here and New Orleans was limited, difficult, and tardy; but since that time (1842) there is scarcely a day without the arrival of goods or passengers from that place. And merchandise or passengers leaving the city on the morning of one day, will be in Woodville on the afternoon of next day, a distance of one hundred and eighty miles; so that if an epidemic should prevail in New Orleans, and the cars here were permitted to run, there would be no difficulty in the transmission of fomites, or sick from that place; and an individual could be exposed to infection there, and arrive here many days before the manifestation of disease.

Although Woodville is incorporated, and we are taxed for the maintenance of officers, and for the purposes of cleanliness, comfort and health, still, I fear, the desired objects have not been attained, as our citizens are well aware there is much filth accumulated and allowed to remain in many parts of town. There are many stables and horse lots, which I think would compare respectably with those of Augean notoriety, and require another Hercules to remove their tons of offensive ordure: there are back yards and privies around the royal oak, which beggar description, and there are cellars and sewers which, in a well regulated city, would draw upon their keepers the censures and punishments of the sanitary police. In the back yards of the groceries and restaurats, and in the streets near them, there was, and is, a large amount of saw dust, in which ice was brought from the city, undergoing decomposition. There was, during the spring and summer, much water collected in the back yards, and under the houses in the same neighborhood, which was permitted to stand until it was coated with a thick green scum, part of which was drained off early in September, leaving a large muddy spot exposed to the action of the sun and air, and a considerable quantity of water still remained under one of the houses. There was so much drained off, that it ran down the street nearly two hundred yards, and was so offensive as to attract the attention of persons passing by. There is a brick cistern in the same vicinity, which held water until it became green and offensive. Near the same place is a slough, or rather a wash, which has been filled up with shavings from the more elevated parts of the street. All this is on the west side of the square. On the south side, on what is called commercial row, the houses are very compact, forming a solid block of buildings, and under some of them are cellars which held rain water; one especially, was very offensive on this account, and the occupant of the house was carried off by the fever in its most malignant form. There are back yards on this row, which are very filthy. On the east side of the square, an old cellar, where the building had been destroyed by fire, in 1837, was the receptacle of much trash and filth, and during the months of April and May it was cleaned out and enlarged for the purpose of erecting a new building on the same site, but the dirt and filth was piled and scattered in the streets, where it threw off a good deal of stench and malaria. Two other buildings were erected near the same place at that time, one being a wooden building, the other two, of brick. Another house had been put up in the fall and winter previous, north-east of the court house, and during the summer of 1844, some alterations and excavations were made about its cellar and foundation ; water was frequently standing under it for weeks during the spring and summer. Besides these, there were some more houses put up in different parts of town. During the spring, summer, and part of the fall, the *streets all through town*, were *ploughed*, *ditched* and *repaired*, by which, much dirt was turned up to the action of the sun, and some very nasty places were hereby opened.

During the months of May, June and July, a diarrhœa prevailed so generally here, in town, and in some portions of the adjacent country, that many felt disposed to accord to it the name of epidemic. The cases generally, were very obstinate, and some were totally unmanageable, terminating fatally in a few days, in spite of the best directed medical skill. All the remedies usually employed in this disease, were powerless in many cases which came under my observation, and medicines which might succeed in checking its progress for a few hours, would be found to be inadequate to the cure, and more powerful agents would have to be called it. May it not be likely that this diarrhœa had some agency in preparing the systems of the people for the overwhelming onslaught of the yellow fever?

I would beg leave, also, to record here, some remarkable deaths which took place in this county, not that I think they had any connection with the epidemic, but merely from their singularity. Mr. Johns, after eating a hearty breakfast, and expressing himself as feeling better than he had for years, fell dead at his work bench, in March, aged 73 years. Mrs. Diana Dawson died of phthisis on the 19th May, aged 69 years. Mr. J. Riddle, subject to strangury, ate supper on 31st May, and died that night before eleven o'clock, aged 75 years. Mrs. R. N. Johnson ate breakfast on the 9th June, shortly after, complained of pain in the chest, around the heart, and died in less than five minutes, aged 73 years. Mrs. Susan Scott, relict of Gov. Scott of this State, died, after a long illness, in the month of July, aged 55 years. Mrs. Schwartz, drank ice cream on the 4th of July, cholera supervened, and she died on the 5th, aged 30 years.

There were vast swarms of flies early in the summer, and musquitoes by thousands, which annoyed us incessantly. The latter were nearly as troublesome in the day as in the night, and fumigations could not expel them from the houses.

Rains were very constant during the spring and summer, so much so, as to prove very irksome to our planters in the cultivation of their crops. The streams were repeatedly swollen beyond their banks, carried off fences, and inundated portions of the crops, which were planted in the bottoms. And every reader will call to mind the unusual overflow of the Missouri and Mississippi rivers.

I will here give some particulars of the weather as it is conceded on all hands, that epidemics are influenced thereby : Drs. V. and L., in their report, p. 241, say, "the whole month of June was rainy." From the 20th of July, to the 15th August, there were many vicissitudes and sudden changes of temperature, attended with showers. On the 1st, 2nd, 3d, 5th, 6th and 10th August, there was more or less rain, but especially on the 1st, 2nd, 5th and 6th, accompanied with remarkably loud thunder. The wind at this time, up to the 4th August, was from the S. S. E. ; it changed, and was on the 5th, 6th, 7th and 8th, from the N. and N. W., but on the 9th, it again veered and came from the south, without any material variation for two weeks, or up to the 21st, when it changed again and came mostly from the east, varying occasionally to S. E., but oftener to N. E., the cardinal point, however, preponderating. It continued from these points for nearly six weeks. On Wednesday night, 21st August, a remarkable thunder storm passed over this town, accompanied by a wind of considerable force, principally from the north, but although the lightning and thunder were peculiarly brilliant and loud, there was not rain enough to run from the roofs of the houses, or settle the dust in the streets; somewhat heavier showers fell that night on the northern limits of the county. From this last date till the 8th of September, the air was dry, warm, and extremely debilitating; the thermometer ranged from 80 to 98 degress, and some have alleged, that it reach 100 degrees in the shade. The sun's rays beamed upon us with torrid fervor, undimmed by a cloud; and from sun-set till sun-rise, a peculiar aroma pervaded the atmosphere, causing oppression and a sense of suffocation, attended with nausea in some persons, especially when coming in from the country. I believe more persons were taken down from the 3rd to the 8th of September, than at any subsequent period of the same length. A shower fell on the 8th, between two and three P. M., for about fifteen minutes, but the air was so warm, and the earth so parched, that it was soon dissipated by absorption and evaporation, and an unpleasont aroma arose from the ground. No more rain fell till the night of the 26th, and on the 27th, when there were copious showers, and the wind came from the north, producing a sudden diminution of temperature, which was detrimental to the sick. The epidemic was decidedly checked by this change of temperature, as there were few fatal cases after it. However, the weather became warm again, and continued so until after the 15th of October; nor was there much rain during this time. On the 18th and 19th of October, the weather became cooler, the thermometer ranging from 70 to 84 degrees, when on the 19th it fell, in the course of six hours, nearly 22 degrees, and we had frost that night. On the 20th of October, there fell much rain, especially at night. During the height of the fever, all who could leave town, did so; many houses were tenantless, either from death or flight, it was seldom any were seen in the streets, except to attend a funeral, but now (21st October) many ventured back amongst us, and life returned where so long had been silence, gloom, and wo!

Perhaps it may not be improper to state, that fruits were scarce during the summer. There was an abundance of plums, but peaches were a rarity; and, owing to the wet season, melons were scarce; apples are not cultivated generally here.

Having thus succintly stated the condition of the town, the atmosphere, and the people, I shall proceed to the

Symptoms of the disease; Although this may well have been omitted, still, I think, the report would be incomplete without it. The first indications of an attack were a sensation of langour and debility, repugnance to any kind of exercise; chilliness, gaping, stretching; flushes of heat; severe pain in the head; nausea; severe aching of the back, especially of the lumbar region; of the extremities, especially the joints; sometimes cramps of the limbs; spasmodic movements of the arms and legs; burning of the eyes and a flow of tears; full strong, bounding pulse from one hundred to one hundred and forty. A majority of the attacks came on at night, between ten and three o'clock; and I think these cases were more rapid in their progress than those which came on in the day.

The tongue was always coated with a thick fur of various shades, from ash to brown, a brownish yellow being the most usual color. This organ was also, sometimes, covered with a coat. which, when touched, felt precisely like that of the cow, or cat; it was very rough. There is only one case of paralysis in my knowledge as a sequela of the fever, and that is in a negro man who had been engaged often in digging and repairing wells in town. He lost all power of motion in the inferior extremities during the attack, but by vesicatories, unguents and frictions, he has been partially benefitted, so that he can now walk a little, by using two sticks.

The case of Dr. Proctor was notable for large abscesses on the fore-arms.

The majority of cases were of the continued type, lasting from three or four days, when it either ceased or assumed a malignant character; there were some of a remittant and others, of an intermittent type. In my own case, the fever lasted four days without any material diminution, when, by the use of quinine, it left and never returned. Black vomit came on from the third to the fifth day, seldom earlier or later.

I have nothing worthy of notice as regards the period of *incubation.* Some were attacked with the fever in the course of a few hours, after exposure to the miasm, and I know of more cases than one which were seized nine days after being in town.

Great thirst; clamminess of the mouth; sordes on the teeth and gums, and when the mouth was closed the teeth seemed almost cemented by the viscid matter. The *breath* was remarkably offensive from an early stage of the attack; for a healthy person to inhale it, frequently produced nausea and vomiting.

Stomach.—There was much gastric irritability in a majority of cases, even from the commencement of the attack, and sometimes great quantities of bile were discharged. This irritability was difficult to manage, and there were many cases where it never was subdued. There was complete *globus hystericus* in some female patients.

Intestines.—In the majority of cases the bowels were easily acted on by cathartics, and in fact, in some, the discharges were spontaneous, copious and debilitating, requiring medical interference to arrest them.—Constipation was rare, and I heard of only two cases in which it was obstinate.

Kidneys.—These acted very variously in different patients, and even in the same case at different times The urine was almost uniformly highly coloured, and in fatal cases became of a dark and muddy hue.—Hæmaturia occurred in many cases; when it was discharged it produced burning of the urethra, especially in females. In many cases there was suppression of urine, requiring relief by cups, leeches and diuretics.

Uterus.—This organ was, in some cases, the immediate cause of death from excessive hæmorrhage. It was dangerous for women to have the yellow fever during the existence of the catamenia, as it was so apt to run on to excessive menorrhagia.

Liver.—Sometimes the liver was greatly engorged, enlarged, and hard to the touch; sometimes slow in its secretory action, or torpid; but for the most part there was no lack of bile.

The Spleen sometimes was engorged and swolen, attended with pain and tenderness.

Skin.—The perspiration was very variable and irregular in its appearance. In most of the cases the skin was pungently hot, dry and harsh. If perspiration appeared, it was only for a short time, soon drying up and leaving the skin in its former condition. There was, in my own case, and I observed it in several others, a chilly sensation, succeeded by warmth, and that by perspiration, all perhaps in half an hour; and this would continue alternating for two days or longer, passing through all the stages of fever, *in miniature*, more than a dozen times a day, the pulse at 100 all the time. In the case of Wm. Smith, which terminated fatally, during the fever there was a herpetic eruption on his arm as complete as any I ever saw.

Parotid Gland.—I know of two cases of this gland being the seat of the most violent inflammation, viz: in the young man named above, and in Mr. G. Rivercomb, who was attended by Dr. Brown; the glands suppurated in this last case and were punctured. As the cases advanced and assumed a grave aspect, the tongue was clean at the point and edges; sometimes conical and pointed, at others, flat and flaccid, the patient protruding it with difficulty and allowing it to remain exposed; sometimes it trembled and fluttered like a leaf; these were always premonitions of death.

Therapeutic Agents.

I shall endeavor to give a fair statement of the plans of treating the fever, and the results of cases, as far as I am able, and commencing with the most important, I shall take up

Venesection.—This was the herculean remedy in the management of the yellow fever. Few cases recovered where it had been neglected, and for success to attend its employment, it was necessary to resort to it early; the first twenty-four hours was the time specified. For my part, I preferred the first ten hours; as there were cases where bleeding had been used in twelve hours, which nevertheless proved fatal. To prove serviceable, a free bleeding was required, and if necessary, to repeat it; we seldom subjected ourselves to the cramp of ounces or measurements, but were guided by the impression made on the pulse. At the first appearance of the epidemic, some of our physicians were opposed to the abstraction of blood in this way, but the fatality which attended their practice soon disclosed the error, and when they commenced the employment of venesection freely, "victory perched upon their lance." In cases of most intense agony and suffering, pulse bounding and throbbing, heart laboring like a curbed steed, and the patient rolling and tossing from side to side, and it seemed that to relieve him required superhuman skill and power, by the touch of the lancet all pain was relieved and calmed as it were by magic, and the fever would shortly vanish, never again to return. *Vesicatories* were used in almost every case, in order to relieve the bowels or brain, and prevent fatal inflammation. In many of the fatal cases these surfaces became the sources of slight hæmorrhage, or were blotched with echymoses. I believe every case, where blisters were applied to the abdomen, was troubled with strangury.

Cups and Leeches.—These were the next most potent agents employed, and when freely used, answered an admirable purpose in eradicating local congestions, pains, or excitement. When they were resorted to for the purpose of removing or lowering arterial excitement, they were applied to the dorsal region of the spine, and were satisfactorily efficient when re-applied often and long enough.

Baths.—These were so seldom employed, and in fact so difficult to be had, that I can say but little concerning them. Early in an attack, a tepid bath assisted in lessening the fever, but when they were used as a means of stimulating the patient in the latter stages, they proved abortive, if not deleterious, by heightening the debility.

Cold affusions were very grateful and beneficial in the stage of high febrile excitement. When there was great determination to the head and brain, (which occurred in nearly every case,) I often opened the temporal vein or artery. In one case, where the patient was nearly distracted with the cephalalgia, on opening this artery, he fell asleep while the blood was running. Every one experienced great relief from this practice.

Mercurial preparations, in which list the *submurias hydrargiri* held the most conspicuous place. This grand remedy, which has received the praises and gratitude of myriads and the censure of hundreds; which has cured its millions and punished thousands for a reckless misuse of it; which has acted the most conspicuous part of any therapeutic agent for the last century; which can be administered by persons of the least as well as of the greatest intellect; by the quack as well as by the scientific physician; this remedy, I say, was the grand lever which rescued hundreds from impending death, and, mayhap, which caused either immediately or remotely the destruction of a few by its abuse. Let us ever scan minutely the nature of disease and justly weigh the power of our remedies, and not employ them rashly to the injury of our patients and the discredit of our profession. There is no medicine which has been so misused, so imposed upon, as calomel; and certainly there is none which can be given in such variable doses and with so great impunity as this. While by some *ten grains* is considered an ample dose, sufficient for all ordinary purposes, by others it is looked upon as a mere nothing, and from *forty* to *one thousand* grains are administered with as much confidence and *sang froid* as though it was magnesia or gum arabic. Where is there another remedy which can be so safely abused as this? Can we give opium in this way? Will the human system bear quinine in this manner? Would we thus play with antimony, or cantharides, or any other known powerful agent? The unanimous response is, *No! we dare not!* And still there is a great hiss and cry against calomel as a poison, a monstrous scourge, an execrable vampyre lapping the blood of thousands, a hideous hydra roaming the earth seeking whom it may devour and sapping the very life strings of the human race! leaving desolation, loathsome disease, misery and death in its path! When the truth is, the medicine is innocent, and the physician should bear the odium. There is no medicine which can boast of as many trophies as this. The whole nosological catalogue has bowed in submission to it. The most simple forms of disease and the most complex, the mildest and the most malignant have all found a conqueror in calomel. In the management of yellow fever in the early stages, after the due abstraction of blood, it acted a noble part in relieving the alimentary tube of its offensive ingesta, restoring tone to the liver and other viscera, and proper action to the skin. I never gave more than twenty grains at a time.—Sometimes it acted as an emetic first, causing the ejection of immense quantities of dark green bile from the stomach.

Blue pill was an important adjuvant as the case advanced; and when there was a torpor of the bowels, the combination of colocynth answered an admirable purpose. It was observed, I think, by every physician that ptyalism was more easily produced than ever known in common bilious fever. This proved very troublesome, as hæmorrhage was very apt to supervene, which was obstinate, and detrimental to the patient, if not fatal. In a majority of cases, *castor oil* acted unfavorably, by inducing or aggravating gastric irritability. It produced great distress in many; the most oppressive, enervating sensations before the cathartic effect.

Emetics were eschewed; the irritability of stomach contra-indicated their use.

Serpentaria Virginiana was slow and uncertain in its action, liable to increase the gastric irritability, and consequently was not much used.

Spts. Æth. Nitrosi was a much preferable remedy, and acted more advantageously than any of the diaphoretics.

Seneca.—This I used with marked advantage in some cases, when there was great debility and prostration. There was one case in particular of a little boy about five years old, in which the fever was very obstinate, and in which I experienced marked benefit from its use in the following formula:

℞ Contus. Rad. Senegæ, ℥ ss.
Sulph. Magnes. ʒ ii.
Aqua bull. Oi.

After the infusion had cooled, ʒ f ii. were given every hour or two.—This acted on the intestines and skin gently, and imparted tone to the general system.

Sulph. Quinine.—This was considered a hazardous, a dangerous remedy, by the most of the medical gentlemen here, in the treatment of the epidemic, because it produced, or aggravated, gastric irritability. I know in some cases which I treated, it did act thus when given *alone*, but by combining it with other remedies this difficulty was obviated. There are many medicines which, taken alone, act unfavorably; but when associated with another, the most advantageous results are accomplished. We all are well aware that by thus combining medicines, we can direct, or divert, their force to or from any organ or set of organs, and act with double or diminished energy, as may be desired. For instance, in the action of what are styled emmenagogues, this truth is palpably evident, and we observe that nearly every formula, or recipe, which is prepared to increase the action of the uterus, has *aloes* in it, because it is known to spend its force on the rectum and inferior portion of the colon, and by this medicine giving a direction to the balance, we obtain the combined action of the whole. And thus it was I addressed myself to the direction of the action of quinine; and I combined with it the Spts. Æth.

Nitrosi in order to direct its force to the skin and kidneys, in the proportion of ʒ i. to each grain. I found difficulty in only one case, and that was obviated by administering the sup. carb. sodæ immediately after taking each dose of quinine, which quieted the stomach, the medicines were retained and the patient convalesced rapidly. Some of my patients objected to the use of quinine at first, but were forced to acknowledge their error after taking a few doses of it thus combined.

Another combination which I employed was the following:

℞ Sulph. Quinine, ℈ ii ;
Lupuline, ʒ ss ;
Ol. Pip. Nig. ʒ f i ;
Tinct. Opii, ʒ f i ;
Gum Arab. q. s. ft. pil. 40.

I usually gave one every hour, or two every two hours, as the urgency of the case demanded. They uniformly acted well when preceded by venesection and cathartics, producing gentle perspiration, composing the system, and imparting tone to the stomach. *Huxhum's tincture of bark*, was employed by some of the physicians in the stage of the convalescence. Many other tonics, stomachics, stimulants, &c., were employed as the attending circumstances of different cases seemed to demand, but which it is not necessary to mention here.

Post mortem appearances. I am extremely sorry that people are so much opposed to the opening of the bodies of their friends, as there are thousands of rich facts and stores of knowledge thereby withheld from us and from the world. Such a repugnance prevails here against this practice, that it is seldom that ever the meanest servant's body can be examined, no matter what disease may have existed there. There were numerous efforts made by myself, and others, to obtain the liberty of a *post obit*, but there was only one granted, to Dr. A. C. Holt, in the case of a little negro. I hope physicians will all unite in an effort to subdue this repugnance.

The cause and origin of the Epidemic.

It is with some reluctance that I enter upon this part of the subject, as I have been preceded by Drs. De Valetti, Logan, and Stone, and there are errors in their statements which it devolves upon me to correct, in justice to myself. Drs. V. and L. acknowledge errors in their article, and they are pardonable, as there was so much confusion and distress at the time of their visit, that it was next to impossible for them to obtain a full and satisfactory account. Before their report appeared in the New-Orleans Medical Journal, I published a short letter in the New-Orleans Picayune, stating what I then considered the origin of the fever; I was ridiculed by many for this opinion, but subsequent research has borne me out, and those who once laughed at my statement, have since found that I was not far from the truth. I should have imparted that information at the time to Drs. De Valetti and Logan, had it not been for the reasons that I knew the Journal was ready to be published, and their report was in it; and, furthermore, there was much doubt and speculation abroad as to whether it was yellow fever we had here, and how it originated. The fact of there being no epidemic in New-Orleans, or on the river, was considered as proof demonstrable that the yellow fever could not be here; or, if it was, that the question was now settled as to the spontaneous production and local origin of the disease. I had been of the same opinion, till I heard the facts as I recorded them in the Picayune ; if I had known them when Drs. De Valetti and Logan were here, I should most certainly have imparted them to them. When I was first satisfied that the disease was yellow fever, I entertained the belief that it was of local or spontaneous origin, and was pleased to think that that question would be so completely settled in my day, yea, under my own observation. And I hope that the facts in regard to this epidemic, will have a powerful agency in settling the question in favor of the other side, i. e. of transmission, infection, tropical origin, &c. I think the facts are conclusive and irrefragable, myself; and if I understand Dr. Logan's language, (which seems a little obscure or non-committal), on p. 529, he has been converted to this side of the question, even by the statement of Dr. C. H. Stone. There are many valuable facts in Dr. Stone's article, and some of them have materially helped to establish more firmly my opinions on the subject ; there are some very important ones which I was ignorant of till its appearance.

The reader will bear in mind my account of the condition of the town, the atmosphere, and the people, at the time of the appearance of the first cases of fever, or at the 15th of July, viz: much filth, moisture, ploughed streets, excessively warm weather, hot sun. &c.; all of which were calculated to prepare the human system for the reception of disease ; and I am satisfied if the germ of the yellow fever had not been brought here, there would have existed much severe sickness of other types. Epidemics are worse in large cities when there has been an unusual influx of strangers, upon whom the disease vents its wildest fury ; and here was a population of nearly 900, only a few of whom had had the yellow fever. It will be borne in mind, too, that the fever raged as an epidemic in Galveston, Texas, and that it appeared there early. Mr. Thurber says, so many were falling sick on the 6th, 7th, 8th and 9th of July, as to be remarked by several. This gentleman left Galveston on the 9th of July, arrived in New-Orleans on the 11th, and in Woodville in the afternoon of the 12th, thus being only three days from Galveston ; he came from Bayou Sara on the cars, in company with Mr. Shaw, an old friend of his.—He took his lodgings with Col. J. S. Lewis, near the rail road depot, whose house is a large commodious brick building, in a beautifully shaded yard, and in a thinly settled neighborhood in the south-east part of the town. Mr. Thurber occupied rooms up stairs, in the front part of the house, with large windows opening on the front; there is a portico to the house on the east side, where the family mostly sit to spend the afternoon and converse with their visiting friends. Mr. Thurber says he was taken sick in the night of the 15th July, but felt unwell before going to bed, and on the Sabbath night preceding the complained, while in the pulpit, of indisposition. Dr. Stone, on p. 532, says, Mr. J. S. Collins visited Colonel Lewis' house on the 15th, and wishes hereby to show that the infection could not have spread from Mr. Thurber before the development of the fever. I think it necessary to dwell on this point, as it may be considered important by some. Well, Mrs. Lewis says, she is of the impression that Mr. Thurber was sick at the time of Mr. Collins' visit, and that she, her sister, and Col. Lewis, sat on the porch for half an hour, or more, in conversation with Mr. Collins. The object of Mr. Collins' visit was to purchase some corn, which he received the next day from Col. Lewis' overseer, and the entry is made, by the overseer, of the 16th July. Mr. Collins says he was taken *that day* (that is, the day he received the corn) with premonitory symptoms of fever; and he applied to me for medicine *the next day.* Dr. Stone, now, has put down the date of Mr. Collins' illness on the 16th from that fact, and the fact that I stated, that I was called on to attend Mr. Collins on the 19th July, and that he then *had been feeling unwell for a day or two.* After reflecting on the matter, and recollecting attendant circumstances which had been forgotten, I have come to this conclusion: that is, that the date of the entry of the corn, by the overseer, was wrong, *by one day*, and that my statement to Dr. Stone, of the correctness of the 16th as the day of Mr. Collins' premonitory symptoms, is wrong, for these reasons: on examining my books more closely, I find medicines charged to Mr. Collins on the 18th, which he came *himself and purchased*; and then, my first visit is dated the 19th July, when his case was fully and severely developed; so I conclude that he must have visited Col. Lewis' house on the 16th, had the premonitory symptoms on the 17th, called on me for medicines on the 18th, and on the 19th he was very sick, and required active treatment. It might be asked though, by some, how could the halitus from Mr. Thurber's room, above stairs, affect the air in the porch? Mr. Collins' visit was late in the afternoon, when the moisture and density of the atmosphere might very well favor the transit of the miasma; and besides, Col. Lewis had been in his room to attend to the wants of the sick man, and the halitus might easily be carried about, for so short a distance, in the clothes of Lewis, and other members of his family, and diffused into the air around. However, the case of Mr. Collins does not materially affect the merits of the question, as it is admitted on all hands, that Mr. Thurber was *the first case* in town, and it is demonstrable, by the list of cases furnished by Dr. Stone, that it spread from him, and attacked those who visited the family. The case of Mr. Shaw is in point—he had visited Mr. Thurber the first day or two of his illness, when he was taken, and nearly died at Mr. Thirrell's boarding house, where there were many persons collecting, and passing every day into all parts of the town. There was another person who visited the house of Col. Lewis, (Miss M. Hester) and who was taken shortly after and came near dying; also, Mrs. Kennear, who was sick for some time; she, however, remained in the house till she was restored to health. There were some other cases, in the same neighborhood, at Mr. Posey's and Colonel Hamilton's, in persons who had visited the family of Col. Lewis about this time. And I would further state here, for the benefit of persons at a distance, that Mr. Simrall, Mr. Posey, Col. Gordon and Col. Hamilton, are all next door neighbors to Col. Lewis, between whose families there is constant and promiscuous visiting. As to the case of the negro girl of Mr. J. S. Collins, upon whom I attended, and who was first taken sick at Judge Gildart's, and upon which Dr. Stone places some stress, (see p. 536) I will satisfactorily clear up, by stating that she visited her master's family on Sabbath, while her master was very sick, and waited in the room; and assisted in nursing him some time during the afternoon. Mr. C. was taken down on *Friday*, the 19th; Saturday was the 20th, and *Sunday* was the 21st, and here is where she imbibed the germs of disease; so that the difficulty of the "north-west wind" is removed, and those long "800 yards" are "dwindled to the shortest span." In the case of Mrs. Lewis, it was not necessary for her to see Mr. Thurber, for his nurse, Alfred, was very sick, (and was taken before Mack and Mary) upon all of whom she attended as physician and nurse.

In the list furnished by Dr. Stone he seems to wish to fix each individual to one spot, so many yards away from Thurber, and to forget that in a place, the size of this, where nearly every body is acquainted if not familiar, there must of necessity be much intercourse in various ways. He seems to endow Mr. Thurber with an exclusive patent for yellow fever, and if a person has not visited *him*, that they could not possibly contract it elsewhere; although there were several other cases in distant parts of the town who had obtained privileges of discrimination from him (Thurber). I would further state here, that Mrs. Lewis thinks she contracted the yellow fever in 1839, in Fort Adam; as she was seized there with violent fever after landing from a steamboat coming from Cincinnati. She entered a house in which persons had died of yellow fever, and a gentleman who was in the same room, was taken and died in four days with black vomit. This may account for her not being sick earlier this time. Col. Lewis had had the fever twice before, many years ago. Mr. Wailes' family is another good example. While Mr. Simrall was sick, he sent his negro boy there to assist in attentions about the family, and he was taken down with the fever in a day or so, and in a few days his other servants and children were nearly all down in like manner; no case, however, proved fatal. It may be well to state that Mrs. Simrall and Mrs. Wailes are daughters of Mrs. Newell, and hence the intimacy between their families; and this may throw some light on the case of the boy Moses, who was Mrs. Newell's coachman, and was frequently at both places.

There were numerous instances of persons coming into town and remaining a short time, who were very sick in a few days in the country; some in fact who passed through town with very short delay, were nevertheless attacked. Servants sent in all haste into town for a physician, were almost invariably seized with the fever in a few days. And we all know how much negroes in the adjacent country are in the habit of visiting town clandestinely at night, and if there was any apprehension of censure or punishment the best of them will deny it, if questioned with regard to it. And it is probable that many cases which occurred of this character, may have been thus exposed to the infection. Moreover, as the wind during the worst part of the epiemic came from the E. N. E. may it not have had some agency in producing those remarkable cases of fever in the western and south-western portions of the country.

Thern was one case which I think worthy of notice, and that was Mr. S. B. Leatherman, who visited a sick friend in the country, when he was rather over the worst of his attack, and *shaved him*, and in the course of a day, he himself was taken very sick with all the symptoms of yellow fever, and was with difficulty saved. While on the other hand, here thise case of M. M. Hester, who was in the town nursery, attending and setting up at night with the sick, and yet his symptoms were so slight as not to oblige him to take his bed.*

Dr. Stone seems to be mistaken as to the time of the visit of Mr. McCausland to Mr. Stockett. Mr. Stockett had been in town on the 3d of September, was attacked on the 7th, and during his illness, Mr. McCausland visited him more than once, and he himself was taken down on the 18th September. It may be that his visit to town may have sown the germs of disease, and his visit to Stockett caused them to be developed.

As to the appearance of cases of yellow fever in Natchez, I would state there were several persons who went from here there during the epidemic, and were sick there. One young lady left my house about the 12th of September, went to Natchez, was attacked with the fever, and was attended by Dr. Cartwright; and a young man from the eastern part of this county passed through Woodville on his way to Natchez, was taken there, and attended to at Dr. Davis' Hospital. And Dr. C. and Dr. D. both told their patients, in their convalescence, that they had had yellow fever. And it will be borne in mind that a large company of gentlemen left here on the 4th of September, at the worst time of the epidemic and went to Natchez, some of whom were taken sick on the way there; one died in Natchez, and others sickened on their return to Bayou Sara.

The first deaths which took place here in town were Miss M. Parten, on the 16th August, and Mr. James Radford on the night of the 17th. The young lady turned very yellow a day before she died, the young man became so shortly before death, and discharged black vomit in considerable quantities a few minutes before dissolution; after death he turned very yellow. There were no more deaths from yellow fever then till the 4th September, it is true Mrs. Stanley died on the 26th August, but she had a chronic disease of long standing. There were other deaths about the same time, as old Mr. T. Laud, 26th; Mrs. Davidson on the 28th; a child of P. W. Farrar on the 26th; T. Rollins on the 29th from dissipation, and J. Argus on the 3d September, of consumption.

Another fact which has been noticed by old writers on epidemic yellow fever was, that the *prisoners* in the jail escaped; not one had the fever; while every member of the turnkey's family were sick, and he himself came near dying. The jail is enclosed within a brick will 12 feet high and 80 by 125 feet square; the prisoners are confined in the upper story and the jailors family lived below.

There were more cases amongst males than females, and certainly a much greater number of deaths, which probably was owing to the exposure of the former class to the sun, and other causes, which the latter were exempt from. I am informed by Mr. J. H. Stanwood, that on his return from the North in the month of December, he opened a closet containing some woolen goods which had been placed there the summer previous, and that on opening them he became suddenly nauseated, and was so sick for a day or so as to be forced to the use of active medicines to restore his health. There had been patients sick of the fever, during the fall, in the same room.

No case recovered after the appearance of black vomit. Mr. Miree

(No, 83) lived three days after its supervention, and No. 84 lived three or four days. Hopes were, at one time, entertained of ultimately saving both cases.

There were some persons of my acquaintance who were in bad heath at the time of being seized with the yellow fever who, after recovery, were reinstated in robust health. It seemed to give such a shock to the system as to remodel it and impart new vigor by removing all germs of other diseases.

The case of Mr. J. Keller, (No. 89) is worthy of notice. When the epidemic was raging most violently in September, he removed from the town with his family, some of whom had been sick. After a few days stay in the country he was taken with *a fever* from which he recovered imperfectly. After several frosts he returned to his house, about the first of November, which had been partially occupied by two young gentlemen during his absence. He, with his family, had been in town five days when he was seized again with fever, and after seven days of illness died with yellow fever.

Another instance worthy of notice occurred in the person of Mr. J. M. Baily. He had the yellow fever early in the month of October, had recovered and on the 23d of November went in the woods gunning; retired to bed early after supping lightly; in the course of the night he became nauseated and vomited freely, and went to sleep again, when just at the dawn of day he was heard breathing heavily by Mrs. B. who instantly sprang to his assistance, but he died before any thing could be done to relieve him.

The case of Mrs. G. (No. 80), was somewhat similar. She had recovered from the fever and was preparing, during the night of the 9th October, to depart next morning on a journey. The nights were quite cool; she arose before day and walked out into the air and was returning to the fire when she said she felt chilled through, but had barely uttered the expression when she fell and died in the course of a few minutes.

I know of but one case of ascites which can be properly said to follow, as a sequel to the yellow fever.

After deliberately examining the facts attending the appearance and progress of this epidemic, I have come to the following conclusions.

1. We should have had a year remarkable for the amount of sickness and the severity or fatality of the cases even if the germs of yellow fever had not been brought amongst us.

2. If there had not been such an accumulated number of causes for sickness, i. e. moisture, elevated temperature, filth, ploughed streets, stagnant water and unwholesome winds, even if the germs of yellow fever had been introduced in our midst we would probably have escaped an epidemic.

3. We had a notable combination of causes for disease, and at a favorable time for the germination and propagation of disease the seeds were deposited in the nidus, and a fearful progeny sprang forth, which, with relentless fury, spread misery, desolation and death.

May, 1845.

A LIST, as complete as could be obtained, of all who died from the 2*d August to to the* 14*th November*, 1844, *with their dates and diseases. Those who died of the epidemic will be marked Y. F.; those who died in the country will be marked C.*

	Names.	Dates.		Diseases.	
	Dr. S. Bradford	2d	August		c.
	Miss M. Parten	16	"	Y. F.	
	James Radford	17	"	"	
	F. Laud	26	"	Bil. Rem. Fever.	c.
5	Mrs. Stanley	"	"	Chron. Stepatitis.	
	P. W. Farrar	"	"	Conges. Fever.	c.
	Mrs. C. L. Davidson	28	"	General decline.	c.
	T. Rollins	29	"	Drink. Apoplexy.	
	Jos. Ayres	3d	Sept.	Phthisis and y. f.	c.
10	Rev. H. Beach	4	"	Y. F.	
	Garret Stone	"	"	"	
	J. Sims	"	"	"	
	H. Connell	5	"	"	
	N. McKenzie	"	"	"	
15	J. Riley	"	"	"	
	R. Singleton	"	"	"	
	R. Richardson	"	"	Fever, Child.	c.
	F. Keller	7	"	Y. F.	c.
	C. Lancaster	8	"	"	
20	S. Haas	"	"	"	
	J. Moore	"	"	"	
	W. E. Draughton	"	"	child.	c.
	Dempsey	9	"	"	
	E. G. Binning	"	"	" girl child.	
25	Mrs. A. Oswald	"	"	"	
	L. D. Brown	10	"	"	
	B. Ferguson	"	"	"	c.
	Judge J. Walker	11	"	"	
	Wm. Parker	"	"	"	
30	Mrs. A. L. Hubbard	"	"	"	
	Mrs. C. Bryant	12	"	"	
	Jane Conner	"	"	child.	c.
	L. W. Hudson	"	"	" died in Natchez.	
	C. M. Stewart	"	"	" Bayou Sara.	
35	J. Schlachtberger	14	"	"	
	W. M. Williams	"	"	"	
	P. H, Cotter	"	"	" child.	
	H. B. Scott	15	"	"	c.
	Miss A. Staumps	"	"	"	c.
40	J. Bergen	"	"	"	
	G. L. Boland	"	"	"	
	F. H. Binning	"	"	" Child.	
	Mrs. E. Stockett	"	"	"	c.
	R. D. Holt	16	"	"	

	Names.	Dates.		Diseases.	
45	R. McCulloch	16	Sept.	Y. F.	c.
	Miss M. Brown	“	“	“	
	J. J. Yates	17	“	“	
	Miss C. Lawrence	“	“	“	
	M. Combs	“	“	“	c.
50	Uleman	“	“	“	c.
	D. Brown	“	“	“	
	Mrs. Barnes	18	“	“	
	Peter Courad	“	“	“	
	M. Scott	“	“	Cong. Fev., child,	c.
55	S. W. Foster	19	“	Y. F.	
	M. Newman	“	“	“	
	F. Woll	“	“	“	c.
	C. Simon	“	“	“ girl child.	
	M. Simon's *infant*	20	“		
60	Wm. Binning	“	“	“ child.	
	Wm. Smith	“	“	“	
	J. Kann	“	“	“	
	Dr. J. M. Currier	“	“	“	
	Mrs. Sarah Scott	“	“	Cong. Fever,	c.
65	Wm. McNeely	26	“	Y. F.	c.
	Wm. King	21	“	“	c.
	Miss R. Paaret	27	“	“	
	G. L. Estes	30	“	“	
	J. W. Smith	“	“	“	
70	R. Phipps			“	c.
	Mapes			“	c.
	G. L. Baldwin			“	c.
	J. Lanahart			“	c.
	R. S. Morris	1st	Oct.	Inf. Remit. chiid,	c.
75	H. C. Broom	2	“	Nephritis, g. child,	c.
	T. S. Herbert	3	“	Cong. Fever.	c.
	Dr. H. N. Martin	4	“	Y. F.	c.
	D. Conner	“	“	child,	c.
	Miss M. Pea	10	“	“	
80	Mrs. N. Griffin	“	“	Apoplexy,	c.
	Geo. Martin	11	“	“	
	B. C. Bryant	14	“	“	
	A. S. Miree	17	“	“	c.
	T. L. Johnson	“	“	Y. F.	
85	Child of J. D. Kaigler	20	“		c.
	Ms. C. F. Kaigler	22	“	“	c.
	McNeely	1st	Nov.	Ascites.	c.
	J. Dillahunty	“	“	Y. F.	c.
	J. Keller	14	“	“	
90	B. D. Ogden	“	“	Hepatitis.	c.

There were other deaths besides these from the epidemic, and other cases in the country, but I had no opportunity of ascertaining correctly their names and dates. In addition there were several *negroes* in town and on the adjacent farms. I have a list of 27 only, although there were certainly more than these.

In the above *table* those marked Y. F. C. had been in town, and some in attendance on the sick. Some of the dates I expect are incorrect, and some I left blank as I was unable to approximate nearer the time than merely the *month*. All the females and children are specified. In the list there are only 19 females, and 14 children of both sexes, the remainder were from 15 years up to 80. Adding the 27 negroes to the 90 whites, we have 117 deaths in three and a half months; 36 died in the country. In August, 8 died; in September, 65; in October, 14, and 4 in November, 72 being the victims of the epidemic, and with the 27 negroes make 98 in all.

The vast disproportion between the number of deaths in the two races is worthy of notice. I believe the negroes were no more exempt from an attack than the whites.

PART SECOND.

PERISCOPE OF PRACTICAL MEDICINE; OR SPIRIT OF THE MEDICAL JOURNALS, FOREIGN AND DOMESTIC.

I.—*Modern Improvements in Surgery.*—We make the following extracts from a very able Lecture introductory to a Course on Surgery, in the School of Medicine, Park street, Dublin; by John Houston, Esq., M. D., M. R. I. A. Delivered on the 4th Nov., 1844. The author gives an interesting review of some of the most striking improvements of modern times, and displays a laudable familiarity with the present state of medical science.

"To render fully intelligible the modern improvements in surgery, it would be necessary to begin by demonstrating the modern improvements in anatomy and physiology, on which, especially, many of the former are founded; but whilst I cannot here stop to do more than allude to one or two, in illustration of the bearing of such investigations on practical surgery, I may take the opportunity of apprizing you that there never was a period in the history of the profession in which an exact knowledge of the structure and functions of the body—a knowledge to be obtained only by the combined aid of chemistry and histology—bore so directly on medical theory and medical practice. The retort and the microscope are now becoming as much the pocket companions of the practitioner as the lancet and the stethoscope: and it is curious to observe how the up-hill tracks of the stethoscope to public favour, in former days, have shadowed out that through which the microscope is now destined to wind its tardy way.

Recent researches in the physiology of the blood, in connection with the process of inflammation—that morbid condition which bears so important a part in all organic lesions—have thrown much light on disease.

The great mind of John Hunter saw and believed that the blood possessed in itself an independent life, even while circulating loosely in the bloodvessels; but he knew not the nature and seat of that vitality. The discovery was reserved for the physiologists of our days. There are particles termed globules floating in this liquid, about the 3000th part of an inch in diameter, or so small that myriads of them are contained in a single drop. It has been ascertained, respecting these globules, that they are, each and all, endowed with a definite and uniform shape, and with a development, in virtue of which they pass, by successive transitions, from a condition of origin to one of final evolution—a veritable organization, in other words—properties which give them a claim to the title of life as much as those which justify the application of that term to the ovum from which proud man himself dates his being. The atomic particles of which the blood is composed being thus individually alive, collectively, they form a mass, of which it may literally, as well as allegorically, be said, "For it is the life of all flesh; the blood of it is for the life thereof." "For the life of the flesh is in the blood."

But there is still another discovery which has been of late made, and which promises to be of great value in organic physiology—viz., that there are at least two kinds of globules in the blood, one yellow or red, the other transparent or white, and which differ from each other essentially, both in form and organization. The uses of these particles respectively, although involving some great and important functions in the animal economy, are not yet known with sufficient exactitude to be enunciated as ascertained facts in physiology. I may state, however, as a prevailing opinion, that the red globules, which consist each of a vesicle holding a ferruginous fluid in its interior, are the receptacles and carriers of the oxygen which is destined to kindle and maintain the excitability of the different organs of the body, in the way that the oxygen of the atmosphere, propelled upon heated coals, raises and keeps up a flame; whilst the white globules are exclusively and essentially nucleated cells, floating stores of living elements derived from the food eaten—the true pabulum vitæ destined for the nutrition and growth of the frame. In furtherance of this latter view, Mr. Addison, of Malvern, has instituted experiments which show that an animal structure, bearing all the characters of cellular or rather fibrous tissue, may be formed, synthetically, out of the contents of these cells, by the addition of an alkali, and he ventures on the moral position that some analogous amalgam may take place in the growth of animal tissues from the blood, both in health and disease. Now, without offering any opinion on that part of the theory of Mr. Addison which has reference to the exclusiveness of the white globules in this office, I may state that I concur in his statements respecting the fibrillation of their contents by the action of a dilute alkali, and their fecundity in the production of granules. And I will even go further with him and say, that those granules are themselves, each and all, possessed of an independent life. I have repeatedly watched them, and have shown them to others, when burst from their cell-membrane, performing sundry independent, and apparently voluntary, evolutions in the field of the microscope, until, to the eye, the whole looked like a moving mass of creeping things. In this view, then, the blood is doubly alive, as exhibited—first, in its forming and taking part in the repairs of the animal machine, and, secondly, in the independent movements possessed by the ultimate particles of its matter.

The red globules are small and pliable, and glide with a facility which partakes almost of a repulsion, through the fine capillary tubes of the vessels, whilst the white ones, of a size nearly one-third larger, round and determinate in form, lag slowly along, as if influenced by a sort of prospective attraction to their walls.—You can observe, in the vessels of the web of the frog, under the microscope, this double current—viz., one of red particles running in the centre of the stream, and another of white ones, stealing along the sides of the vessels; and if in the frog, so equally in man, the only difference between them in this respect being, that in the former, where the vessels are transparent, the blood is to be seen by the eye, whilst in the latter, in which they are opaque, the fluid is hidden from the sight.

Such being, incontestably, the manner of the circulation in health; let us inquire what it becomes during inflammation. Under inflammation, the tendency of the white globules to linger in the part is increased. They accumulate in the capillaries, and so stop up the stream that the mass of the blood actually stagnates there; and hence the origin and final manifestation of the *rubor*, *tumor*, *calor*, *cum dolore*, of an inflamed spot. These magnified diagrams of the circulation in the web of the frog's foot, taken from Dr. Hughes Bennett's new "Treatise on Inflammation," Dr. Williams' "Principles of Medicine," and Mr. Addison's "Experimental Researches," and which, particularly the first, must be attentively studied, will enable you to comprehend readily all that I cannot here stay to inform you of.

Whether this stoppage be the consequence of the white globules sticking mechanically in the capillaries, in such a way that they cannot get on further; or whether it arise from a want of tone in the blood-vessels increasing the disposition to attraction between them and the globules—which of these, I say, is the true explanation, is a point that at present engages the attention of pathologists. Be this as it may, however,, the fact, and it is a very remarkable one, is as I have stated. The part being thus laden with an over abundance of the organizable materials of the blood, its condition becomes somewhat analagous to one of excess of nutrition, and as such the state of inflammation is regarded by some pathologists. The process of adhesion in soft parts; the development of bone in connexion with bone, of muscle with muscle, &c., as the result of the inflammatory state, are all easily understood under this view.

In respect of union by the first intention—a process of great importance in reference to surgical operations—we are no longer puzzled and perplexed, by vague and fanciful notions, about vessels uniting mouth to mouth, or shooting their tendril-like tubes from one side of a wound to another. The ancients were perfectly acquainted with the fact of such inosculations, but they knew not the *modus operandi* of the process. A beautiful experiment, instituted by Troja, shows how alive was their attention to this subject. He cut across the leg of a fowl by three sections, in such a manner and at such intervals as to allow one section to be well healed before he made another, but so completely, on the whole, that no part remained which had not been divided. He then killed the fowl, and threw injection into its blood-vessels, when he found that the injection had passed as fully into the toes of the amputated as of the unamputated limb, proving, of course, that it had found a way through the vessels formed *de novo* in the recently united textures.—But Troja knew then no more of the matter than that the fact was so. Recent and valuable discoveries have, however, filled up this hiatus in pathology, and we now know that it is by a process of cell-genesis that the act is accomplished—viz. that the nucleated cells generated in the lymph exuded at the wound, and which serves as a sort of plasma or manure for their propagation, arranging themselves into all sorts of appropriate and necessary forms, are converted into textures analogous, in every respect, to those from whose neighborhood they have sprung; forming thus, in one place, blood-vessels, in the very walls of which, as proving their cell-origin, the nuclei of the cells are discernable, lying before the eye, like pieces of wood-pavement, at definite given distances, and that even long after the growth of the vessels is completed; forming, in another place, nerves; in another, cellular tissue; and so on, until a complete re-establishment of the living organized medium is effected. These newly discovered facts, in connection with others to which they naturally lead, bearing on the maintenance of a healthy condition of the cells, as well as of the plasma in which they are to be developed, must have a very important influence both on the theory and practice of surgery.

For the present improved system of treatment of many surgical affections, much is due to the *surgeons of the army*. It was in the field of battle that a practice which had existed for ages—that of dipping the stumps of amputated limbs into boiling pitch or oil, to stop the hemorrhage—was first discontinued. On one occasion, after a great slaughter in battle, and when his stock of boiling oil had run out, Ambrose Pare was obliged to leave the stumps of many of the wounded to what he considered the more unprofessional plan of wrapping them round in wet clothes, and in the expectation of finding, also, such dead by the next day. Matters turned out quite otherwise, however; for whilst those treated by the oil had been, of course, in pain, sleepless and feverish, those whose wounds were dressed simply, had enjoyed ease and sleep, and lay comparatively cool.

From this incident, the resources of the constitution in arresting hemorrhage began to be understood, and a more humane and judicious practice to be adopted. The reputation of the surgeon, at the time, became universal. The soldiers disregarded danger whenever he was present. On an occasion in which Metz was besieged, and the wounded were dying without medical aid, Pare was brought into the city. The soldiers, when apprised of his arrival, cried out, "Our Pare is with us! we have nothing to fear!" and then fought to conquer. The late Continental wars found an Ambrose Pare in every regiment. Our own Hennen and Guthrie, and S. Cooper and Ballingall, and the great Baron Larrey of Napoleon, have transmitted to us the records of the discoveries and improvements, as well as of the humanity and bravery, of their respective soldier surgeons. Civilians, even, have been found zealous and courageous enough to quit the safe retirement and practice of the domestic circle for such scenes of carnage and of danger, in order to be able to bring back to us intelligence regarding the nature and treatment of wounds, as noticed in such wide fields of observation. John Hunter, John Thompson, and Sir Charles Bell, have immortalized themselves by their devotion and services on this head.

In regard of *amputations*, the greatest modern improvement is, the frequency with which they are abstained from. When surgeons first got into the way of operating, limbs were removed without scruple, and, frequently without just cause. They would appear sometimes to have been lopped off, as if to prove how well the body could maintain its existence without them. Morand relates that in the Hotel des Invalides, at Paris, mutilated objects are in recollection, who had lost their thighs and arms, so that, unless assisted, they could not stir, and it was necessary to feed and wait upon them like new-born infants. That such a state of things has long passed away is quite true, but even within our own time still further improvements in this respect have been made, and many limbs are now saved, that, not long since, would, to a certainty, have been condemned to the knife. Sir Benjamin Brodie informs us, in the last edition of his work on the "Diseases of the Joints," that it was the practice which prevailed in his early days, of amputating white swellings, as soon as their character as such was determined, that gave him those opportunities of investigating the disease in its early stages, on the pathological facts derived from which the chief value of his book depends. Our museums in this city, likewise, bear evidence to the same practice of early amputation; and those who possess such preparations of disease will do well to take care of them, as they are not likely to get many other similar specimens from the hand of modern surgery.

The same observations apply equally to many other cases, such as diseases of the mamma and of the testis, ulcers of the legs, hernia, injuries of the head, compound fractures and dislocations, &c., all of which yield, oftentimes, to improved plans of treatment, short of having recourse to the knife. Regarding hernia, the name of O'Beirne will be hereafter associated with it, as having introduced a plan calculated to save many a valuable life. I allude to his method of drawing the gas from the interior of the bowel by means of a long gum-elastic tube; and to the efficacy of which there is now abundant evidence from all quarters.

In respect of operations, then, true surgery rather avoids than courts them; and in this respect, unlike what takes place in all other professions, the improvements introduced into it cause a diminution in the emoluments derivable from the practice of surgery. It is a well-established fact, that the incomes of medical men are much reduced from this cause, and yet, nevertheless, they persevere with laudable disinterestedness in their endeavors to effect still further improvements. Is not this the highest degree of philanthropy? In this city, more perhaps than in any other in the world, is this statement—regarding the avoidance of unnecessary operations—true. I do not hold out to you, therefore, the prospect of the exhibition of numerous surgical operations; on the contrary, I promise to you that, whatsoever hospital you may select for attendance, you will there see every judicious effort made at cure, by remedial means, before having recourse to the knife. The principle that you will see acted upon is, that to preserve a limb is better than to cut it off." * * * *

"On the subject of *Tracheotomy*, a very remarkable incident has recently occurred, suggesting new views regarding the performance of that operation. The case of the celebrated engineer, Mr. Brunel, in which half-a-sovereign entered by accident, and remained in the wind-pipe, has been heard of by all the world. The mechanical ingenuity of Mr. Brunel suggested to himself the plan of inverting his body so as to allow the coin to run out of it, as it had run into, his patulous trachea, and he accordingly made the attempt; but there was an element, a principle here to be encountered, such as is never taken into calculation in estimating the facilities and difficulties involved in the construction of locomotive or atmospheric engines, and which the medical man alone is competent to deal with—namely, a self-acting spasm, the effect and attribute of vitality, resisting the passage of any foreign irritating substance. Mr. Brunel's plan, in itself, therefore, failed, and the practical cause of its failure lay in the violence of the spasm which the coin produced in the muscles of the rima glottidis at the moment of its contact with that opening; indeed, he was nearly suffocated in the attempt. And here it was that the knowledge of the surgeon, superadded to that of the mechanist, completed the triumph of genius and art, and saved the man. Sir Benjamin Brodie made an opening into the wind-pipe close behind the point at which the spasmodic action was known to occur, partly with a view of quieting the spasm (an effect, by the way, first pointed out by my friend Professor Porter, as one of the results of tracheotomy), but chiefly to give air to the lungs by a new and artificial route, and thereby afford time and opportunity for the foreign body to get past the obstruction. Mr. Brunel's body was then, as before, inverted, and, as was anticipated, the coin ran without further obstruction from the lungs into the mouth. Never did artistic and scientific skill, combined, produce a more marked, a more happy effect; The whole world rang with applause and congratulation.

There are some of what are considered purely medical subjects, to which surgical assistance has been of late most felicitously applied. One of these is what may be termed *acute hydrothorax*, a rapid and great effusion into the bag of the pleura, and the practice to which I allude is that of tapping the side with a fine trocar, so as to give escape to the fluid. A remarkable instance of this novel and bold practice lately occurred in this city, in the practice of my friend Dr. Stokes. A lady, the subject of this affection, was in great agony, and in danger so imminent that her pulse was gone, and she had passed from delirium to coma. On Dr. Stokes' recommendation, the operation of paracentesis thoracis, in the manner just spoken of, was performed. Immediate relief was given, and the patient got rapidly well. Another instance has been related to me by Dr. Stokes, of the same affection, only more chronic in character. Here the bag of the pleura was so filled and distended, that the heart was pushed by the fluid from the left into the right side of the chest, and the ribs were driven out in the opposite direction. The same plan of treatment was adopted, and with equal success. Even during the process of drawing off the fluid, which amounted to several gallons, the lung could be followed by the stethoscope rising into its place against the ribs, and the heart could be tracked from right to left, until it had finally settled in its original position. A remarkable phenomenon attended on the sudden change of place in the parts, even although that change had been from an unnatural to a natural position. The lungs and heart became both so violently agitated by the movement, as almost to have had their functions suspended; but, in a little time, and aided by due medical attentions, they settled again—if I may so speak—to their work, and the patient got completely well. The innovation and improvement, as regards treatment, are here twofold: first, in operating at all in such cases; and, secondly, in substituting a small trocar for the scalpel—the latter change being of so radical a nature as almost to convert the practice by operation into one without operation.

The same kind of practice has been applied, by Sir Henry Marsh, to excessive *distention of the abdomen from flatus*, with the most soothing result, and without any bad effects from the operation. I knew a lady on whom Mr. Cusack operated several times in this way, in consultation with Sir Philip Crampton and Sir H. Marsh—the operation being performed with a fine trocar and canula,—and each time with much relief to the patient from the most painful suffering.

Even the cavities of the skull are not beyond reach of the exploring hand of the surgeon. Many cases have been of late recorded, and particularly one by my friend Mr. Richard Butcher, in which the fluid of *hydrocephalus* has been drawn off by tapping, and, as is stated, often with favorable results." * * *

The improvements effected of late years in the diagnosis and treatment of *syphilitic diseases*, are most important and satisfactory. Syphilis, when first introduced into Europe, three hundred and fifty years ago, raged like a pestilence. The world was paralyzed with terror at the mortality which attended it, as, when once it got into a family, the innocent equally with the guilty were in danger of becoming its victims. So great was the terror with which the Parisians regarded the disease, that, by a decree of Parliament, dated the 6th of March, 1497, all persons infected with it were ordered, if strangers, to quit the city; if possessed of a home, to shut themselves up there, so as to be seen by no one; or if citizens, without any fixed place of abode, to repair to St. Germain des-pres, where lazar-houses were provided for them, and that within twenty-four hours from the date of the order, under penalty of *death*. In the month of September, in the same year, a proclamation was issued by King James the Fourth, in Edinburg, commanding all persons so affected to repair, before sunset on the next day, to the shore at Leith, where they would find boats provided, ready to transport them to the island of Inch, there to remain until cured; and what was still a harder case, the very surgeons who took charge of such patients were doomed to the same banishment—all equally under penalty of being burned on the cheek with a branding iron. The disease has at the present day lost much of its malignancy, and although some persons die of it, such a result is not common. The difference is explained by some on the supposition of the poison being at its highest state of malignancy in its first appearance, and now naturally grown milder. This is probably true, in some measure; but it is still more due to prompt succour afforded to the afflicted, and to the perfection to which the treatment has been brought; for it is a fact, that wherever the science of medicine is most cultivated, there syphilitic diseases are the least virulent. * * *

The great superiority of the surgeon of the present day lies in his skill in distinguishing the true syphilis from simple sores, and in applying to it, and to it alone, the appropriate and necessary remedies. A discrimination between syphilis and simple primary sores was rarely, in former days, attempted. All sores in genital regions were treated with suspicion—*in dubiis suspice luem*, being the motto—and terrible injury to the health, and injustice to the character, of individuals was frequently the result. But under our present improved state of knowledge, the well-informed surgeon will never commit such errors. Whenever he determines that mercury is necessary, he knows exactly the time for the administration of the remedy, the symptoms which betoken its salutary effects, the length of time it should be persevered in, and if it disagrees, how to remedy the evil. He can bring out, in short, every beneficial influence of the medicine on the disease with the utmost exactitude, and, at the same time, steer his patient in safety past all its quicksands and dangers. This, I say, is one of the many illustrations which I have to offer of the high state of perfection of modern surgery; and I feel proud in adding, that the labours of our distinguished compatriots, Mr. Carmichael, Mr. Colles, Mr. Hewson, and Mr. Wallace, have largely contributed to such a result. I believe I am safe in saying, that to Mr. Carmichael, in particular, mankind owes a larger debt of gratitude than to any other individual, for the turn which his investigations and writings have given to the treatment of venereal diseases, and for the salutary check put by them to that wholesale and indiscriminate abuse of mercury which disgraced the medical practice of the last century.

Still farther, as regards this subject, a new and additional remedy has been of late discovered, of incalculable value; one which, however, although not, perhaps, like the mercury, a genuine antidote, nevertheless harmonizes and dovetails with that medicine in a most beneficial manner—being of service at the moment when the mercury should be stopped; acting kindly and well, sometimes, when the mercury disagrees; and often, when given at the same time and in combination with that medicine, producing its own good effects while promoting those of the mercury. I allude to the *hydriodate of potash*. Bark, and mineral acids, sarsaparilla, &c., are often given at the same time, and act variously in keeping up and improving the general health while the poison is being neutralized and eliminated from the system. With these remedies in hand, the well-informed practitioner can, with confidence, promise a safe and satisfactory issue in almost every case. And, after such assurances, if the public contine to place their ailments and their lives in the hands of the mercenary quack—the man to whom all that I have told you, and every thing else on the subject, is unknown—it may truly be said of them, that, in the midst of light, they prefer walking in darkness; that, disregarding the proferred benefits of the accumulated experience of ages, they voluntarily throw themselves back into the condition of danger of those who were banished, by royal ordinances, to desert islands, in company with medical attendants very little better informed than themselves.

A nation's gratitude is due to the medical gentlemen of modern days for the light which their researches have thrown on the subject of *syphilis during intra-uterine life*. It has been discovered that the presence of an occult taint of long-bygone syphilis is prone to be kindled up afresh in the offspring, receiving, as it were, fresh venom, by the new birth, and re-assuming, in the new being, almost all its pristine virulence. Such an infant rarely sees the light of day alive; or, if it do, it is only to propogate to its nurse, or others, one of the worst forms of syphilis. This is, with a vengeance, visiting the sins of the fathers upon the children. Now, the surgeon has not only traced all this evil to its source, but discovered for it a remedy. And here it may be truely said, that his saving hand reaches even to the unborn, staying the course of death, and arresting the spread of evil. A case of this kind, of no uncommon occurrence is the following:—A gentleman marries; he believes himself, and every one else thinks him to be, in the best of health; his virtuous wife is equally so; and every prospect of happiness is open to them. In due time, perhaps prematurely, a child is born, but it is not half thriven; it has the shrivelled skin of old age; it cries continually with a squeaking voice; and is, perhaps, spotted with some eruption. It lingers for a few weeks, or months, pining away daily, instead of adding to its growth, and dies, at last, a miserable object. Well, better hopes are entertained for the next time, and the utmost anxiety exists as the period of the birth of a second infant approaches, in the hope that *it* may be, unlike the first, perfect and healthy; but such joyous anticipations are again doomed to be blighted, for the infant, when born, wosre, perhaps, than its predecessor, may be dead, putrid, and offensive. [Drawings.] Again and again are these disappointed hopes submitted to, and these painful scenes of premature death enacted, the parents themselves all the while continuing in apparent good health. A medical gentleman, properly informed on such matters, is, at length, consulted. He questions the father respecting his past history. He finds, on a special inquiry into the point, that the gentleman, at one time, had some venereal affection, but long before his marriage, and under such circumstances that he had supposed himself perfectly cured of it. But the acknowledgment is sufficient to confirm his attendant in a well-grounded suspicion, that syphilis is at the root of the evil; and full of the confidence which learning and past experience gives, he administers, at the proper time, and with care and judgment, the appropriate remedies to both the parents, who now both require them; after which, the succeeding offspring are delivered into the world, full-grown and healthy—sources of great joy to their parents, and living evidences of the improved state of modern surgery;—I say modern surgery, because the discovery of the connexion of such mortality in new-born infants with syphilis, together with the proper application of the remedy, almost dates within the limits of the last half-century. The "Treatise on Syphilis," by Dr. Colles, contains a chapter on this subject, full of observations of high import and originality.

Gentlemen,—time will not permit me to speak of several other facts which it was my intention to have introduced to your notice—especially some brought to light by the microscope, and which promise to be of great value in practice; such, for example, as the curious and interesting discovery of Mr. Liston as to the presence of spermatozoa in the fluid of some hydroceles: that of M. Gruby, and Dr. Hughes Bennett, regarding the vegetable parasitical nature of certain cutaneous eruptions; and, above all, the novel and important lights shed on the pathology of cancer by the histological researches of Valentin, Gluge, and Muller. I trust, however, that what I have said may be sufficient for the object aimed at in this lecture—namely, that of apprising you of the extent, importance, and difficulty of the profession in which you are embarked, and of inciting you to the necessary energy in its prosecution."

ART. II.—THE CELL THEORY,—We abstract the following synopsis of one of the most beautiful and plausible theories ever projected from a review of a late edition of *Carpenter's Principles of Human Physiology*, in the April number of the Medico-Chirurgical Review :

There is no discovery of modern times which has worked such mighty changes in established opinions, as that connected with the *cell theory*, which, originating in the researches of Schleiden into the ultimate texture of vegetables, was extended in the celebrated work of Schwann (Mikroskopische Untersuchungen, &c.,) to the organization of animal structures. By these investigations, it has been ascertained, that with some few exceptional cases, every part of every animal and plant is, in the first epoch of its formation, developed from a nucleated cell; that by the inherent and independent powers of this cell and by the metamorphoses it experiences, all the organic tissues are formed; and, more than all, that every act of the process of nutrition—secretion—absorption—assimilation—growth, and decay, instead of being, as until this theory they were in all the higher animals invariably considered to be, immediately dependent upon the blood-vessels and absorbents, are, in reality, accomplished by parts which are essentially extra-vascular. Although the facts here announced have received the concurrent sanction of all accurate observers in every part of Europe, yet as we have reason to know that, among many of our professional brethren, a lingering attachment to former doctrines, and an indisposition to admit the somewhat startling deductions which have flowed from the observations of Schwann and his numerous followers, still prevail, a few remarks on the subject may not be misplaced.

When we recall to mind the extreme simplicity, the undeviating uniformity, the mathematical exactness of all the great laws of Nature, it is not difficult to comprehend that the mechanism by which one great class of vital actions, the nutrative, are sustained, should present a common type. The humblest of the cellular plants displays phenomena which are essentially the same as those which in the aggregate constitute the organic life of the most complex animal:—it absorbs nutriment—it grows—it reproduces, and thus maintains its own existence and that of its species. A similar series of actions are performed in the simplest classes of the animal creation, and in both instances the only organs which can be detected are—cells. Now, if we turn to the most elaborately constructed animal, to the human body itself, what do we find? The embryo, as we learn from Martin Barry's admirable investigations, taking its rise from a single cell—the formation of a membrane, the vesicula germinativa—the development of a considerable extent of the nervous centres—the deposition of the commencing vertebral column—all these phenomena occurring antecedently to the appearance of blood-vessels and of their central organ, the so-called punctum saliens. If from the nutritive process of the embryo we turn to that of the adult, the same kind of evidence is afforded of the predominent action of cells, and the subordinant agency of vessels. Thus the active and essential part of every organ, that in fact which constitutes the organ, is extra-vascular. No artery penetrates the sarcolemma, the ultimate nervous tubule, or the efficient agent of the various glands. Again, few organs appear to be more vascular than the mucous coat of the small intestine; and yet here the parts engaged in the secretion of mucus and the absorption of the chyle, are seen to consist of epithelial cells; even the more simple membranes, the serous and the synovial, those in which, if anywhere, the direct action of the capillary vessels might be expected, present in the place of the exhalent arteries of former anatomists, a layer of delicate epithelial cells, the true agents of the secretion.

All this appears somewhat startling, but when the matter is presented to our notice in its simplest form, the truth of it becomes obvious:—for example, the animal machine consists, in addition to blood-vessels, of several different solid substances, muscles—nerve—bone. Now, as two bodies cannot occupy the same space at the same time, it plainly follows that all these substances must be placed beyond the tubes carrying the blood, and this fact being once understood, all the details respecting degrees and relations of vascularity become questions of secondary importance. The axiom, therefore, that organization is co-extensive with, and dependent upon vascularity, must henceforth be abandoned, and with it will vanish a crowd of errors and apparent anomalies respecting the vitality of parts wanting blood, such as the epidermis and cartilage in vertebrated animals, and the entire organism of many of the more simple classes among the invertebrata. We will only further remark, that the great principle thus established, that throughout the organic creation, the nutritive process is effected by one typical formation, the nucleated cell, is a generalization which promises to effect for structural anatomy, what the law of correlation has accomplished for comparative anatomy, and that of definite proportions for the science of chemistry.

Dr. Carpenter, who has adopted in all their extent the views at which we have glanced, regarding cells or vesicles as the primordia of all vegetable and animal tissues, gives a comprehensive account of the subject, from which, however, we can only extract one or two passages.

"A very large proportion of the vegetable organism, (in the simplest plants, the entire structure,) is made up of *cells* or *vesicles;* which are minute closed sacs, whose walls are composed in the first instance of a delicate membrane, frequently strengthened, at a period long subsequent to their first formation, by some internal deposit. The form of these cells is extremely variable; and depends chiefly upon the degree and direction of the pressure, to which they have been subjected at the period of their origin, and subsequently to it. Sometimes they are spheroidal; sometimes cubical or prismatic; sometimes cylindrical; and sometimes very much prolonged. These cells may undergo various transformations.—One of the most common, is the conversion of several into a continuous tube or duct.

"The animal body exhibits phenomena, of a character essentially the same. Even in the fully-formed organism, many parts may be found, which are composed, more or less evidently, of isolated cells or vesicles, analogous to those of plants; and it has been clearly proved that, in its early condition, the whole fabric has this character. In fact, it has been shown by the researches of Barry, Schwann, and Valentine, that the whole structure originates in a single cell; that this cell gives birth to others analogous to itself, and these again to future generations; and that all the varied tissues of the animal body are developed from these, although no difference can be in the first instance observed among them.

"From what has been stated, it appears evident that the process of nutrition mainly consists in the growth of the individual cells composing the fabric; and that these derive their support from the organic compounds with which they are supplied by the blood, just as the cells composing the simplest plants derive theirs from the inorganic elements which surround them: and as different species of the latter select and combine these, in such modes and proportions, as to give rise to organisms of very diversified forms and properties, so is it clearly intelligible that the different parts of the fabric of the highest animals should exercise a similar selective power, in regard to the materials with which the blood supplies them. The structure composing every separate portion of the body has (what may be termed) a *special affinity* for some particular constituents of the blood; causing it to abstract from that fluid, and to convert into its own substance, certain of its elements. The conversion is termed *assimilation.*"

ART. III.—PHTHISIS PULMONALIS.—We extract the following from a review of the recent works of *Louis* and *Evans,* upon this disease, in the Med. Chir. Review, April, 1845.

Pathology of Phthisis.—Dr. Evans believes that attention has been unduly directed to the nature, detection, and removal of *tubercle,* as if it were the *cause* of the various symptoms, whereas it is but the result of a peculiar form of inflammatory actoin, occurring in persons possessing the *phthisical predisposition.* To the examination of this predisposition, as the first stage in the production of phthisis, he attaches great importance; and, after alluding to various circumstances which may give rise to it—as hereditariness, exposure to cold and damp, absence of light, the debility caused by various diseases, &c.,—he thus speaks of its nature :—

"From a careful consideration of the causes which tend to predispose to the development of tubercle, I think you are justified in coming to the conclusion that this predisposition consists in a deficiency of that manifestation of vital force whereby the tissues are enabled to grow at the expense of the circulating fluid. This deficiency may be congenital, or it may be the result of external circumstances: it may be confined to an organ, or it may implicate the whole system. But we are led by strict induction to believe that when this predisposition exists, the slightest local inflammation is liable to terminate in that peculiar variety of fibrinous secretion intermediate between lymph and pus, to which has been given the name of tubercle.

"Comparative anatomy and embryology prove that the development of muscular tissue is the product of an action of growth, of a higher order than that which gives rise to cellular and nervous tissues, it consequently follows that in a general arrest of development in the organism, the muscular tissue should suffer first, and in the causes which tend to produce atrophy, this tissue should first present a deficiency of nutrition. Therefore it is, that in the predisposition to tubercle, we find a want of proportion between the red and white tissues, the latter are present in excess, and it has been supposed that an excessive development of the white tissues predisposed to tubercle. But from what we have seen you can evidently understand that it is not an hypertrophy of the white tissues, which constitutes the predisposition: but that the same causes that predispose to tubercle, produce likewise atrophy of the red tissues.

"You may perceive how well the locality which tubercle generally occupies, corresponds with the two-fold method of its production; we might be led by pure calculation to conclude that the lungs, of all organs in the body, ought to be most liable to inflammatory affections, subject, as they are to every atmospherical change, and kept in never-ceasing activity; and then, on the other hand, the upper lobes are those in which nutrition is least active, and the left side, again, is in all the higher animals, the least developed."

We may conclude this section of the subject with quoting the summary of the opinions held by Dr. Evans, as stated in certain "Pathological Propositions."

"1. Phthisis is a disease characterized by a deficient force of growth, together with symptoms both local and general of active pulmonary congestion. 2. The preponderance of white tissues in this disease is due to a diminished force of growth, whereby the tissues generally, but the red in particular, are rendered incapable of attracting from the blood their normal quantity of aliment, and by which their power of resisting decomposing influences of external agencies is diminished. 3. The diminution of the force of growth depends upon abstraction of natural stimuli and aliment; for example, want of heat, light, oxygen in the blood, &c., and the food being insufficient and innutritious. 4. The active pulmonary congestion depends upon the application of stimuli too violent and too prolonged, and may display itself either in the form of bronchitis, hæmoptysis, or pneumonia. 5. The symptoms of active pulmonary congestion in this case, are hectic fever, hæmoptysis, catarrh, cough, altered voice, together with derangement of the digestive and uterine functions. 6. The pathological appearances of the active pulmonary congestion are those of bronchitis, pulmonary apoplexy, or of pneumonia, in the stage of engorgement. 7. The same causes which produce the symptoms of phthisis, are likewise apt to produce the secretion of what is called tubercle, an albuminous substance, intermediate between coagulable lymph and pus. 8. The parts of organs that have secreted tubercle, are subsequently disposed to ulcerate and suppurate, and the tubercle, at the same time, to soften in part, into a fluid similar to pus. 9. Abscesses formed by the softening of tubercles, and the ulceration and suppuration of surrounding parts, are subject to the ordinary law of abscess—viz., burrowing to and bursting from the surface presenting the least resistance, following the least organized track in their fistulous course, cicatrizing by the means of a lining membrane, &c. 10. Masses of tubercle and tuberculous cavities are generally surrounded with indurated lungy substance, of a black, yellowish, or grayish colour. 11. In proportion to the amount of this induration will be the signs of impeded circulation, namely, dilatation of the right cavities of the heart, and enlargement of the extremities of the fingers. 12. The existence of tubercles is not signalized by symptoms, nor their absence a cause of amelioration in disease. 13. The presence of tubercles never causes inflammation in the surrounding tissues. 14. The state of emaciation being a direct consequence of diminisned force of growth; and this latter being the predisposing cause of phthisis, we ought to expect emaciation, or something analogous to it, to precede in general the local signs of phthisis. 15. The lesions in phthisis most important to be kept in mind, are the deficient force of nutrition, and the local pulmonary irritafion; and the symptoms of this disease, namely, the emaciation, hæmoptysis, hectic fever, cough, alteration of voice, loss of appetite, thirst, constipation, diarrhœa, amenorrhœa, &c. are all, more or less, the consequences of these lesions. 16. Hæmoptysis, when very profuse, may be the cause of the diminished growth, and pulmonary irritation of phthisis. 17. Sub-acute gastritis may predispose to phthisis. 18. Excessive discharges, as diarrhœa, menorrhagia, &c., may produce diminished force of growth, and thus predispose to phthisis. 19. The suppression of menstruation in a person predisposed, may produce active pulmonary congestion, and thus give rise to phthisis." 32.

PROGNOSIS.—The question of *curability* of phthisis is one of as difficult decision as of vast importance. Judging from the vaunts so frequently made in the present day of the specific powers possessed by so many medicinal agents, we might seem to have arrived at that stage of the inquiry which does not admit of farther doubt being entertained. But alas! when we come to examine into the history of reputed cures, how often are we met with evidence of want of good faith, and still oftener of accurate knowledge, upon the part of the observer. The well-known uncertainty of the duration of a given case of phthisis, and the numerous instances in which the symptoms have been arrested, or have stopped short, for an extraordinary length of time, open a large field for charlatanism, and for self-deception; while numbers of cases have been set down as cured, were never examples of phthisis at all, and remain mere monuments of the ignorance in diagnosis of those who have had the management of them. Believing this, however, we think it no less the duty of all who have the opportunity of pursuing the investigation [and who has not?] to enter diligently upon the trial of various modes of cure, even when as empirically suggested as many of those to which we have alluded are; for not only is the possibility of the eventful discovery of the right one not unreasonable, but many incidental advantages may spring from the researches made in quest of it. To the alchemists in their vain search we are indebted for the discovery of many valuable processes and preparations. But, if we mean to maintain the character of sober, honest, and rational inquirers, we shall employ a little more caution, both in assuring ourselves of the presence of the disease to be cured, and in the frequency with which, and duration of time during which, we test our means of relief, before we offer the results to the profession, much less to the *public*, than has of late been the fashion.

M. Louis refers to three cases in which the disease, not having made much advance, became permanently arrested; and he considers that the researches of Laennec and others have proved beyond coubt the possibility of the cure of phthisis. Those of Dr. Rogee, quoted upon this point, may be alluded to. From them it would seem that the cretaceous or calcerous concretions, formed at the apex of the lungs, are always the sequel cured or transformed tubercle. Now as 51 out of 100 women, opened by him at Saltpetriere, without selection, presented one or more of these concretions, phthisis appears to be more frequent in its occurrence, and far more frequently disposed to stop short in its career, than was imagined. It is however difficult to suppose these cases could ever have been of a very serious kind, or that large cavities are ever transformed into these little masses. Even the period of life when such anatomical changes commenced is very doubtful, observed as they were in aged subjects.

"It appears from all that precedes, how difficult a matter it is to form a prognosis in phthisis, and into what an abundance of errors we should be drawn, were we to attempt to establish it on the invasion of the affection. On the one hand, a certain amount of severity in the symptoms of the outset does not always prevent the disease from stopping short in its course. On the other hand, we have seen that the affection, after having advanced for a certain number of years with much, nay, extreme slowness—perhaps even after having stopped completely for a season—may suddenly assume all the characters of severity, and cut off its victim in a very short space of time. Perforation of the lung may take place very soon after the invasion of the malady; tubercles may form in the meninges; or chronic inflammation seize on the peritoneum. It is impossible, in the present state of knowledge, to forsee the time at which these symptoms will manifest themselves in individuals destined to experience them, or to prognosticate who will be afflicted with, and who exempt from, their developement. There are reasons in abundance to prove the wisdom of caution in prognosis:—while at the same time their general tendancy is to display the danger of the affection, and the small hope of prolonging existence, when the symptoms assume a severe character at, or soon after, the period of invasion."

Dr. Evans takes far more, and, unfortunately, a far too favorable view of the prognosis of phthisis:—

"If you insist on the discovery of means whereby tubernacles may become absorbed, or induration be resolved, or abscesses be made to disappear, and be replaced by normal structure, I confess that I am ignorant of any remedial method, whereby such wonders can be effected. But if you look on a pleurisy as being cured, although the side be contracted and the lung compressed; if an ulcer is considered healed, although an unsightly cicatrix occupy its site; then I promise you, that by pursuing a proper line of treatment, you will be enabled to cure many cases of phthisis in every stage. In many cases of incipient phthisis you may succeed in removing every trace of the disease, and leave your patients in as good health as before they were attacked; and in other more advanced cases, you may at least alter the disease from being a rapid and fatal one, into a malady, troublesome no doubt, and requiring constant care, but no more distressing than the generality of chronic affections. These are strong statements, gentlemen; but I hope to be able to support them by equally powerful evidence."

TREATMENT.—M. Louis passes in brief review "the principal means which have of late years been brought forward, as best calculated to arrest the progress of phthisis."

Protiodurect of Iron was introduced with great laudation by M. Dupasquier, but neither this nor any other preparation of iron has proved useful in the hands of M. Louis. He delivers a caution well worthy of notice at the present time:—

"It is true that, in order to obtain accurate conclusions, I took care not to put my phthisical patients on any of these ferruginous preparations till six or eight days after their admission; because daily experience shows that a few days' regulated diet, combined with the use of diluent drinks, will suffice, perfectly unassisted by active treatment of any kind, to produce an improvement in the state of their various functions, to cause decrease of thirst, improvement of appetite, a better appearance of the sputa, greater facility in expectorating, &c. It is perfectly clear that, unless the precaution to which I now refer be taken, a certain improvement, in reality depending upon regimen alone, may be ascribed to the influence of some pharmaceutical preparation, and the observer be thus deceived into most serious errors. It is more than probable, that omission of this precaution accounts, in a great measure, for the utterly different views of practitioners concerning the action of medicines."

Chloride of Sodium was recommended by M. Latour in doses gradually increased from ʒ 1-2 to ℥ 1-4, and continued for two or three months, but, after a patient trial, M. Louis found the remedy to be worthless. Of the *Subcarbonate of Potass*, of large doses of *Sal Ammoniac* and of *Chloride of Lime* he has had no experience. The inhalation of *Chlorine* has been attended with no success in his hands. The few cases he has tried *Digitalis* in have furnished no satisfactory results, nor has he been more successful with *Prussic Acid*. Of *Iodine* and *Kreasote* he knows nothing, and the fame of *Naphtha* had not extended to Paris at the period of his publishing this edition. The scepticism with which most observers regard the loudly vaunted pretensions of new medicines for the cure of phthisis, has a tolerably good justification in the history of the above-mentioned ones. Each, upon its introduction, has been accompanied with vouchers of having cured consumption, no matter how advanced, and yet have they all, one after the other, sunk into merited oblivion.

M. Louis' chapters upon the prophylactic and palliative treatment of this distressing malady, although containing many useful remarks, are by no means equal to the other portions of his work; and we may now turn to Dr. Evans' "Lectures," in which decisive efforts are recommended, not for the mere retardation of the disease, but for its curative treatment. He strongly protests against the too prevalent custom of treating the symptoms of diseases as they arise, without endeavoring to refer them to the lesions upon which they are dependent; and adds, that any benefit which can be expected to accrue from the treatment he recommends, will be only proportionate to the accuracy with which the different stages of the disease are recognized. The lesions he distinguishes as characteristic of phthisis occur in the following sequence: a diminished force of growth and reparation, with its consequent excess of nervous excitability: local pulmonary irritation: local pulmonary inflammation with the secretion of tubercular matter: a state of pulmonary induration: a condition of ulceration and suppuration of the lungy substance.—*Tubercles* are only the *result* of phthisis, "they are incapable of producing any of its symptoms, and their presence has very little influence on the progress of the disease." *Their* treatment must never be the object we have in view. "We have no positive means of judging when they are present. When present we know of no treatment for them; and the best method of preventing their deposition, is by checking the local pulmonary inflammation, which is their immediate cause."

ART. IV.—DR. STEWART on resuscitation from drowning:

I have been induced to make these few remarks from having recently seen the beneficial results of the practice here recommended; indeed, the only successful case out of five or six, in all of which insufflation was practised. The case to which I refer, was that of a child about five years of age, who had fallen in a large covered excavation, containing about four feet of water. When I saw him, he was, to appearance, perfectly dead, without respiration, entirely destitute of pulsation at the wrist, nor could any movement of the heart be detected. The body was very cold, and the limbs hung lifeless, as he was carried by a man to be rolled upon a barrel. I directed him to be wrapped in some warm clothing, first wiping the body dry, and taken to the nearest bed; at the same time I sent a bystander for a blanket and another for some warm water from the first fire where it could be found. All these directions were promptly followed. The child had already been placed in a warm bed, before any of the articles were procured; as soon as they could be used, the body was completely enveloped in the blanket saturated with hot water, when the child made one deep inspiration. A long interval followed, when another inspiration took place; the intervals between the inspirations gradually lessened, until in about an hour the respirations had become regular, but very laborious. The child continued in a state of stupor for about twelve hours; he gradually recovered from this state, when violent reaction occurred to such an extent as to endanger his lief from inflammation of the brain. This state was only subdued by prompt blood-letting, and a week elapsed before the excitement of the circulation was effectually subdued.

In the directions published for the information of such as happen to be present upon taking the body of a drowned person from the water, I would suggest that such only as tend to impart warmth, be the means adopted. The inflation of the lungs, even if it is not positively injurious, is an operation requiring much skill and delicacy, and some amount of anatomical knowledge that cannot be imparted by any popular instruction. The application of heat is one which any person can perform, while professional assistance is sent for. A practice, the very reverse of active measures, almost universally prevails: no attempt, whatever, is made to warm the body, or to do anything else, except, perhaps, rolling it on a barrel, or leaving it exposed in its wet clothes, surrounded by idle spectators, inactive from ignorance of what to do, or from a natural unwillingness to undertake a strictly surgical operation. Let them be instructed to adopt a measure which is evidently founded alike upon common sense and physiological principles, and which is so easy to put in practice; while other measures are left for the professional man. Let them be told that nothing is required of them but the application of warmth, and the careful handling of the body, as is always done in cases of other accidents.

The best method of applying heat is that which can be done with the least delay. Dry heat for various reasons would be decidedly preferable, but I know of no method that could be relied upon to apply heat in this manner, with the necessary promptness. The mode which, under ordinary circumstances, can most readily be put in practice, is to envelope the naked body in a large blanket which has been thoroughly saturated with hot water, a small quantity of which can always be found where there are any houses; and if it is but a quart it will be sufficient to wet the blanket for all purposes required. As it is always necessary to simplify the directions, so that no confusion or delay may arise from the suggestions of different methods, the object would perhaps be best attained by directing this alone to be done. While, therefore, the body is carefully conveyed, if possible, to a bed, and there divested of its wet clothing, and immediately covered with anything that will prevent the further loss of heat, let some of the bystanders be sent to obtain water from a neighboring fire; then direct a blanket to be wetted with it, and with it cover the entire body and limbs, leaving the head exposed to the cool air. By attending to these simple directions, an invaluable period of time can be improved, and prompt and effective assistance rendered by any one, without the possibility of erring, until the services of a medical man can be obtained.—*New-York Jour. of Medicine.*

The Relation of a Physician to a Colleague.—[We cordially subscribe to the following principles, and wish that every member of the profession would engrave them upon the tablets of his memory.—ED'RS.]

This relation is twofold. The first embraces mutual respect, and where that is not possible, let indulgences at least be the principal law of conduct.

Nothing is more difficult than to judge others, but nowhere is it more so than in the practice of medicine. It is therefore unpardonable in the public; but it is revolting to hear physicians, who know the difficulties of the art, and of forming opinions regarding it; judge their colleagues with severity, harshness, contempt, or disclose their faults, and try to raise themselves by lowering others. Oh, that I were able to impress the minds of my brethren with the truism, as forcibly as I am penetrated by it! He who degrades a colleague degrades himself and his art. For, in the first place, the more the public becomes acquainted with faults of physicians, the more will physicians become exposed as contemptible and suspicious, and the more will such exposure impair confidence; and confidence in the whole body being diminished, every single one, and the censurers included, will lose a share of it. The public will be less prone to censure the medical profession, and its faults would not be a favorite topic of conversation, if the members themselves did not broach it, and set the bad example. It shows a shortsighted selfishness, and want of common spirit, when a physician acts in such a manner and thereby hopes to raise himself, as he degrades others.—*Lancet.*

V.—*Of the chyle in a pathological state by* M. BOUISSON.—(*Journ. des Connaiss. Med. Chirg.* 1845.)

As yet, we do not know enough on this subject to trace the regular pathological history of the chyle, but the few authenticated facts which we possess, and an attentive examination of the alterations of the chyle, will dissipate much of the obscurity which hangs over the pathology of humoral affections. The formation of this fluid is influenced in many *general diseases*, when there exists a diminution of the forces of the organism. Imperfect chylification gives rise to a product which is itself imperfect, and which, by deteriorating the blood, tends powerfullyto maintain the morbid affection. According to Lind, the chyle of *scorbutic* patients is viscous and readily putrifies ; the blood in such subjects contains an excess of alcaline principles. The mesenteric ganglions, in such, are generally unsound, red, softened in their centre, and they complete the alteration of the qualities of the chyl ea'ready began under the influence of the digestive organs.

Scrofula is likewise developed in certain states, and with certain characters, which induce us to admit a deterioration of the chyle. We know the part which nutriment of a bad quality plays in the etiology of that disease ; the lesions which the mesenteric glands frequently experience the predominance of the albuminous portion of the blood and the deficiency of fibrine ; finally, the general languor of the digestive apparatus. M. Bouisson states that the chyle, in scrofulous subjects, experiences a sort of aqueous reduction, or becomes watery. Dr. Klenke says he has often detected corpuscles of chyle, *sans noyau*, (without a nucleus), or adipose corpuscles, as well as the absence of lymphatic globules, in chachectic individuals, whose bodies he had inspected a short time after death. The same experimenter has, besides, detected the existence of a material alteration of the chyle in animals affected with chachectic diseases analogous to scrofulous affections. The elements of chyle, when mixed with the blood, experience, in some of the affections of the *respiratory* or other organs, a retardation in their ultimate assimilation. In certain cases, they are even directly eliminated. It is more especially true in regard to the fatty part of the chyle, that the debility of the organic acts which tend to assimilate the blood, manifests itself. It remains visible for some time in this latter fluid, until it escapes into those tissues which become impoverished by the effect of its combustion in the lungs.

There are, moreover, other conditions of the economy, whose true nature remains undetermined, and in which the fatty matter, conveyed by the chyle, escapes with the secretion, without being made to contribute to the acts of nutrition.

Thus *chyleuse urine*, as mentioned by a number of authors, has been witnessed in modern times. Haller appears to have had this affection, according to De Haen.

The chyle may experience pathological changes in many of the *diseases of the digestive tube and its annexæ.* Its formation seems to be suspended in the various species of diarrhœa, of a certain degree of violence, particularly when it runs into a *lientery*. The ancients believed that the chyle was thrown directly from the system in the disease which they called *cœliac flux*, and which appeared to be but a form of mucous or serous diarrhœa, coinciding, perhaps, with an exagerated secretion of pancreatic fluid. In diseases of the liver, which modify the biliary secretions, chylification is obviously deranged. Abundant biliary fluxes determine a peristaltic movement, which is opposed to the formation of chyle. It is the same thing in cases of retention of bile, or the suspension of its secretion. In icterus, fatty matter, in lieu of passing into chyle, is often found in the faeces.

When the passage of the chyle is obstructed, either by the engorgement of the mesenteric ganglions, or by the obliteratoin of the thoracic duct, or by the presence of tumors exercising pressure on the canals through which it travels, different morbid phenomena are the consequence of the interruption of its natural course. The obstruction determines atrophy, although it has been demonstrated that certain tumified ganglions may be traversed by injections.

Obliteration of the thoracic duct, when there exists no supplementary route for the transmission of chyle, and the compression of this canal, are likewise followed by atrophy and wasting. Death may result from it; it is ordinarily preceded by a tumor, formed by an accumulation of chyle, below the obliteration, and which may be followed by rupture. * * * * * * * *

The chyle may be altered by mixing with the various products of the system. Sœmering and many other modern anatomico-pathologists, have found pus in the lymphatics of the intestines. M. Cruveilhier, and M. Bouissere, have detected tubercular matter in those vessels. M. Magendie has signalized the development of *vibriens* in the canal. Does this state, asks the author, proceed from the appearance of microscopic entozoæ, which the more modern researches have detected in the blood?

Doctor Klenke attributes to the chyle a peculiar mode of vitiation, in which the saline matters are more abundant than ordinary, and which are spontaneously deposited in a crystalline form. M. Bouisson finally points out the existence of a rare pathological phenomenon to which the chyle gives rise. The museum of pathological anatomy at Strasbourg, contains the rarity to which our author alludes. In the receptaculum chyli, which was greatly dilated, M. Bouisson found a fibrinous substance deposited from the chyle. This concretion is as large as a nut; very consistent; of a white color, and resembles in appearance an organized clot of blood; it adheres by a narrow surface to the internal tunic of the reservior of Pecquet.

VI.—Academie des Sciences.—Paris. (Sitting of the 10th November, 1844.)

M. Amussat laid before the Academy the second part of his researches upon *wounds of the bloodvessels*. The main object of his investigations is to point out the important sole which the clot performs in arresting hæmorrhages. M. Amussat has arrived at the following conclusions: First, When both carotid arteries are divided by making a large transverse wound of the neck, death is not instantaneous, as is generally supposed. The hæmorrhage continues for several minutes, whilst the animal retains all his faculties. Second, The carotid arteries do not remain open after their division, as might be supposed; and, notwithstanding the volume of these vessels, a clot soon blocks up their orifices, as after the division of a single carotid. By examining the plates, (we presume the author here presented them to illustrate this doctrine), which represent the arteries of dogs, and particularly the carotid of the ox when butchered after the Jewish method, we find that the organization of the clot is the same as that pointed out in a former memoir. Third, The simultaneous division, or at short intervals, of the eighth pair of nerves and of the carotid arteries, in the middle of the neck, does not exercise any immediate influence upon the color of the jet of blood, nor in the formation of the clot in the carotid arteries when completely divided crosswise. Fourth, The spontaneous clot formed at the extremeties of the divided arteries, is composed of two clots; one exterior, and already described, the other internal, which is nothing else than a coagulum as completely organised as that which is formed after all the artificial means of obstruction, comprehension, cauterization, ligature or torsean. Fifth, The retraction of the internal and middle coats, which is the product of the three properties of the arteries, so ably described by M. Florens, enables us to explain the formation of the clot and the difficulty of finding it in the midst of the tissues in which it is buried. Sixth, Spontaneous obstructing clots are often dificult to detect. In order to do this, we must bear in mind the known anatomical disposition of the divided artery, and observe the pulsations at the extremity of the divided vessel. Besides, we may detect by the touch the small mass of blood which constitutes the clot. Seventh, Finally, it has been sufficiently demonstrated that it is always by a clot or an obstructing coagulum that hæmorrhages are spontaneously arrested, whether the animal survives or not. Thus then, the doctrine of a *spontaneous clot* exterior or anterior, as obstacles to the escape of blood from ar teries when completely divided, is the only true one, and contrary to the opinion of Jones and Beelard, the artery alone accomplishes this itself. Doubtless, continues M. Amussat, the fact established in my memoir, is but a small addition to the theory of J. J. Petit, considered in a philosophical point of view, but when regarded in a practical sense, it is of great importance, as is demonstrated by all grave and fatal hæmorrhages which occur; because we are unable to find the disfigured artery when masked in a clot.

Neuroses of the Ganglionic system of Nerves.—An essay was received from M. Merat on this subject. The author asserts that the therapeutics of this class of affections is far in arrears. He believes the most efficacious remedies for these affections are the stimulating and heating antispasmodics, such as ether and the alcoholic tinctures. He strongly recommends the powdered root of the valerian; the powder of St. Johneswert, and the *sedum acre* in large doses. *Progress of the spermatic fluid in the genital apparatus of the female mammifera.* M. Pouchet related the rasults of the experiments which he had made upon the female rabbit, during the first 24 hours after copulation.

From the 16th to the 25th hour, we shall always find the living *zoospermes* in the vagina and the fallopian tubes. At the 21st or 23d hour, these animalcules are very active; but afterwards they begin to loose their activity and die towards the 25th hour. We find after this period only lacerated zoospermes, the tail is separated from the enlarged extremity. The author has occasionally found living zoospermes at a depth of one or two *millemetres* in the uterine extremity of the fallopian tubes; he has likewise observed them occasionally and in small numbers at the distance of between ten and twelve millimetres in these tubes. According to our author, they never go beyond this distance. M. Pouchet believes that it is in tho uterus, and perhaps also in tubes, near the uterine cavity that fœcundation takes place. He thinks M. M. Bischoff and Wagner deceived when they imagined that they had discovered sperm in the ovaries.

Gangrene from secale Cornutum. M. Bonjour related two cases of poisoning by the secale cornutum. Two children after a hearty repast of bread made of grain containing a large portion of the ergot, presented the phenomena of gangrenous ergotism. One, aged ten years, had both legs amputated. In the other, aged about 28 months, the right leg separated spontaneously and dropped off. They both survived. Tne other members of the family suffered, but less seriously.

Acarus in the Urine.—M. Fee, Professor of the Strasbourg faculty, communicated the results of a miscroscopic examination to which he had submitted the urine in a healthy state. He found in a drop of this fluid an *acarus* unlike that detected in the itch.

(*Sitting of the 30th December*, 1844.)—*On the Structure and some of the diseases of the lungs.*—M. Rochoux read a memoir under the above title, in which he developed the results of some miscroscropical examinations. He discovered cellules amounting in number, to 600 millions. Of these, 17,790 were grouped around each terminal branch. The lamellæ which compose these cells are constituted of extremely delicate filaments. It is in the angles resulting from the intersection of these lamellæ that the capillary and sanguine vessels are distributed. The disease which occupied the attention of M. Rochoux, are emphysema, tubercles and empyema. On this subject, he comes to the following conclusions: First, Emphysema from dilation of the pulmonary cells, as admitted by Lannec, does not exist; it is even impossible; and hypertrophy and dropsy of the walls of the pulmonary cells, although admitted by many physicians, is yet to be demonstrated. As yet emphysema by infiltration of air in the tissue of the lungs, has alone been sustained. Second, Tubercles, like all accidental productions susceptible of degeneration, should be studied exclusively in the first stages of their formation, consist of a tissue at first filamentous, singularly interlaced, and then of a pale orange color, which soon passes through all the degrees of degeneration described by authors, beginning with the military form. Third, the existence of a fibrous membrane or at least a peculiar texture of the membranous tissue, of which the lungs are essentially composed, is the main cause of that almost irremediable contraction which that organ experiences in inflammatory effusions which emanate from affections of the pleura pulmonalis; hence is deduced the general precept, that we must operate as promptly as possible in such cases, and before the pulmonary tisue has undergone that species of shrinking which will never allow it to resume its primitive normal state, although freed from the liquid which compressed it.

Nature and treatment of Typhoid Fever.—M. Léopold Twick, physician at a Plumbiéres sent in a memoir on the nature and treatment of typhoid fever. In this memoir, the author proposes to demonstrate: 1. That the habitual lesions of the ilium in this disease are secondary, arising chiefly from chemical influence; 2. That the increased volume of the spleen, as frequent at least as the internail lesons, is of far more value as a pathognomonic sign, since this alone has led us to believe that typhoid fever bears strong analogy to intermittent fevers, which usually likewise produce the same alterations. 3. That the majority of the facts furnished by modern nosographers as examples of continued typhoid fever are double pernicious testians; true "*fievres larvées*," continued fevers, such as was described by Torti at the commencement of the last century; 4. That these diseases have become so grave and fatal simply because we have overlooked their remitteut character, and the special treatment which they demand. M. Turck regards typhoid fever as a general disease which attacks the entire economy, and against which bleeding is of no avail. He has satisfied himself that the disease is of an intermittent form, and hence he advises the administration of the great antiperiodic *par excellence.* He employs the vin. chinconæ in the form of general lotions, the sulphate of quinine in lavements, and the infusion as a drink. The author concludes his paper by citing several observations well calculated to sustain the views above developed, and in the treatment of which his plan proved eminently successful.

(*Sitting of the 6th January*, 1845.)—M. Chatin read a memoir, entitled: *Studies of vegetable physiology conducted by means of arsenious acid.* The author has deduced from his experiments some observations applicable to ruval economy, to legal chemistry, and to therapeutics which may be summed up in the following propositions:

Agrictlture.—Arsenic does not destroy the rust in the cereales, nor the cryptogames in general, or the *uredo cardo* in particular. This fact will demonstrate the necessity of prohibiting the use of arsenious acid for agricultural purposes.

Legal Medicine.—The elimination of arsenious acid by plants, in a given time, proves that no traces of that article remain in the cereales; whose seeds have been exposed to the vapor of arsenic in autumn.

Therapeutics.—First, a calm and humid atmosphere, obscurity, electricity, acting constantly, would appear favorable both to man and animals in the first stage of poisoning; and, on the contrary, a dry and disturbed state of the air, a brilliant light, would seem to be beneficial, at the period when absorption has taken place, because these external influences seem to facilitate the elimination of the poison Second, The complete neutralization, by the chloride of calcium, of the the arsenious acid absorbed by plants and entering into a state of saline combination with the alkaline bases of their juices, and the alkaline nature of the blood of animals, which render so likely the formation of a combination analagous to the preceeding at the very moment when they absorb the arsenious acid; indicates very clearly that this combination of chlorine must prove an antidote to the arsenious acid absorbed by these latter.

Diseases of the middle and internal ear.—M. Wolf read a paper upon a new mode of treating these affections. He thinks M. Deleau has acted wisely by substituting injections of air for those of fluids. By charging the air with the vapor of resinous and balsamic substances we shall have at our command an excellent remedy for catarrhs of the middle ear." The preceeding mode is unavailing when we desire to introduce into the eustachian tube substances which require a high degree of heat to be volatilised. M. Wolf has devised an apparatus which enables him to inject the vapor of water, either simple or medicated. The water contained in an iron vessel and heated by means of a spirit lamp, is made to boil, and the vapor which escapes is conducted by a tube into a second vessel larger than the first, containing a third vessel filled with cold water. At the upper part of the second vessel there is a tube through which the current of watery vapor, whose temperature may be regulated at pleasure, escapes. With this apparatus the author introduces medicaments which have not been previously dissolved in water.

If, for example, we wish te introduce acetic ether or other medicines which volatilise at a low temperature, M. Wolf pours them into the third vessel, and their vapor escapes with the current of the heated air; if we desire to employ any of the balsamic vapors such as benzoin, we put the benzoin finely pulverised into a small vessel and place this into the large vapor vessel, and the aqueous vapors will become charged with the volatilisable particles of these substances. M. W. has employed in this way, various substances for diseases of the ear.

When both eustachian tubes are free and pervious to the current of air (as in nervous deafness). it is not necessary to introduce the vapor through the sound; it will answer to carry the conducting instrument towards the mouth of the tube. M. Wolf's conducting tube is then a large caoutchouc canula, which is introduced from 5 to 8 *centimetres* into the inferior nasal canal, to one extremity of which is adapted the tube of the apparatus from which the vapors escape. This procedure is extremely simple, free from pain, and does not require, as the catheterrism of the eustachian tube, special tact in these kind of operations.

Grafting of Nervous Cords.—M. TAVIGNAT made a communication relative to some experiments which he had made upon this subject. if, says M. T., we embrace in the same ligature, two nervous cords near each other, with a view to effect their simultaneous division, we shall soon find developed between their four extremities a sort of nerviform ganglion common to both and in which the fibres of the two nerves and their functions seem to be confounded. A section of the two nerves made a little distant the one from the other, executed in such a manner that the superior end of the one may be adapted to the inferior extremity of the other, gives rise to the formation of a new nerve which preserves its functions entire. When this communication was read, M. Flourens stated that he had published some years since, similar experiments, with like results. He wished to effect a union of several different nerves, for example, that of the superior with the inferior nerves of the brachial plexus, and even that of the cervical with the pneumogastic nerves. In both cases, he effected a complete union, and, in some cases an entire restoration of function was brought about.—*Journal des Connais. Med. Et. Chirurg.* 1845.

VII.—*Of the Physiological and Pathological Antagonism between the Thyr oidGland and the Lungs. By* Dr. RICHE, *of Obernai.*

In the September number, for 1844, of the *Journ. des Cennaiss. Med. et Chirurg.*, we find a Doctor Hamburger, of Bohemia, has proposed to treat *phthisis pulmonasis* by producing an artificial development of the thyroid gland. To justify this novel and singular mode of treatment, he bases his arguments, first, upon the probable important part which this organ plays during embryotic life as a *branchial* apparatus, and upon the antagonism, which, according to him, exists between these two apparatuses in many cases; then upon the fact that an attentive observation has proven to him that the thyroid gland diminishes in persons predisposed to phthisis, and that it disappears entirely when the tubercles are softened—when secondary fever is developed.

In cases where Dr. Hamburger has succeeded, he has observed a consecutive development of the thyroid gland; and in individuals predisposed to phthisis and cured of goitre, he has observed phthisis to declare itself after the goitre has disappeared.

On one occasion he tried to produce hypertrophy of the thyroid gland by the free use of chalk-water. He succeeded to a certain extent; but suddenly the hypertrophy disappeared, and phthisis made such rapid progress that the patient died in a short time afterwards.

These observations and reflections are extracted from the *Almanach Medicale* of Dr. Sachs, of Berlin, for 1844' To these M. Riche has added some others. Observers, says he, have already remarked that iodine, which may be advantageously employed in scrofulous phthisis, is, on the contrary, hurtful in genuine tubercular consumption. These facts, he thinks, go to support Dr. Hamburger's ideas, already quoted. He likewise thinks that the supposed intimate connection and reciprocal influence of these two organs should render practitioners more circumspect when they are called to treat goitres in subjects of a tubercular tendency. He thinks, that in such cases, we should associate with iodine the calcarious salts and the martial preparations; or, that in cases of goitre we should stick to the administration of powdered spunge, carefully torrified, whose composition ap proaches more nearly the mixture which it is intended to represent.

M. *Riche* believes that we might excite a hypertrophy of the thyroid body, by the repeated application of cups—of blisters, of irritating pommades; and he proposes to try if the thyroid region might not be chosen, with success, as the seat of election for a revulsion in cases of diseases of the lungs. M. Riche concludes by urging his honourable confreres of Strasbourg to make some experiments upon this subject, and to make known the results through the medical journals.

Remarks.—Without being seduced by the novelty, or led away by the speculations of these gentlemen, we have thought them sufficiently curious to give them a place in the Journal. Whatever may be calculated to shed light upon the best method of treating a disease which has slain more, perhaps, than the sword, should be carefully collected and faithfully recorded. Who knows but that the thyroid gland may, like the spleen for the stomach, serve as a diverticulum for the lungs? Indeed, its spongy nature and high vascularity would strengthen such a supposition. From its position, situated between the lungs and brain, may we not reasonably *conjecture* that it is placed there as a sort of *hæmasometer*, to regulate either the cerebral or pulmonary circulation, or peradventure both, according to circumstances? If nature, as philosophy inculcates, does nothing in vain, we must suppose this gland to be either an exception, or placed there for purposes utterly unknown.

In a lady, aged about 76 years, of delicate constitution and excitable system, we found the thyroid body unusually developed at particular periods. Any sudden emotion of mind, especially of anger, caused the thyroid gland to swell, become turgid, florid, and to increase at least fourfold in size. As the mental perturbation passed away, the gland shrank to its usual size and assumed a healthy appearance. From the conformation of the chest of this female, we supposed her predisposed to tubercular deposits.—(*N. O. Edrs.*)

VIII.—*The Guinea-Worm*—(*Dranunculus.*)—M. Maisonneuve, the clever Paris surgeon, so well known to many of our readers, has lately had a very excellent opportunity of studying the guinea-worm, (filaria medinensis,) and has published in the *Archives* some interesting details respecting it.

In October, 1843, he received into his wards, at the hospital of St. Antoine, a man named Ede, aged twenty-eight, who had, five months previouslly, returned from Senegal, where he had served as a soldier two years and a half. About a month before he entered the hospital, he perceived on his foot, for the first time, a small tumor, which gave rise to a dull pain in the vicinity of the articulation, and was accompanied by considerable itching, for which he in vain tried poultices, rest, and various other plans of treatment. M. Maisonneuve was surprised to see a vigorous man enter an hospital, for what appeared nothing more than a very small phlegmonous tumor on the level of the posterior extremity of the fourth metatarsal bone, and merely made a small incision, afterwards ordered a poultice. On examining the wound the following day, he observed a kind of white filament, which he seized with his fingers; it gave way, but broke when he had drawn out about six inches. On ascertaining from the patient that he had lately inhabited Senegal, it occurred to M. Maisonneuve that it might be a guinea-worm, and he then examined the man more attentively. In the vicinity of the furunculous tumor there was a slight degree of œdema, which extended to the malleoli; pressure over this region was painful. At the superior and external part of the same leg, just below the head of the fibula, there was another small tumor, very similar to the first. It was indolent, merely gave rise to slight itching, and had only been perceived by the patient about fifteen days previously. From this tumor there passed a kind of flexuous, irregular cord, which he at first thought was a varicose vein; after turning round the anterior part of the leg, it lost itself in the calf; its consistence was hard, like that of whip-cord. It was evidently a second worm. The man was muscular and in the full vigor of youth. He had never suffered anything of the kind whilst in Senegal, nor had his white companions, although it was common among the negroes. In the course of the fortnight which followed the man's admission, several phlegmonous tumors formed round the external and internal malleolus, and on being incised, exposed different portions of the filaria. It was entirely extracted, but not without some trouble, as it broke repeatedly. One day, on pressing the superior furuncular tumor, a few drops of white fluid, like whey, escaped. This fluid was examined with the microscope, and found to contain myriads of small cylindrical worms, with thin, pointed tails. They were amazingly active in their motions, and on being examined in water, were not found to present any tentacula or appendages of any description. Some of them remained alive several days. The head of the filaria showing itself at the tumor, M. Maisonneuve seized it with his fingers, but it broke. He then took up a fold of skin over its course, and by a transverse incision exposed several circumvolutions of the worm. It was situated in the lamellar subcutaneous cellular tissue; he dissected it out with care, passing a sound underneath, and was thus able to extract all the superior portion. It was about as thick as a crowquill, and very like the vas deferens near the epididymis. On exercising traction by the part exposed, it broke, and a second incision and dissection was resorted to. The remainder of the worm was found curled up on itself in the cellular tissue, like the superior part, and was obtained in the same way; a small piece of the caudal extremity was left, but extracted the following day by the patient himsef.

M. Maisonneuve remarks that this case is very interesting, as it establishes a point in natural history which has been much debated—viz., the existence of the filaria as a distinct species of animal, capable of propagating itself whilst in the human economy. As long, in all probability, as the dranunculus merely increases in size, its presence, says M. Maisonneuve, gives rise to scarcely any local disturbance, but when the period of reproducing arrives, it makes an effort to perforate the skin, and thus occasions a furuncular tumor. It is singular that these parasitical animals should thus discharge their young externally.—*London Lancet.*

IX.—*Inflammation, Ulceration, and Induration of the Neck of the Uterus.*

In the London Lancet for May, 1845, we find a very interesting paper on this subject by J. Henry Bennet, M. D., who has long resided in Paris, and is intimately acquainted with the treatment of female diseases practised in that city. We regret not being able to make room for a more lengthy extract from Dr. Bennet's paper, in which he gives an interesting account of the *Special Hospitals* of Paris—separate establishments for the young, the adult, and the aged, &c.—Since the recent advancement of medical science in our own country, and particularly in the South, it is ascertained that many females who have long been in delicate health, labor under some local affection of the uterus or vagina, which, under the old system of blind empyricism caused them to linger out a miserable existence. We will only give two of Dr. Bennet's cases, which will speak for themselves.

Case. I. At the beginning of 1844, a gentleman who had been married about four months, requested me to see his lady, who had, he stated, been suffering for some time. The lady, four-and-twenty years of age, was apparently in the enjoyment of robust health, the various functions being all accomplished with great regularity. On inquiring minutely, however, into her state, I found that she had experienced pains in the loins nearly ever since her marriage; and these pains had gradually increased, had lately been accompanied by slight pain behind the pelvis, and by a deep-seated sensation of heat in the same region; that intercourse, at first unattended by pain, had, a few weeks after marriage, become painful, and was then unbearable, from the last mentioned cause. There was no perceptible leucorrhœal discharge. Being convinced that inflammation and ulceration of the uterine neck were the cause of these symptoms, I obtained the consent of the parties to an examination. On practising the toucher, I found increased heat in the superior region of the vagina, and a large tumefied, but soft and pulpy, cervix uteri. The anterior lip was evidently much more tumefied than the posterior; on its margin, I distinctly felt a superficial induration of several lines in length, presenting a rather uneven surface. The conical speculum having been introduced, I found the mucous membrane of the lower two-fourths of the vagina perfectly healthy, but the superior fourth was red, inflamed, and partly covered with a muco-purulent secretion, especially where in contact with the inflamed cervix. The latter was of an uniform red color. The anterior lip was so much congested and swollen, as to occupy nearly all the concavity of the speculum, and to cover the orifice of the uterine cavity, and the under lip. On its being pushed back so as to expose the latter parts, a circular ulceration, of the size of about a shilling, was discovered around the os, but more especially extending on the anterior lip. The pressure of the speculum was found rather painful. A slight oozing of blood took place on the copious mucoso-purulent secretion, which covered the ulcerated surface, being wiped away. When this had been done, the mucus passing from the interior of the cavity of the neck was found quite transparent, shewing that the internal surface of the uterine cavity was not inflamed. The entire surface of the cervix, the upper part of the vagina, was painted over with the solid nitrate of silver, which was passed two or three times over the ulcerated region, and into the cavity of the os for a couple of lines. The application of the caustic was scarcely attended with any pain. The patient was then told to use cold water vaginal injections several times a day, for two days, and, after that period, injections with the sulphate of zinc. She was also told to remain quietly at home, on an easy chair, or a sofa, and, as a matter of course, forbidden any communication with her husband. A couple of days after the cauterization, the pains in the loins and pelvis had much abated, as also the other symptoms above mentioned. On the eighth day, the cauterization was repeated, the tumefaction of the cervix had much diminished, as also the inflammatory congestion. The ulcerated surface was decidedly smaller. The same local treatment was pursued. On the sixteenth day, nearly all pain in the loins had disappeared; the cervix uteri was evidently rapidly regaining its natural size, and the ulceration had still further diminished. She was allowed to ride out in a carriage, and even to walk with moderation. Cauterization with the nitrate of silver was again resorted to on the twenty-first and twenty-fifth day, but much more slightly, and on the thirty-second she was quite cured.

The ulceration had cicatrized, without leaving the slightest induration behind it. The tumefaction of the uterine neck had disappeared, and it had regained its usual coloration and unctuous feel to the touch. I need scarcely say that not a vestige of the symptoms experienced during the preceding months remained. I gave her no medicine internally during the treatment, because she did not require any, and did not even think it necessary to modify her usual diet, which was simple.

The above is a very instructive case, embodying, as it does, most of the symptoms which are observed in the more intense cases of this form of inflammation and ulceration. When the disease is as severe as in this instance, I think it very probable, that, if left to itself for years, as is no doubt often the case in England, general induration and hypertrophy of the cervix may take place even in women who have never borne children. The cause being local, if that cause be subtracted, and proper local measures taken to subdue the inflammation, the cure in most instances takes place rapidly. That this disease is worthy of more attention than it meets with, is certain, not only because it is a source of misery in married life, but because it is, no doubt, often a cause of sterility. That such is the case will readily be understood when we consider that the congestion of the uterine neck, and the copious purulent secretion, which clogs up the uterine orifice, must frequently obliterate the latter. Young females seldom become pregnant whilst suffering from this affection. Messrs. Gendrin, Emery, and Jobert, whose experience of uterine affections in private life is very great, have repeatedly told me that they have known many young married women, their patients, who were laboring under this form of disease, and had remained sterile, become pregnant, as soon as cured.

The case which I have just related is an example of ulceration in a severe form. The one which I now intend to give is equally interesting as illustrating the incipient stage of the malady, and also as proving that it is not necessary for intercourse to be carried to excess to give rise to inflammation of the cervix. This is a point which is not without some importance, as if we admit, which I believe we ought to do, that the cervix in some is so sensitive that very slight irritation is sufficient to inflame it, we shall be induced to look upon our patients in a different point of view to that which would otherwise obtain.

CASE II.—In Paris, as all who are acquainted with Parisian matters well know, the police is very severe, and exercises great scrutiny and control over all persons who are not regularly domiciliated householders. In pursuance with this line of conduct, domiciliary visits are made at irregular periods, in the middle of the night, in the lower order of hotels or lodging-houses, and also in those inhabited by students. This is a precaution rendered absolutely necessary by the irregularity of the lives of some of these gentlemen, and by the circumstance of their congregating, to the number of fifty, a hundred, or more, in the favorite hotels of the "Pays Latin." When these "*descentes*," as they are called, take place, every room is visited, and all persons whose passports are not found in order, as also all "ladies" on a visit, are marched off to the prefecture de Police. The following morning the latter are sent to St. Lazare, (the hospital and penitentiary for unfortunates,) unless claimed by two respectable householders. I have known nearly a dozen thus caught at one draw of the "police net," and among them more than one fine lady, who did not most certainly anticipate such a "denouement." On one of these visits, a young female, named Jourg, eighteen years of age, was taken, and, not having any friends, was detained by the police. In the course of a few days, she was examined by the police medical authorities—a precaution usually adopted in these cases, previously to being discharged, and was found by them to be laboring under slight ulceration of the os uteri.

It was thought that the affection might be syphilitic, and as she was not an enrolled woman of the town, she was sent to a general hospital, (L'Ourcine,) and not to the infirmary of St. Lazare. The hospital physician kept her a few days, and then not thinking her affection sufficiently severe to require further treatment, sent her back to the police. Here she was again examined by the police physician;

who found that the ulceration had not been cured, and sent her into M. Emery's wards at St. Louis, where she consequently came under my notice.

On examining with the toucher, July 4, 1843, the cervix uteri appeared small and soft, and there was a scarcely susceptible, very superficial, and very circumscribed induration; no pain on pressure. The speculum shewed the vagina to be narrow, and of the natural hue unto very nearly its superior extremity, where it became rather red and injected. The cervix was small, about the size of the ungueal portion of the medius finger; it was evidently congested, but soft, offering little or no resistance to pressure. On its anterior aspect there was a small abrasion, about the size of a sixpence, covered with minute red granulations, and a little semi-purulent mucus. There was no other mucoso-purulent discharge in the vicinity. The mucus issuing from the uterine orifice was perfectly transparent; no pain whatever in the loins, or hypogastric region; no heat or burning sensations; no leucorrhœal discharge; health perfectly good. The patient said, that had she not been told that she was ill, she should not have thought there was anything the matter with her. She stated that she had been brought up in the country; that eight months previously she had come to Paris, and had lived since her arrival by working as a sempstress; that she had made the acquaintance with a student who had persuaded her to accompany him home to his lodgings the night she was seized by the police, and that it was the first and only time she had known any one —an assertion which the state of the genital organs tended to corroborate. She had menstruated for three years, had never experienced any leucorrhœal discharge whatever, and had always been in excellent health. M. Emery, the physician who at first examined her, told me that the lesion, which was very slight indeed, had increased since then; whilst she was at the Ourcine she had been treated by emollient injections. It was therefore considered that these means were not sufficiently energetic, and the ulcerated surface was cauterized by the acid nitrate of mercury. Emollient injections and general baths were also resorted to. The tumefaction of the cervix and the ulceration increased under the influence of the first cauterization, (it was evidently rather too energetic,) but decreased under that of the second, third, and fourth, which were performed at intervals of six days. On the 5th of August the tumefaction and redness of the cervix had disappeared, and the ulceration was all but healed. Astringent injections were then alone used, and on the 15th of August she left, perfectly cured. In this case, as usual, the cauterizations were scarcely felt by the patient, owing to the natural want of sensibility of the cervix uteri; no lumbar or hypogastric pains or other symptoms developed themselves during the treatment. Had this girl been a young married woman, in the course of a few months she would most likely have been in the same state as my first patient.

X.—*Causes and Treatment of Deafness.*—Deafness, of course, will arise from the presence of foreign bodies, or from the swelling of the meatus or parts around it. It frequently takes place in consequence of the accumulation of cerumen, and, indeed, this is the most common cause of deafness, and nearly the only one that you can with any certainty count upon removing. Old people are often relieved from deafness of long standing, by having foreign matters washed out from the external meatus. There has been a discussion as to what is the best solution to employ for dissolving the accumulated secretion. You will, in one of the volumes of the "Edinburgh Medical Essays," find a very goed paper on the subject, in which are detailed trials with various alkaline and other solutions, and the conclusion arrived at is, that tepid water will answer as well as anything else. You use for the purpose a tolerably large and well-valved syringe, with a properly-fashioned nozzle; now and then you use a scoop, and pull out portions that would not otherwise come away. You often find large plugs of cerumen mixed with cotton and wool, which patients have put into their ears, to prevent cold air getting in, as they say. It is also said, that deafness arises from there being too little wax, from the ear being dry, and in these cases all sorts of stimulating oils have been dropped into the ear, with, I believe, but little advantage. Then you find deafness arising from obtuseness or paralysis of the nerve, and that is thought sometimes to be remediable. You may endeavour to rouse the sensibility of the parts by blistering over the mastoid process, and sprinkling the raw surface with strychine in minute quantities.

Deafness also occurs, to a greater or less extent, (and this is irremediable,) where suppuration has taken place in the internal ear, and where the bones of the tympanum have been destroyed. You cannot restore these parts, and you cannot expect the functions to be restored, for there is great change of the structure of the organ. Patients often continue to have a discharge from the ear for a long period; there has been an attack of inflammation and suppuration during some inflammatory affection or fever of childhood, as scarlatina; it goes on for a long time, and parents desire to have it dried up; but beyond having the part kept clean, I advise you not to interfere. If you employ a strong injection, you may give rise to disease in deeper seated parts, and endanger life.

There are discharges from the external ear now and then connected with abscess situated deep in the temporal bone. I have seen two or three cases where the matter which had formed within the cranium was discharged by the ear. There may be abscess of the brain, or abscess between the dura mater and the anterior aspect of the temporal bone; and this certainly cannot be remedied by any external application. I have seen the discharge checked by long exposure to cold and wet, fever has ensued, and the patient has speedily perished.

The functions of the ear are sometimes interrupted in consequence of affections of the throat. Common people know that when the "almonds of the ear," as they say, come down, they are deaf. If the tonsils remain enlarged and swollen, the hearing is obtuse. The same thing occurs from polypi and other tumours of the throat. In cases in which I have removed them, the patient had been deaf for years. I have mentioned in one of my books the case of an old gentleman who had been deaf for very many years; he went regularly to church, for form and example' sake, it would appear; for after a great many polypous tumours had been extracted, he declared that he never, till then, heard one word of the service. The tonsils have been removed now and then with great advantage in cases of deafness, but though relief has been thus afforded, I would not by any means advise you to resort to it in all cases. You are not warranted in cutting away the tonsils or uvula under the assumption that they are the cause of the deafness, more especially when there is but little, if any, abnormal change in them.

After all, there is not a great deal to be done for the cure of deafness. There are many cases easily remedied, but a great many others are quite irremediable. The deafness which ensues in consequence of the deficiency or destruction of parts cannot be remedied; in many cases where the nerve is affected, no relief can be afforded, but where it arises in consequence of obstruction of the Eustachian tube, or the presence of tumours, the surgeon may sometimes afford considerable relief. It has been said that deafness arises from the Eustachian tube being closed up by mucus, and in order to remove it, all sorts of quacks have been in the habit of injecting air and fluids into this passage; but the only effect of this process, so far as I can see, would be, to drive the vitiated secretions further into the passages, and impact them there.

In some cases of permanent obstruction of the Eustachian tube, in cases of thickening and induration of the membrane of the tympanum, and in collections of blood in the cavity, the drum of the ear has been perforated. This is an operation not attended with difficulty. The only trouble is in keeping the opening from closing again. The operation may require repetition, unless it is performed in such a was that a portion of the membrane is wholly removed. This may be accomplished d means of a sort of punch dextrously applied. The instrument must be so guidemes to avoid the attachment of the handle of the malleus. I have, in a few instnadbo seen patients thus slightly benefited, but great success cannot be anticipated from the proceeding. * * * * * * *

QUARANTINE LAW IN FRANCE.

XI.—We give below the remarks of the editor of the London Lancet upon the subject of quarantine in Europe, and ofterwards his equally interesting observations on the hygienic state of modern Egypt. It is certainly not a little curious to observe the diversified opinions and conclusions entertained in different countries and at different periods, in regard to the propriety of quarantine laws. These laws originated in ignorance of the cause and nature of the dreadful pestilences that now and then make their appearance at unwonted places; they have proven a God-send, it is true, to many officers and attendants who would otherwise be at a loss for the means of making their daily bread; but at the same time they have trammelled and clogged the wheels of commerce, inflicted cruel and unreasonable inconvenience upon the traveling community, been attended with heavy expense, and given but little benefit in return. The countries that tried them first, most rigidly, and the longest, are gradually relaxing the discipline, while strange to say, *we*, or at least *some* of *us*, here in the *very clime and cradle* of the Yellow Fever, Cholera, and Plague, are calling upon government to protect us from their ravages. Has the labor been lost that brought forward the immense array of facts and arguments upon this question, with which the records of medicine have been enriched within the last half century? Who now among the advocates of *infection*, brings to the support of the doctrine, any *new fact or argument*, or any thing more forcible, than what has been already adduced and refuted, over and over again? How much better would it be for us to look carefully *at home* for the morbific cause, and to devote all our attention to its removal? No one can be at a loss for the cause of plague, after reading the subjoined, account of the "Hygienic state of modern Egypt;" nor, in our opinion, ought he to expect much benefit from quarantine laws, after making a careful and minute survey of the purlieus of New Orleans, But let us hear Mr. Wakley:

Quarantine Law in France.—IN THE LANCET for March, 1844, we persented to our readers an elaborate account of the state of the quarantine laws in the more important European kingdoms, and discussed at some length the question of the incubation of the poison of the plague, as elucidated by the new data brought forward by M. AUBERT ROCHE. We then stated, that, stimulated by the late immense increase of our communication with the East, through the Mediterranean—rendered confident by our northern climate, our improved hygienic state, and the complete immunity from pestilential diseases which we have enjoyed from the middle of the eighteenth century,—the Board of Trade had so far relaxed in England the rigor of the quarantine laws as to render them, in our ports, *a mere form;* and that this had been done without the sanction of any public medical body, as we possess none such to be consulted. We also stated that the French Government, still trembling at the recollection of the terrific plague of Marseilles, (which occurred not much more than a hundred years ago, and was imported directly from Egypt in in a merchant vessel,) and aware that their ports on the Mediterranean are now, owing to steam navigation, only eight days' distance from the permanent focus of that fearful disease, had not dared to modify their quarantine regulations, although commerce greatly suffered by their severity.

Since we then wrote, the cause of free intercourse between nations has been agitated with so much success by members of the Academy of Medicine, by many intelligent medical practitioners personally acquainted with the disease, and by the public press, that the Government has relented at last, and a first important modification has been made in the laws which regulate the communications of France with the East. We must in justice, however, say, that the researches of M. AUBERT ROCHE have, no doubt, had the greatest share in producing the change. It will be remembered that M. AUBERT ROCHE proved, by the examination of the registers of the lazaretto of Marseilles, for the last hundred and twenty years, that during that period not one case had occurred in which the plague had primitively appeared on board, or in a lazaretto, eight days after the departure of the vessel from the infected locality; or, in other words, that there was not on record, during that lengthened period, a single instance in which the plague had appeared amongst the crew or passengers of a vessel, either previous to, or after, landing, if they had been free from it during the first eight days of the voyage.

On the 14th of last August (1844) regulations were published by order of the French Government, according to which passengers landing from a vessel with an "unclean Bill of Health" will have in future to perform a quarantine of fourteen days instead of twenty-five; those landing from a vessel with a doubtful Bill of Health, twelve, instead, of twenty; and those landing from a vessel with a clean Bill of Health, nine days instead of fifteen. By these regulations, however, passengers with an unclean Bill of Health only gaining nine days, and those with a clean Bill, six days, the French Mediterranean packets will still labor under a disadvantage, as compared with the English, whenever they have unclean Bills of Health. The passage from Alexandria to Marseilles, by steamer, takes seven or eight days, that from Constantinople nine or ten. With an unclean Bill, consequently, twenty-four days must elapse from the time of departure from Constantinople to that of being set at liberty at Marseilles, and with a clean Bill, nineteen; twenty-one and eighteen from Alexandria. In our own ports, at Southampton and Falmouth, for instance, fourteen days' quarantine only is exacted, and the quarantine is allowed to commence from the day of departure from the East. In the case of a suspicious death occurring on the passage, however, the fourteen days' quarantine would recommence from the date of the death. Thus, except on such an occasion, the quarantine actually expires before the arrival of the steamers in England, the voyage taking sixteen or eighteen days from port to port. It must be also recollected that French passengers are much nearer to Paris at Southampton than at Marseilles. The difference is, therefore, still sufficiently great between the time employed in the two routes to induce even the most patriotic Frenchman to choose the Southampton steamer in preference to their own if much pressed. France is still more accessible to travellers from the East by the continent than it is by our steam line of communication. The Austrian Government, under the pretence that a quarantine is now enforced at Constantinople against Egypt and the adjoining countries, has given up enforcing any quarantine whatever on persons coming from that city, so that by the Austrian steamers and Trieste a traveller may be in Paris from Constantinople in twelve days, or even less.

The above details are now supplied in order to show clearly the nature and extent of the changes that have taken place in France since we last noticed the subject, and to point out the present position of quarantine law in Europe. In England, these laws have already been nearly thrown aside, owing to the opposition of those merchants with whose commercial interests they had interfered, and to the general confidence generated by our long-continued freedom from even the appearance of danger. In Austria, the great commercial and political importance of a free and unrestricted communication betwee that country and the states on each side of the Danube, has been the real cause of the total abolition of quarantine regulations, as far as the Eastern states of Europe are concerned, and not the very nugatory measures recently adopted by the Turkish Government. But in neither England nor Austria has the great change in quarantine law been effected by the medical profession. The commercial interests have, no doubt, had the tacit approbation of the medical practitioners who knew what was taking place, but they have not been the agents by which the prejudices and fears of former times have been overcome. In France there are commercial interests of considerable importance involved, anxious to obtain unrestricted communication with the East; but alone they were powerless against the fears of Government. Since, however, the cause of free intercourse has been warmly espoused by the members of the medical profession, Government has lent a more willing ear to their requests, and has become a partial convert to the doctrine of non-contagion, as evinced by the late changes. The influence that the medical profession exercises over the executive and legislative bodies in France, is to be attributed to its powerful organization. The Paris Faculty of Medicine is composed of twenty-five professors, who have all gained their appointments, as it were, at the point of the sword, by concours, after years of labor and struggling. They are consequently a choice and select body, comprising many of the most illustrious medical practitioners of the day. Their value is fully appreciated, as well by the non-medical as by the medical public, and their opinion is listened to with respect and deference.

This remark applies still more forcibly to the Academy of Medicine, a more numerous body, and one which is therefore able to receive within its ranks nearly all those who distinguish themselves in their profession. It is to the Academy that the Government refers in all questions of difficulty, and its recommendations are adopted in nearly every instance. On the present occasion the executive applied as usual to be enlightened with respect to the conflicting statements and opinions which were brought before it, respecting the necessity of enforcing severe quarantine laws, and last year the Academy named a committee to investigate the subject, and to report thereon.

Although the advocates in France of unrestricted intercourse are much dissatisfied with the limited extent of the change that has recently been effected, an impartial observer must confess that the Government of that country has perhaps gone as far as it was authorized to go in the present state of the enquiry respecting the degree of transmissibility of the plague. The data advanced by M. AUBERT ROCHE, to which we alluded above, have been partially denied, although erroneously so, as far as we can judge. Still, even according to the account of that writer, the plague has manifested itself sixty-four times during the last hundred and twenty years, in vessels bound from the East to Marseilles, and persons belonging to these vessels haver epeatedly died of the plague in the Marseilles Lazaretto. He admits, also, that the incubation of the plague may last eight days, and the voyage from Alexandria is sometimes performed in seven. On the other hand, even supposing that the hygienic state of Marseilles, as well as that of most other modern European towns, is so far improved as to place it in a toially different condition to what it was in 1725, and that the Oriental plague, like our own typhus fevers, is propagated more by infection than by contagion, yet were it once to obtain a hold on the poor inhabiting the low and populous parts of the city, favored by a southern temperature, we might perhaps again see renewed the horrors of past ages.

Many interesting documents have been laid before the committee of the Academy of Medicine, which is still sitting, with reference to the plague, and among them one on the hygienic state of Egypt, which throws so much light on the origin of the dread disease, and on its perpetuation in that unhappy country, that it is impossible to give too great publicity to it. We shall, therefore, speedily revert to the subject, and lay before our readers some details of great interest relating to the present hygienic condition of the unfortunate Fellahs, who recognise the "paternal" sway of MEHEMET ALI.

The Hygienic state of Modern Egypt.—In the account which we gave in our last number, of the present state of quarantine law in France, and in Europe generally, we alluded to a communication lately presented to the Academy of Medicine, at Paris, by M. HAMONT, a French physician, respecting the hygienic state in Egypt, We consider this document to be of so important a nature, from the light it throws on the causes which engender and perpetuate the plague in that country, for so many centuries the principal focus of the disease, that we shall at once, without waiting for the decision of the Committee of the Academy, lay before our readers a condensed account of M. HAMONT's interesting memoir. The physician, having resided fourteen years in Egypt, has had every opportunity of forming correct views as to the state of the population of that fertile but unfortunate country. It will be seen that his statements tend greatly to corroborate the views now very generally entertained respecting the nature of the plague—viz., that it is merely *the* malignant typhus of the East, created and propagated, like our malignant typhus, by infection or contagion, according to circumstances.

It is now, says M. HAMONT, an universally admitted fact, that the plague is endemical in Egypt, although it is not so generally known that this is the case only in Lower Egypt, in the Delta of the Nile, and not in Upper Egypt. It is not however, the climate of the Delta that is in fault,—for the climate of every part of Egypt is beautiful, admirable,—but the inhabitants themselves who appear to do their utmost to create causes of death for their own destruction. Their dwellings —the air which they breathe—their food—all contribute to vitiate the elements of life.

The FELLAH constructs his hut with mud, on the margin of stagnant water, where the bodies of numerous animals macerate and rot. Against it his neighbors erect a second, a third, so as to form a group of huts closely approximated one to the other, and to leave little or no interval for the circulation of the air. In these miserable huts, men, women, and children, half naked, lie mingled on the damp floor, from which, generally speaking, they are only separated by a rotten mat. Around these wretched habitations you tread on the recent excrements of men and cattle, on heaps of refuse, where famished dogs dispute the possession of the putrefying flesh of a cow, a camel, or an ox, frequently dragging the mangled intestines to the very threshhold of the houses. As the Delta is a plain of a uniform level, in order to construct their habitations, the peasants are obliged to dig excavations, which they do in the immediate vicinity of their future residence. The pits thus formed are filled with water at each inundation of the Nile, which remains stagnant during the remainder of the year; millions of insects give life to them, and they soon become as repulsive to the smell as to the sight. Yet it is from these sinks of corruption that the peasants draw the water they drink and use, and in them that they perform the daily ablutions enjoined by their religion. The Fellah himself, nearly naked, sows and works in the mud of the rice-grounds, near which he often sleeps. When his wife and children are not assisting him in the fields, they collect the excrements of men and cattle deposited near their dwellings, and mixing them with the muddy, fœtid water, work the nauseous compound, with their hands and feet, into small, round, flat masses, which they stick against the walls of their habitations to dry, and which they use as fuel. The air which the Fellah and his family breathe is thus perpetually tainted and corrupted by the noxious emanations proceeding from vegetable and animal substances in a state of putrefaction. It would seem as if every act of his life was destined to be the very reverse of what it ought to be, hygienically speaking. Around his residence he creates a focus of corruption so horribly offensive, that its proximity becomes painfully evident to the traveller long before he reaches it. Nor is the contamination confined to his own dwelling. In order to purify himself before he offers his daily prayers in the Mosque, he visits an outer yard devoted to this purpose, and then washes himself in an adjacent tank of putrid water. The contents of these yards, where fifty or a hundred Mussulmans may be seen at a time, flow into an uncovered canal, which generally terminates in a large ditch in a public square, near the habitations. This ditch is not closed, and never emptied. Its contents overflow, soak into the earth, and, like a black lava, penetrate everywhere into the roads and houses. It is easy to conceive how offensive the atmosphere must be in such a place during the hot months of July, August, and September; and wherever there is a Mosque, such is the state of things. A stranger may always discover the Mosque, in Lower Egypt, by attending to his sense of smell. The more fœtid the air becomes, the nearer is the Mosque.

In order still further to concentrate the pestilential exhalations which he generates about him, the Egyptian surrounds his village, constructed in a low, damp soil, with a heap of rubbish of all kinds, thus rendering the ventilation of the locality in which he resides still more difficult, and making the refuse which might fertilize his land an additional source of disease to himself. Cooped up in this disgusting retreat, he seems as if he had done his utmost to create for his own use a focus of disease and corruption, and to poison the valley of the Nile, in which Providence has thrown, with profusion, elements of conservation. Infested with vermin, he seldom changes his linen, and, as if his nose and mouth were not enough to absorb the mephitic air which he "composes," he is generally clothed in rags, or in a linen tunic, which leaves the greater part of his skin exposed.

In his food, the Fellah shews an equal contempt of all the laws of hygiene. The butcher generally chooses the dirtiest spot in the village for his operations, and prepares his meat on a dunghill, surrounded by animal remains in a state of decomposition. The animal's throat is cut, the skin is taken off, laid on the ground, and the butcher, with a hatchet, divides the carcass into joints. The meat is then washed and squeezed by the butcher and his assistants, nearly always covered with vermin, in a state of squalid filthiness, until all traces of blood have disappeared, when it is hung up for sale on an adjoining wall. Whilst the butcher is thus engaged, filthy old women seize the intestines and empty them, defending their prey with difficulty from the dogs that crowd around. The meat, if not sold immediately, is attacked by myriads of flies, and rots, but still the Fellah purchases it. On certain market-days, large quantities of badly-salted, half-rotten fish are sold. On these occasions, the effluvium emitted is so horrible, that a stranger exposed to it falls to the ground, unless he escape by flight.

To complete this scene of misery, and as if entirely to annihilate the beneficent influence of the climate, the Egyptian not only leaves the cattle that die, to rot in the fields, or on the roads, where they fall, without an attempt to bury them, but also acts so as to make the remains of his fellow-creatures a source of infection to the survivors. The dead are laid either in or on the earth, covered with a thin layer of sand, of baked bricks, or of mud, or placed in tombs. In the first case, the covering is soon dispersed, or cracks, and the body remains more or less exposed. It is then visited by swarms of flies, who luxuriate on the decomposing tissues, and become themselves a source of infection to cattle and men, on whom they afterwards alight The tombs, in those parts of the country in which they are used, are long, rounded, or square constructions, closed by a stone at one end, and placed side by side. Each tomb is a small cemetery in a larger one. The dead are thrown into them, one on the other. Imperfectly constructed, and imperfectly closed, they are often in ruins, and always partly open. Dogs, hyænas, and jackals, attracted by the smell, penetrate into the cemeteries, and into the tombs, drag out the bodies, and leave the mangled remains exposed to the air. The emanations from an Egyptian cemetery, mostly situated in the centre of the dwellings of the living, are a permanent source of disease. Every night after sunset an impure atmosphere issues from them, the effects of which are perceptible even on the natives themselves. If such is the state of things in ordinary times, what must it be when a pestilence rages; when the cemeteries of fifteen or twenty villages, near to each other, or those of a large town like Cairo, are thus filled, to overflowing, with human bodies, in a state of putrefaction, thrown confusedly into these open tombs, not interred; and yet it is in vain to reason with the Fellah. If you point out to him the inevitable results of these odious practices, he thinks you are an atheist; and tells you, showing the sky with his finger, that everything comes from heaven, that our days are numbered, and that if he were to die to-day, it would be because he had arrived at the end of his destined course.

In winter, violent rains, frequent in the Delta, fall like torrents on the mud huts of the Egytians; stir up, divide, and liquify, the heaps of refuse which surround their houses and villages; as also the earth of the cemeteries, causing the putrid ditches and tanks to overflow; and the corrupt stream penetrates, without meeting any obstacle, into their habitations. The maceration of hemp adds to the deleterious influence of these noxious streams. When the Nile is about to overflow, the hemp is placed in ditches, and partly covered with stones or earth. On the river's retiring, these ditches are found to contain the bodies of an immense number of rats, mice, insects, reptiles, fish, &c., which macerate and rot, giving rise to the most frightful fœtid effluvia. It is, indeed, next to impossible to remain in the vicinity of these foci of putrefaction, so much is the atmosphere contaminated.

The Egyptian in good circumstances lives well, but more than three-quarters of the population live on substances the very sight of which is sufficient to occasion nausea. The meat which they eat is the flesh of oxen, camels, or sheep, that have died of disease—of carbuncle, or dysentery; and even for that they are made to pay. The bread is made of Indian corn, when it can be obtained, but often of cotton-seed, of linseed, of the stones of dates, powdered, and made into cakes. The most common article of diet is old rotten cheese, made with bad milk, and kept in pots in which may be seen thousands of maggots. To this cheese they often add sour oranges, to the flavor of which they are partial. In addition to the above, they habitually eat the leaves of the marsh mallow, of the thistle, and of white clover, bad green rotten dates, raw onions, and cucumbers, a kind of melon without flavor, radish leaves, and rotten fish.

The Fellah, besides his own labors, is continually called upon to perform the most irksome tasks—such, for instance, as the cleansing the canals. For this latter occupation, the young and the old, the lame and infirm, are all put in requisition. The mud is scraped up with the hands, and thrown on the side of the canal. The miserable creatures pass the day in the dirt and wet, and sleep at night on the side, covered with mud from head to foot, and merely protected by a mat, when one can be obtained.

Although thus wanting food, clothing, everything, the Fellah is, nevertheless, pursued from morning to night by the agents of government. If he be unable promptly to satisfy the insatiable exigences of the tax-gatherers, he is seized, bound, thrown into a damp prison, and beaten. When he has nothing left, when he has sold his last rag, he is tortured, hung, impaled, and his ears and nose cut off, or his teeth pulled out.

As M. Hamont justly says,—What can we expect of an organization formed under such influences as those which we have described? Living in the midst of everything that is repugnant and disgusting; placed from the day of his birth to that of his death in an abnormal and exceptional position, it is surprising that the diseases to which the Fellah is exposed should also be exceptional.

The number of those who are suffering from disease in Lower Egypt is immense. Eighty out of every hundred Fellahs, at least, are laboring under chronic incurable ophthalmia; and a large proportion are, besides, attacked with scrofula, porrigo, scabies, elephantiasis, diarrhœa, dysentery, marasmus, cachexia, and lastly, with the plague.

The plague is a permanent pestilence in this wretched country, but it is only about every ten years that it assumes a violent epidemical character. In the interval of these decennial ravages, says M. Hamont, it is often seen in a chronic form. Many of the Fellahs present tumors in the axilla or groin, which increase gradually, without much pain, suppurate, and disappear, which M. Hamont considers to be a chronic kind of plague. This dread disease generally appears at Cairo after the solstice of winter, and ceases at the end of June; but this is not always the case. When it reigns epidemically, its ravages are frightful. In 1835, two hundred thousand people died in Egypt. In April of that year, seventeen thousand persons died at Cairo alone; in May, ten thousand. M. Hamont thinks that these periodical, epidemical invasions may be attributed to the saturation of the constitution of a large proportion of the inhabitants with the morbific agencies which we have enumerated. This part of the population being swept away by the disease, the country again enjoys comparative freedom for a certain period. M. Hamont was attached to the Cairo hospitals for some time, and had the plague-patients taken to tents erected at a distance from the town; he also examined the dead on a table in the open air. In not a single instance was the disease communicated; whereas, in the hospitals where this precaution was not adopted, many medical men, as well as attendants, and other patients, fell victims. Although M. Hamont looks upon the plague as only infectious, and not contagions, he does not think that the French government would be authorized to do away with the quarantine, or to reduce it more than they have done by the regulations of last August.

We cannot read the details furnished by M. HAMONT without being struck with the great similitude of the circumstances under which the plague arises in Egypt, and malignant fevers in our large towns. It is in the badly-drained, badly ventilated suburbs of London, Edinburgh, Dublin, Liverpool, &c., among a population of badly-fed, half-starved individuals, that malignant typhus makes its ravages. Wretched, however, as is the hygienic state of many of the inhabitants of these localities, it is immeasurably superior to that of lower Egypt, and it is perhaps to this cause that England owes its modern immunity from the plague. Formerly, when ventilation and draining were unknown, when the population of large towns were cooped up, in England, as elsewhere, behind fortified walls, the plague paid us periodical visitations, as it now does Egypt; nor was it possible to trace its origin to communication with the Continent. It is evidently to the gradual improvement of our hygienic state, and to the spreading of the inhabitants of large towns over a large area, that we owe our superior sanitary condition. When men congregate, they become a focus of infection and death to each other, and it requires all the ingenuity which our superior mental organization has endowed us with, to mitigate, to ward off, the elements of destruction which we create around us. It is because the modern Egyptian has failed to avail himself of his intellect in order to neutralize the influence of his own impurities, that he may be said to live in the jaws of death, that the population of his native country has fallen from ten millions, to which it amounted in former times, when hygiene was known and practised, to less than a million and a half. The lesson ought to be a fruitful one to us.

TRANSYLVANIA UNIVERSITY, AND THE NAVY.

The following correspondence was sent to us in a slip from "the Observer and Reporter," a newspaper printed at Lexington, Ky., and we see it also published in the Western Lancet. In accordance with the expressed wish of the Dean of the Faculty, we insert it in our Journal with pleasure; feeling happy at having it in our power to aid in disabusing the public mind in regard to an erroneous impression, and in rescuing a very respectable Institution from "the base efforts of falsehood and calumny." The evidence here brought forward is amply sufficient for this purpose, and we hope and trust the contemptible and malicious slander will recoil with merited severity upon the head of its author. The Transylvania Medical School, planted as it was in the depth and bosom of the mighty West, the pioneer of science in a land but recently reclaimed from savage barbarism; and attended as it has been by the bold, talented, but often rudely educated sons of the West, could not be expected to send forth graduates altogether as accomplised as the *alumni* of older and more favored institutions. But who will contend that *the school makes the man?* If any there be; let him search the records of science and of fame, and he will find that great men have sprung up from all quarters, and that genius will triumph over all obstacles; that great physicians have emanated from the humblest school; whilst all the facilities and advantages of the most renowned universities have been lavished in vain upon dullness and folly. If Transylvania has done harm (and we do not defend her against the charge) by cheapening medical education and lowering the standard value of the diploma, thereby inducing many young men of limited means and deficient primary education to desert the counting house, the work bench and the plough, for the more arduous and responsible career of medicine, her rivals have imitated her faults; while none of them have yet excelled the talents, energy, and fame of her professors; nor have we reason to believe they annually turn away from their halls, a better qualified set of graduates. We are of those who believe that the standard of medical education demands elevation throughout the union, and we are greatly mistaken if Transylvania would not readily co-operate with the other medical schools, in the accomplishment of the object established.

MR. EDITOR:—In the New York Herald of April 7th is an article purporting to be a letter from a Washington correspondent, dated April 5th, from which we extract the following:

"It is a remarkable fact, mind that, a remarkable fact, that at the last examination of applicants for the appointment of Assistant Surgeons for the Navy, out of thirty examined, only sixteen were found qualified, and that the fourteen rejected as incompetent, were graduates of the Medical College of Lexington, Ky., and Cincinnati, Ohio. Pray, Drs. Dudley and Locke, how is this? Is it possible your Professors award diplomas initialized M. D., to green-horns, not even qualified as Assistant Surgeons in the Navy? Look sharp, gentlemen, look sharp, or the Louisville College will carry off the premium."

When this foul slander first met our eye, we pronounced it a base and malignant falsehood, got up for no obvious purpose, save to gratify a spirit of incendiarism, that glories in its own shame, and would prostrate in the dust the fair fame of individuals, as well as of public institutions.

Determined to have the truth fully out, we addressed a letter to a member of the Navy Board, whose prompt reply is here presented and speaks for itself:

PHILADELPHIA, April 21st, 1845.

SIR:—I have this day received your letter of the 15th inst., proposing the following queries, viz:

"How many candidates for examination for the place of Surgeon or Assistant Surgeon in the Navy have you known to have reported themselves as graduates of Transylvania University?"

"How many of the same have been found unqualified?"

It gives me pleasure to state in reply, that of the candidates for admission into the Medical Department of the Navy, rejected by the last Board of Naval Surgeons, not one was a graduate of either Lexington or Cincinnati. Nor has any graduate of the Transylvania University yet presented himself before any Board of which I have been a member. I am, very respectfully,

Yours, &c.

SAMUEL BARRINGTON,

Surgeon U. S. Navy.

To THOS. D. MITCHELL, Prof. Mat. Med. and Therap. Transylvania University.

If our recollection serves us, Dr. Barrington has been one of the Examining Board for at least three years, and his testimony will satisfy every honest man. We are authorized to state further, that of a large number of candidates examined some six or seven years ago, for the place of *Army* Surgeon, the gentlemen who stood at the highest point of professional merit, in the judgment of the Board, was a graduate of Transylvania University, and a native of Kentucky. He is yet in the army, and a highly valued officer.

The conductors of the public journals who are disposed to rescue time-honored and useful institutions from the base efforts of falsehood and calumny, are requested to copy this article. We feel that we have a right to claim an insertion in every paper in which the slander of the Herald has appeared. The scandalous injustice perpetrated on the school of Lexington, (for which the libel was mainly designed) is so manifest, that every man of integrity and fair dealing will be ready to award the full measure of infamy, so richly earned by the mercenary and unprincipled author.

THOS. D. MITCHELL,
Dean Med. Fac. T. U.

Lexington, Ky., May 3d, 1845.

PART THIRD.

BRIEF NOTICES OF MEDICAL LITERATURE.

Art. I—*Principles of Forensic Medicine.* By William A. Guy, M. D., *Cantab. Fellow of the Royal College of Physicians; Professor of Forensic Medicines, &c. &c., First American Edition, with notes and additions*; by Charles A. Lee, M. D., *Professor, &c.* New York: Published by Harper & Brothers, No. 82, Cliff street, 1845, pp. 700.

The object of forensic medicine is chiefly to qualify medical men to give an opinion, in questions relating to murder, whether by poisoning or otherwise; mental diseases and a number of different questions which may grow out of this subject. It embraces the entire circle of medical science, and no small portion of that of jurisprudence. If the public, especially that portion of it, disposed to commit murder by secret and unknown means, were convinced, that by the help of toxicological science, medical men would be enabled to discover the agent by which death is brought about, we are disposed to believe that crimes of this sort would become less frequent, because its punishment would be more certain and inevitable. By becoming good medical jurists, we should diminish the number of criminal prosecutions, because a well founded apprehension of being detected and punished, would inevitably disarm another " fell Locusta" and frighten from his infamous purpose the cowardly assassin! The ingenious arts and premeditated tricks with which the artful murderer attempts to conceal his infernal purposes, are based upon the presumption (too often realised alas!) that he will outwit the doctors, and set the laws of both God and man, at defiance. In former times, when medical jurisprudence scarcely existed as a science, murder by poisoning, was far more frequent, although the number known was much fewer than now, when this science has attained a degree of certainty which must and does carry terror to the hearts of those who would seek to destroy life by poison. This is a view of the question which we think should stimulate the efforts of all philanthropic physicians to collect and publish every important fact which may tend to shed a ray of light upon the difficult science of "forensic medicine."

That this is one of the most *useful* branches of the profession, and one too about which our information should be as accurate and clear as possible, cannot be questioned, if the reader will take the trouble to examine the work of Mr. Guy; in this, he will not fail to remark the great diversity of opinions advanced by medical witnesses, on principles and facts, in medico-legal questions, about which they might agree, if well informed on the subject in the premises. It is not to be expected that every practitioner is to become a profound toxologist, as an Orfila or a Christison, or as able a medical jurist as Guy, Taylor, Zacchias or a Beck, but each should feel it his duty to acquire a competent knowledge on the subject, in order that when called upon by the laws of the land, he may clear up doubts and difficulties, and thus add weight and dignity to the profession. When it is remembered that the *opinion* of a medical witness is received as evidence in cases of murder, by poison or other agents, it will be readily conceded that we should be extremely circumspect in making up an opinion, in cases of doubt and uncertainty.

As it is mainly upon medico-legal question, the most difficult of all, that the physician is called upon to testify, before the public, it will be at once seen that if we are unprepared to make the case clear to the court, in the eyes of the world, the witness is stigmatised as stupid and ignorant ot science.

Gentleman of the bar, who like hungry wolves are let loose upon the poor medical witness, expect him to give decided and positive answers to a thousand questions whose bearing they neither see nor understand ; for it is emphatically in questions of this sort that the fool may baffle the philosopher. The lawyer being but little acquainted with the medical sciences, imagines, that as in the law, so in physic, we must be governed by a precedent which holds unconditionally in all cases.

To suppose a case—a man receives a blow on the head from his antagonist; the scalp is divided, but not extensively; some blood flows ; the patient seems to be doing well, when suddenly erysipelas attacks the scalp ; fever is kindled up; delirium supervenes ; finally effusion upon the brain, followed by coma and death. The assailant is cited to appear before the Criminal Court ; the medical witness is placed upon the stand, and is gravely asked by the *learned* attorney, to define the difference between a wound which is mortal, and one which is not so ! Failing to do this, he is made an innocent object of ridicule at whom the withering sarcasm of the bar and bench is levelled. Whereas, in truth, it is well known, that the most trifling injury is sometimes followed by speedy death ; and in other instances, from the most dreadful lacerations, the patient may recover. In questions of this sort, we can not be too guarded, both for our own, and the standing of the profession. For a vast deal of valuable information on this subject, we refer the reader to the work under review ; it will be found to contain the experience of the profession on the various subjects of which it treats.

It has been remarked by some, and we believe that it is generally accredited, that Mr. Guy has forfeited all claims to authorship, that he has in fact copied from Mr. Taylor's work on medical jurisprudence. The facts relating to a subject of this nature, special in its bearing, belong to any man who may undertake to collect and arrange them for the instruction of the profession. If the charge of plagiarism be well founded, which we will not undertake to deny, still this fact does not diminish the great value of the book, although Mr. G. be shorn of the honors which appertain to the author.

To be familiar with forensic medicine, we must have correct notions on all the different branches of medicine; anatomy, physiology, pathology, therapeutics, chemistry, &c. must be invoked to aid us in solving difficult medico-legal questions.

In the universities of Austria, it was formerly made obligatory upon the lawyer, to make himself well acquainted with forensic medicine; but it was soon found that in order to do this, they had to master the science of medicine; consequently government gave it up, and now the Austrian is no better informed on this subject than the American attorney.

If medical men in this or any other country, will pay some attention to forensic medicine, we shall have no other business for the lawyer than to institute and conduct the suit in criminal prosecutions of this sort. As a book of reference, we place a high value upon the work of Mr. Guy. It contains a full exposition of all medico-legal questions, and with the additions by Professor Lee, it will be found perhaps the most complete work on the subject in the English language.

As regards the charge of plagiarism preferred against Dr. Guy, by Mr. Taylor and some of the medical journals of the day, it may be said in extenuation of so grave an accusation, that Mr. G. attended Mr. T.'s lectures on the subject, and it was natural to expect the pupil should adopt the ideas and even, in some instances, the very language of his master, when writing on the same subject.

We shall however leave this question to be settled by the parties concerned. It is certain that Mr. Guy's work contains much the largest amount of matter, and we think much better adapted to the wants of the profession in this country.

Mr. J. B. Steel, 14, Camp street, has the work for sale.

ART. II. *A practical treatise on the diseases peculiar to women, illustrated by cases derived from hospital and private practice.* By SAMUEL ASHWELL, M. D., *Member of the Royal College of Physicians, London, and Obstetric Physician and Lecturer to Guy's Hospital. First complete American from the last London edition. With Notes by* PAUL B. GODDARD, M. D., &c. &c. &c. Philadelphia: Lea & Blanchard, 1845. pp. 520.

We are called on to notice another valuable work on obstetric medicine. The recent works of Chailly, Lee, and Colombat de l'Isire have already received our cordial approbation, and after a careful examination we feel no hesitation in pronouncing the production of Dr. Ashwell equal in value to any that has ever been produced upon the subject. Perhaps some allowance should be made for the partiality that is apt to be engendered in behalf of the book that has last interested and engaged our attention, and we should be sorry to be guilty of bestowing unmerited praise, but we have found this work of Dr. Ashwell so replete with practical knowledge, and at the same time so concise and well arranged, that we cannot withhold from it our earnest recommendation. It has received the commendation of the medical press throughout Great Britain and America, and will make a valuable and appropriate accompaniment to some one of the able systems of midwifery recently published. It supplies a mass of practical knowledge in regard to the diseases peculiar to women, that is never found and indeed cannot well be embraced in such works. The rich field of observation presented by Guy's celebrated hospital, and an extensive practice in modern Babylon has been assiduously cultivated by our author, and from it he has garnered up and distributed among the profession at large, treasures of knowledge that entitle him to the gratitude of mankind. We heartily concur with him in the opinion "*that practitioners who hold important public appointments are bound, so far as their source of authentic information can be made subservient, to improve and increase the common stock of professional knowledge.*" It surely is a reproach to any man to *cumber* a post of honor, obtained perhaps by improper influences, and without regard to merit; to neglect the advantages within his reach, or to obstruct the march of improvement. Yet instances of the sort are often seen, and no where more frequently than about large hospitals. A selfish desire of personal aggrandisement is the ignoble motive that actuates them, and it should be visited by the contempt it merits. The example of Dr. Ashwell and other distinguished names abroad, is well worthy of imitation. We highly approve of the authors' plan of introducing *numerous cases*, which he says, "are narrated in order that their symptoms may show whether the histories of the various diseases are accurately given, and from their successful or unfavorable issue, the danger of the malady and the worth of the treatment may be demonstrated."

We also highly approve of the copious *formulæ of remedies*, appended to the various chapters. Although practitioners are familiar with the general virtues and powers of medicines, yet particular *formulæ* of doses and combinations that have received the sanction of extensive experience, are always acceptable, and especially to the junior members of the profession. Next to direct observation which we may not have opportunities of making, comparison offers the best medium for conveying instruction, and here the advantages of the authors' plan are most evident; after giving the history and pathology of a disease, he details the symptoms and treatment, and thus offers to the practitioner that may consult him the surest guides to success.

The work consists of two grand divisions; Part first, on functional diseases of the uterine system.

Part second, on the organic diseases of the internal and external female genitals. To these are added an appendix, on the morbid consequences of *undue lactation*.

The first part is divided into nine chapters, of which those treating upon *leucorrhœa, dysmenorrhœa, menorrhagia, and disorders attendant on the decline of menstruation*, are replete with interest.

Part second is divided into eight chapters, comprising all the organic affections to which the female system is liable, from the trivial to the most serious and dangerous, including displacements of the uterus, &c; and enriched as they are with well reported cases, will be found of great value to the retired practitioner, to whom such cases are at any time liable to occur, though rarely. We can scarcely imagine a difficult or perplexing case among the diseases of women, to which a parallel might not be found in the work of Dr Ashwell. The resemblance between comparatively simple and malignant diseases is frequently so striking as to render the distinction exceedingly perplexing; but the diagnosis of such lesions is a matter of great moment. Dr. Ashwell says—"I know practically, that it is sometimes almost impossible, with every aid, to arrive at *certainty* respecting the precise character of complicated diseases of the womb; but I also know, that they are often overlooked or misunderstood, from the want of sufficiently and careful investigation. Nor is this delay to be altogether ascribed to the practitioner: there is, amongst delicate females, a natural although an unsafe repugnance to the early and necessary examination; and the concealed situation of the uterus, within the pelvic cavity, renders the task however ably performed, by no means a simple one. It is scarcely necessary to remark, that to do so successfully, *the healthy condition, and the healthy varieties of the female generative organs must be understood*. It will be in vain to attempt to appreciate morbid deviations, if this previous knowledge be not possessed. * * * * * From two sources, important facts may always be obtained, and from the discharges, knowledge, illustrative and conformatory of the true pathology of these affections, may generally be elicited.

The history of the symptoms, and the examination by touch, afford, in every instance of organic uterine disease, certain and indispensable information: whilst, the speculum, the stethoscope, and the discharges, will often assist; and may occasionally, lead to an incontrovertible opinion." His chapter "on premature labor in pregnancy, complicated with organic disease" is one of peculiar interest. Dr. Ashwell claims the credit of first suggesting and recommending this practice, and gives an interesting narrative of the cases that prompted him to think of it. It is one of almost equal importance with the induction of premature labor in great deformities of the pelvis, which has now received the sanction of the most enlightened portion of the profession, especially in Great Britain and America. The cases under immediate consideration are chiefly those attended with tumors in the vagina, a number of very interesting ones are related.

The chapter "on organic diseases of the cervix and os uteri" are rich and instructive; also those "on the displacements of the uterus;" and "on the diseases of the ovaries;" containing the latest improvements and discoveries.

The subject of "*the morbid consequences of undue lactation*," is ably discussed in the appendix, and illustrated by half a dozen instructive cases. It is a subject in regard to which, we venture to say scarcely a practitioner can be found who has not at some time been consulted, and it is one of very grave importance. Who has not witnessed one or more instances of delicate and interesting mothers having been brought to untimely graves by undue lactation, founded upon the *dread of increasing families?* Truly it is an awful and melancholy alternative, whether a poor helpless woman shall have her constitution broken down and exhausted by the cares and sufferings attendant upon too frequent childbirth, or *be driven into consumption* by nursing too long from her bosom, the beloved offspring, whom, she feels conscious she has barely strength enough left to raise and maintain! yet such is the dilemma in which many most inestimable women are placed, and it is one worthy the serious and deliberate attention of the medical philosopher and the phylanthropist: But the subject is one of too delicate a nature to admit of discussion, and it is probable we shall continue to see women who have the misfortune to be *delicate*, sacrificed upon the altar of hymen, the *poor* doomed to a life of toil and care, and the world filled with helpless superabundant population.

We cannot make room for further remarks upon this valuable work. It should be in the hands of every practitioner, and will generally be found an able counsellor under painful and trying difficulties. The author will certainly not have occasion to complain of the American editor for obtruding too many of his remarks upon the attention of the reader. He has left him the field almost exclusively, and we suppose only claims the credit of having brought so valuable a work to the notice of his countrymen.

The volume may be had at the Bookstore of Mr. J. B. Steel, No. 14 Camp street.

ART. III.—*Principles and Illustrations of Pathological Anatomy; being a complete series of Colored Lithographic Drawings.* By J. HOPE, M. D., F. R. S., *physician to the St. Mary Le Bone Infirmary; Mem. Hon. de la Société de Statistique Univvrsalle; Extraord. Mem. and formerly President of the Royal Med. Soc. Ed., &c. First American Edition, Edited* by L. M. LAWSON, M. D., *Professor of General and Pathological Anatomy and Pathology, in Transylvania University,* Cincinnati: Desilver & Burr, Lexington: A. T. Skillman, & Sons 1844, pp. 259. With 250 Lithograpic drawings, and over 200 pages of explanations.

The reputation which Dr. Hope acquired in the profession, by his splendid work on the "Diseases of the Heart," will prepare us to expect much from the same able pen on the subject of general pathology. The work before us, the last, we believe published by the author, is a welcome and a valuable accession to the works on pathology which we already possess. Dr. Hope seems, from this specimen, to have had clear conceptions of the real objects and advantages of pathological science to the young practitioner. He describes and represents the more obvious, and the more striking changes wrought in the system by disease. He does not consume his own and the valuable time of the student in writing long chapters on the morbid alterations of insignificant cryptæ, or mucous glands, whose very existence, is alone revealed to a powerful microscope. Refinement in descriptive pathology is too much the fashion of the day; and there are some who give more of conjecture than demonstration. Now we are disposed to acknowledge that without some acquaintance with minute and internal pathology, our therapeutics would be based upon great uncertainty; yet it must be confessed, that some distinguished practitioners, disregarding the theories of the mere pathologists, prescribe with a bold hand, a course of treatment, based upon the general features of the disease,—the obvious symptoms, and neither know nor inquire into the mysteries of special pathology. By this, we would not seek to justify an ignorance of pathology; but if we become thoroughly imbued with the doctrines of the day, we shall assuredly become *ultraists;* afraid even to give the slightest stimulant, lest we aggravate an ideal—a supposed inflammation. The pathologist, in typhus fever, forbids the internal use of stimulant tonics, for fear of increasing the irritation seated in the small intestines; but the shrewd and observing physician will support, be these very means, the exhausted and sinking system. And what is the result. The former suffers his patient to die, and comes out with a flaming, and a most verbose description of the pathological changes, produeed in the glands of Pyer, Brunner, &c ; thus proving his diagnosis, and adding to his reputation ; whilst, the latter cures his patient and is set down as stupid and utterly ignorant of the pathology of typhus.

Dr. Hope limits his deseription to those sensible, obvious and appreciable changes which result from diseased aetion ; and henee the great value of this work to the young physician, who is desirous to learn so mueh of pathology as will enable him to institute a rational and sound system of practice. Although we do not condemn eolored plates, yet we reluctantly confess, that without a familiar acquaintance with morbid specimens, we can have no adequate—no elear conception of the ehanges both of color and consisteney, effected in the tissues of the body, by disease. As far however, as these helps can go, to instruet us, the plates here furnished, will be found *generally* correct. To those, whose opportunities for the study of morbid speeimens, are few and far between, these plates may be of essential service, being well ealeulated to give sueh a general idea of structural changes as will enable them to treat disease upon sound and rational principles. We would respeetfully suggest to those who undertake to delineate the ravages of disease, the great utility, in our humble opinion, of always representing the same parts both in a sound and diseased state, upon the same plate ; for few young practitioners have clear and distinct notions of the appearances of sound structures exeept when contrasted with the same organs in a pathlogieal state—Andral himself, who has made over 5,000 autopsies, aeknowledges this difficulty, and devotes a long ehapter in his work on pathology, to this subjeet. As the complexion, and color of the skin of different individuals differ greatly, so we might find a corresponding difference in the eolor &c, of the internal surfaees, especially the gastro-enteric, in different subjeets. Yet this would not justify us in noting down these differenees as pathological ; as a departure from the normal, the pathological state. We venture to assert that the gastrie mueous membrane of many persons, even in health, presents a degree of redness, injection, &e., which in other individuals would amount to aetual disease—a moderate attaek of gastritis—

"Quot. hemines; tot corpora."

If therefore we overlook these constitutional peeuliarities, we shall give a false coloring to nature, and mistify rather than elueidate an important branch of medical scienee.

The "*diseases of the respiratory apparatus*," are first described ; these are made to inelude *cardiac affections*, *bronchitis*, *&c.* The second divis, ion comprehends the "*diseases of the liver* ;" as the "*spleen*, *kidnies*- *bladder*, *peritoneum*, *uterus*, *&c.* The last part treats of the diseases of the "brain and spinal cord." On the diseases of each of these organs, Dr. Hope has given a clear and concise deseription, aiding the student when necessary, by pretty aceurately drawn plates. More verbal descrip tions of morbid anatomy, will not make a lasting impression upon the mind ; hence our author has endeavored to give a correet transcript of pathological alterations, as these will serve to refresh the memory of the practical pathologist, while they will convey to the less experienced a pretty general knowledge of morbid changes. Plates, however eorrect should never supercede the necessity of minute post mortem expmination, when this is practicable; for, although they may be colored to (*death*) nature, still, much may be gained by *touching* and *handling* the diseased structure. In receiving instruction, we should never rest content with the exercise of one sense, when two or more may be called to aid the understanding.

The original cost of this work was so great, that it was necessarily limited in its circulation. The object of the American editor, Professor L. M. Lawson, was to bring it out in a neat and cheaper form, and thus enable the profession at large, to procure, at a trifling expense, one of the most valuable works of the day. The notes by Professor Lawson, are exceedingly judicious and appropriate. He deserves, for his labor, the thanks of the profession.

ART. IV.—*The Principles and Practice of Dental Surgery.* By CHAPIN A. HARRIS, M. D. *Professor of Practical Dentistry in the Baltimore College of Dental Surgery, and member of several learned American Societies, second edition: revised, modified and greatly enlarged. Illustrated by* 69 *wood engravings.* Philadelphia: Lindsay & Blackston, 1845, pp. 600.

This is the second edition of the principles and practice of dental surgery; and this fact is the best assurance of the intrinsic value of the work. Dr. Harris has long enjoyed the confidence of the dental profession, and an experience of over twenty years practice in dentistry, is a safe guaranty that his theory and practice will stand the test of time and experience. This, the second edition, is greatly augmented, and contains in addition, the "anatomy of the mouth," at least as full as will be found necessary for practical purposes. We have examined the work under notice, and although unacquainted with the minute details of practical dentistry, yet we know enough of the elements of the art, to hazzard the opinion that this work will prove to be high authority on the science of which it treats.

Part first is devoted to the consideration of the "anatomy and physiology of the mouth" and the various subjects connected with this part of physiology. In his "*second part*" he examines the principal characteristics of the teeth, gums, and salivary calculus," including the "fluids of the mouth," the characteristics of the lips and of the tongue. *Part third*, is dedicated to the "diseases of the teeth," in all their multiform variety, comprehending several of a special nature. The *fourth part*, is taken up with the consideration of "salivary calculus," and diseases of the gums. In the *fifth part*, he encroaches upon the confines of surgery proper, but legitimately claimed by the dentist, viz: the "diseases of the maxillary sinues." The teeth are frequently exciting causes of abscesses in the maxillary sinus; and this fact should direct the attention of the surgeon and dentist to the teeth in the superior maxilla, when this cavity seems to be diseased. In his *sixth* and last part, he enters upon the practical, the "*mechanical*" branch of dentistry. Here Dr. Harris is quite at home; he lays down admirable directions for rectifying the diseases and deformities of the organs of mastication. He is no quack in his profession; he examines his subject thoroughly and developes its principles with the skill and science of a chief chirurgical dentist. Throughout, the work is illustrated with handsome wood engravings, no small merit in a book of this kind. The paper, type, &c., are of the best quality, and the whole work will compete, in mechanical execution with the best medical works of the day. We recommend this work to the dental profession throughout the country;it will guide and direct them in all important operations, and make them useful in their profession. Of one thing we are positive, no dentist should punch out our teeth, who had not given Dr. Harris' book a thorough perusal.

"We say then to one and all, call on Mr. Steel, 14 Camp street, and buy Harris' Principles and Practice of Dental Surgery."

Art. V.—*Experimental Researches upon Febrile Caloricity, both before and after death. Post Mortem Fever.* By Bennet Dowler, M. D., *of New-Orleans.*

These researches were originally published in the *Western Journal of Medicine and Surgery;* but they now come before us in the form of a neat pamphlet numbering over 50 pages.

The author of these curious—these original researches, is well known in this city as a close observer of facts and an ardent cultivator of pathology. To demonstrate this, we have only to refer to the present, and his other essays, published in the Medical Journals of the day. The foundation of these post-mortem researches was laid in the dead-house, attached to the New-Orleans Charity Hospital. Here, he watched and noted the phenomena which are manifested when the elements of life are about to be dissolved to form new combinations, new life, different powers and affinities.

Whether the facts developed by Dr. D. be of any practical utility or not, *certes*, he has opened a new field of inquiry which, if properly cultivated, may lead to results of the most novel and gratifying character. Few men ever live long enough to develope to its full extent of practical bearing, a *single* idea of any magnitude; instance, Fulton, Franklin, Bichat and others, yet their successors may widen the field and thus extend the benefits of a new discovery to distant and future generations.

We have a pressing desire, but neither time nor space to analyse and sum up the observations as set forth in this pamphlet by our ingenious friend. But when it is known that Professor Louis of Paris, to whom Dr. D. transmitted a copy of these researches, viewed them with approbation and addressed an encouraging and complimentary letter to the author, we shall have said enough to establish Dr. Dowler's claims as a man of science and research.

In order to give our readers some conception of the very ingenious experiments instituted by Dr. Dowler to ascertain the rise and fall of the temperature of different *cadavera*, we shall make room for the following analysis of some of his tables, showing the "post-mortem fever of regions."

"43 dead bodies; mean time after death, when the observations began 29.5 minutes; maximum delay in three cases, 3 hours; minimum 1 minute; mean duration of the experiments 1 hour and 32 minutes; 4 maxima, being 1 of 4 hours and 8 minutes, and of the air 84.4, partly taken in the dead-house, and the residue from the other wards; mean of the axilla 104.71°; maximum 113°. minimum, 100°; mean of the rectum, 104.05; maximum, 111°. minimum, 100°.; mean of the epigastrium, 105.48; maximum, 111°; minimum, 101°; mean of the chest, 101.95°; maximum, 107°; minimum, 97°; mean of the heart, generally of the right side, 103.5°; mean of the brain, 98.71°; maximum, 102°; minimum, 95°. The liver was observed in nine cases, and gave a mean of 106.33°; a maximum of 112°, and a minimum of 102°. The perineum without inscion gave a mean of 104.45; a maximum of 109°, and a minimum of 101°, in sixteen cases. The pelvis and lower (part of) abdomen in 9 cases averaged 105.05°; maximum, 107, minimum, 109°."

This table is sufficiently curious, but we utterly despair of showing from it, by a comparison of organs at different periods, the surprising phenomena manifested by *post-mortem* fever, in particular regions remote from the centre, at prolonged intervals after death, requiring hours to reach its maximum; then sometimes declining to the temperature of the centre, both keeping pace for a time, and then the centre falling more rapidly, leaving the thigh stationary, at perhaps 106° for many minutes—reversing all the laws of refrigeration known to philosophers. So we leave this subject for the present."

Since inditing the above, we have received the *Philadelphia Medical Examiner* and the *Boston Medical and Surgical Journal*, both of which seem to regard Dr. Dowler's researches as deserving attention; the former journal states that the facts developed in this paper by our author, are deemed so novel and striking, that they will be repeated in Philadelphia, in the course of this summer.

Dr. Dowler has a large amount of matter relating to *post-mortem* phenomena, which may hereafter be brought to light. It will go to corroborate what he has already published and add materially to the interest of the subject.

Art. VI.—*The Perkins Institution and Massachusetts Asylum for the Blind. Annual Report of the Trustees*, 1845. Boston.

We beg the respected author, Dr. S. G. Howe, to accept our sincere thanks for this most interesting pamphlet. Amid the pile of books, journals, pamphlets, and papers, lying upon our desk, it came near escaping our attention, an oversight which we should have deeply regretted; but thanks to Laura Bridgeman, her name caught our eye, as we accidentally opened the work, and from that moment we could not lay down the Report until we had read it through. Who is not familiar with the case of Laura Bridgeman, the extraordinary and interesting *blind and deaf mute*, so minutely and touchingly described by Dickens, in his "Notes on America?" Her afflicting story made the *very soul* of his otherwise contemptible book. In this pamphlet may be found a continuation of Laura's history, from the pen of her preceptor and benefactor, which, together with the Doctor's admirable observations, will be found fully as interesting as the first account. Nineteen pages are occupied with the Trustees' Report of the history and condition of the Institution during the past year. There was no death in that time. The number of inmates, 82. The Report is ably drawn up, and contains many very judicious observations in regard to blindness and its unfortunate victims. To this, Dr. Howe has added an Appendix, in which he gives a truly interesting account of seven blind and deaf mutes; two of which, Laura Bridgman and Oliver Caswell, reside in the Institution; four he found in Great Britain and Europe, where he has recently been sojourning; and one on Blackwell's Island, New York. Dr. Howe's reflections in relation to *common instruction*, show him to be a man of philosophic mind, as well as of uncommon benevolence. We cannot withhold the following beautiful and well-merited compliment which he pays to the SISTERS OF CHARITY.

"I had the satisfaction of visiting Bruges, the excellent establishment conducted by the learned and benevolent Abbe Carton, who is known over Europe for the zeal and talent with which he has labored for the blind, and the deaf and dumb. This school contains both classes, who are taught principally by ministering angels, in the guise of Sisters of Charity. I had before seen them in all parts of the world, tending upon the wounded, ministering to the sick, in hovels as well as hospitals, and giving spiritual comfort to the dying sinner, while they wiped the damps of death from his brow; but in Belgium I first saw them managing excellent prisons for female convicts, and bringing the influence, which woman can best bring, for softening the heart and reforming the life of hardened criminals. In Belgium, too, I first found them engaged in the more difficult and responsible task of drawing out the intellect where its natural channels were blocked up, and in both cases saw them leading their willing charge, by gentle love and sweet importunity, as far on the path of improvement as man, by stronger and sterner efforts, has even forced his. Individual men may have subdued, within themselves, the love of self, and learned to turn meekly even in spirit the unsmitten cheek to the smiter, but as a class or profession they have never done it, and Christianity owes to woman the nearest approach to perfect impersonation of its pure spirit upon earth, as it is shown in the Catholic order of the Sisters of Charity. I cannot refrain from these remarks even if they be misplaced. I feel that I must pay the tribute somewhere, and certainly the most interesting thing connected with schools for the deaf mutes and for the blind, which I saw in Europe, was the part borne in their instruction by these devoted young creatures. And I was much pleased to find a man of the Abbe Carton's talents and experience. confirmed all that I believed of woman's peculiar fitness for this most difficult kind of instruction."

This pamphlet deserves a general circulation.

The British American Journal of Medical and Physical Science—*Edited* by ARCHIBALD HALL, M. D., *Edin. Lecturer on Chemistry, University of McGill College*, and one of the PHYSICIANS, *Montreal General Hospital. Montreal*, April, 1845. Monthly.

We have received the first and second numbers of this new Medical Journal, and find its table of contents richly stored with interesting matter. From the prospectus we learn, that the Journal is not to be limited to Medicine exclusively, but will embrace the physical sciences, natural history, geology, mineralogy, &c. Placed as we are, on the *extremes* of the domain of Science, in North America, on our part, we will with pleasure exchange with our cotemporary all that we can collect of interest in the South for whatever he may be pleased to send us from the North.

It certainly augurs favorably for the advancement of Medical Science, to see so many journals springing up throughout the land. We heartily wish our cotemporary all success.

Proceedings of the Medical Society of the State of Tennessee, at the Sixteenth Annual Meeting, held at Nashville, May, 18 .5.

This little pamphlet gives rather a beggarly account of medical proceedings in a state society of sixteen years existence; the following resolution, however, adopted by the Society, is somewhat encouraging.

Prize Essay.

"Resolved, That a premium of fifty dollars be awarded to the author of the best essay on some medical subject, to be submitted to a committee by the time of the next regular meeting of the Society, who shall award the prize."

The subject chosen, is *Scrofula*. Essays are to be transmitted to Dr. Felix Robertson, Nashville, Tennessee, (post paid) on or before the first Monday in March, 1846. There are physicians in Tennessee capable of writing, if they can only be gotten at it.

PART FOURTH.

HEALTH OF THE CITY AND COUNTRY—TOGETHER WITH AUTHENTICATED REPORTS FROM THE NEW-ORLEANS HOSPITALS AND INFIRMARIES.

NEW-ORLEANS, JULY 1st, 1845.

We greet our patrons on the commencement of the second year of our Journal. Our first has been a year of anxiety and toil—such anxiety as must of necessity attend the undertaking of an experiment which involved our reputation, threw upon us fearful responsibilities, and placed us before the public view ;—such toil as is harrassing, but unavoidable, in a new and untried career. We have worked hard, though our only reward is the *general commendation* of our friends. The hope that our time and labor have not been spent in vain, but that some good may result, inspires us with the determination to pursue our object with undiminished energy. We commence our second volume under more favorable auspices ; we perceive that the Medical profession in the South, is, in a great degree, roused from its lethargy, and we look with confidence for a hearty co-operation in the cultivation of medical science. Notwithstanding the difficulties with which we have had to contend, we have incurred additional expense in the improvement of our Journal, and if our subscribers will second our efforts by prompt remittances of their respective dues, we contemplate still farther improvement. It will be perceived that the Periscope of the present number is printed in smaller type, by which a greater quantity of matter is condensed into the usual space allotted to this part.

We have devoted a larger space in the first part of this number to original essays, than usual, and still were compelled to exclude a very valuable paper from the pen of Dr. Stone, of Woodville, on the Yellow Fever of that place. This we very much regret, for although there would then have been so large an amount of matter in regard to a single disease, yet inasmuch as Drs. Stone and Kilpatrick take opposite grounds in regard to the origin of the late extraordinary epidemic at Woodville, and the occurrence was one, which, if faithfully recorded, ought to throw considerable light on the still mooted question of contagion, it was very desirable that both should appear in the same number. On account of the late arrival of Dr. Stone's paper, it was unavoidably excluded.

Dr. Logan, one of the committee sent by the Medico-Chirurgical Society to examine the Woodville fever, and whose report has already been published in this Journal, has recently read a long paper on the subject before the Society, in which he candidly declares that a more careful examination of all the facts that have been brought to light, has caused him to change the opinions he once entertained in regard to the *infectious nature and transmisibility* of yellow fever. He now believes that the disease is infectious and may be carried from place to place.

We deem it sufficient to publish *all the facts*, which will have been done when Dr. Stone's paper, now received, shall appear, and leave our readers to draw their own conclusions.

Dr. Hort's paper, on quarantine, will be found to abound in valuable facts, strong arguments, and wholesome advice. By order of the *Physico-Medical Society*, it has been struck off in pamphlet form, and distributed amonst the city authorities, and the community at large. We give in this number a great deal in relation to quarantine, but we deem it important, as a diversity of opinion appears to prevail on the subject.

Dr. Montgomery is entitled to our thanks for his valuable paper on the continued fever that prevails in the South. We are aware that very vague ideas prevail in regard to its pathology and treatment. We shall always be pleased to hear from the same able source.

Dr. Binaghi's paper on a rare disease that prevails in Mexico, will be read with interest. Dr. B. is an Italian physician of merit, who resided several years in the city of Mexico, but is now permanently settled in New Orleans.

We have had the good fortune to make the acquaintance of Dr. Monteit, *chief Surgeon* to the principal Hospital at Vera Cruz, and are authorized to expect some valuable communications from him. Dr. M. is a French physician of the most respectable attainments, and like the majority of his order, who have received European educations and are now scattered throughout the world, is ever ready to contribute to the advancement of medical science.

We have established a new department in this part of our Journal, to be entitled Health of the Country, under which we intend to give an account of the health of the South-West beyond the limits of New Orleans. We are about establishing a correspondence with some twenty-five points, at proper distances apart, extending from the Gulf of Mexico as far North as Nashville, Tennessee, which we think will accomplish the object, and render the Journal interesting to many unprofessional as well as the strictly medical reader. We are only able to give in this number the desired information from a few neighboring points.

A correspondent has obligingly sent us a brief historical notice of the *Alabama Medical Society*, but we cannot make room for it in the present number.

We regret to find ourselves again a few days behind our time. This misfortune is exceedingly vexatious, and our best efforts will be continued to prevent it in future. Our friends may rest assured that the work will be continued in spite of all obstacles.

HEALTH OF NEW ORLEANS.

Our city continues in the enjoyment of almost uninterrupted health. There was probably never less disease here at this season, and but few cities are blest with better health than New-Orleans, when not under the influence of epidemics. Since our last date, scarlet fever has prevailed to a greater extent, especially in the lower part of the city. There has been a good deal, recently, in the upper part also, yet we have heard of but very slight mortality. Intermittent and remittent fevers, diarrhœa, and dysentery, prevail to a limited extent. We have recently heard of several violent cases of cholera morbus. We again hear of numerous cases of *coup de soliel*, or sun stroke.

The weather is extremely hot and oppressive at this time. During the month of May, and up to the 15th of June, a large amount of rain fell; since the last date it has been more dry and sultry. Our meteorological table will give a correct general idea of the weather.

The river is in just the opposite condition to what it was this time last year; it is extremely low, though we hear of quite a freshet coming down. Our steamboats travel so rapidly, that we are enabled to hear of an overflow in the upper rivers long before the flood reaches this place.—Whilst twelve months ago the Mississippi at this place was swollen to its very brim, and the gutters of all our cross-streets afforded continued streams of water running from the river to the swamp, thereby carrying off an immense amount of filth; now it is so low as to expose to the sun an extensive *batture*, or bank, covered with *debris* of every kind falling from the shipping, steamboats, and flatboats lying along the Levee; and the gutters are only daily watered by gushes from the hydrants, that supply streams which require the aid of the scavenger's broom to push them to the rear of the city. There they become stagnant and extremely offensive.

The streets are very filthy from the want of sufficient water to cleanse them. We mentioned in a previous number, that a public spirited and influential member of the Council had proposed a plan for watering the city, which we deemed of great importance in a hygienic point of view. The subject is at present before the City Council, and referred to a committee. It is greatly to be hoped it will be carried into execution. There is no telling the beneficial results that might arise from a good system of sanitary regulations well executed in this city. Who knows but that it might be rendered as free from epidemic visitations as any city of its size?—Whereas the only wonder now is, that with so many apparent causes of sickness, so little is witnessed.

Yellow Fever.—We are, as yet, totally exempt from this disease. On the 21st of June, a communication appeared in one of the city newspapers, apparently from a medical correspondent, which announced that the schr. Water Witch had just arrived from Tobasco, having nearly all her crew down with yellow fever, and that the disease was raging violently at that place. We immediately repaired first to the U. S. Marine Hospital (now in Circus street, under the care of Dr. J.J. Ker), where we were informed the sick had been taken, and also to the schooner, where the following facts and particulars were ascertained: The Water Witch, with a crew of eight persons on board, left New Orleans on the 26th April; she arrived at Vera Cruz on the 5th of May, *all well*; staid there nine days; heard of perhaps a case or two of yellow fever, *but saw none*; *crew healthy all the time there;* went thence to Tobasco, where she arrived on the 17th May, *all well;* staid there till the 9th June; heard of no sickness prevailing there; is in the habit of trading to that place, and never knew yellow fever to prevail there. At this place two young men of the crew, on a Sabbath day, took a pistol, a bottle of liquor, and a skiff, and went off on a frolic. The weather was very hot and rainy—they got wet, and drunk, ate a quantity of bad fruit, and returned in bad plight. One of them was seized with intermittent fever the next day, and had a chill for several successive days. He was bled, took several doses of medicine, and was considered convalescent when the schooner left on the 9th. He did not improve, however, but continued to linger on the voyage to New Orleans, and when he arrived here was extremely feeble, but presented none of the appearances of yellow fever. He has since recovered.

His companion was attacked on the 8th, six days after the first, and the day before the vessel sailed. He complained chiefly of an oppression at the stomach, but had no chill until the fourth day of sickness; he then had fever, pain in the stomach, &c.; took a dose of salts, one of calomel, and an emetic; he continued to be sick, but was not thought to be in danger. Immediately on arriving here he was taken to the U. S. Marine Hospital, was found to be in a moribund condition and died soon afterwards, but presented none of the appearances of yellow fever, as I was informed by the surgeon, Dr. Ker. There was *no other sickness on board the vessel.*

Such are the facts that called forth the communication of the sapient correspondent of the Bulletin. It produced a slight panic that frightened away some who would have left the city in a short time any how. The President of the Board of Health promptly enquired into the circumstances, and published a correct statement of the facts. Our city papers should be careful in publishing communications calculated to cause alarm, and always see that their statements be correct. The period is near at hand when we may expect the appearance of yellow fever, and we hope that every physician in the city will pay particular attention to its origin.

Health of the West Indies, Vera Cruz, and Galveston.—After careful enquiry, we have heard of but little sickness at any of these places, during the spring. There has been no epidemic at Kingston, or Havana, or any of the places between which and this, there is much intercourse.—Early in the month of May, the U. S. ship Vandalia was reported to have suffered greatly from sickness, (yellow fever we presume) and to have put into Port-au-Prince, in distress. Since that she has gone to Pensacola, where we have heard of no sickness. By the last arrival from Vera Cruz we learn that the *vomito* is prevailing to a considerable extent at that place. There is no sickness at Galveston.

We here allude to the health of the last mentioned places, because some of our physicians, and other citizens, believe there is always a *striking connection* between the prevalence of sickness in that region and this city. If there be such a connection, we will endeavor to trace it up.

We have now reached the dullest season of the year, in our city. The buisness season is closed—the vast amount of produce brought to this market from the fertile valley of the Mississippi has nearly all been sold and exported, and thousands of onr wandering citizens have taken their flight to other climes ; some in pursuit of health, others to revisit distant relatives and old friends, whilst others again, who have not become acclimated or identified with the city, have gone to spend their leisure and their money far from the influence of the dreaded epidemic. We wish them all a safe return, and hope they will find us here to greet them when they come. Almost every art, business, and profession, *except Medicine*, has its period of leisure and rest, during which its followers may relax their exertions and even enjoy the luxury of a change of scene ; but the practicing physician, like Sisiphus chained to the rock, can never leave his post ; whilst others may roam in pursuit of health or pleasure, he must remain to defend the Household Gods.

HEALTH OF THE COUNTRY.

Under this head, we expect in future, through the aid of correspondents at different places, to give an account of the existing state of health throughout the south west. We did not send out circulars until very recently, and have therefore heard from but few places. We are much gratified at the readiness with which our friends have thus far responded to the call. They will please accept our thanks.

From *Mobile* and *Natchez*, we learn there is very little sickness.

Vicksburg, June 25th, our correspondent writes :

"Since the commencement of this year, Vicksburg has been the victim of two or three epidemics, more general and more fatal than it has been my lot to witness at any similar period during my residence here. The contemporaneous prevalence of these epidemic diseases, forms not the least striking feature in their history. Almost uninterruptedly, from the beginning of January, until the latter part of May, has this been the case. Scarlatina, the first cases of which, were noticed about the 20th of December, of the past year, was during the period mentioned extensively prevalent, and alarmingly fatal ; in some instances, as many as two or three members of one family falling victims to its attack. Very much about the same period, but commencing a few weeks later, we were visited by an epidemic of measles—its termination as *an epidemic* being a few weeks later than that of scarlatina. Among the children of the white population, the mortality from scarlatina, was greater than that from measles, though, I think, that among the negro children, there were a larger number of fatal cases from measles. Form this last, I know of no reason, unless the fact that measles spread more generally among the negroes, than scarlatina, affords an explanation.

"Among the adult negro population, the cases of measles were much more numerous than I have ever before noticed ; the cases frequently running into diarrhœa, bronchitis, or pneumonia, and terminating fatally after an illness, sometimes of several weeks. Measles appeared to be considerably modified by the co-existence of scarlatina, angina maligna being an almost universal attendant upon cases of the former. With regard to the catarrhal symptoms, they did not appear to present a greater degree of violence, than we ordinarily see in this disease. Pertussis during the same period, has also been exceedingly prevalent, indeed its existence as an epidemic, may be confidently affirmed. Among the adults of the city proper, during the same time, there has been but little sickness, should we except such as were unprotected by a previous attack of the above diseases. Some few cases of malignant erysipelas were noticed by me in my own practice, but they were of a mild character, and yielded readily to treatment. The cases, if any other than those mentioned, must also have been mild, as I do not remember to have heard of any deaths in the city fiom that cause. In the county, however, a number of cases occurred on the Yazoo river, some of them of an extremely severe and fatal character. In this instance, the disease appeared to be entirely local, being confined to two or three contiguous plantations; it was evidently highly contagious. One of its most remarkable features was the variety of appearance which the difficult cases presented. It prevailed during the last half of the month of March, and all April. Some few cases of diarrhœa and dysentery, but not generally of a severe type, were noticed in May. Measles and scarlatina were, however, still prevalent among children, but seem to have lost their epidemic form towards the last of this month, (May); some few cases, however, have been seen even up to the present time; some of them fatal. With this last exception, the present month (June) has been more than usually healthy, the few cases that demand attention being chiefly bilious and intermittent fevers, but under every aspect of the case, our city can confidently be asserted healthy at this time."

Woodville, Mississippi, June 24th. Our correspondent at this place writes:

"It is very healthy here, just now. There *have been* some cases of enteritis, which were troublesome; some catarrhs &c. The earth in this immediate vicinity is parched; no rain for six weeks."

Montgomery, Alabama, June 24.—Our correspondent writes:

"At present we have a few cases of remittent fever; principally quotidians and double tertians of a mild character, though a few of them resist treatment with more obstinacy than cases have done in other seasons, presenting, apparently, the same assemblage of symptoms. There is a tendency to gastro-enteric complication. Of intermittents, I have seen no cases for several weeks. Such an exemption is, I think, rather rare with us at almost any season. We have a few cases of diarrhœa, presenting in some instances, if neglected, dysenteric symptoms. They are not so numerous as they were a few weeks past. A rare case of cholera morbus now and then presents itself. Occasional cases of pneumona now and then supervene, proving obstinate, if not fatal, in cases of children laboring at the same time under whooping cough. This latter prevails now to a limited extent only, perhaps for want of susceptible subjects, as it has been with us some twelve or eighteen months."

Selma, Alabama, June 17th.—"For two months our community has been healthy; a very few cases only of intermittent and remittent fever having as yet occurred. But we had a disease amongst us during the winter and most of the spring, which I think deserves some notice. It resembled very much those epidemic influenzas which we see described as having frequently occurred in Europe since the year 1311. There has been something of the kind here occasionally, for the last two years, but it commenced in good earnest about the 1st of November last, and disappeared in the latter part of April. It prevailed as an epidemic and was confined to no particular class; every age, sex, and condition being alike obnoxious to it. So general was it too, that no one escaped it altogether." Our correspondent then goes into a very interesting detail of the symptoms and treatment of this affection, for which we have not room at present. We may insert it amongst the original communications of our next number.

Rodney, Mississippi, June 25th. Our correspondent says:

"Our professional brethren in this vicinity have had but little to do as yet, as the season thus far, has been unusually healthy."

Jeanerette's Post Office, Parish St. Mary, June 16th, 1845.—Our correspondent writes:

"In several parts of this parish the whooping-cough and measles are prevailing extensively, and of a severe character. In the vicinity of Franklin the measles have spread extensively, and have been attended with great mortality. I have heard of two plantations, on each of which from ten to fifteen deaths have occurred from this disease. The measles are also prevailing in New Iberia, in the Parish of St. Martin, but of a mild type. On one plantation in my neighborhood there are a number of cases of diarrhœa accompanied [followed ?] by dropsy; the treatment of which, in negro subjects, I always find difficult. It is somewhat strange that I have prescribed for more such cases on this plantation than on all others to which I am called. I can only account for it from the fact that for several years the proprietor has been clearing and improving land, thus necessarily increasing the exposure and hardships of the negroes. I have recently seen a fatal case of fever, which, from a mild remittent was converted into a severe Typhus, the nearest approach to our northern winter Typhus that I have met with in the South; duration 22 days.

Jackson, Mississippi, June, 24th 1845.—"In December last there were a few cases of measles in this city, but all mild and easily managed. During the winter and spring we had, prevailing to a limited extent, what is here called Pneumonia or Pleurisy. This is a common form of disease in this part of Mississippi, but I do not think the name will give a correct notion of the disease." Our correspondent thinks that genuine pleuritis is very rare in that region, but that pneumonia attend with a low form of remittent fever is common, and that "this is the most formidable prevailing disease to be met with in Mississippi, during the winter and spring season. In this county, and Madison, this disease has prevailed, from a mild kind of catarrh up to a high degree of pulmonary congestion, frequently terminating fatally in a few days; sometimes in a week or ten days. Adults and laborers appear to be more particularly its subjects. Diarrhœa has prevailed, though not extensively, during the spring. Intermittents are the *prevailing disease* here since the 1st April, chiefly of the tertian form. Remittents have occasionally appeared, but of these there have been comparatively few, though these have not yielded very readily to treatment. Up to the present date, I have not seen or heard of any malignant intermittent, (or "congestive case," as we call them here), nor of the congestive remittent. So far, our fevers have been easily managed or cured, and the cases, but "few and far between."

In our future numbers we expect to give a fuller account of the *health of the country*.

SURGERY IN NEW ORLEANS.

For want of a medical journal in this city scarcely any of the important surgical operations, that have been performed in the South West, have ever been made known to the world. Almost every capital operation has, within a few years, been performed by the surgeon of Louisiana, and we now propose to notice briefly such as we have either witnessed, or have been related to us from authentic sources.

1. *Amputation at the Hip—Successful.*—Some years since, Dr. Walter Brashear, of ——, now retired from practice, performed this bold and terrific operation with a skill and success that would have done honor to the immortal Larrey. The case, we believe, has never been published, but we now take the liberty of placing the fact upon record, and beg leave to relate an anecdote in connection. During the winter of 1843–4, when the Hon. Henry Clay was on a visit to this city, we had the pleasure, together with some twenty-five or thirty physicians, of spending the evening with him at the house of a medical friend. Whilst at table, one of the company proposed "*the health of the venerable* Dr. Brashear, *the first and only surgeon in Louisiana, who had successfully performed amputation at the hip-joint.*" Mr. Clay, who was sitting by the side of Dr. B., with characteristic good humor immediately observed—"*he has you on the hip, Doctor,*" to the great amusement of Dr. B. and the rest of the company.

2. *Amputation at the Shoulder Joint.*—This operation was performed at the Charity Hospital, about two years since, by Dr. Stone, for a desperate gun-shot wound. The operation was well performed, and offered the only hope of life, but the case was too desperate, and the patient soon sunk. It was also performed at the Charity Hospital, last year, by Dr. A. Mercier, for gangrene of the arm, after a maltreated compound fracture; but the case fell into the hands of Dr. M. too late, and although the operation was well performed, the patient sunk.

3. *Excision of the Parotid Gland.*—This operation has been twice performed successfully, by Dr. C. A. Luzenberg,—the first, about eight years ago; the patient is still living in this vicinity; the second was performed, in April last, upon a negro woman, about 35 years of age, for schirrus. The gland was enlarged to nearly the size of the fist, very much indurated, and unmovable. This operation was performed in the presence of some dozen physicians, ourselves amongst them. As a preliminary step, dictated by prudence, a ligature was cast around the common carotid, *to be tightened if necessary.* Dr. L. then made an incision through the integuments around the whole base of the tumor. The dissection was then rapidly progressed with, but the hemorrhage soon became so profuse that the ligature on the carotid had to be tightened. This being done, the diseased gland was quickly, and in the opinion of all present, *completely removed.* A large and deep chasm was left in its place, which was filled with lint, covered with a compress, and the patient put to rest. The operation was well and rapidly performed, and the time occupied was about 30 minutes. The patient bore the painful operation with extraordinary firmness; having uttered but one or two groans during the time. No unfavorable symptom has followed, and the wound at this time is almost entirely healed.

4. *Amputation of the Mamma, and Ligature of the Femoral Artery.*—These operations have both been successfully performed, recently, by Dr. W. Stone ; the ligature, for aneurism of the femoral artery ; and the amputation for schirrus of the mamma. In the latter operation, Dr. Stone left sufficient integument to cover the wound ; it was brought together by adhesive straps, and quickly united by the first intention. We saw Dr. Stone perform the same operation for the same complaint, last year. The subject was likewise a negro woman. She was frightened nearly to death, and could not be prevailed on to submit with any composure. While she was writhing and screaming with all her power, Dr. S., with characteristic firmness, proceeded with his incisions, and removed the entire mamma, as also an indurated axillary gland. The wound was then covered with the flap, and quickly healed. It is now twelve months since the operation, and we learn the woman has no return of the disease. We have seen both of these operations performed by Dr. Luzenberg, within the last two years. Indeed, these two gentlemen have performed almost every capital operation in Surgery since they have been practising in New Orleans, and with a skill and success that would do credit to any city.

5. *Tying the Subclavian Artery.*—We have seen this difficult operation twice performed by Dr. A. Mercier ; first, for false aneurism of the axillary artery, last year—unsuccessful ; and very recently for aneurism of the subclavian itself; both on the left side. In the first instance, he operated below the clavicle, and the patient sunk from secondary hæmorrhage. On the 21st of June last, he performed the last operation in the presence of a large number of physicians and other spectators. The subject was a negro woman, aged about 30 years. She belonged to a baker, and had long been in the habit of carrying a basket of bread upon her head which she held chiefly with her left hand. The aneurism made its appearance about eight months since, and continued to grow until it attained the size of a man's fist; situated just below the clavicle, and extending from the border of the axilla half way to the sternum. Dr. Mercier operated above the clavicle, and after the tedious labor of more than an hour, which he performed with great firmness and skill, he succeeded in fixing his ligature, and completely arresting the circulation. The case has since progressed most favorably, and there is now every prospect of a successful issue.

6. *Ligature of the External Iliac Artery.*—We are informed that this operation was successfully performed, a few years since, by Dr. H. Daret, upon a well known and worthy gentleman of this place, for aneurism situated very high on the femoral artery. The gentleman may be seen any day on our streets, and is indebted to skilful surgery for his life.

7. *Ligature of the Carotid Artery.*—This operation has been often performed in this city, but we cannot forbear mentioning the following interesting case. A few years since a gentleman of Natchez, Mississippi, had the misfortune to receive a pistol ball about the centre of the left cheek. It penetrated deeply, and the hæmorrhage was so profuse as to place the life of the gentleman in the most imminent danger. Several days afterwards he was brought to this city in this perilous situation. He arrived here in the night, and Dr. Stone was called to see him. He at once saw that the only hope depended on tying the carotid ; he immediately performed the

operation by candle light, and with complete success. The gentleman is now living in this city.

8. *Excision of the Maxilla Inferiora.*—This operation has been successfully performed three times in this city, by Dr. J. F. Buegnot, for *Osteo-Sarcoma*—the first, in 1839, by dividing the bone with a chain saw, at the symphisis, and above the angle. The second, in the same manner. The third operation was performed in 1840. The disease in this case had progressed so far, that it was found necessary to disarticulate the bone at the *temporo-maxillary joint*; and thus the entire half of the bone was removed. The patients all recovered entirely.

We have thus enumerated some of the most important surgical operations that have been performed in this city and vicinity; there doubtless are others equally important, that have not come to our knowledge. The minor operations would be too numerous to mention.

HOSPITAL REPORTS.

Charity Hospital.

The monthly reports for May and June, show the admissions into this Hospital to be about the same as usual; the discharges were more numerous. and the deatl s fewer than perhaps ever were known at the same period, and in proportion. The wards, both medical and surgical, have abounded in the most interesting cases, and we are pleased to see that the strictest attention is paid by the attending physicians and surgeons. The most remarkable incident to be noticed, was the admission of about 20 unfortunate victims, men, women and children, that were horribly scalded and otherwise mutilated by the explosion of the steamer Marquette, as she was leaving our wharf. This occurred on the 1st July at 4 p.m. The boat was not large, and did not have many passengers. Several persons were killed at the instant, some torn to pieces and others drowned. It was most painful to witness the groans and agonies of those who still survived, but doomed too soon to perish. The most of the sufferers were taken to the Charity Hospital as soon as it was practicable, where every thing was done for their relief that could be; but on the following morning the dead house presented the melancholy spectacle of six persons who had been suddenly snatched from existence whilst in the full enjoyment of youth and health. Of 20 admitted, 10 have since died, This is one of those dreadful calamities too common on the Mississippi river, that sometime rushes a crowd of desperate patients into the Charity Hospital. But few accidents are more fatal than extensive burns.

We have on hand a number of valuable cases, which we have never been able to publish for want of room, and are now limited to the following brief observations.

MEDICAL WARDS.

Service of Dr. T. M. Logan.

By an arrangement with Dr. Mercier, this gentleman has taken in charge the Female Surgical Department. The majority of the cases, which are received into this service, are, for the most part, those of the primary and secondary forms of syphilis, gonorrhœa, urterine diseases and chronic ulcers. Sevarel interesting cases, however, have passed under

Dr. Logan's treatment, and we extract a few from his note-book, as worthy of particular notice.

Hydrarthrosis Genu. Acupuncture. Cure.—Mary Dunn, ætat 36, while walking in the streets, slipped and fell upon her hands and knees. Some time afterwards she experienced pain in her left knee, and entered ward No. 7, of the hospital, on the 25th April, 1845. On the 26th, she was transferred to ward No. 5, appropriated to the infirm and children in the service of Dr. Logan. Upon examination, the anterior part of the knee joint immediately over and around the patella, presented a tumefied appearance, and a distinct fluctuation was perceptible upon pressure—the fluid escaping from under the fingers from one side to the other, under the posterior surface of the ligamentum patellæ. From the previous history of the case, the feeling of the parts, and the indolent character of the swelling, almost entirely free from pain, except in the kneeling posture, an accumulation of synovial fluid was suspected. The patient was put under the hydriodate of potash treatment (℈ ii daily), and a mercurial plaster was ordered to the part. On the 29th, the tumefaction was found to be rather increased, and the patient complained of costiveness. A dose of castor oil, preceded by 4 grs. calomel, was ordered, with a continuance of the hydriodate of potash. Acupuncturation was now practised in several points in the most dependent part of the swelling, and on the needle being pushed in until resistance was overcome, and then withdrawn, a quantity of a straw colored alkaline, albuminous fluid, possessing all the characters of synovia, escaped. The patient was directed to preserve the recumbent posture, and Emplast. Vigo was ordered to the parts. A repetition of the acupuncturation was practised daily until the 5th of May, when it was found that from the repeated application of the needles, hyperæmia, and one of its consequences, hypertrophy of the parts, (the skin and subjacent tissues being much thickened, hard, and somewhat cartilaginous,) had occurred. The introduction of the needles was therefore discontinued, and compression by bandages resorted to. The iodine treatment was all-along continued, and the bowels opened whenever requisite, by calomel and castor oil. On the 8th of May, with a view of producing adhesive inflammation between the synovial surfaces of the relaxed sac, a sinapism was ordered for the knee to be tightly bound down by a roller bandage. The student, in charge of the patient, having gone out, the nurse neglected to remove the mustard, and consequently, when the bandage was taken off in the evening, the parts were of a purple color, hot, and partially blistered. Cold applications were directed to relieve the pain. From this date to the 15th, the case continued to improve, and the sac was nearly obliterated, when the patient, in the fulness of her gratitude, went upon her knees to return thanks for her recovery, and so broke up the adhesions, and otherwise injured her knee, that in a few days it was as much tumefied as ever. A blister of cantharides, 5 by 6 was now applied and bandaged. It drew well, and a quantity of serum was evacuated. The recumbent posture, and the hydriodate of potash was persevered in until the 27th of May, when the patient was discharged cured.

Hemiplegiæ—Strychnine—Cure.—On taking charge of ward No. 6, on the 9th of April, 1845, Mary, a negro slave, aged 18, presented herself for treatment. She was suffering under complete hemiplegia of the left side, emaciated, pulse feeble, and skin pale and cold. She had been in the hospital under a variety of treatment, without any amelioration of any of her sufferings since December 28th, 1844. From her own account, she had suffered from fits, epileptic; about twelve months ago, which she attributed to taking a quantity of cinchona, and the use of cold applications to the head, during treatment for an attack of fever. Since which, she has had no return of the catamenia. Counter-irritation along the spine, by setons, blisters and tartarized ointment were persevered in, together with stimulating frictions to the extremities, without any material improvement, except in her general health. The appetite continued good, and the bowels were carefully attended to. The most unpleasant symptom in the case was a *stillicidium urinæ*, the bladder being perfectly paralyzed. Strychnine was at last resorted to according to the formula of Magendie, viz:

℞ Strychnin. pur. gr. ii.
Conserv. Rosar. ʒ ss.

Divide into XXIV pills, one night and morning. After the use of these pills for about one week, she was found to be in the following condition: pulse fuller and quicker, pain in the head, tetanic movements alternately in the paralyzed limbs, with pricking of the surface. The strychnine, having evidently made an impression, was discontinued; the hair was cut off, and cold applications applied to the head, and hot mustard baths to the feet.

From this period the patient began to improve; by the 20th of May, she had a good use of her limbs and was able to walk about. The incontinence of urine, however, still persisted with the amenorrhœa. Embrocations, to the hypogastrium and lumbar region, consisting of ℞ nucis vomicæ ℥ i.; liq. ammon. fort. ʒ ii, were tried for one week, but without any benefit. On the 28th of May, the following mixture was ordered with the double view of arousing the dormant uterine functions, as well as to correct the atony of the urinary organs: ℞. Tr. hellebori nigri ℥ ss. Tr. myrrhæ ℥ i. Tr. cantharid. ʒ ii., thirty drops three times a day in sugar and water. Under this treatment a gradual improvement of her unpleasant symptoms is taking place, and there is every reason to anticipate a perfect restoration of the uterine function.

Oblique Fracture of the Femur—Death 6 weeks after. This case serves as an illustration of the difficulty, or rather impossibility, of curing a fracture in extreme old age. The subject was an old woman, upwards of eighty years of age, and a slight fall while in the act of walking in a garden, was sufficient to produce an oblique fracture of her femur in its superior part about one inch below the great trochanter. The double inclined plane splints were used to reduce the parts, and every measure taken to assist the feeble energies of nature. But in spite of every care bed-sores formed on the nates and other dependant parts, and the patient gradually sunk into a typhoid state, about 6 weeks after the accident. In the post mortem examination no callus whatever was discovered and a quantity of dark bruised blood was found extravasated within the muscular tissue. The parts to all appearances were *statu quo*, as at the time of the accident, and the recuperative powers of nature had completely failed.

Hysteroptosis. This was one of those cases which are beyond the resources of the Medico-Chirurgical art, and is mentioned only as an instance of the length of time during which the disease may be endured without compromising the life of the sufferer.

The patient was an old Quarteroon, upwards of three score and ten, and stated that she had been in the same condition, in which she then was, for upwards of 20 years. The procidentia was complete. The whole tumour was about the size of a man's fist including the wrist, its tissues were thickened, and its mucous surface much roughened and indurated. The attempt at reduction gave so much pain, and offered so many difficulties that there was greater reason to dread the induction of an inflammation more dangerous than the prolapsus—to which, indeed, she had become perfectly habituated. Even if reduction could have been effected, in all probability it never would be permanent, inasmuch as the most important element in the pathology of the case was the relaxed condition of the muscles and tissues composing the pelvic floor. An enormous prolapsion of the rectum corroborated this opinion; and altogether the case presented so strongly one of those monstrosities of disease which defy the resources of medicine, that the patient, at her request, was discharged, unrelieved.

SERVICE OF DR. E. D. FENNER—WARDS 10 AND 12.

The following cases are deemed worthy of notice, though we can make room for but little more than a bare enumeration of them. Some of them will be reported at length, as soon as we can admit them.

Case 1.—*Softening of the Brain.*—The subject was an Italian laborer, aged 46. About eight years since, while in Europe, he received a severe injury from the falling of a heavy stone across his back. He had hæmorrhage from the mouth, nose, and anus, but was skilfully treated, and was able to be up in about two weeks. From that time he has been subject to an habitual hæmorrhage from the bowels, which continued up to December last. This discharge then ceased, and he soon became affected with vertigo and headache. On the 2d of February last, he was seized with fever, with violent convulsions. A physician of this city had him bled, cupped, purged, blistered, &c., but without relief; and, finding that the convulsions continued from day to day, and the man being very poor, he advised him to be sent to this Hospital. It was not then done, and he continued to suffer in this way until the 1st of April, when, a blister having been applied over the top of the head, he suddenly became *perfectly blind.* He then went on in the same way, having spasms every day, and violent pain in the back of the head, and in his limbs, until about the 1st of May, when he became *completely deaf.* From this time he was a raving maniac, suffering as before, and tormented with the most horrible dreams and illusions. His wife gave him a solution of morphia every night, and this was the only medicine he took for about sixty days—he had only two small evacuations from the bowels during this long period. He entered the hospital on the 8th of May.

Existing state—skin cool and moist; pulse 90, and somewhat corded; tongue slightly furred and dry; is hungry and thirsty; costive; blind; deaf; delirious; pains in the head and limbs; occasional spasms. His disease was diagnosed both by Dr. Lewis, who first attended him, and by Dr. Fenner, to be *Rammollissement of the Brain.* The case was looked upon as hopeless, but an active course of treatment was determined on

He was cupped along the upper spine, blistered, tartarized, purged, &c ; and the improvement for a time was surprising. His spasms ceased; his pains were alleviated ; he had a good appetite, and really looked like recovering. The iodid. potass. and quinine were tried on him, but without good effect. On the 9th of June, he began to get worse, and three days afterwards died in convulsions. It is worthy of remark, that in this case the spasms were generally worse on the *right side* ; when under them, his head was inclined that way. There was never permanent contraction or rigidity of any part, nor any paralysis.

Autopsy, thirteen hours after death.—This was confined to the brain and upper spinal marrow ; the suspected seats of lesion. (We were requested to disfigure the body as little as possible.) The *dura mater* and superficial vessels of the brain were very much injected. *The entire posterior lobe of the right hemisphere of the cerebrum was completely disorganized.* The medullary substance was here changed into a soft gelatinform appearance, some portion of which was firmer than the rest; the most of it as soft as curdled milk, or clabber; a portion somewhat resembled pus, another was more like gelatine. In the anterior superior part of this lobe, was an oblong spot, about an inch in length, that looked different from the rest, and somewhat resembled an *old cicatrix*. The posterior and inferior portion was softest—being semi-fluid. The *pons Varolii* and *medulla oblongata* were perfectly sound. The ventricles were filled, but not distended, with limpid serum. Nothing abnormal was observed in the *corpora striata*, the *thalami nerv. optic.* or *tubercula quadrigemina.*

Spinal Marrow.—On raising the spinous processes from the third cervical down to the fourth dorsal vertebra, the theca of the spinal marrow was found very much injected. On laying this open, the medullary substance appeared *very white* and *very soft;* much more so than the medulla oblongata—indeed the upper and lower extremities exposed, were much firmer than the intermediate space.

Here is a well marked case of *rammollissement* of the brain and spinal marrow, though the symptoms were somewhat different from any we have seen recorded.

2.—*Chronic Diarrhœa with Diabetes.*—The subject of this case was a man aged 44, who had been living in the Yazoo swamp for the last four years, in the rudest manner imaginable ; sleeping on the ground, and eating what he could get. He had chronic diarrhœa and intermittent fever nearly all the time. About the middle of March last, the diabetes commenced, but he paid no attention to it. He entered the Charity Hospital on the 3d of May, with both diseases still upon him, though he told us nothing of the diabetes until he had been under treatment about fifteen days. He was then discharging about six quarts of urine daily. This now became the predominant disease, and he died on the 23d of May. The autopsy displayed one kidney *completely atrophied*, leaving nothing but an *empty sac*. The other was hypertrophied and soft. The case is an exceedingly interesting one, and will be reported at a future time, in connection with two others that we have, one of which recovered.

3. *General Paralysis.*—P. A., an Irish blacksmith, aged 32, for ten years past a resident of Vicksburg; was in the habit of drinking pretty hard for five years previous to 1844 ; then signed the *temperance pledge*,

and drank no spirits for twelve months. Has generally been healthy and done a great deal of work, although not athletic. About the 15th February last he was taken with a vomiting and purging that ran on for two weeks and reduced him very much, before he suspected the true cause of it. He then discovered that *his wife was giving him arsenic, every day, in his food and drink.* He was then taken to the Vicksburg Hospital, where he was treated two days, but without benefit. He then observed that his extremities began to lose the power of feeling and motion. He left the Hospital and went to a boarding house, where he was attended by several physicians, fifteen or twenty days. By this time *all his extremities were paralyzed*, and he set off for New Orleans. He entered the Charity Hospital, March 27th. When Dr. F. commenced service, on the 7th of April, his condition seemed so desperate that it appeared useless to attempt to cure him. On raising up his musquito bar, he beheld a miserable object, reduced to a skeleton, all his extremities paralyzed, and an incessant expectoration of frothy mucus. Dr. F. passed him by, telling the nurse to let him have some porter, and any thing else he wanted. On the following day to his surprise he found him no worse, and *prescribed the sulphate of quinine, a purgative enema, a liniment to the extremities, porter and full diet.* April 9th. Found him much better The treatment was continued and he improved from day to day. After a week or ten days he had electro-magnetism applied several times. It caused a good deal of pain, but no improvement. He then gave him the 12th of a grain of strychnine three times a day. He took two grains in this way, continued the frictions, porter and nutricious diet, and improved astonishingly. After a while the strychnine produced such a disagreeable jerking and twitching of his arms and legs, especially during the night, that it was discontinued, and the quinine ordered again. This has been his course of treatment ever since —the strychnine and quinine alternately, frictions, nutritious diet, &c. He now feels very well, has a good appetite, has gained flesh and strength, sleeps well and is cheerful, puts on his pantaloons, but is unable to walk; the sensation and motion of his arms and legs are good, but he cannot move his toes and fingers well; his extremities are still quite emaciated. The *heat* is broken out all over him, but he is otherwise comfortable. As two weeks feeding on arsenic did not kill this man long since, it is hoped he may recover.

There have been *two other cases of paralysis*, in these wards—one an old toper from the Balize, supported in the pay ward by his friends, but not under treatment; the other. *paraplegia* from injury of the spine. This man came down from Arkansas under the hope of being benefited by electro-magnetism. Dr. F. had it applied two or three days, but without benefit, and he soon left the Hospital, a hopeless case.

4. *Three Cases of Dropsy.*—One of them cured, and two now under treatment. These last were subjects of chronic diarrhœa previous to the dropsical swelling and are found difficult to manage.

5. *Eighteen Cases of Intermittent Fever treated with Salicine.*—The object of these observations is to ascertain the virtues of salicine, and to what extent it may be relied on as a substitute for quinine. In the vicissitudes of commerce and of governments it might happen that we should be cut off from the supply of this valuable medicine, which is entirely of

foreign growth. It is, therefore, very desirable, if possible, to discover a substitute for it at home. By a communication which recently appeared in the Washington "*Union*," we are informed that the British government are now endeavoring to acquire a monopoly of Peruvian Bark. If they succeed, the price of quinine will probably be greatly increased. In view of this, we learn that the United States Army Medical Service has determined to make an extensive trial of salicine, the active principle of the willow bark. We have thought that the fine opportunities presented by this large Hospital should not be neglected in this investigation. Dr. F. has now tried the salicine in eighteen cases, but deems the number quite too small to justify a report. So far it appears greatly inferior to quinine. Its virtues are somewhat enhanced by combination with piperine. As the article has been very little used within the last few years, the quality may not be first rate. It is now dearer than quinine, on account of the larger doses required, but if it be found to answer as well *in any dose*, it can be made cheap, as the supply of willow bark in our country is inexhaustible. A report will be made at a future time.

6. *Hepatic Abscess.*—This is an exceedingly interesting case, now under treatment. It will be reported at a future time.

The following interesting autopsy has been kindly furnished us by Dr. O'Regan, a very intelligent and frequent visitor of the Charity Hospital:

Granular Disease of one Kidney; Atrophy of the other. A laborer was admitted into the New Orleans Charity Hospital on the 11th June, 1835; a Spaniard, but a resident of the city for the last five years, 25 years old; unmarried. His lower extremities were œdematous and the cutaneous surface pale, lips livid, countenance bloated; he had several times obtained admission into the hospital within the previous five years, for his constant complaint, œdema of the lower extremities. The minute history of his case, I cannot be expected to relate, never having seen the patient but once during life. This man died quite unexpectedly. One of the sisters of the hospital came to ask him if he would have breakfast; on her return with the meal, he was dead.

Post mortem.—The muscles were dark colored, but by no means atrophied; the veins were distended with a watery and highly carbonised blood. On cutting into the cava, where it joins the right side of the heart, all the blood contained in the body, flowed in a continuous stream as in those who die by the electric fluid; the pericardium contained about half a pint of serum; the heart itself was enlarged, the right side had its ventricle and auricle dilated, its parietes thinned, its columnæ carneæ trophied, each chorda tendinea as attenuated as a hair and the valves opake, but no deposit on their free margin. The left ventricle had its walls one inch in thickness, in other respects it was similar to the right. The lungs were completely collapsed. The pleura pulmonalis and costalis were perfectly free from disease. The liver was congested; the gall-bladder somewhat distended; the venous congestion of the intestinal canal, particularly in the ileum about six inches from its junction with the cœcum, was intense; this accounted satisfactorily for the intestinal hœmorrhage which existed for some time. There was also epistaxis at various intervals. The spleen was not altered in shape, consistence or size. The mesenteric glands were enlarged and gorged with blood. The left kidney contained a serous cyst; on taking off the fibrous envelope, the surface was granulated; on cutting into it, its structure was dense, pale, and cut like fibro-cartilage. The right kidney was no where to be found, nothing remained but its envelope, which was very thick and opake. The bladder was contracted and in other respects natural. *Remarks.*—When granular degeneration is established in the kidneys, it suffers the albumen to pass, which performs a highly important part in the animal economy.

We find during the prevalence of renal dropsy

In the Blood:	*In the Urine:*
Defect of albumen,	Excess of albumen,
Excess of urea,	Defect of urea,
Excess of water,	Defect of water.

But in Diabetes Mellitus,

In the Blood:	*In the Urine:*
Excess of sugar,	Excess of sugar,
Excess of water,	Excess of water,
Urea unknown,	Excess of urea.

I think these facts point distinctly to the conclusion, that in the case of renal dropsy, the kidney deranges the constitution of the blood by imperfect performance of its function. While, in diabetes, the secretion is deranged by the organ being compelled to remove from the blood, elements exceeding in quantity and number those which fall under its charge in health.

SURGICAL WARDS.

SERVICE OF DR. A. MERCIER.

Before mentioning that which the service of Dr. Mercier presents interesting, during the months of May and June, we shall enumerate some interesting observations, which we have already alluded to in our last number. The young man who had, at the same time, a fracture of the *femur*, of the *fibula*, and of the *radius*, was subjected, as we have already mentioned, to the treatment by the *immovable apparatus*. Scarcely had the bandages become perfectly dry, when the patient began to walk by the aid of crutches; in a short time from this he left the Hospital, and took passage on a steamer bound for Tennessee.

The man who had a fracture of the *humerus*, and who had also been treated by the *immovable apparatus*, has also left the Hospital, the bone having become consolidated.

We have already said that the treatment by the immovable apparatus was a favorite plan of Dr. Mercier; we have likewise said that all hobbies, however, are apt to succeed better in the hands of their particular riders than with the generality of practitioners; however, in viewing the two successful cases above mentioned, it will be perceived that the treatment by the immovable apparatus may produce results which cannot be brought about by other measures, and, moreover, by this mode of treatment patients are enabled to walk, and are not compelled to remain in bed for fifty or sixty days during the summer. Hence it is to be regretted that surgeons in this country are not generally in favor of this mode of treatment.

The patient who had been treated by the hydriodate of potassa for a *hydrarthrosis of the right knee*, left the hospital eight days since, without any swelling. The fluid effused in the joint has completely disappeared; the relaxed ligaments have recovered their natural length and strength. The swollen extremities of the femur and of the tibia, have shrunk to their normal state; in a word, the cure is complete.

Among the numerous admissions in the wards of Dr. Mercier, during the months of May and June, as usual, he has received a considerable number of indolent ulcers on the lower extremities. The treatment with the hydriodate of potassa, and dressing with chloride of sodium, and the aromatic wine, as mentioned in our last number, has appeared the mode which produced the most satisfactory results.

Among the operations performed by Dr. Mercier, the following may be mentioned:

CASE 1. *Double Hydrocele. Treaed by Injections of Tincture of Iodine.*—W. W., aged 27 years, in the enjoyment of good health, had been affected with double hydrocele for six months. After having submitted him to a proper regimen, Dr. M. punctured with an ordinary trochar, and injected into each tunica vaginalis from three to four ounces of a mixture composed of equal parts of simple water and the alcoholic tincture of iodine. A small portion of the mixture was suffered to remain in the sac. Two days afterwards inflammation was fully developed; it was combatted by emollient cataplasms, and strong lead water. On the 12th day after the operation, the patient left the Hospital perfectly cured. We shall seize the first opportunity to develope the superior advantages of this treatment over all others.

2. *Disarticulation of the Second Phalanx of the Left Index Finger.*—This little operation was performed on a young man, (in consequence of a wound of that joint of the finger,) with success.

3. *Luxation of the Shoulder. Reduction. Cure.*—X., aged 30 years, having fallen on the left side, dislocated the shoulder. The head of the humerus was lodged in the axilla on the border of the small pectoral muscle, at the junction of the outer third of the clavicle, with its middle third. The patient was fixed to a wall by means of a folded sheet; another bandage placed round the affected limb, was entrusted to four assistants, who made gradual extension. Dr. Mercier, with one hand in the axilla, grasping, with the other, the elbow of the patient, suddenly approximated the limb, and brought the articulating surface into their natural position. The patient was dressed as for a fractured clavicle. In a short time he left the Hospital.

4. *Aneurism of the Axillary Artery.—Ligature of the Subclavian on a level with the external border of the Scaleni. Cure.*—We shall add nothing to what we have already said of this operation in our remarks, entitled, "Surgery in New Orleans." Suffice it to say that this is one of the most difficult, and important operations of modern surgery. The success obtained by Dr. Mercier in this operation, will doubtless induce him to make known its details to the medical world.

SERVICE OF DR. A. HESTER.

Dr. H. has drawn reports of a number of cases, but we can only make room for the following:

Aneurism at the arch of the Aorta—Rupture—Death.—An Irish laborer, aged about 35, of light sandy hair, rather tall and lean, was picked up on the banquet, in the evening of the — May, 1845, and brought to the Hospital about 7 o'clock, p. m. He was pale and almost pulseless; extremities cool; difficult and hurried respiration; in a word, in a moribund condition. Stimulants externally and internally were applied, but without effect. He continued to sink, and in a few hours died that night. About two weeks anterior to his death, this man had been treated in the medical wards of the hospitel for a slight attack of *bronchitis.* By a regulated diet, and *tartarized* expectorants, he was relieved of the thoracic symptoms, and in a few days was discharged. Nothing more was known of his case, until he was brought to the hospital as above stated. It was not known of what affection this man had died. The morning following his death, we inspected the body. The muscles were not rigid; the entire surface of the body extremely pale. As the lungs seemed to be the seat of the cause of death, his chest was first examined. In the right side of the cavity, we found more than half a gallon of fluid blood; he had completely occupied all the space between the lungs and thoracic parietes. The lungs were compressed towards the vertebral column; the functions of the right lung had been abolished by the presence of the blood; on cutting into the lung, its air cells were found filled with blood mixed with froth; all the air cells were impervious; it presented the appearance of a lung in the first stage of intense inflammatory engorgement.

Turning our attention towards the heart and great vessels, we found a large, almost empty, *aneurmysmal sac*, capable of containing about 10 oz. of fluid; it was formed on thearch of the aorta.

We found on the right side of the sac, looking towards the cavity of the chest of the same side, a perforation large enough to admit the end of the index finger; this opening was of a circular form, but with irregular ulcerated edges; some fibrinous matter, of a dark color, was found in the sac. The walls of the sac were about the thickness of those of an ordinary stomach. The sac was attached to the vertebræ on both sides; it embraced the four upper dorsal, in fact these might be said to form its posterior wall. The cartilage which covered the bodies of the vertebræ, was completely absorbed, leaving the bone uncovered, but smooth to the touch. As in similar cases on record, doubtless if death had not put a period both to life and the disease, the bodies of the vertebræ would have been gradually absorbed, according to the well known laws of certain morbid actions. The left lung was sound; so also the heart and its valves. All the rest of the organs normal. but rather exsanguineous, as usual when a patient expires from the sudden rupture of a large aneurysm.

The entire sac with the vertebræ was removed, and is now in the museum of the Louisiana Medical College.

Dislocation of the head of the humerus in the axilla, of 4 weeks standing. Reduction. J. M., an Englishman by birth, and a sailor by vocation, had his left shoulder dislocated on the 14th of May, by the violent and compulsory measures used to force him on board a vessel about to sail from Boston.

Soon after the accident, the ship sailed for the port of New Orleans, and as they had no surgeon on board, the reduction of the limb was not attempted, nor was any thing done for his relief, although he suffered considerably on the voyage. He arrived here on the 13th June, and on the following day, the 14th, he came to the Hospital to be relieved.

He was a robust and muscular man, and about 25 years of age. Having explained to him the nature of the injury, and the pain he would suffer, as well as the risk to be encountered, he consented to have it reduced.

We found the head of the humerus firmly fixed in the axilla, and thrown a little forward under the pectoral muscle. It was immovable by ordinary force, and the arm was confined to the side. It is unnecessary to go into detail; the diagnosis was easy. In the presence of, and by the assistance of Drs. Mercier, Henderson, O'Regan and Mr. Vandergriff, we subjected the patient to the pullies; after one or two efforts, directed with great care, we succeeded in breaking up the adhesions which the head of the humerus had contracted with the circumjacent soft parts, as was easily recognised by the starting of the bone, and the distinct cracking at the same time, and brought the head of the humerous on the internal edge of the glenoid cavity. At this point it remained, notwithstanding a second effort at reduction. The chief obstacles to reduction lay in the powerful muscles which surround the scapulo-humeral articulation. To obviate this difficulty—to relax the muscular system, now that the adhesions were broken up, a vein was freely opened in the arm, and the blood allowed to flow, (the patient being all the while seated in a chair) until syncope threatened. Before the patient could rally, we resorted again to the pullies; and soon had the satisfaction to find the head of the bone gradually approaching the glenoid cavity. At this moment we ordered the extension to be quickly suspended, and grasping the elbow of the dislocated arm with our right hand and with the left made pressure upon the head of the humerus upward and backward, suddenly brought the arm forwards and inwards, and the reduction was effected. The patient bore this painful operation without a murmur, or the least expression of suffering. To maintain the parts *in situ*, the hand of the dislocated arm, was carried over the chest, and made to rest upon the opposite shoulder, and confined in this position by a bandage.

For two or three days, the patient complained of considerable pain and sorensss about the joint; a mild cathartic and quietude soon made the patient easy, and some six days since the reduction, he expresses himself nearly well, and will be discharged in a few days.

Fracture of the fourth and fifth Cervical Vertebrae.— Charles H. Englishman, aged 25 years, of robust frame, was accidently precipitated head foremost down the hold of the ship Creole, just arrived from Havre, about 11 o'clock on the night of the 5th june, 1845. He was not discovered for some time after the accident, and when picked up, was found affected with loss of motion and sensation of both the upper and lower extremities. On the following morning, (the 6th), he was brought to the Hospital. We found him as follows: complete loss of motion and sensation of thoracic and pelvic extremeties, he felt neither the pricking of a sharp instrument, nor was he conscious when the limbs were moved; complained of great soreness of the entire scalp; cried out with pain and complained of great agony when his head was moved; described the pain as seated on the back part of the neck just above the points of the scapulae. On inspection, we discovered a contused spot, at, and a little above, the junction of the cervical with the dorsal vertebra. The soft parts at this point, were discolored, red, and swollen; pressure here caused the poor fellow to cry out. From the thickening and tenderness of the parts, we were unable to decide upon the exact nature of the injury. The integuments above a line drawn across the chest parallel with the mammæ, were exceedingly tender on pressure; indeed the sensibility of these parts seemed greatly exalted. The respiration was easy and natural; the skin hot but soft; the pulse 90 and full; the superficial veins were considerably distended; his tongue was brown and dry in the middle; considerable thirst; intellect quite clear; no discharge of urine or fœces since the reception of the injury. *Ordered:* vs, to 25 oz. *catheterism, cath. enema, cold lotions to head, cups in the evening to cervical vertebræ.* The v. s. produced little or no relief; several ounces of clear urine were drawn off, with the catheter; the cathartic enema had no effect, and the cups were not applied. In a few hours after the bleeding, the pulse became very rapid; the skin hot; the mind wandering; the respiration a little hurried; *vomiting* supervened; a large quantity of dark turbid fluid, bearing a striking resemblance to the *black vomit*, was ejected several times from the stomach; the tongue assumed a dark colour, became also very dry. About five o'clock in the evening of the same day, he died.

Post mortem, 15 hours after death. Nothing unusual about the cadaver. A large quantity of very dark thick blood escaped on removing the scalp, about the junction of the sagital with coronal sutures and for some distance along the course of the longitudinal sinus; showinng clearly that this part had brushed some hard body, in the fall. The dura-mater which corresponded with this region, was covered with an effusion of blood. All the membranes of the brain were highly engorged.

The substance of the brain itself was injected; a little bloody serosity in the lateral ventricle—it was more abundant at the base of the brain.—After the brain was removed, twenty or thirty ounces of black fluid blood escaped from the divided vessels at the base of the scull. We next turned our attention to the cervical vertebra; after removing the thick layer of muscles which occupy this region, we found the spinous process of the 4th vertebra fractured; the soft parts contused and ecchymosed, and the muscles bruised; on removing the part of the bone which was fructured, we found the spinal marrow crushed; the membranes which invest it, ruptured, and the medulla protruding through the wounded membrane. The bodies of the fourth and fifth cervical vertebræ were fructured and displaced, thrown backwards or outwards, thus crushing to an impalpable mass, the inclosed spinal marrow. Beneath and around the seat of fracture, some coagulated blood was found, between the bony canal and the membranes. We removed the fractured bones, laid open the theca of the spinal marrow, and about two or three ounces of a bloody serum escaped, showing conclusively that a large quantity of sero-sanguinolent fluid had been poured out between the membrane and the marrow, throughout a greater portion of the tract.

MONTHLY REPORTS of the N. O. Charity Hospital, for the months of May and June, 1845.

MAY.

MAIN BUILDING.

Admitted,	403
Discharged,	384
Died,	41
Remaining on the 1st June,	328

LUNATIC ASYLUM.

Admitted,	27
Discharged,	25
Died,	2
Remaining on the 1st June,	86

JUNE.

MAIN BUILDING.

Admitted,	452
Discharged,	393
Died,	28
Remaining on the 1st July,	359

LUNATIC ASYLUM.

Admitted,	35
Discharged,	33
Died,	5
Remaining on the 1st of July,	67

NOTE.—It must be remarked that all the patients admitted into the Lunatic Asylum are not insane. It is customary to place here cases of *delirium tremens*, and any other cases of delirium that are refractory, and disturb the inmates of Medical and Surgical wards. The Lunatic Asylum is in the immediate proximity to the main building, and the rooms are perpectly neat and comfortable.

MORTALITY OF NEW ORLEANS.

List of DEATHS and DISEASES in the City of New-Orleans, during the months of April and May, 1845,—*viz :*

April.—Bilious Remittent Fever, 1 ; Intermittent Fever, 1 ; Congestive Fever, 2 ; Typhoid Fever, 1 ; Adynamic Fever, 1 ; Puerperal Fever, 2 ; Injury of Brain, 1 ; Apoplexy, 7 ; Cerebritis, 1 ; Meningitis, 6 ; Angina Tonsillaris, 2 ; Croup, 1 ; Bronchitis, 5 ; Hæmoptysis, 1 ; Pleuritis, 1 ; Phthisis Pulmonalis, 43 ; Hypertrophy of the Heart, 1 ; Cancer of Stomach, 1 ; Gastro-Enteritis, 2 ; Enteritis, 10 ; Cholera Infantum, 2 ; Diarrhœa, 7 ; Colitis, 3 ; Dysentery, 4 ; Vermes, 1 ; Ascites, 3 ; Hepatitis, 2 ; Nephritis, 1 ; Cancer Uteri, 3 ; Lumbar Abscess, 1 ; Injury of Spine, 3 ; Trismus Nascentium, 10 ; Tetanus, 2 ; Convulsions, 10 ; Delirium Tremens, 5 ; Poison by Opium, 1 ; Scarlatina, 9 ; Erysipelas, 2 ; Scorbutis, 1 ; Dentition, 2 ; Marasmus, 2 ; Sciatica, 1 ; Intemperance, 1 ; Fracture, 1 ; Amputation 1 ; Accident, 1 ; Poison unknown, 1 ; Gunshot, 2 ; Burn 3 ; Drowned, 11 ; Still Born, 18, Old Age 2 ; Unknown 20 ; Total, 226 ; Males, 140 ; Females, 86 ; Adults, 136 ; Children, 91.

May.—Bilious Remittent Fever, 3 ; Pernicious Intermittent Fever ; Congestive Fever, 2 ; Typhoid Fever, 4 ; Typhus Fever, 2 ; Compression of the Brain ; Congestion of Brain, 5 ; Apoplexy, 3 ; Meningitis, 10 ; Cerebritis, 2 ; Softening of Brain, 1 ; Brain, not specified, 1 ; Gastritis, 2 ; Angina Tonsillaris, 1 ; Croup, 3 ; Bronchitis, 8 ; Pertussis, 2 ; Pleurodynia, 1 ; Pleuritis, 1 ; Pneumonia, 5 ; Pleuro-pneumonia, 1 ; Phthisis Pulmonalis, 30 ; Hypertrophy of Heart, 1 ; Aneurism of the Aorta, 2 ; Pericarditis, 1 ; Gastro-Enteritis, 8 ; Gastro-hepatitis, 1 ; Enteritis, 9 ; Colitis, 1 ; Diarrhœa, 10 ; Cholera Morbus, 6 ; Dysentery, 10 ; Atrophia Abdom. 1 ; Hepatitis, 1 ; Disease of Liver not specified, 1 ; Ascetis, 1 ; Icterus, 1 ; Vermes, 1 ; Rupture of Bladder, 1 ; Metro-peritonitis, 1 ; Menorrhagia, Caries, 1 ; of Vertebra, 1 ; Luxation of Cervical Vertebra, 1 ; Injury of Spine, 1 ; Convulsions, 5 ; Delirium Tremens, 4 ; Trismus Nascentium, 10 ; Tetanus, 1 ; Epilepsy, 2 ; Insolation, 1 ; Scarlatina, 9 ; Rubeola, 2 ; Erysipelas, 2 ; Syphilis, 2 ; Scrofula, 3 ; Anæmia, 1 ; Rheumatism, 2 ; Dentition, 8 Dropsy, 1 ; Aneurism, 1 ; Intemperance, 2 ; Gangrene, 1 ; Poison, 1 ; Fracture, 3 ; Accident, 3 ; Drowned, 9 ; Still Born, 12 ; Old Age, 1 ; Unknown, 31 ; Total, 276 ; Males, 176 ; Females, 95 ; Adults, 145 ; Children, 126.

From Dr. Lewis, Secretary of the Board of Health.

ABSTRACT OF A METEOROLOGICAL JOURNAL FOR 1845.

By D. T. Lillie, at the City of New-Orleans

Lat. 29° 57′ Lon. 90° 7′ west of Greenwich.

1845. Months.	Thermometer.			Barometer.			RAINY DAYS.	PREVAILING WINDS.	orce of winds	RATIO 1 TO 10	QUANT OF RAIN.	
	MAX. 0 tenths.	MIN. 0 tenths.	RANGE. 0 tenths.	MAX. 0 hund.	MIN. 0 hund.	RANGE. 0 hund.					INCHES.	THOSANDS
MAY.	89.5	64.0	25.5	30.25	29.98	0.27	15	S.E.	2	1-4	4	959
JUNE.	90.0	70.0	20.0	30.27	30.01	0.26	7	S.	2		2	795

REMARKS.—The Thermometer used for these observations is a self registering one, not attached to the Barometer, and is placed in a fair ex posure. Hours of Observation, 8 A. M., 2 P. M. and 8 P. M.

The Barometer is located at an elevation of 19 feet above the level of the ocean, and is suspended clear of the wall of the building. The Rain Gauge is graduated to the thousandth part of an inch, and the receiver is elevated 40 feet from the ground.

Darling's Truss. We would call the attention of the profession and others to a new truss, invented by an ingenious mechanic of this city, Mr. Darling It will be found on the cover of the journal. It has received the sanction and approbation of several eminent physicians and surgeons of this city, and from a careful examination of its structure, we feel satisfied that it is, perhaps, the best instrument of the kind now in use. It is extremely neat, can be adapted to the retention of any reducible hernia, may be worn with but trifling inconvenience, and we feel satisfied that it will be generally adopted, when better known. We saw it applied a short time since for the retention of a scrotal hernia, and nothing could exceed the ease and comfort with which it was adjusted to the parts.

PATIENTS admitted into the United States Marine Hospital at Mobile, during the quarter ending 30*th June*, 1845.

R. GALE, M. D., Surgeon.

DISEASES.	Numbr	Disch'd	Died.	Rem'g	DISEASES.	No.	Disch'd	Died.	Rem'g.
Abscessus	2	2	0	0		1	1	0	0
Ambustio	1	1	0	0		1	1	0	0
Bronchitis	1	1	0	0	Hepatitis	1	1	0	0
Bubo (Sympathetic)	1	0	0	1	Paraplegia	9	9	0	0
Contusio	10	6	0	4	Pneumonia	1	11	0	1
Colica Biliosa	3	3	1	0	Rheumatismus Chron.	14	13	0	1
Phthisis Pulmonalis	2	1	0	0	" Acutus	2	1	0	1
Dysenteria	11	10	0	1	Morbus Venereus	6	6	0	0
Diarrhœa	3	2	0	1	Strictura Urethræ	3	3	0	0
Epilepsia	2	2	0	0	Ulcus				
Febris Intermittens	15	12	0	3	Vulnus				
" Biliosa	4	3	0	1					
Fractura Cruris	4	4	0	0					
" Comp.	1	1	0	0					
	60	48	1	11		83	35	00	3
					TOTAL,	98	83	1	14
Remaining over last quarter,........................									
Typhoid Pneumonia,................................						1		1	
Scrofula,..						1			1
						100	83	2	15

THE NEW ORLEANS

MEDICAL AND SURGICAL JOURNAL.

SEPTEMBER, 1845.

Part First.

ORIGINAL COMMUNICATIONS.

ART. I.—*Remarks on Yellow Fever.* By JOHN HARRISON, M. D., Professor of Physiology and Pathology in the Medical College of Louisiana.

To those at a distance, the mention of New-Orleans calls up the idies of disease and death as inseparable associations; yet, during eight months in the year there is not, perhaps, a healthier city in the Union. In the four remaining months we are liable to suffer from the Yellow Fever—the pestilence of the South—the great obstacle to increase of population in her cities, and, of course, to all other advantages which increase of population brings with it. Hence the causes of this disease, its pathology, and the methods of treating it, become of immense importance to the public, and hence, too, a duty rests upon those who have seen much of the disease, to impart their experience.

I purpose in the following pages to state the symptoms of Yellow Fever, and, as far as practicable, the lesions found after death; to sift, as far possible, its pathology, and to discuss the best methods of treating it. In so doing, I shall confine myself to my own convictions and experience without reference to the writings and opinions of others. What I write is the result of observations made during thirteen years' practice in the city of New-Orleans; during ten of which I was connected with the Charity Hospital, either as house-surgeon or visiting physician. In that noble retreat for the unfortunate, and in private practice, I have treated many cases of the disease, and have made or assisted at several hundred *post-mortem* examinations. In addition, I have experienced the disease in my own person. I make these remarks for the purpose of showing what opportunities I have enjoyed of studying the disease; and if, in the present paper, there be found little that is new or of much value in a positive sense, it still may, I hope, be of some service in destroying erroneous notions of the disease, contracted by those who have never seen it.

In a paper like this it is necessary to be brief. Yellow Fever, like all other diseases, has its specific symptoms, developed in its rise, course, and terminations, and by these we designate the disease. But the law of variety prevails in disease as well as in organology, and in none more than in Yellow Fever; and, as we cannot expect to find two faces or two leaves exactly alike, we may also expect to meet with varieties in cases of diseases arising from the same cause. The most important varieties in the symptoms of Yellaw Fever will be mentioned, but the minute detail of cases avoided; inasmuch as it is only from the essential characters of the disease, sifted from accidental or unimportant occurrences, that we can expect any general and practical good.

The Yellow Fever makes its earliest appearance about the latter days of July, and continues until the appearance of frost, which is usually about the middle of November. Frequently, however, it appears later in the season, and sometimes disappears, at least as an epidemic, before frost—probable for want of fuel. A few cases, are also met with some weeks after cold weather has set in; but the disease invariably ceases as an epidemic after a frost sufficiently severe to kill the leaves of trees and annual plants. Many attempts have been made to connect its appearance with meteorological phenomena, but all, so far, have been unsuccessful. I have observed that in those years in which epidemics prevailed to a greater extent than usual, they were preceded by intermittents of a bad type. These last prevailed during the latter part of May, in June, and the earlier part of July. Whether they are the constant forerunners of Yellow Fever or not, can only be decided by a long series of observation. It attacks only strangers, those born in the city being perfectly exempt from the disease, though it is still a question whether they do not pass through it in infancy. The creoles of the State, residing out of the city, and never subjected to the disease, are as liable to attacks of as grave a character, as those born farther North. Upon recovery, the citizen is said to be *acclimated*, and enjoys an immunity from the disease; but this though general, is not universal, for I have known several who have been attacked a second time. I have never known, however, such cases to terminate fatally. Some persons, also, (and those not a very few) pass through the most violent epidemics without being attacked at all, and are then considered fully acclimated. This rule, however, does not apply to those who have remained in the city during what are termed *mild* epidemics; in other words, those who pass through such epidemics as occurred in 1833, 1837, 1839, and 1841, without an attack, are considered as safe as those who have recovered—otherwise not. Persons coming from cities where the disease prevails, and in this respect similarly situated to New.Orleans, also enjoy an immunity from the disease. Such is Charleston, S. C. Again, the susceptibility to the disease seems to be renewed by long exposure to cold weather, as young men born in this city, but sent to Europe or our Northern colleges, are sometimes attacked on their return. Negroes certainly have the diseases in a much milder from than the whites, and the mortality is far less among them. I cannot say that I have absorved any difference as regards the sexes, either as to the number or the malignity of the cases. As a general rule, children have it milder than adults.

From the preceding remarks it will be seen that the Yellow Fever is by no means regular in its visits, nor are those visits of equal malignity. Of the years in which I have observed it, the following is a summary:

1832, Epidemic.
1833, Violent epidemic.
1834, Epidemic.
1835, Mild epidemic.
1836, Very few cases.
1837, Violent epidemic.
1838, Few cases.
1839, Violent epidemic.
1840, None.
1841, Violent epidemic.
1842, Epidemic.
1843, Epidemic.
1844, Mild epidemic.

The terms "mild epidemic," "epidemic," and "violent epidemic," are intended to express degrees, both as to the prevalency and malignity of the disease.

In 1832 a violent epidemic of Asiatic cholera raged at the same time the fever prevailed. In 1833, 1834 and 1335, there also existed sporadic cases of cholera.

When I add that the fever is confined to the city and neighboring towns, or to those who enter them but afterwards return to the country; that persons residing in the country around, even though they be unacclimated, are safe from its attack, and that, in all years in which it appears, we see very mild as well as very malignant cases, I have mentioned, I believe, the principal points counected with the appearance of Yellow Fever.

Let us now examine the symptoms.

SYMPTOMS.

Omitting individual peculiarities, let us sum up those symptoms by which the disease is recognized. We will suppose a person who has been protected, in the best way possible, from those obvious causes of disease which may affect the health at any season. He is well lodged and clothed; he is temperate in his diet, and is careful not to expose himself to the sun, to wet weather, or the night air; he is abstemious with regard to alcoholic liquors. These precautions, however, avail him little. In the midst of excellent health, he is stricken down. He experiences a rigor, which sometimes ends in a violent ague; in a few hours a burning fever comes on, with distressing pains in the head, back, and limbs. The tongue, however, is as yet moist, and the urinary secretion, copious; but the eyes are generally dull and heavy, and intolerant of light.

In the course of 24 or 36 hours, the usual consequences of violent fevers ensue; the secretions are diminished in quantity, and altered; the tongue becomes red around the edges, pointed and furred, with a white or yellowish down in the middle; sometimes, though rarely, it is dry. Sordes appear upon the teeth. The urine is highly colored, and in many cases highly corrosive. The skin is usually moist, with sudamina scattered here and there—principally over the breast. It is, however, sometimes dry, and very hot. The pulse continues strong and quick, beating at the rate of 108 to 120, or over, per minute.

Towards the close of the third day, or beginning of the fourth, the fever intermits. The prostration of muscular power, which has been increasing from the first moment, is now complete—the patient being scarcely able to turn in his bed. The pulse falls in frequency even below the natural standard, though in general retaining its usual fulness. The stomach now becomes more or less irritable, being unable, in most cases, to bear even a spoonful of cold water. The skin and eyes assume a yellow tinge, and both are highly injected. This injection, however, does not appear to be attended with high action, for the skin is now rather cold to the touch and the secretions from it seem altogether to have ceased. If we press with a finger upon the surface of the body, we observe, upon removing it, a white spot, which slowly and gradually resumes it former color. This is strikingly in contrast to the quick flash wherewith the blood returns into the tissues on the first or second day. This injection in truth is of a passive character, and is undoubtedly one of the consequences of the foregoing violent actions to which the whole system has been subjected, and by which the organization of the tissues have suffered. In short, the parts are changed in structure—have lost in consequence their natural elasticity—make little resistance to the blood coming from the heart,—and are injected as we might inject a sponge with a syringe.

From the condition last described, the patient gradually returns to health or dies. If death is to be the result, we shall see the irritability of the stomach growing almost hourly greater—even a teaspoonful of cold water being thrown up the moment after being swallowed. An indescribable *malaise* afflicts the sufferer, although he appears at the same time to be without any fixed or local pain. A continual sighing, involuntary groans, extraordinary restlessness, great dimunition, or a total stoppage of all the secretions, announce the approach of the fatal symptom—black vomit. On the fourth, fifth or sixth day, this is thrown up, and death soon closes the scene.

The matter first thrown up consists almost entirely of the drinks taken. A few flocculi of mucus may be discerned floating here and there in the liquid. Towards the approach of black vomit these flocculi increase in quantity, and are of a deep gray color. Mixed with them we may often find, upon a close examination, a few striæ of a darker matter—in other words, of black vomit.

This last mentioned fluid is not thrown up in the manner that emesis usually occurs. The muscular motions, and the sounds accompanying the ejection, are are peculiar. There is no violent retching; a sound is heard caused apparently by a hiccough mingled with a cough, and the black matter is ejected. In many cases this is done so violently as to send it many yards. I have seen it, in the Hospital, thrown entirely over the bed of the next patient and fall on that adjoining.

The conditions of the patients when throwing up black vomit, vary most remarkably. Some are quiet—answer questions—and appear rational, but indifferent to their fate; so much so, that they will frequently respond to questions concerning their condition, by saying "they have the black vomit." Some will even get out of bed and walk about—declare they are perfectly well, and wish to dress themselves. I have seen this occur, and death take place in half an hour afterwards. Others are delirious, and force is required to keep them in bed; others lie in a semi-comatose state, and keep up a constant and most distressing moaning.

Such is the usual course of the disease; but there are a vast number of individual difference which we ought to expect, since it would be difficult to find any two persons in precisely the same condition at the moment of attack; and, therefore, it is but in the application of the well known law "that the same cause acting on different subjects must produce different effects," that we should be led to expect individual differences in all epidemic diseases. Let us take notice of some of the most remarkable.

A. The symptoms of Yellow Fever will vary according to all those general circumstance which modify those of other diseases. Thus, we may have the congestive, inflammatory, or typhoid from, according to circumstances. In many cases we have gastric irritability from the very commencement, together with pain or pressure at the epigastrium. Towards the close, we have, in many cases, passive hæmorrhages from the application of cups, from leech bites, from wounds given by the lancet in bleeding, &c. They also occur from the gums and tongue, from the nostrils, from the bowels, from the scrotum, from the eyes, (though rarely,) and sometimes from the ears. These hæmorrhages, though dangerous, are not however indicative of a desperate case, since I have known many of them recover. In one case under my charge, the patient bled for three days from the gums and tongue, losing about a pint a day, when it was arrested by the use of kreosote, and she recovered. The blood lost coagulated to some extent, but very imperfectly.

B. In some few cases there is no actual chill at the commencement of the disease; and indeed, no fever ever makes its appearance. These cases are characterized by extreme restlessness from the beginning, and by an indomitable disposition to walk about; hence, they are sometimes called "walking cases." They present, as is well known, by far the most formidable variety to the physician, and indeed are generally regarded as incurable. The patient in this form of the disease, presents a natural eye, tongue, and pulse. The skin is also natural, except that towards the extremities it becomes cold, and very often the hands and feet look as if they had been subjected to the long action of water. When the patient is questioned, he seems loath to answer—returns sullen replies, and tells the physician there is nothing the matter with him. There is no gastric irritability, and no pain or pressure at the epigastrium. The stools are fluid, and the urine copious and limpid. In all this absence of the usual symptoms which mark the disease, there is, however, a very peculiar and characteristic expression of the countenance, which cannot be described, but when once seen is never forgotten.

The condition above described, continues until the end of the second or beginning of the third day, when a change occurs. The patient lies down from inability any longer to keep up; the pulse sinks in volume, but increases wonderfully in frequency; the skin grows hot; the stomach swells up, protruding the walls of the abdomen before it; black vomit in large quantities is thrown up at the first gush, and death follows very shortly afterwards. This variety is exceedingly rare.

C. In others cases, we have all that tribe of symptoms which characterize ataxic fevers; such as, great irregularity in the capillary circulation; certain parts becoming suddenly cold and pallid, and the next moment red and hot; copious perspiration, alternate with hot and dry skin; the bowels are now loose, and a few hours afterwards the patient may be suffering from the torments of constipation; the respiration is hurried after the slightest exertion; the urine, in the morning, limpid, and in the evening perhaps highly colored; the tongue may be this moment moist, and in a short time, without appreciable cause, perfectly dry; chills and rigors supervene from the slightest application of cold; tremors, nervous dilirium, &c., also occur in this form of the disease. It is generally fatal.

D. Towards the close of the attack, the brain frequently becomes affected. The physician may have flattered himself that his patient is perfectly safe; the fever has subsided for 24 hours or more; the tongue and pulse are good: there is no gastric irritability of any consequence; urine has recently been passed; all things, in short, tend to convince him that the case will end in a speedy cure. After a while, some slight remark from the patient arrests his attention; the nurse informs him that she thinks the patient has been "wandering in his mind;" upon further questioning and trial, however, he can discover nothing more. There is, however, an unquiet glance of the eye which still further excites his suspicions. He pays a visit earlier than usual and enters the sick chamber with a foreboding heart. He finds his worst fears realized—nervous delirium has set in, and he abandons all hape.

In several cases I have seen the brain seriously affected in the first few hours after the attack. In some, convulsions occurred, ending in coma, from which last they never recovered. These cases, however, were in children.

In other cases, though there were no convulsions or coma, the brain was attacked in the begenning, and continued affected throughout the disease. These cases are not characterized by *delirium*, property speaking. The little tricks—the preverse pleasure of thwarting their physician and nurses—the great delight shown upon the success of their schemes, characterize the mental perversion of the insane, rather than of the delirious. These cases, though not altogether hopeless, are exceedingly dangerous.

Delirum often comes on during the first or second day. It is generally relieved by depletive means, and is not considered a very dangerous symptom.

I may here relate a case which occurred in my practice in 1837, and which presents some singular features. The patient, a robust young man, about 25 years of age, had gone through a severe attack of fever, and on the 6th day was pronounced safe. Thinking himself so, he desired his nurse to bring him a mint-julep from the bar of the hotel in which he lay. On her refusing, he insisted that the physician had ordered it. It was brought and drunk. The next day I found him perfectly insane, but without fever or any physical symptom of disease. His insanity manifested itself by loud talking—wild, but somewhat coherent fancies, hysterical laughter and tears; paroxysms of rage, succeeded by whining complaints of ill treatment; insomnolency, and odd remarks, that provoked to laughter the most saturnine of his attendants. Under the use of opiates and the shower bath, he recovered, and is still living in good health; but it was more than two months before he was completely relieved.

Another affection of the encephalic organs is manifested by an unusual disposition to sleep. It appears soon after the patient is taken down. There does not appear to be so much suffering as in ordinary cases; the pains in the head, back and limbs, are by no means so violent. The pulse is frequent and quick, but small and weak. The skin is usually pale, or rather, sallow. But the most formidable symptom is the tendency to sleep. It at once alarms the nurse and attendants; and it may well do so. The patient may be easily roused, will answer questions put to him, but while talking, falls away into slumber. These sleeping cases, as they are called, are justly considered as being among the most desperate the physician can meet with.

There is another phenomenon which is of very general occurrence when the case is about to terminate fatally. I allude to an exquisite tenderness of the epigastric region, supervening between the 4th and 6th days. The slightest attempt to press upon the parts is resisted by the patient with all the expressions of intense agony and horror. What is the cause of this extraordinary sensitiveness? Is it inflammation? Assuredly post-mortem examinations flatly contradict such a supposition. It is known to exist most intensely in cases where, after death, we find but very trivial lesions. Besides, it is not experienced in other diseases, in which, after death, we find much greater lesions of the stomach and duodenum. Nor does this phenomenon resemble that produced by pressure at the epigastrium in cases of simple gastro-enteritis. It rather resembles the effects produced by pressure, in cases of violent peritonitis. But peritonitis does not exist. I have never met with decided peritoneal inflammation in all the autopsies of Yellow Fever that I have witnessed. Besides, this exquisite tenderness is not confined to the epigastrium. It is experienced all over the abdomen and even in the limbs, but is certainly most severe over the stomach.

It appears to me that this morbid sensibility must be referred to a condition of the nervous substance, induced by the preceding violent actions to which the whole system has been subjected. The tissues have been changed in their constitution; not only as regards the minor points of structure, but also chemically. The nervous substance has suffered in common with the rest, and hence the effect. In short, the exquisite pain is owing to a morbid condition of the nervous substance, not to increased action in the parts. It is a most fatal symptom.

Another phenomenon which seems related to that of passive hœmorrhage, is the appearance of petechiæ. They never appear until the febrile symptoms subside, and seem, in truth, to be nothing more than small spots of blood congested in the tissues. Musquito bites also become dark and livid, and much resemble petechiæ. They are both outward signs of that complete prostration of the powers of life, that torpor and want of action, in which the system is left after the preceding violent actions have subsided.

Another and most fatal symptom frequently appears towards the close of the attack. It is a total suppression of the urinary secretion. The kidnies appear to have been completely disorganized by the foregoing violent actions.

There are also some minor varieties of symptoms which it may be worth while to take notice of, and which I shall here group together. The bowels are sometimes costive and with difficulty operated on, while in other cases, or at other periods, they are extremely relaxed. The dejections also differ exceedingly in appearance. At times they are exceedingly large and offensive; at others, they are watery and slightly tinged with coloring matter. Their color, too, varies, being sometimes of a clayey or ash-colored hue; at others, they are composed of dark, tarry, fuliginous matter. Sometimes they seem to be made up almost entirely of water and bile, which in certain cases is secreted in immense quantity. The two last mentioned evacuations, it has been supposed, are caused by the action of calomel; but I have met with them often in cases treated by myself, in which not a grain of calomel or any other mercurial had been employed. The dark and tarry dejections have also been esteemed a certain sign of a favorable termination; but I have met with it repeatedly in post-mortem examinations in the largest quantities—the bowels being literally full of this substance.

The tongue also presents different aspects. Instead of being dry and furred, it is frequently moist—preserving, indeed, an appearance almost natural throughout the case. In truth, those who see much of Yellow Fever soon learn to distrust the tongue in their prognosis.

Though most of the fatal cases are accompanied with black vomit, the rule is by no means universal; nor is black vomit necessarily a fatal symptom. I have never passed through an epidemic in which there were not a few cases of recovery after decided black vomit had been thrown up. In one case, it was thrown up for two days, and recovery took place.

Towards the close of the attack, a most distressing hiccough sometimes supervenes. It is exceedingly obstinate, and will yield to nothing; though not always fatal, it is nevertheless a very dangerous symptom.

The skin, too, presents many and remarkable varieties of color. Sometimes it is pallid, or of a bluish hue—at others, of a beautiful pink, diffused more particularly over the chest, throat and face. Its usual color, however, is (towards the close) of a dun yellow. In some cases the yellow hue comes on only after death.

Active hæmorrhages from the nose or bowels sometimes occur on the first or second day. These must not be confounded with the passive hæmorrhages already spoken of. When not too severe, they are rather favorable than otherwise.

Such are the most prominent symptoms which characterize Yellow Fever during the life of the patient. Let us now follow him to the dissecting rooms, and see if we can ascertain from the dead body the causes of this frightful train of morbid phenomena.

POST MORTEM APPEARANCES.

The Skin.—Our attention is first called to the state of the skin. A few hours after death, nothing is more common than to find all the lower or depending parts of the body in a state of congestion—literally black from accumulation of blood. And this is not confined to the external parts; we shall find the same thing at the base of the brain; in the depending portions of the intestines, and particularly of the ileum. We shall also find this congestion more or less in the lower portion of the lungs, and I believe it constitutes in many cases, what is mistaken for inflammation of the stomach towards the cardiac orifice. As I have already observed, the tissues seem to be partly disorganized by the disease ; they have lost, in a great degree, their natural elasticity ; the capillary spaces are enlarged, and the blood settles down from the mere effect of gravity.

The yellow hue of the skin, what is it owing to? An obvious answer is, that it is caused by the absorption of bile. But this admits of much question. The color, in the first place, does not so very closely resemble the yellow skin of jaundiced persons—an experienced eye may perceive the difference. In jaundice, the urine is colored by bile ; but it is not so in yellow fever. To be sure, jaundice sometimes supervenes in yellow fever, but this is rare, and when such is the case, the urine is tinged. I rather attribute the yellowness to the change of condition which the blood and tissues have undergone. We know, indeed, that a similar appearance may be produced artificially—by a contusion, for example. And, indeed, the color of old contusions resembles the yellow skin of patients in this fever, much more than that produced by jaundice. Again, in cases that die of jaundice we shall generally detect a complete obstruction in the ductus choledochus, an obvious cause for the absorption of bile. In yellow fever subjects, though I have often made the investigation, I have never discovered any such obstruction.

But let us open the body.

The Brain.—This organ is sometimes congested with blood ; at other times it contains a little water in the ventricles and under the arachnoid. The pia-mater is sometimes finely injected ; the dura-mater is rarely affected, and when so, presents only a few small sanguineous spots on its serous surface. In most cases, the brain presents no appreciable lesions whatever. The like may be said of the spinal marrow and sympathetic ganglia.

The Lungs are sometimes obviously congested with blood. They do not retract, as they usually do, when the sternum is removed. They are also, in parts, much discolored. In one case, examined in 1839, a portion of the left lung about, the size of a dollar, was found in a state of apoplexy—the blood was extravasated and coagulated. The mucous membrane lining the trachea and bronchia, is also in many cases finely injected, or spotted with blood. In numerous cases—perhaps in a majority—the lungs present no lesion that we can detect.

The Heart is very rarely, if ever affected. The endocardium is sometimes slightly discolored, but I believe this is only met with in subjects that have been some hours dead, and appears to be occasioned by absorption of the coloring matter of the blood left in the cavities. Small blood-spots are also sometimes found on the endocardium, and seem to be analogous to tho petechiæ on the skin. We generally find coagula in the heart in this disease, but they contain more coloring matter, and are softer than those we meet with in cases of death from most other diseases.

The Liver.—I have never seen any lesion in this organ which could be attributed to the effects of the disease. There is no organ in the body which presents such various appearances as this ; at times, being very dark ; in other cases, presenting a pale yellow aspect. In the cases examined at the Charity Hospital, it is not unusual to meet with chronic affections of this organ, but as we meet with an equal number of cases at other seasons, it is plain they have nothing to do with yellow fever—either in cause or effect. Indeed, as I have heard it sensibly remarked, there is no organ in the body with which the disease may take so many liberties, without material injury to health, as the liver. We frequently find it in conditions which are evidently of long standing, and such as to produce astonishment that the individual could have lived without manifesting his disease, by striking and unequivocal symptoms.

The liver sometimes contains less blood than we usually find in the viscus, and in those cases it is paler and drier than usual. At other times, however, it is engorged with blood, and bleeds freely when cut; but these appearances it is subject to in common with all the organs, and the existence of one or the other appears to depend much upon the condition of the patient at the time of the attack, and the treatment he has undergone. In cases in which the lancet has been used freely, we shall generally find a pale yellow liver.

The Gall Bladder in most cases contains its usual quantity of bile, which is to all appearances healthy. Sometimes it is greatly inspissated; in other cases the bile is more mixed with mucus than usual. I have sometimes found the gall-bladder containing only a little glary mucus; these cases are rare. The mucous coat of the organ is sometimes, like other mucous tissues, injected or spotted with blood. In most cases, it is not affected.

The Spleen is usually sound. It is sometimes engorged with blood, and in consequence, is enlarged and softer.

The Pancreas.—I have never seen this organ diseased in yellow fever.

The Urinary Bladder is rarely changed in appearance. Its mucous secretions are sometimes increased in quantity, and in a few cases the mucous membrane is dotted with small points of blood.

The Kidnies sometimes contain a great deal of blood. When cut into, we can seldom find any appreciable lesion. In some cases, the mucous membrane of the pelvis and infundibulam is, like the mucous coat of the bladder minutely spotted with blood.

The Stomach and Intestines.—In a great majority of cases, the stomach is finely injected with blood. Not only is the mucous membrane thus discolored, and that, too, in cases examined almost immediately after death, but we find also abrasions of the tissue in pit-like holes and furrows. It is beyond all doubt that this engorgement existed before death. In some cases the whole surface of the stomach is affected; in others, the effusion and injection is confined to the cardiac or pyloric portion. The sub-mucous cellular tissue is also generally injected. The duodenum and a large portion of the other small intestines are frequently found in the same condition.

In some other cases, though these are far more rare, the stomach, duodenum and other intestines present us with an almost entire absence of appreciable lesions. Prying and curious eyes have found here and there a few slight red spots, and have convinced their owners that they detected thickening and softening of the mucous membrane. I confess that I have not been able to see this, and the conviction has been forced upon me that in these cases we could not lay our finger on any lesion which would account for the foregoing phenomena, or the death of the patient. But by these remarks, I do not intend to be understood as inferring that these organs have not been diseased. On the contrary, I believe that in all cases of death from Yellow Fever, the stomach suffers, and suffers most severely. All I contend for is, that we are not entitled, from the facts before us, to say that it has been inflamed—and inflammation in my creed is not synonymous with *disease*, but expressive merely of a particular species of morbid phenomena. Those who contend that the stomach must necessarily have been inflamed, reason, not from facts to theory, but from theory to facts. They are guilty of hypothetical, not inductive reasoning. The stomach has doubtless suffered, since all the organs in the body—every tissue—must, more or less, be altered in its constitution, after undergoing such violent morbid actions. But in the cases we now speak of, the stomach seems to have suffered not a whit more than the other organs.

As the stomach usually contains black vomit, it may be argued that the tissues have relieved themselves, by pouring the contents of their vessels into the cavity of the organ; but if this be true, how comes it, that in other cases, in which we also find the stomach full of black vomit, we meet with a mucous membrane literally engorged with blood?

The large intestines and the lower portion of the small, are not so often found congested as the stomach and duodenum, yet such a condition is by no means rare.

Before leaving the stomach, it may be well to remark that, in some cases, it presents a very singular aspect caused by artificial means. It is difficult to describe. The mucous tissue, when washed, has a sort of marbled appearance, with faint lines running here and there, and intersecting each other. This is owing to the acid solution of sulphate of quinine administered in the latter stages of the disease. That such is the fact, has been proved by immersing a stomach not having this appearance in a solution of quinine.

A remarkable feature in Yellow Fever is, the frequent occurrence of intussusceptions of the small intestines. These were exceedingly common in autopsies made in 1839. The quantity of intestine invaginated sometimes exceeded a yard.

In certain cases we found Brunner's glands presenting a miliary aspect. Whether this was connected with the disease or not, I do not know.

In some cases of a typhoid type, in which there existed before death a low nervous delirium, we found, sometimes ulceration, and at others, hypertrophy and softening of Peyer's glands.

Ulceration sometimes occurs in Brunner's glands, but rarely, and when it does take place we generally have hæmorrhage from them.

The mesenteric glands are sometimes considerably enlarged. This occurs generally in cases in which death occurred after the 7th or 8th day. I have also seen them much enlarged in cases treated on the mercurial plan.

The Blood.—This fluid does not present to us any strong evidence of those changes which we might expect after the system undergoing such violent actions. It has been said that it loses its coagulability. Whether such is not the fact in certain cases I will not undertake to say, but in a great majority it is certainly not true, for we find coagula in the heart, and blood taken from the larger vessels generally coagulates after a while. It is unquestionably true, however, that it requires a much longer time to coagulate than blood usually does, and that the coagulum is larger and softer. Blood drawn from the arm rarely if ever presents a buffy coat; nor have I ever seen it cupped.

Black Vomit.—Of this fluid mention will be made under the head af Pathology.

The rest of the body, such as the cellular, fibrous, cartilaginous, osseous tissues, &c., present nothing remarkable. The muscles in many cases are darker and drier than usual.

PATHOLOGY.

The nature of this disease must be inferred from the history of its appearance—from its course and symptoms—from some points in its treatment (to be spoken of hereafter), and from the lesions found after death; for the special cause of it is utterly unknown.

Without troubling ourselves with nosologies or general treatises, it will suffice for our purpose to say that from the earliest times medical writers have in general admitted the existence of two great divisions of fevers, which we may designate by the terms symptomatic and idiopathic. How do these fevers differ from each other? And to which class does yellow fever belong?

A person in good health breaks an arm or leg; receives a pistol shot; is burnt or scalded; in short any mechanical injury may be inflicted on the system, and after the cold stage (into which he is at first thrown) passes over, he is afflicted with high fever. Examining the seat of injury we shall find it to be inflamed. Now, the fever supervening in such cases as these, is consequent, as must be obvious, to a local affection—the phlegmasia—or local affection precedes, and is unquestionably the cause of the fever or general affection.

The means by which the local affection becomes general are the nerves and spinal marrow, the centre of the nervous system; the condition of the nervous substance at the seat of injury is propagated to the spinal marrow; that organ, in truth, takes on the same condition, the nerves going out from it to all parts of the body, are in turn affected—the affection is transmitted to every molecule of the tissues—the relation between the tissues and the arterial blood is changed, and the local affection becomes general. We have fever supervening on a local inflammation.

In the cases above cited the original cause of all this is known—but it frequently happens that we cannot determine the cause of the original inflammation. Thus we often meet with cases of hepatitis, nephritis, cystitis, etc., which we cannot trace to any mechanical or chemical causes. But we must in such cases reason from analogy, and infer their existence. We know that cold applied to certain parts of the body, whilst the rest remains covered, will produce rheumatism and fever. In some cases pneumonia or inflammation of some other organ is the consequence. Indigestible diet produces gastritis, enteritis, or dysentery. In short, we can trace these connections in so many cases as to enforce the belief that all these fevers arise from the same general cause, viz., from local lesions produced by mechanical and chemical agents. They may also be produced through the influence of the nervous system, particularly from irritation of the spinal morrow as in some cases of acute rheumatism. Certain conditions of the blood appear, also, to give rise to local inflammations which may be followed by fever, as anthrax, for instance.

We have mentioned *chemical* agents as among the remote causes of symptomatic fever. But in all such cases it must be understood that the chemical agent acts *locally*, and thus in the same way as mechanical causes. Thus boiling water will produce a scald ; red-hot iron, a burn ; caustic potash and mineral acids destroy the skin ; arsenic and corrosive sublimate disorganize the mucous membrane of the stomach, etc. When the chemical agent is absorbed into the system, the effects are complicated, and the general affection assimilates to idiopathic fevers.

The fevers thus produced by local lesions will of course be modified in their symptoms, duration and results, by all those causes which modify disease in general. The condition of the patient—the organs affected—his constitution—the treatment employed, and many other minor circumstances, must of course produce their effects. Of these causes of modification, this is not the place to speak, but in many important particulars it will be found that symptomatic fevers differ from the idiopathic.

In the first place, their duration is by no means circumscribed within the same limits. They may sometimes be cut short, as it were, by judicious treatment, and the patient be convalescent in a few days, or perhaps hours. Even after they are well formed, depletion appears to abridge their duration. In other cases, from peculiarity of constitution, neglect, or injudicious treatment, they may be prolonged for months, taking on the chronic form. Now, in idiopathic fevers we see on such thing. No one ever cut short a typhoid fever, small pox ; scarletina, etc., by the use of the lancet. The disease will run its course, and the part of the physician is to watch its progress, interfere when important organs are affected, and restrain the officious interference of others, and the imprudence of the patient himself. To attempt to *cure* the disease, as we may cure a pleurisy or pneumonia, in the commencement, is just pure absurdity.

Again, though not universally, symptomatic fevers are generally accompanied by local pains—pains referred to the organs in which after death we find unequivocal proofs of pre-existing inflammation. Thus, pleurisy is accompanied by pain in the chest ; gastritis, by pain in the epigastrium ; enteritis, by pain in the abdomen, &c. All this is strikingly in contrast with the general course of idiopathic fevers. There are certain pains common to all of them ; pains in the head, loins, and inferior extremities. Small-pox, simple continued fever, Yellow Fever, &c., may all set in with exactly the same symptoms, namely, high fever, pains in the head, loins, and limbs, so that it is impossible to make a diagnosis simply from the symptoms. We infer the existence of this or that disease from other circumstances, or its nature is developed in its progress.

The blood, drawn by the lancet or otherwise, also presents some peculiar and interesting differences in these two classes of fever. In the symptomatic, we have the buffy coat, and frequently that phenomenon called "cupping." These phenomena, it is now well known, are owing in these fevers to an increase of fibrine, and though they may not always appear, yet we are assured by Andral * that such increase ot fibrine does always exist. "In the phlegmasia," says he, "there is an excess of fibrine relatively to the globules, that is to say the reverse of what takes place in typhus." (p. 61.) Again: "No matter what may be the state of the system, the representation of an acute phlegmasia involves necessarily, and in every case, the increase of the fibrine of the blood beyond its normal quantity. This law, too, prevails amongst animals as well as amongst men, as I have become well assured from analysing the blood of dogs, horses, neat cattle, and sheep, attacked with various inflammations which had been discovered during life, and after death." (p. 67.) And again: "The formation of an excess of fibrine in the blood is uniformly the accompaniment of inflammation of a grade high enough to give rise to fever; but this fact acquires a greater importance, both as a diagnostic sign, and as an element of the doctrine which relates to the production of disease, if we reflect that it never takes place, unless an inflammation exists somewhere. An excess of fibrine in the blood becomes, therefore, a pathognomic sign of such inflammation." (p. 78.) He also says that the increase of fibrine is strictly proportional to the intensity of the inflammation, and a degree of symptomatic fever accompanying it.

Now let us turn to the idiopathic fevers which Andral terms *pyrexiæ*, in contradistinction of the *phlegmasiæ*. He says: "in my first memoir upon the alterations of the blood, I have proved that the fibrine *never augments* in the pyrexiæ, supposing them divested of all phlegmasial complication; that it often remains in normal quantity, and that sometimes it diminishes to a point at which we do not find it in any other acute disease. I have shown that the pustules of variola, and the dothinenteric *plaques* of typhoid fever, do no have the power of increasing the cipher of the fibrine; and finally, I have shown that with all the possible proportions of globules, whether they were very abundant, or whether they have become very rare, a pyrexia could equally arise with all its varieties of form and gravity," (p. 53.) I must refer to this valuable and interesting work for further particulars; yet I cannot refrain from extracting the following passage which bears powerfully upon the subject in hand.

"At every period of clinical observation, and upon whatever theoretical point of view, the observer was placed, it has been recognized that amongst the pyrexiæ, there were some unattended by any grave symptoms, which marched naturally towards a favorable termination; while there were others which, either at their commencement, or during their course, were accompanied by accidents of such a nature, that it seemed as though the forces which rule the organism were either vanquished, or profoundly disordered to such an extent that the extinction of life must be the consequence; and at the same time it was found that in such cases the blood presented an altogether peculiar appearance; it was observed that, become less consistent, it seemed to tend towards a sort of dissolution. Admitted at all periods, but differently explained according to the prevailing theories, this condition which may develope itself in any pyrexia, and towards which several seem to tend naturally, has been

* Essay on the Blood in Disease. American edition.

called turn by turn putrid, adynamic and typhoid state ; it has its greatest development in the typhus fevers properly so called; it is in some sort inherent in them ; it is as it were their essence. The pyrexia now called typhoid fever, presents it in a slight degree from the invasion and the grave cases of this disease are its marked representation. It does not ordinarily exists in the eruptive fevers, but it often complicates them, and constitutes one of their dangers. Finally, in addition to the pyrexiæ with well marked characters, which have a fully determined place in nosological systems, there are others to which no name has been given, which may yet present in a high degree the different symptoms to which the ancients attached the idea of the putrid state. This is because there may exist in effect, in all the pyrexiæ, a common alteration of which the blood is the seat, and whose existence constantly coincides with the appearance of those phenomena always the same, attributed by vitalism to adynamia, by solidism to relaxation of the fibre, and by humorism to putridity of the humors. This alteration of the blood consists of a diminution of its fibrine ; it is consequently an alteration the inverse of that which betrays in the blood the phlegmasial condition."

But the most remarkable difference in the two classes of fevers exists in the lesions found after death. In symptomatic fever we almost invariably find coagulable lymph effused either into the substance of the organs or on the serous tissues ; and in many cases this lymph is partly organized. The mucous tissues, too, in certain instances, as croup, bronchitis, etc., effuse a matter which, if not identical with coagulable lymph, is exceedingly like it. In other cases we have pus, or muco-purulent matter thrown out from these membranes, and almost always meet with a fine *arterial* injection of the mucous, or sub-mucous coat. Now, although I do not deny the occasional occurrence of coagulable lymph and pus in idiopathic fevers, such occurrence is by no means general ; in fact, it is the exception, not the rule. In the most malignant, most fatal cases, those which run their course most rapidly to a fatal termination, it is notorious that we find nothing of the kind. Who has ever observed hepatization of the lungs, sphacelus of the stomach, effusions of lymph on the pleuræ, pericardium, peritoneum, etc., in such cases ?

We may here digress to say a few words concerning the local inflammations sometimes found in idiopathic fevers. A person may be laboring under a phlegmasia at the time that an attack of idiopathic fever comes on ; the inflammation may run a certain course, and its effects be produced, before the idiopathic fever supervenes on the symptomatic. If, as hereafter will be maintained, idiopathic fevers all arise from poisons, this can easily be conceived. Again, the patient may be attacked with a phlegmasia while in a state of convalescence from an idiopathic fever, and death be the consequence. In such cases we may have buffy coat on the blood, or effusions of lymph, etc. And again, the nature of the morbific cause may have its effect. If the poison be mild in character, and not such as to overwhelm the nervous system at once, the reaction of the system may bring on active local congestions, particularly in robust constitutions ; and this, it appears to me, causes the difference between synocha and synochus. The type of each is not produced by the quantity taken, or by the intensity of the poisonous agent, but by a difference of *kind* in the poisons. The like may be said of *synochus*, *typhoid* and *typhus*. It is true, that some confusion has crept into pathology from the fact, that these terms are often applied to mere conditions—thus, we frequently hear of typhoid cases of small-pox, scarlet-fever, etc. This is to be regretted, but cannot well be remedied. A distinction must therefore be made in the application of these terms; for although persons in weak health, of anœmic habit, or ataxic temperament, or other vice of constitution, will present certain symptoms in any epidemic different from those of the strong and robust; still, independently of these individual varieties, idiopathic fevers have general characteristics marked in their accessions, symptoms, course, and termination—and by these we name them. These general characters, preserved through whole multitudes of patients, can only be referred to a peculiarity in the cause producing the disease.

But to return from this digression; we have seen that idiopathic fever rarely present those morbid lesions in the tissues so common in the symptomatic. There is still another fact with regard to these lesions to be here mentioned. Symptomatic fevers arise from local lesions; thus in pleurisy, an affection of the pleura, precedes the fever; on the other hand the lesions found after death, in idiopathic fever, are the consequences of the disease, and are developed during its course.

Finally, the sudden accession of the fever, the rapid and complete prostration of the powers of life, the tendency to passive hæmorrhages, the appearances of what are termed *crisis*, and the rapid amelioration which follows, together with the effects of remedies, all go to show conclusively, that the idiopathic are diseases essentially different from symptomatic fevers.

The causes of idiopathic fever must therefore be entirely different from those of the other. What are those causes? We know that some of them, as in small-pox, are organic poisons, and it is highly probable, that most, if not all varieties of idiopathic fever are produced by the same class of agents. But we must refer our remarks on this subject to another head.

After these observations, it can scarcely be necessary for me to add, that I regard Yellow Fever as an idiopathic fever *sui generis*, and to be caused by a poisonous agent, also *sui generis*, and of an organic nature.

Taking for granted then, that Yellow Fever is produced by poison (an assumption to be maintained hereafter), the question that first presents itself is, how the poison affects the system. Poisons can operate on the body in three ways only. 1st. By attacking and chemically changing the tissues; such is the action of corrosive sublimate, arsenic, mineral acids in a concentrated state, etc. 2dly. By acting directly on the nervous system, that is, by affecting the nerves expanded or diffused in the tissues, which affection is transmitted to the central organs. 3dly. By being taken into the circulations. Of the first of these modes, it suffices to say, that though the poisons might, thus acting, produce a symtomatic fever, it is obvious from what has already been said, they could not produce an idiopathic fever; moreover, idiopathic fevers do not set in with such lesions. With regard to the second mode, though I shall not here deny in toto the sympathetic action of poisons, still the experiments of Magendie, Delille, Emmert, Verniére, and a host of others, sufficiently prove that they act principally through the medium of the circulation, and render it extremely probable that their specific constitutional effects are produced in that way only. The experiments of Hering and Blake on the rapidity of the sanguine current, also, add great force to such a view.

I shall then take for granted that the morbific cause of Yellow Fever acts upon the system through the medium of the circulation. In what manner does it enter the blood? There are, obviously, but three channels of admission: by the skin, by the intestinal canal, or by the lungs. The skin is protected by the epidermis, an obstacle to absorption, and we may therefore justly conclude that the passage of the poison is in some other direction. If we suppose that the poison is soluble in water, our drinks may be the vehicle of its introduction into the system. If we suppose that it is held in solution in the atmosphere, the lungs are evidently the channels by which it is introduced. It is most probable that the latter supposition is the true one; for the lungs present a vast extent of surface, and absorption and exhalation are continually going on through the air vesicles. Not only is the oxygen of the air taken in, but many other fluids, contained in solution in the atmosphere, are also absorbed; thus persons sleeping in a newly painted room give from their urine the odour of turpentine; and not only is carbonic acid gas thrown of from the system, by the lungs, but the odour of many substances, such as camphor, onions, etc., taken into the system by other inlets, may be detected in the breath.

The poison having entered the circulation, other questions arise. Does it primarily effect a change in the blood—and is that change a necessary forerunner to the constitutional symptoms; or, is the blood a mere carrier of the poison to the tissues? In the present state of science these questions cannot be answered.

It is a well known fact that certain poisons remain in the system for a long time without producing any obvious effect. That of hydrophobia is a familiar example. Now we cannot suppose in such cases that the morbific agent remains inert. It is thrown by the blood upon the tissues and must produce its effect. But the primary effects may be slight and thus escape observation. The changes, however, go on—one following another, until the constitutional symptoms are produced. The poison of Yellow Fever unquestionably acts in this manner, for instances occur in all epidemic years, of persons leaving the city in good health and being attacked on board of boats going up the river, or over the lake—or even several days after arriving at the end of the journey. They must therefore have carried the seeds of the disease within them.

We must now examine the *modus operandi* of the poison. Having been taken into the circulation it will wander with the blood to the different tissues. It matters not for our present purpose, what particular tissue or tissues it may fall upon. It must, whenever it falls, set up a morbid action and this, we are not obliged to suppose must be one high intensity—that is, inflammation. The actions of life go on between the molecules of the tissues and of the arterial blood. Any foreign substance in solution with the blood, must produce some perversion of the vital process, be that perversion great or small; the effects will depend on the chemical nature of the agent. The actions set up may be above, or below the normal standard so far as regards intensity—or they may remain of the same intensity. In other words, disease and inflammation are not synonymous terms; and this is particularly observable in Yellow Fever.

The fever subsides on the third or fourth day, the pulse falls to the normal standard, the skin is cool—all evidence of high action is wanting, but the disease goes on, and for several days. The skin becomes yellow, the tongue dry, the urine scanty, the eyes injected, hæmorrhages occur, etc. In short, the patient is precisely in the most dangerous period of the attack. All this is in striking contrast with a case of symptomatic fever such as pneumonia, pleurisy, etc.

As the poison is carried by the blood throughout the system, it is evident that some portion of it may fall upon the central axis of the nervous system, (the spinal chord) and effect that organ. Now the spinal cord cannot be affected without every tissue in the body being also affected through the medium of the nerves. Modified in its own actions, it will radiate its affections as from a centre. Hence in diseases arising from poisonous agents introduced into the circulation, there are *two* sources of the constitutional symptoms—one in the tissues generally, or wherever the poison may be thrown—and the other, in the spinal chord. In this, again, these fevers differ from the symptomatic.

The spinal chord being affected to a certain point, the constitutional symptoms break forth, a rigor is experienced, followed by violent reaction.

But though this is the usual course of Yellow Fever, it is by no means universal. We sometimes meet with what are termed congestive cases, in which there is no action of a high grade. On the contrary, the system is depressed below the normal state. The pulse is slow, and the hands and feet cold. The term fever applied to such a condition, is evidently a misnomer. We might with as much propriety apply it to a case of collapse in Asiatic Cholera, or to a case of poisoning by a large dose of laudanum. The poison in these cases so profoundedly alters the nervous substance, that reaction cannot take place. The peculiar disease goes on, however, though fever be absent, as is evidenced by the blood, its want of fibrine, the occurrence of passive hæmorrhages, black vomit, suppression of urine; in short, all the characteristics of the disease.

With respect to the lesions found after death, they are evidently the consequences of the disease; and if any proof were wanting of this, it may be found in the fact, that in the worst cases—those of a congestive character—no lesions occur. I regard the lesions in the stomach and intestinal canal, therefore, as I do the yellowness of the skin, the injection of the eyes, the passive hæmorrhages from leech bites, the scant and high-colored urine, etc., not as the causes of the fever or other symptoms, but as effects. They are all but so many different results of the morbid actions to which the system has been subjected; and he who attributes the appalling train of morbid phenomena which characterize this disease to the lesions he finds after death, judges, it seems to me, about as wisely as he who should attribute the conflagration of his dwelling to the ashes which he finds in its place. Nor need I be told that I am admitting the existence of a disease which has no location. I do no such thing. I am as well aware as any one, that disease is no entity—that it is a word expressive merely of perverted actions in the tissues. What I contend for, is, that Yellow Fever is not gastro-enteritis, or inflammation of any other particular organ—nor proceeds from such. In short, that black vomit, suppression of urine, passive hæmorrhages, and the rapid and fatal course of the disease, are owing to other causes—to the influence of poison.

But are those congestions which we find in the stomach, intestines and elsewhere, to be considered as the results of inflammatory action? I very much doubt it. We sometimes find them in the lungs, than which no organs are more disposed to the effusion of coagulable lymph. But we never find hepatization of the lungs in Yellow Fever; I mean, of course, as caused by that disease; not by chronic affections. They seem indeed to be connected with the fever, for the higher the febrile symptoms, the more frequently we find congestions in the different organs. But on the other hand, we can witness with our eyes these congestions taking place long after the fever has entirely subsided: we see petechiæ forming, the eyes becoming hourly more injected, old leech bites becoming livid, the gums and tongue swollen and red, etc. Now in all these cases it is simply passive congestion. There is no action characterizing inflammation, such as increased heat, increase of sensibility, or stronger or quicker pulse, attending the formation of these hyperæmia. If such is the case on the exterior, why should not the same process take place in the interior of the body? There is not one fact to militate against such a supposition.

But at the same time, if these congestions are not evidences of pre-existing inflammations, it must be allowed, that the previous high fever, has prepared the tissues for them; since, (as has already been mentioned), in congestive cases, we rarely find them.

The passive hæmorrhages, so often mentioned, require a few words. They occur, as has already been related, from the bowels, gums, nostrils, etc. The whole system seems to have been profoundly altered. The normal relations between the blood and solids are broken up. The blood arrives in the tissues and passes through, wherever it can find an exit, by the mere mechanical *vis à tergo* action of the heart. It coagulates with extreme difficulty. In many cases every variety of styptics have been used in vain. Even actual cautery is of no avail—the blood oozes forth from beneath the eschar. The only means are mechanical, which suffice when they can be applied.

One of the most striking traits of Yellow Fever is the occurrence of black-vomit. It has been correctly described as resembling coffee grounds in a thin solution of gum arabic, or infusion of flax-seed. But it varies greatly as to color. Sometimes we can see but a few striæ mixed with the flocculent grey matter already spoken of. These striæ are most apt to be found on the sides of the basin. In an hour or so the fluid ejected from the stomach becomes darker on account of their increase. Sometimes, instead of the coffee grounds appearance, the fluid thrown up approaches in color that of venous blood. In some cases the vomit can be distinguished in nothing from blood in an uncoagulated dissolved state. In short, between decidedly formed black-vomit and blood, there are numberless shades--they run into other by imperceptible degrees, and the distinctions that have been made by some authors in the appearances of the matters ejected from the stomach are altogether artificial.

In the quantity thrown up, there is also great difference in different cases. Some throw up enormous quantities—others die after having ejected but a few striæ.

This fluid has never been subjected, that I know of, to a complete chemical investigation. I think, however, that enough is ascertained concerning it, to satisfy us of its origin and general nature, to wit :

It is composed of solid particles held in suspension by the liquid—since they may be separated by filtration.

It is acid in re-action. Litmus paper is turned red, and turmeric paper, changed by an alkali, is restored to its original color.

A white precipitate is thrown down by nitrate of silver, which is again re-dissolved by ammonia, but not by nitric acid. This indicates that the precipitate is probably a hydro-chlorate. Hydro-chloric acid, it is well known, has been detected in the stomach by almost all experimenters.

A fluid, so like it as to deceive most experienced persons, can be artificially formed by pouring a little hydrochloric acid upon blood. The addition of a little mucilage will render the resemblance still stronger.

I once, with Dr. Thos. Hunt, of this city, performed the following experiment. A man was brought into the dead house, while we were there. Upon examination, there was no black vomit in his stomach, but a whitish acid smelling liquid, amounting to about half a pint. Into the stomach, containing this liquid, some blood from the vena cava was poured. At first, we thought the experiment had failed, and we returned to other investigations. Upon examining the fluid, however, after the lapse of 10 or 15 minutes, it was impossible to distinguish it from specimens of black vomit with which we contrasted it.

Now when we take into consideration these facts, and also that, in place of black vomit, it is not unfrequent to find blood in the stomach, and that between blood and black vomit there are numberless shades, we can hardly, I think, avoid the conclusion, that black vomit is simply a passive hæmorrhage which has taken place from the coats of the stomach. It is absolutely the same in nature with the hæmorrhages from the gums, nostrils, bowels, &c., and different from them in color, merely because it has come in contact with the acid contents of the stomach.

That it is a much more fatal symptom than other hæmorrhages, is easily explicable from the great importance and manifold sympathies of the organ which pours it forth.

A reason for the frequent occurrence of hæmorrhage in the stomach, may also be found in the structure of the organ—being, at the same time, one of the most porous and lax in its texture, and one of the best supplied with blood in the body.

[*To be continued.*]

Art. II.—*Medico-legal Considerations respecting Perforation of the Stomach from Disease, and its relations to Corrosive and Irritant Poisoning—With a recent Case.* By W. M. Carpenter, M. D. Professor of Materia Medica and Therapeutics in the Medical College of Louisiana.

The following remarks have been elicited by the circumstances of the case, related as occurring in our own region, and coming directly under our own observation. But few physicians or lawyers give any especial attention to forensic medicine, and it may readily happen that some of them may have cases of the kind submitted to them for their examination, and for the purpose of obtaining their opinion in criminal trials. These remarks by no means cover the whole ground of the particular subject under review, and are by no means intended to throw light on the diagnosis of these cases, or the means of detecting poisons. The sole object had in view, was to impress the necessity of caution in all medico-legal cases of the kind.

It has long been known, that in some cases, the stomach is rendered thin, or softened in some of its parts by the partial destruction or disorganization of one or more of its coats, by disease; and that this condition may exist, for a length of time, without any very decided decline of health, or other symptoms, which would lead to the suspicion of serious disease. This thinning or softening sometimes goes on, until some occasion, when, after a hearty meal, generally dinner, the parietes give way, the contents of the organ are discharged into the peritoneal cavity, and death supervenes, with many of the symptoms generally regarded as characteristic of poisoning by arsenic or some other corrosive substances. Suddenly the abdomen swells, with excruciating pains, and accompanied with violent retchings; the pulse sinks, the surface of the body becomes cold and clammy, the features collapsed and cadaveric, coma sometimes comes on, and the patient dies after a few hours of extreme suffering. The abrupt appearance of these symptoms directly after taking a meal, and in persons supposed to be in the enjoyment of good health, always excites the suspicion of poisoning; and the condition of the stomach, as discovered by post-mortem examination, is too apt to be regarded as confirmatory of this suspicion. In these cases, a perforation is discovered in some part of the parietes of the stomach, without any other lesion of structure in that organ, except in some few cases, a peculiar softening around the perforation, and sometimes there may be traces of inflamation of other parts of the organ. Effusion into the peritoneal cavity is generally discovered, and as a consequence, traces of intense peritonitis. The opening is sometimes circular, with a smooth and even margin, at other times irregular, with a fringed, torn margin, presenting shreds and fragments of membranes. The color of the margins is likewise subject to modifications; sometimes grayish or whitish, with a tinge of blue, at other times brown or blackened. The contents of the stomach, and the fluids found effused into the peritoneal cavity, are likewise often found of a very dark color. Now these are much the same appearances that might be found in some cases of poisoning by corrosive substances, and indeed, Andral distinctly tells us that he thinks *there are no "certain anatomical characters by means of which a so called spontaneous perforation may be distinguished from one which is owing to the action of a poison."* He thinks however, that "when there are a number of sloughs, in several spots in the stomach, there is strong ground to suspect the presence of poison, because such sloughs are but seldom observed in any other case. When, on the contrary, there is no lesion observed in the stomach but the perforation itself, the probability is that it is not the effect of poison; for it is not easy to conceive how a corrosive substance introduced into the stomach could act but on one single point. The thing however, is not impossible."

In order to arrive at more precise notions on this subject, we may regard perforations of the stomach as resulting from either softening of its coats, or from their erosion. This of course is exclusive of carcinomatous perforations.

1st. *Perforation from softening*, during life, is not of frequent occurrence, and its existence, as a specific pathological lesion, has been doubted by some pathologists. In the examinations of bodies, some time after death, it is not unusual to find the parietes of the stomach more or less softened, and even perforated in some part, generally in the most dependent portions; but there is no doubt that this, in general, depends upon the chemical action of the gastric fluids upon the dead tissues. There is, however, unquestionably, a kind of softening, which takes place during life, and which is supposed to be the result of an organic process, *sui generis*. To this variety of softening, Cruveilhier applies the name of "gelatiniform softening" (*Ramolissement gelatiniforme*) to distinguish it from the cadaveric softening, which results from the action of the gastric juice, and which he distinguishes by the name of "pultaceous softening" (*Ramolissement pultacée.*) It is not always an easy matter to decide from a mere anatomical examination, whether the softening and perforation in these cases, existed before, or took place after death; but we may generally decide the question by reference to the symptoms that preceded death; for in cases in which the perforation took place during life, we should have all the violent symptoms which are the necessary consequences of such a lesion. It therefore only remains for us to ascertain, in cases characterized by these symptoms, whether the perforation discovered, is the result of this peculiar morbid vital process, or was caused by erosion. From perforation by erosion, it may generally be distinguished by the peculiar consistence of the tissues which form the borders of the aperture, which are characterized by a peculiar gelatiniform degeneration; without any traces of inflammatory, ulcerative, or other erosive action. This variety of perforation sometimes occurs in persons apparently in the enjoyment of perfect health; causing sudden and violent death, and creating the suspicion of poisoning.

2d. *Perforation by erosion.* These perforations may be considered under two heads, that is, as resulting from an ulcerative process, or from the chemical action of corrosive substances upon the coats of the stomach.

Perforation from ulcerative erosion, is characterized by the aperture generally occupying the middle portions of the ulcer, and the margins of the ulcerated surface generally remain, surrounding the borders of the hole. This gives the edges of the aperture a bevelled shape, that is, they are thinnest near to, and thickest farther from the opening. Ulceration may likewise depend upon different causes; either upon irritation, arising spontaneously, as in chronic gastritis; or upon the application of some substances which occasion and sustain, in that organ, a degree of inflamation, for a length of time. Thus in poisoning by arsenic, if the patient survives three or four days, ulceration will frequently be discovered. But in cases of ulceration from irritant poisoning, perforation probably never occurs, as the patient generally sinks before such an event could arise; the only ulcerations which are observed to cause perforation, are those dependant upon chronic gastritis. This variety of perforation, caused by the slow progress of insidious disease, sometimes takes place in persons who have no suspicion of the lurking mischief within, who have experienced no serious indisposition, and who would be regarded by their friends as enjoying a moderate share of health.

Perforation by Chemical Erosion.—This kind of perforation is produced by the application of corrosive or caustic substances to the coats of the stomach. The action of these substances is rarely limited to particular parts of the organ, and we generally have accompanying the perforation, a considerable degree of inflammation throughout a great part of the lining of the organ. The œsophagus, mouth and fauces are likewise generally more or less inflamed, in consequence of the temporary contact of the corrosive material. In cases of this variety of poisoning, pain and constriction about the fauces are generally prominent symptoms, but cannot be regarded as pathognomic, as we have sympathetic affections of the fauces, in some other cases of gastro-intestinal and hysterical disorders, which cannot be distinguished from those which accompany poisoning.

Perforation of the stomach by irritant and corrosive poisons, is by no means of common occurrence, and can only result from the operation of a limited number of corrosive substances, and that in a concentrated form. Most of these corrosive substances leave some peculiar marks, or discolorations of the tissues acted on, which serve to suggest, in most cases, what particular article produced the slough.

The following are, to the best of our knowledge, the only substances regarded as capable of producing corrosive perforation of the stomach, and their action is, in some degree, characterized as follows: They act chemically on the tissues with which they come in contact, and generally produce inflammation of the stomach, and all, except, perhaps, arsenious acid and phosphorus, invariably produce inflammation of the mouth, fauces and throat; and the reason why these do not, is, that they are very slightly soluble, and their solutions do not act promptly upon tissues.

Sulphuric Acid blackens, or renders brown the tissues on which it acts.

Nitric Acid renders them yellow.

Hydro-chloric Acid turns them yellowish, or dark green.

Arsenious Acid.—Perforation of the stomach by arsenic is exceedingly rare, as there are but three cases on record. In those cases, the tissues around the orifice were described as of a gelatinous consistence, or of a reddish brown color. Even ulceration is not to be expected, unless the patient survives a day or two.

Bi-chloride of Mercury gives the disorganized tissues an ash grey color.

Phosphorus gives a peculiar garlicky odour.

Iodine, and some other substances, have been supposed, by some, to produce perforation of the stomach, but we are not aware of cases in which such effects could be attributed to them.

The above mentioned discolorations, though perhaps not invariable, are sufficiently so to serve as a general character. But we must be careful not to mistake for these, the discolorations of the tissues which are frequently found in dead bodies: thus, the stomach and intestines are frequently seen to be colored reddish, brownish, yellowish, or greenish, by the proximity of the liver, spleen, or other tissues and organs.

Bearing the above facts in mind, we may often decide whether the perforation has been spontaneous, or the result of a corrosive substance; and even to infer what the substance was. But the mere anatomical inspection in such cases, conducts at best, to nothing better than mere probability, and gives us no information sufficient to justify the formation, or expression of an opinion.

Neither do the symptoms, or other features of the cases, generally, afford any more certain grounds for diagnosis ; but there are additional ascertained points and facts of a general nature respecting the different kinds of perforation, which are worth bearing in mind, as they sometimes indicate, with a degree of probability, the nature and cause of the lesion.

Perforation from Disease.	*Perforation by corrosive Poisons.*
1. Not a very unusual occurrence.	1. Occasionally observed when the strong mineral acid, or other concentrated corrosives have been taken. Perforation resulting from the irritant poisons, as arsenic, is hardly known, and there are but three cases on record.
2. Spontaneous perforation of the stomach most generally occurs in young females, of from 18 to 23 years of age. (*Taylor Med. Juris.*)	2. Occurs without reference to age or sex.
3. The severe symptoms frequently come on during the appearance of perfect health, or after slight indisposition.	3. Occurs without any reference to previous health.
4. The attack generally comes on soon after a meal, and the symptoms *become suddenly severe.*	4. The symptoms of irritant poisoning frequently appear soon after taking a meal, but the suffering comes on gradually, and increases slowly in severity.
5. Vomiting rarely takes place, and though there are violent retchings and efforts to vomit, but little, if any of the contents of the stomach are brought up.	5. The vomiting, in cases of irritant or corrosive poisoning, is always severe.
6. Purging is not usually seen as a symptom in cases of spontaneous perforation of the stomach.	6. Purging is a common, and almost invariable attendant of irritant poisoning.
7. It is not usual to find traces of general inflammation of the stomach in these cases, and even around the orifice they may be wanting.	7. The stomach is generally found highly inflamed, in the greater part of its extent, in cases of irritant and corrosive poisoning. But this is not always the case, as there are three or four cases on record, in which poisoning was clearly proved, and arsenic found in the stomach and intestines, but there were no traces of inflammation discovered in the stomach or bowels.

The foregoing facts are the most important, if not the only ones, that would be likely to indicate what was the cause of death in a case of the kind under consideration. But it will readily be perceived, by the attentive consideration of these data, how little reliance is to be placed upon an opinion derived solely from the history and progress of such cases, and from the character of the anatomical lesions detected by post-mortem examinations. The *ensemble* of the symptoms and post-mortem appearances, will certainly, in many, perhaps in a large majority of cases, afford very strong reasons for or against the *probability* of poisoning; but an exclusive reliance on these data subject the examiner perpetually to errors which may have the gravest consequences. The position of a physician as a medical jurist, always demands that he should be cautious and circumspect in the expression of his opinions; as they might otherwise tend to awaken or confirm injurious suspicions against the innocent, cause long and distressing imprisonments, and even jeopardize the lives of persons, against whom suspicion would never be breathed, were it not for the hasty expression of an almost groundless belief, by the attending or examining physician. But in these particular cases, there is often an additional and particular circumstance which places us under especial obligation to withhold any expression of opinion that the deceased was poisoned, until every chemical test had settled the matter conclusively, and left no shadow of doubt. This circumstance is, that spontaneous perforations and its consequent violent symptoms, and death, generally occur in young women, and at an age when the suspicion of poisoning might, and sometimes does, give rise to the most painful conjectures, and serves as the basis of the most malicious reports. The example given is by no means the only one in which the virtue of the lady has been impeached, and the death regarded as suicide by poisoning with a view to conceal her shame.

Most of our readers will be familiar with the case of Charles Angus, tried at Liverpool in 1808 for administering poison to Miss Burns, thereby procuring abortion and murder. In this case there was a perforation of the coats of the stomach between the size of a crown piece and the palm of the hand. The medical testimony proved that the poisons suspected could not have caused her death.

A case was tried in France in 1814. A young woman, near Montargis, died suddenly, with suspicious symptoms, and a large perforation being discovered in the stomach, six physicians who examined it, without considering an analysis necessary in a case so strongly marked, declared that she came to her death by the effects of some corrosive poison. Her husband and mother-in-law, against whom there was really no ground for suspicion, were thrown into prison and tried for their lives. A physician of the place, seeing the error of his professional brethren, endeavored in vain to induce them to reverse their opinion; but he induced the authorities to refer the case to the Faculty of Medicine of Paris. That body, with Chaussier at their head, gave an unanimous opinion that there was no proof of poisoning; and that the woman could have died of nothing else than spontaneous perforation.

Taylor* relates the following case, which recently came before himself

* Medical Jurisprudence, p. 58.

and Mr. Hilton for examination. "A female of noble family, aged 23, died suddenly under suspicious circumstances. One afternoon, about three hours after her last meal, she was suddenly seized with the most excruciating pain in the abdomen, and violent vomiting. Her skin was cold and clammy, and the abdomen tender and painful. It was suspected that she had taken poison, and magnesia and sulphate of magnesia were given to her. No poison was found in the room, and she strongly denied the imputation. The symptoms became worse, and she died the following morning, about fifteen hours after her first seizure. On inspection, all the organs were found healthy, except those of the abdomen. There were strong marks of peritoneal inflammation; the intestines were loosely adherent to each other, and a quantity of lymph was effused around them. The cavity contained about a pint of liquid which had escaped from an aperture in the stomach. The liquid was reserved for analysis. The stomach was laid open by making an incision along its greater curvature. It was empty. At the upper and posterior part, near the pyloric end of the smaller curvature, was an opening of an oval shape, about half an inch in its longest diameter. The edges were firm, hard and smooth, presenting not the least appearance of ulceration or laceration. They were bevelled off from within outwards, being thinned towards the peritoneal coat, the aperture in which, was much smaller than that in the mucous membrane. There was no sign of inflammation in membranes around; but the peritoneum, about the edge of the aperture, had a black appearance. At the lower part, near the large curvature, there were thick, irregular, black striæ, the mucous membrane being raised and blackened, but not softened. These striæ appeared like those produced by sulphuric acid; but there was no corrosion, and no acid reaction on test paper. The black matter was interspersed with a yellowish colored substance.

"The contents and coats of the stomach were analyzed, but no poison could be detected. All of the particulars of the case were considered, and the medical opinion given was: 1st., that the deceased had died from peritonitis, caused by extravasation of the contents of the stomach. 2d. That this extravasation was owing to a perforation of the coats of that organ, caused by slow and insidious disease, and not by poison."

The following case occurred in Louisiana, and the facts, with the stomach, its contents, and the liquid effused into the peritoneal cavity, were submitted to me for examination. A young lady, aged about 18 years, apparently in good health, was attacked a short time after dinner with excruciating pains in the abdomen, accompanied with severe retchings to vomit, but little, however, was thrown up by these efforts. The symptoms came on suddenly and increased in severity very rapidly. The pulse was variable, but small, the skin became cold and clammy, the abdomen swollen, tumid and tender to pressure; the face at first flushed, soon became pale, and the features collapsed, and she died about 12 or 14 hours after the accession of the attack. From the severity and rapid course of the disease, but little doubt was entertained that she was poisoned. Some trivial circumstance led to suspicion being attached to a negro woman, an old servant of the family, who was consequently taken up and sent to prison to await her trial. Suspicions of a different nature were entertained by some, who suggested suicide by poisoning. The examination of the body at once and satisfactorily negatived this latter idea, by showing the non-existence of the motive to which the act was attributed; but was regarded as decidedly confirmatory of the suspicion of poisoning. No trace of disease was discovered except in the abdomen. The stomach was found nearly empty, and its contents effused into the peritoneal cavity, which was intensely inflamed. The stomach was discovered to be perforated anteriorly near the middle of the greater curvature, by a nearly circular aperture larger than a dollar, the margins of which were rather even, and of tolerably firm consistence, and having a bevelled form in consequence of the inner coats being removed to a greater extent than the peritoneal covering. No other corroded point was discoverable in any part of the organ, nor was the mucous tissue softened, and showed no traces of high inflammation. The margins of the aperture were darkened or rather blackened, and black striæ or marks were observable in other parts of the organ. The fluid taken from the stomach and peritoneal cavity, were examined and tested by means of the ammoniated sulphate of copper, but without the requisite precaution of eliminating from the suspected liquid the organic matters, which were mixed with it. A green color, regarded as Scheele's green, being obtained by this test, the presence of arsenic was considered as demonstrated.

When the stomach, its contents, &c., were brought to me, my first impression was that no corrosive could produce such a condition except strong sulphuric acid, and this was not likely to limit its action to any one point, as was the case here. When the organic substances were moved from the fluid, no coloration was produced by the copper test, and the other tests, as well as Marsh's apparatus, were used without detecting a trace of the presence of arsenic or any other poison. A portion of the stomach was likewise submitted to a careful examination with a like want of success in discovering any poisonous substance.

The inevitable conclusions were: 1st. That the lady did not come to her death by poison, but by peritonitis resulting from the escape of substances from the stomach into that cavity. 2d. That the aperture by which this escape of the contents of the stomach took place, was produced by the perforation of the coats of that organ by insidious disease, or by some unknown cause.

The prisoner, unquestionably innocent of the crime for which she was to be tried for her life, was released without a formal trial; for most of those concerned were satisfied that she was not guilty.

This case is doubly illustrative—first, of the necessity of judging cautiously from symptoms and post-mortem appearances—and secondly, of the importance of using every precaution before using chemical tests, for the detection of poisonous substances, in criminal cases. It is hardly necessary here to remark that the appearance of the color produced by the ammoniated sulphate of copper might have resulted from the presence of many organic acids or their salts, and consequently no reliance could be placed on such a result.

Art. III.—*Success of the Cannabis Indica in the treatment of Tetanus. Report of a Case.* By W. G. G. Willson, M. D., of New Orleans.

To the Editors of the New Orleans Medical and Surgical Journal.

Gentlemen:—As the Indian Hemp has attracted much attention, since its introduction to professional notice by Dr. O'Shaugnessey, in the treatment of tetanus, I am induced to believe that, the following case of its successful employment will prove interesting to the readers of your Journal; more especially as (to the extent of my information on the subject,) it is the first instance reported of its complete success this side of India.

May 25, 1845, I was requested by Dr. Farrell to visit, with him, a negro man (belonging to Mr. Yancey), aged 27, and apparently of vigorous constitution. He stated, that he was attacked, two weeks ago, with stiffness and uneasiness in the back of his neck, consequent upon exposure to wet and inclement weather. We subsequently learned the fact of his having suffered, about two months previously, from a punctured wound of his foot, that suppurated, but healed in a short time without any untoward symptom. At the period of our visit, his condition was as follows, viz:

External System.—Expression of countenance was strikingly characteristic and peculiar; it seemed to depict acute suffering and anxiety, and yet he felt no pain. When analysed, it consisted in partial contraction of the orbicularis occuli—causing a half closure of the eyelids. The corrugator supercilii and levator muscles of the angles of the mouth were similarly affected. Skin was of good temperature, and inclined to perspire. Decubitus—on the side.

Nervous System.—He was not conscious of loss of strength; nor was there any deprivation of sensibility, either of the skin, head, or spinal column; muscles of the abdomen were contracted, and the ribs drawn downwards. The dorsal muscles were much more prominently affected. Muscles of mastication, and those of the neck generally, were subject to the same rigidity, but in a less degree. This condition was unattended with pain, unless excited by some effort of volition, which not only aggravated the constant tonic contraction, but also aroused clonic spasm in other muscles During the presence of these spasms, the skin along the anterior margin of the trapezius was always singularly corrugated.

Organic Functions.—1st *Digestive System.*—Deglutition could be accomplished with some little difficulty. The digestive organs were in good condition generally.

Respiratory System.—Articulation required more than ordinary effort, and readily excited spasm. Respiration was exclusively abdominal, and 22 per minute.

Circulatory System.—Pulse 70, full, compressible and regular.

Urino-genital System.—Diminished secretion of urine, with normal frequency of micturition.

The extract of cannabis indica was directed to be taken every two hours, in the dose of two grains combined with one tablespoonful of brandy; the effects of each dose to be carefully observed. The first dose was taken at 1 o'clock, p. m., and repeated at 2, and at 5 o'clock, without any apparent sign of its action, while the disease had obviously progressed, as was plainly evinced by the increased difficulty of swallowing; the greater duration and painfulness of spasm, and by the diminished power of volition over the limbs; requiring assistance to turn in his bed. Pulse had risen to 80. Respiration was still 22.

At 7 1-2 o'clock, I visited him again, in company with Dr. Farrell. Pulse was then 85; increased rigidity of dorsal and thoracic muscles, constituting well expressed opisthotonos; large beadlike drops of perspiration were standing on the surface, particularly about the face and hands.

An attempt was made at deglutition, but caused such violent and distressing spasm as to endanger suffocation, and rendered impracticable any further effort for its accomplishment. For the first time, the jaws presented the phenomenon of trismus.

It was promptly resolved to push the medicine to the extent of bringing the system speedily under its influence. ʒ i. of the tincture, with ℥ iv. of the infusion of flaxseed, was ordered as an enema—to be repeated every two hours. The tincture was prepared from the extract, and contained grs. v. to the fluid drachm. On visiting him again, at 10 p. m. (one hour after the administration of enema), I found him under the influence of medicine. Countenance was composed, and he appeared very drowsy. Pulse 90, full and regular—respiration easy, but entirely abdominal. He could open his mouth partially, and slightly protrude his tongue, without arousing spasm. Decubitus on back, which continued strongly arched.

May 26.—A repetition of the dose, last evening, at 12 o'clock, immediately procured sleep, which continued until 4 this morning, when another dose was administered, and caused him to doze interruptedly until 8 o'clock. He has taken, in all, twenty-eight grains of the extract. Pulse 86—respiration 20; urinary secretion, scanty; clonic spasms less frequent.

Visit at 11 o'clock, a. m.—He has recovered the power of deglutition sufficiently to take medicine *per orem*, though still difficult on account of rigidity. The same quantity of medicine to be given, combined with a tablespoonful of brandy. Bowels torpid. Prescribed ol. tiglii, gt. i.

Visit at 10, p. m.—He has dozed repeatedly during the day, with little interruption from clonic spasm; but the fixed contraction of the muscles of back and posterior of neck persists unremittingly. Croton oil did not operate; was repeated at 4 o'clock, and produced free purgation. Nourishment has been freely supplied him in the form of soup, which excited spasm at first, but after a few trials, he managed to swallow with comparative facility. Pulse reduced to 72—respiration 18. He is free from pain and disposed to sleep.

May 27.—Passed a good night—awaking at intervals of 3 or 4 hours, to take medicine, and soon relapsed into quiet sleep. 68 grs. of the extract have now been administered. Rigidity continues unmitigated; the clonic spasms are less frequent; bowels have been moved three times this morning. Secretion of urine more abundant.

Visit at 10, P. M. Drowsiness with occasional short naps, (as yesterday,) characterised his condition throughout the day. The muscles attached to the lower jaw appear somewhat relaxed, but the clonic contractions have become decidedly more frequent. The intellectual faculties continue dull—the effect of narcotism. Up to this time, neither aphrodism nor delirium have been occasioned—effects usually ascribed to the action of the cannabis. Pulse has risen to 92. Respiration 22. Pupils are somewhat dilated. The dose to be increased to ʒ ii. grs. x.

May 28, visit at 8 A. M. Soon after the exhibition of the first *increased dose* (last evening) he began to slumber; but was equally subject to clonic spasm. A second dose, two hours after, procured tranquil sleep at the same time that it reduced the action of the pulse to 84. He has consumed 133 grs. of the extract. At this time, the remedy more strongly manifests its sedative influence, the pulse being lowered to 72, and of good force and regularity. The symptoms seem to indicate a transition into what might be considered a second stage. The tonic contraction has decidedly abated, while the clonic spasms have alarmingly increased in number, though not in severity.

Visit at 10, P. M. Pulse 85. Respiration 22. Skin cool and perspiring profusely. Bowels so flatulent that every spasm occasions the expulsion of gas *per anum*. Croton oil was directed.

May 29—Visit at 8 A. M. 193 grs. have been taken up to this time. The customary dose was given at 10 o'clock last night, but as he seemed unusually restless at 12 o'clock, ʒ iii. xv grs. were exhibited in conjunction with 1½ table spoonsfull of brandy. He soon became sick and uneasy—cold perspiration broke out upon the surface. But in a short time sleep more refreshing than formerly supervened. The former dose was resumed at 2. He passed the night in dozing; frequently interrupted by spasm. Has slept better since 6 o'clock, and took soup and coffee with less difficulty than yesterday. There is neither increase of rigidity nor diminution of clonic contractions. Drowsiness not unlike stupor, marks the intervals of wakefulness. Pulse 72. Respiration 20. Skin warm and covered with drops of sweat. Croton oil did not operate. Bowels continue flatulent. Secretions of urine good. An enema of ol. ricini, tinct. assafœtida and ol. terebinth. was prescribed, and gave relief.

Visit at 10 P. M. Patient has slept better to-day, and seems to be doing well. Pulse 76.

May 30. A new feature in this case presented itself last night, viz. the secretion of viscid mucus in the air passages, and fauces, which is expectorated with great difficulty; sleep was less interrupted with clonic spasms; skin is warm, soft, and without the disposition to excessive perspiration. Pulse 68, full and regular; respiration 20, and partakes a little of the thoracic. Urine passed in good quantity. Rigidity about the same as yesterday. Increased the dose to ʒ ij ss. tinct. cannab. with two table-spoonfuls of brandy. Visit at 7 P.M., doing well; pulse 78.

May 31. Passed a good night; spasms and rigidity have both declined. Pulse 68, respiration 20. It has been remarked that the pulse is strikingly variable. Dr. Farrell saw him last evening with a pulse near 90 a short time after I had observed it at 78.

Visit at 8 o'clock P. M. Enema required to be repeated before it operated. Spasms have not increased, but pulse has risen to 82.

Visit at 10, P.M. Patient is in great distress, having been attacked with a violent convulsion about one hour ago; immediately followed by quick and hurried breathing, attended with a loose mucous rattle in the trachea, larger bronchia ; viscid saliva mixed with blood and mucus was discharged from his mouth, teeth are firmly set, back strongly arched, pulse small, irregular, and 115 per minute, while the heart's action can only be compared to a mere fluttering. I directed him to be turned on his side with his head dependant, upon which some fluid blood, flowed from his mouth; the posture gave some relief. Ordered tinct. cannab. ʒ iij, brandy 3 table-spoonfuls, to be given by injection.

In half an hour the pulse had fallen to 104, and in 15 minutes more, sleep succeeded; interrupted, however, by spasmodic jerks of the body and extremities, but of which he appeared unconscious.

June 1. Visit at 8 A M. He dozed intermittingly throughout the night frequently subject to short convulsive movements. Back is more arched, somewhat painful, and requires to be supported. Pulse 88. Pupils a little dilated. At intervals of about 2 minutes a feeble spasm attacks him, merely causing a faint protrusion of abdomen of scarcely 2 seconds duration.

Visit at 10 P.M. Pulse 84, respiration 22. Mucous ronchus of trachea. He can swallow well. Continue treatment.

June 2. Visit at 8 P.M. Had a natural stool; passed a large amount of urine. Pulse 68, full and regular, respiration 20 and less noisy. Diminished rigidity of muscles generally; clonic spasms only recur at intervals of 30 or 40 minutes.

Visit at 10 P.M. Slight increase of tracheal ronchus. As usual he has dozed the greater portion of the day, awaking regularly to take medicine.

June 3.—Improved—several hours have passed without any recurrence of convulsive movement. Relaxation of dorsal muscles still more palpable. Pulse 66; respiration continues noisy. Visit at 10, p. m. Doing well ; diarrhœa, unaccompanied with any symptom of distress.

June 4.—Speech much more distinct. He can open his mouth to half the extent of its natural limits, and could even protrude his tongue, but for the apprehension of exciting spasm. Increased relaxation of muscles. Clonic spasms no longer attack him, except when occasioned by repeated efforts of volition. He now possesses considerable command over limbs. Reduce dose to ʒ ii. every two hours.

June 5. The back is no longer arched; the dorsal muscles appear completely relaxed; a little rigidity still remains in the posterior cervical. He continues awake for periods of one or two hours; but there appears no disposition to a return of the spasms; he can get out of bed with trifling assistance and has recovered command over limbs; pulse 68, full, and of good strength.

June 6. Rested well last night, and is sensible of improvement. A short time after taking medicine he becomes drowsy as heretofore, but sleeps much less since the dose was reduced. Pulse 72, secretion of urine abundant; bowels loose; reduce to dose ʒ i.

June 7. Clonic spasms returned, (last night,) the dose was immediately increased to ʒ ii, and gave relief. He is now doing well.

June 14. Since the 7th inst., the period at which the last notes were taken, no remarkable symptoms have occurred. The transition of the muscular system from rigidity to complete relaxation progressed without interruption, with the exception of some stiffness in the back of the neck; while clonic spasms, after being reduced to one or two in the course of a day, ceased to be aroused even by an effort at voluntary movement.

On the whole, his condition has steadily improved. He can swallow perfectly, and has recovered so much strength as to be able to walk about his room and the adjoining gallery. The dose has been further diminished to ʒ ss; but at times it was temporarily augmented to combat any irregular symptom, and uniformly with the happiest success. The supply of hemp was exhausted yesterday, nor can more be procured.

June 19.—The only vestiges of disease now observable, are, the tetanic expression of countenance when the features are excited by any emotion; a disposition to perspire freely; and, an unwillingness to open the mouth to its full extent, for fear of causing a return of convulsions. For several days he has not only been in the habit of promenading his room, but of walking in the yard, &c. We considered the case cured, and directed all medicine (except occasional doses of brandy) to be discontinued.

June 20.—Was attacked with one or two convulsive twitches during the day, caused, no doubt, by exposure to humidity. Prescribed brandy.

June 23.—During the 21st and 22d instants, spasms became more frequent, and were associated with permanent contraction of posterior cervical and dorsal muscles. Dr. Farrell obtained a new supply of extract, cannab. indica, from a different source. But notwithstanding its very liberal administration, it failed to produce sleep until this morning.

June 29.—The condition described above gradually wore off. Occasional doses of hemp have been exhibited up to this period, in conjunction with brandy. Patient seems to repose more confidence in the latter, which was not the case in first attack. He has not had spasm for three days. Rigidity has disappeared, and he is able to walk about without any assistance. The case may fairly be considered as cured. In conclusion, I wish to remark upon one effect of the medicine, which I have omitted to notice; it is, a sense of most insatiable hunger, which made the patient desire food (uniformly) on awaking from the sleep occasioned by nearly every dose of the first stock of the Indian Hemp.

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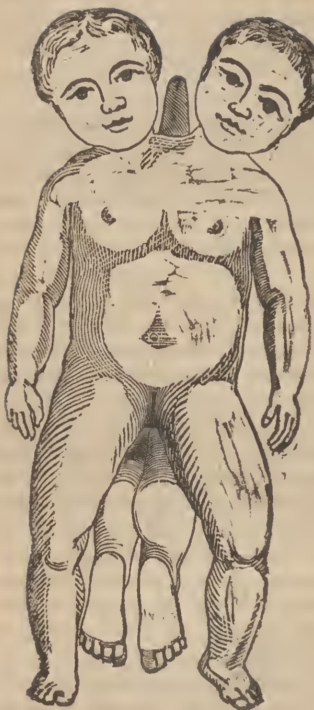
Postscript.—July 15. Since the above report was made out, the case has terminated fatally. The patient continued gradually to improve in strength, and without the least apprehension being entertained concerning his recovery, when on the 3d inst. he was attacked with measles (a case of which had appeared some days previously in the same house). The rash receded, the pulse became greatly accelerated, and immeately clonic spasms supervened, then gradually gave way to severe tonic spasms of the neck, abdomen, and back. The Indian hemp (of the new supply) was given in large doses, also brandy, morphine and quinine, without for a moment checking the progress of the disease. He died on the morning of the 12th, from spasm of the glottis, while attempting to swallow. Two years ago I treated a case of traumatic tetanus with the



ERRATA.

Page 161, 19th line from bottom, for "arm," read *anus.*
Same page, 22d line from bottom, for "arms," read *anus.*

cannabis indica; on the fourth day the symptoms were much alleviated even in a more marked degree than in the preceding cure; unfortunately the supply of the medicine became exhausted, and none could be procured; the case went rapidly on to a fatal termination, dying in 38 hours from the suspension of the remedy. It is necessary to remark, that the extract first used was procured from Mr. Squire, Oxford street, London. A fresh supply was afterwards obtained from a different source, but I am disposed to doubt its goodness, as we could perceive no sign of its action, though given in very large doses, (ʒ vi. of the tinct. within the hour,) without its causing the least disposition to sleep. On the whole I do not think that the termination should in the least disparage the efficacy which the remedy so strongly manifested on the first attack. The patient died on the 47th day. J. FARRELL, M. D.

ART. IV.—*Description of a Double Monster.* By A. J. WEDDERBURN, M. D., *Professor of Anatomy in the Medical College of Louisiana.*

By reference to the plate it will be seen that this Monster has two heads, and necks distinct; a very large body, two perfect arms, and one in a rudimentary state, situated at the point of union between the two necks and the body. There are four legs, the two anterior well developed, and having the appearance of being connected with a single pelvis—the other two present themselves in an opposite direction, and are smaller, seeming to have a connexion with the common trunk, as it were by implantation. The umbilical cord has four arteries and one vein. The vulva is single, placed, as may be seen in the plate, between the anterior legs in a vertical position, and has within it two vaginæ, and two urethræ, which have a direction, respectively, of forty-five degrees from the median line of the fused bodies. The arms of each individual is placed immediately upon the line of direction of the vaginæ, so that when the anterior and posterior legs are placed at right angles with the body, a transverse line will strike each arm, and form, with lines drawn from the vulva in the direction of the same, a triangle which would be divided by the median line into two right angled-triangles.

Mode of union by the Skeletons. In order to give an accurate idea of the fusion of the two skeletons, let us imagine a plane drawn from the anterior border of the glenoid cavity of the scapula, so as to cut through the centre of the acetabulum, and intersect the median plane of the body at right angles; this lateral plane drawn on the left side of the right fœtus, and right side of the left, will remove a portion of each skeleton corresponding to an arc of 90 degrees. Then, (the parts being spread,) bring in contact the lateral cut surfaces with each other, and the anterior being approximated, we will find the union effected after the following manner: Posteriorly, the scapulæ united by the fusion of their glenoid cavities; the ribs, of the approximated sides, which have been shortened, (in consequence of the removal of a portion by the two vertical planes described), united in such a manner as to form a posterior median line for the common body; and below, the union is affected by the iliac bones. Anteriorly, we have the left half of the sternum of the left fœtus, united with the right half of the sternum of the right fœtus; whilst, again, we have the pelvic cavity completed anteriorily by the union of the pubic bones of the two bodies forming a perfect symphysis.*

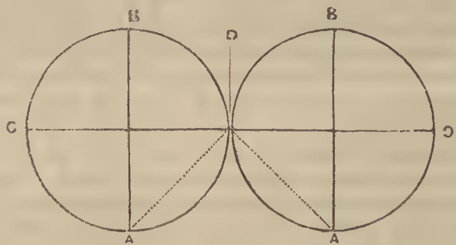
The rudimentary arm has but one bone, the humerus, the head o which articulates with a cavity formed equally by the union of the scapulæ.

The bones of the lower extremities which correspond with the two well developed arms, are also in perfect proportion. The two posterior legs are somewhat less in size than the anterior, the deficiency existing principally in the anterior parts of the thighs; the bones being perfectly formed, with the exception of those of the thighs, the heads of which received in a common acetabulum, formed by the fusion of the iliac bones, as before mentioned, are flattened at their contiguous surfaces.

Placed upon the median line, and standing out at a right line from it, we have a bone bifurcated at its posterior part, so as to have a connexion with both scapulæ. The anterior and free part of this bone is just such as we might imagine to be formed by the fusion of such a portion of two clavicles as would be left, after removing a part of each, indicated by a line drawn from the anterior border of the glenoid cavity to the posterior part of the sternum.

Examination of the Thorax.—On opening the chest, the heart was found to be single, composed of two cavities, one auricle, and one ventricle, contained in a large pericardium, common to both bodies. Posterior to the pericardium, a mediastinum, connecting the same with the point of union between the ribs, and anterior, a mediastinum, having a normal appearance, establishes a perfect division between the two chests.

FIG. 1.



*Let Fig. 1 represent the skeletons of two bodies placed side by side;

A. A. the sternum in each;

B. B. the spine;

C. C. the glenoid cavities of the opposite sides of the two bodies;

D. the glenoid cavities of the approximated sides of the same;

The two dotted lines represent the loss in the fusion.

FIG. 2.

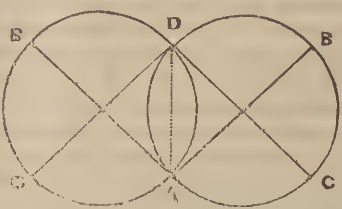


Fig. 2 represents the fused skeletons of the two bodies.

A, the point of union between the right half of the sternum of the right fœtus and the left half of the left;

B. B. the position of the two spines;

C. C. the glenoid cavities of the opposite sides of the two fused bodies;

D. The point of fusion between the glenoid cavities of the approximated sides.

Fig. 2, will also represent the union at the pelvis. A, the vulva, and the symphysis formed by the right pubic bone of the right fœtus, and left, of the left; B. B. the two sacra; C. C., the cotyloid cavities with which the anterior legs are articulated; D., the union of the iliac bones, and of the acetabula forming a cavity common to the heads of the two posterior femora.

The lines A B, will represent the direction of the vaginæ as given in the description.

Lungs.—Right side of right chest, lung with four lobes ; left side two. Left chest, both lungs, two lobes.

Thymus Glands, two—one for each chest.

Diaphragm, very large, with a double set of foramina. On the right side we have the aortic. the œsophagial, and the foramen for the transmission of the vena cava inferior, which receives the blood from the umbilical vein, from the hepatic veins and from the abdominal vena cava,* which winds around the right side of the liver and empties into the cava just as it is penetrating the diaphragm. On the left side we have the aorta and vena cava penetrating the diaphragm near the same point, the vein being on the left.

The œsophagus on the right side passes down in front of the thoracic aorta, which it crosses obliquely at its upper part from right to left, when it penetrates the diaphragm posterior to the foramen transmitting the vena cava. On the left side the œsophagus is situated on the right side of the aorta, which vessel lies between it and the ascending vena cava.

Abdomen.—Liver occupying the entire front of the abdomen at its upper part. Two spleens in their normal position, that is, resting upon the diaphragm on the left side of each spine respectively. The whole of the abdominal as well as the pelvic viscera, with the exception of the liver, are double.

About two or three inches from the pylorus of each stomach, the small intestines, by their corresponding parts, are joined together, and although there are two perfectly distinct canals, they are so completely united throughout their whole course by a short cellular tissue, that they have very much the appearance of but one canal, fringing the free margins of two mesenteries, which are again united after the same manner. Though the union between the two canals and two mesenteries, is quite firm, they can still be separated with a little force.

Particular examination of some of the thoracic, abdominal and pelvic viscera. HEART, as stated before, having one auricle and one ventricle: the latter, somewhat crescentic in shape, resting upon one portion of the auricle which is behind it, whilst the other portion is situated above, surmounting the ventricle in front, and having an auricular appendix hanging from its centre. The auricle receives the blood at its superior part from two vessels about an inch apart, which communicate with the jugular veins of the contiguous sides of the heads. The one on the right side receiving a trunk common to the right arm and head of the same side, and also that of an azygos vein, which is situated on the left side of the thoracic spine. By raising the apex of the heart, we find as before stated, the inferior cava emptying into the inferior part of the auricle. The ventricle on the right side gives off one vessel, from which the pulmonary artery arises at a short distance from the ventricle. On the left side we have two vessels arising which communicate at the arch. The vessel which is the right of these two, after getting as far as the arch, gives

*I have made a distinction here between the abdominal vena cava, and the inferior vena cava of the right fœtus for the sake of description, as the abdominal cava does not hold its normal relation to the liver, but taking the course described above, unites with the vessel receiving the hepatic and placental blood above the liver at right angles.

off branches to the upper part of the body and head, and is lost by its branch of communication with the left, which vessel, at the arch, sends off the pulmonary artery, and then approaches the spine, where we find it between the œsophagus and a thoracic vena cava, which vein taking the course of the aorta, having also a similar arch, receives, just before it empties into the auricle, (behind the two vessels just described,) a vein bringing the blood from the left arm and same side of the corresponding head.

On opening the heart we find the following arrangement. The large auricular portion (corresponding with the [appendix auriculaœ), in front and above, is separated from the posterior and inferior portions of the auricle, by a partial septum, extending from right to left, which has in its centre an oval foramen. The free margin of this septum has an oblique direction from its point of attachment at the posterior-inferior lateral part of the right side, to the anterior and superior lateral part of the left. Behind this septum, then, we find at the upper part of the cavity of the auricle, the openings of the two veins mentioned above. Anterior to the septum below, we have on the right side two openings common to all the veins of the right set of lungs, whilst at the opposite side, there is but one opening for the blood of the left lungs, and above this, we have a large opening, (furnished with a valve), which is the mouth of the vena cava of the left fœtus, before described. Excepting, then, several small openings for the coronary veins, we have but one more, which is immediately in the centre of the most inferior part of this cavity, that of the inferior cava of the right fœtus. The auricular and ventricular cavities, communicate by a single ostium venosum, immediately in the centre, and the peculiar arrangement of the valves, with their columnæ carneæ, gives the cavity of the ventricle the appearance of being divided into three chambers, the two lateral communicating with the central one by large and irregular formed foramina.

Liver.—On first opening the abdomen, this organ presented the appearance of being composed of two, but from a more careful examination, we find the following to be its condition. As seen at first, it is the one belonging exclusively to the right fœtus, with its proper lobes, but somewhat modified in its external appearance, measuring about four inches in its lateral diameter, and about two in its antero-posterior. On the under part of this liver we find that which was the right portion of the one belonging to the left fœtus, blended with the one above, in such a manner as to have the portal vessels distributed to each from a single vena potarum, which is common to the venous systems of the two sets of organs, contained in the common abdominal cavity. This portion of the left liver may be designated by a vertical line drawn through it, immediately upon the right border of the lobus caudatus, which would leave this only portion of the left liver existing without a gall-bladder, so that in the fusion of one entire liver which is superimposed upon only by a small portion of another, we have but one gall-bladder.

The Uteri are bi-horned, having a Fallopian tube and an ovarium for each horn, the anterior of each uterus resting against the point of union between the pubic and iliac bones, and the posterior concealed by the rectum on each side as it crosses it in reaching its normal position, behind the body of the uterus.

The Bladders, (each having its proper urachus, which are situated side by side), are joined at the median line of the common body, by cellular tissue connecting the approximated surfaces of their muscular coats. The peritoneal covering common to both, is reflected over their superior, posterior and external lateral surfaces, in such a manner as to have presented before the dissection the appearance of but one bladder.

Nervous systems. Perfect in both, excepting only the loss in their peripheral portions which correspond with the parts lost in the fusion of the two bodies. The rudimentary arm derives its nerves from the axillary plexus of the two sides approximated, which meet in front of the symphysis formed by the union of the two glenoid cavities, and matted firmly together in a mass of a quadrilateral shape, from which we have, taking a vertical direction downwards, a single nerve, about a line in breadth, which afterwards passes backwards under the united surfaces of the two bones, when it gets into the axilla, and is divided into a number of branches.

The monster just described, will at once be recognized as belonging, according to Breschet and others, to the order Diplogenesis (διπλοος, double, γενεσις, generation). This order is divided by M. Mayer into three groups; 1st, monstruosity by inclusion: 2d, monstruosity by implantation; 3d, monstruosity by juxta-position. This third class has also its subdivisions; first, that in which we have a mono-cephalous monster with two bodies; second, including those which are bi-cephalic with a single body; and third (the one to which our monster belongs), that of diplosomia, in which there are two heads and two bodies more or less fused. Under the head of diplosomia (διπλοοσ, double, σωμα, body,) in the Dictionnaire de Médecine, the author, after having pointed out various methods by which we find this union affected, says: "When the union takes place in the trunk, it is either by the anterior faces, or lateral faces of the thorax, or that of the abdomen; sometimes it is by the dorsal, sacral, or gluteal regions that the two subjects are united. In these different cases, the connexion may be superficial, or deep, according as it is established by the skin, or bones; or the point of junction between the two vis-ceral cavities, apparently isolated, form, on the contrary, but one, whilst there may exist, or not, a beginning of fusion of some of the organs of the two fœtuses. It is thus that, in the union of the anterior part of the chests, the sternum is wanting: the two chests communicate, and the organs which are enclosed present these numerous anomalies. Sometimes two hearts are found well formed, distinct, and separated, having their own pericardium, or enclosed in one envelope, and then they may be united by some part of their external face; sometimes the two hearts have but an imperfect development. In some cases there is but one, the structure of which is more or less irregular, and which seems to be formed by the fusion of the two hearts. There are then a great many varieties in the origin of the principal vessels. A heart regularly formed, has also been seen furnishing a double number of vessels—the right ventricle, two pulmonary arteries—the left ventricle, two aortæ, &c. Sometimes, on the contrary, the heart, placed in the centre of the only cavity formed by the union of the two chests, is composed of but a single auricle, and a single ventricle—the pulmonary arteries coming from the aortæ which divides at some distance from its origin (Meckel). In the union by the abdomen, the digestive apparatus may be simple in a certain part of its extent. The liver is sometimes single, but much larger than usual; or, it is composed of many lobes, provided with two gall bladders. When the junction of the two subjects takes place at the inferior part of the abdomen, there is a fusion, more or less complete, of some of the organs situated in the pelvis. The anus may be single, the genital organs simple. The bones of the pelvis may be then increased in number, or, instead of supernumerary pieces of bone, we scarcely find traces of the pelvis; in this case, the inferior extremities are wanting, or consist only of irregular appendices, fixed at the termination of the spine, by means of imperfectly formed osseous productions. Ollivier, the author of this article, in making some allusion to the different theories that have been advanced on the subject of the development of this class of monsters, says, 'I have mentioned before, that when the union of the two subjects is but superficial, this connexion takes place only through the medium of the skin, and of the fusion of some of the osseous parts of the skeleton. It is rare that the means of union is effected only by the skin. Swingler mentions a remarkable case of it, where the union of two little girls had taken place above the umbilicus, and confined to a small surface; their separation was effected with success by the knife; but such a case must be placed amongst the exceptions.'

"When the diplosomia is so perfect, there is no doubt that it depends upon the fusion, more or less complete, of the two germs, simultaneously fecundated, and enclosed in the same ovum. It is possible also, as I have mentioned, in speaking of the heteradelphia, that the fusion of two umbilical cords, or the presence of a single cord, favors and even determines the adhesion of the two embrio, in the case where their union has taken place by the abdomen. But when the diplosomia is not complete, when this dichatomia of the body is of little extent, and especially when there is no multiplication of some of the parts, is the monstruosity also the result of the fusion of the two germs? The fact is at least doubtful in many of the cases, and is not even probable when there is but an augmentation in the number of some of the organs.

"According to Meckel, the natural duplication of some parts or organs, as that of the limbs, causes by insensible degrees that of the whole body. This distinguished anatomist, views in all the double monstruosities different series, of which some begin by the duplication of the fingers, others by the heart, and ultimately end by the duplication of the entire body. But it is sufficient to recollect, that, in some cases, the heart has been found simple; that is, composed of only two cavities in double monsters, and in some others, a double heart without traces of duplication existing in other parts of the body, to prove how unfounded is the opinion of Meckel. It is the same with the duplication of the fingers, or of the limbs; examples sufficiently numerous demonstrate that it may exist without any trace of duplication in the organs.

"Beclard, and after him Tiedemann, were induced, by the examination of a good number of facts, to conclude that some monstruosities result from the absence, or imperfect development of the nervous centres. According to the German anatomist, the nervous system, pre-existing, regulates the formation and the ultimate development of the embrio, as well as the particular form, and the disposition of its organs; from which it results that the absence of a portion, more or less extensive, of the nervous and cephalo-rachidian centre, determines that of a corresponding part of the trunk, and of the limbs, (Beclard); whilst an augmentation of the development of the same parts of the nervous system, causes the development of supernumerary parts. M. Lauth adopts this opinion, upon which he founds his explanation of diplosomia. It is in the primitive duplication of the cerebro-spinal system that we find the first degrees of the duplication of the entire body. This duplication of the nervous centre always commences by one of its extremities, and is always lateral. If it takes place at the cephalic extremity, the duplication begins by the anterior part of the central lobes, and in that case there are two faces placed one by the side of the other. If the duplication commences in the lumbar extremity, the rachis is double inferiorly, and there are two vertebral columns which are connected by the sides. Starting out from these two points, we can establish two, almost, continuous series of anomalies, which conduct us equally to the highest degree of duplication.

"But here objections present themselves, analogous to those which I have already made to the explanation of M. Serres for the hetarodelphia. If all the monsters which belong to our third class of diplogenesis, had their origin in this primitive and normal division of the cerebro-spinal centre, the supernumerary parts would be always united in the same way, that is to say, laterally, and could not be in any other way; the contrary is proved by well authenticated cases. Moreover, if the primary division of the spinal marrow truly induced duplication of the whole body, was the primary and determining cause of it, it would be necessary, in order that the consequence should be in direct relation with this primary cause, according to Tiedemann, that the double monster should be composed of two halves of a single individual, and not, as is the case, of two individuals more or less complete, since each lateral cord of the spinal marrow constitutes only half of the organ, and not an entire organ. However, I am far from denying the participation which the nervous system may take, in the formation of monstruosities; especially when I consider that, in the diplogenesis, and especially in those of the second and third class, there exists always a notable regularity, a symmetry in the anormal structure of these monstruosities. And if we reflect on this frequent appearance, on this recurrence of the same aberrations, remarkable by the fixedness of their characters, and which seem to reproduce forms as definite as all those of regular beings, as M. Geoffroy Saint-Hilaire has very judiciously pointed out, we are induced to think that these monstruosities are developed under the influence of some fundamental organic condition, which controls in some degree the mode of their formation. Thus, according to the preceding considerations, it is very probable that this organic condition resides in the cerebro-spinal system."

"The writer of the above concludes his chapter by stating the following fact: first, that in all the Double Monsters, there is constantly identity of sex in the two individuals united. He refers to an example reported by M. Hénot de Mentz, which is an exception to this rule, but doubts the conclusiveness of the facts in this, as he does in other cases which have been cited. In the second place, much the largest proportion of these monsters are of the female sex. In forty-two observations by Haller, there were thirty double monsters of the feminine sex, nine of the masculine, two hermaphrodites, and one not belonging to any distinct sex:"

I have given the translation of the foregoing article in consequence of its setting forth, in a very concise manner, the different theories concerning the development of this class of monstruosities, and also from the fact that we have a sufficient number of anomalous conditions cited, to enable us to draw a comparison with the structure of the case described in the first part of this paper.

The classification given by M. Ollivier, which embraces the monster in question, under the head diplosomia, seems to be imperfect, inasmuch as the nomenclature does not convey an idea of the mode of union further than that we have a monster with two heads and two bodies. In the "Histoire des Anomalies de l'organisation par M. Isidore Geoffroy Saint Hilaire," we find the different varieties to which this double monster belongs under the class of monomphaliens.* This class he divides into five genera, first, *Ischiopage*, when the union takes place at the hypogastric region; second, *Xiphopage*, when the union takes place at the lower part of the sternum; third, *Sternopage* at the sternum; fourth, *Ectopage*, by the entire side of the chest; and fifth, *Hemipage*, where the chests and necks are fused. The monster just described then, it will be perceived, has no particular name by which its mode of union can be designated, but it is embraced in the four first genera, and might, according to this classification, be termed an *Ecto-Ischiopage*, as there could not be union by the pelvis and chest, without involving the intermediate parts, which fact would make the term sufficiently expressive.

This monster, it will be noticed by refercnce to the translation from the Dictionnaire de Médecine, presents many interesting points for consideration, with those who pay attention to such subjects. In all the subjects which have been examined before, when two livers have been found fused, or when the liver has been compound in its structure, there have been invariably two gall-bladders, but in this case, we have one liver perfectly formed, superimposed on, and united with a portion of another, with but one gall-bladder, and that occupying its normal position. The heart differs somewhat from those of the same kind which have been described before, and the perfect union, by the entire sides, embracing as it does so many varieties, is calculated to throw considerable light upon the mode of developement in such beings.

This double fœtus was sent to Dr. Stone, Professor of Surgery in the Medical College of Louisiana, by Dr. C. H. Stone of Woodville, Mississippi. It is to be regretted that there is no history of its birth, as the latter gentleman states that it fell into his hands by accident. It is a negro; born no doubt at the full period of gestation, and probably, from its appearance, within the last twelve months.

Art. V.—*Observations on the Treatment of Wounded Arteries, with Cases.* By Warren Stone, M. D., *Professor of Surgery in the Medical College of Louisiana.*

The operative Surgery of the Arteries may be considered complete, and yet but little is said of the minute treatment, and management of wounded arteries. There is no more merit in casting a ligature around

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*μονος one, ομφαλος umbilicus.

an artery than belongs to the skilful mechanic; but there *is* merit in knowing when, and when not, to use the knife. There are cases, when by avoiding a painful operation, life may be saved; on the contrary, by temporising, and allowing repeated hæmorrhages, even from minor arteries, life may be sacrificed. It is not necessary to enter into the pathology of wounded arteries, for it is sufficiently understood. I shall speak of it in connexion with the treatment of particular cases.

When an artery of considerable size is divided, it is our duty to tie it at once, provided the wound is open, and the vessel accessible without an operation, but if this is not the case, and the bleeding can be easily controlled by compression, we are justified in attempting a cure by this means. The usual mode of making compression, in such cases, is to apply a large compress over the wound, and confine it with a bandage, enveloping the whole limb, which serves to prevent the escape of blood externally, but does not prevent its escape into the tissues around the divided vessel, and which, while it may favor the formation of a clot in the artery, tends at the same time to prevent that adhesive inflammation upon which we depend for a cure. Compression should be made immediately over the mouth of the divided vessel, (by whatever means may be convenient), and kept steadily applied, until a coagulum forms in the artery, which will take place in a short time, precisely as if a ligature was applied, provided the compression be complete; but if an occasional escape of blood is allowed, it will break up the forming clot. The length of time required for the formation of the clot, varies according to the size of the vessel. In small vessels, a slight coagulum around the mouth is sufficient to arrest the bleeding, and this may form in a few minutes; but in large vessels, it is necessary for the clot to form in the artery, (which we know will take place, if the bleeding mouth be kept closed), as far back as the first collateral branch. In a large artery, I think it requires from two to three hours for the perfect formation of the clot. When this is accomplished, the compression should be removed or lessened, the wound, if possible cleared of blood, the parts brought accurately together, and no more pressure made, than can be allowed without interrupting the process of union, or the deposition and organization of lymph around the mouth of the vessel. Too strong compression prevents the natural adhesive process, and rather favors secondary bleeding. If the wound presents an unhealthy, sloughy appearance, and secondary hæmorrhage occurs, it is useless to attempt a cure by compression, even in minor arteries; for repeated hæmorrhages will follow, until the patient is exhausted. If, however, the wound is healthy and granulating, slight compression may be made, just sufficient to prevent any injurious loss of blood; the granulations will close the vessel. If compression is strong, absorption of the granulations is effected, rendering the case worse, without affording any additional security against bleeding, which will occur as often as the clot dissolves, and continue until another forms. A light graduated compress of soft dry lint, with light pressure will, so long as the compress remains dry, prevent bleeding in large sized arteries; therefore, the compress should be removed as often as it becomes saturated with the discharge.

CASE 1ST.—*Showing that large arteries will unite without ligature.*—Mr. H., a robust man, in the prime of life, received a gun-shot wound, in

November, 1843, which divided the left femoral artery, at, or above the profunda. The ball entered the anterior part of the right thigh, just below the spine of the ilium, passed through, entered the scrotum anterior to the right spermatic cord, and passing behind the left, came through; entered the left groin, and came out just below, and anterior to, the trochanter, dividing in its course the femoral artery and vein, and producing serious injury to the crural nerve. I was but a few paces from him at the time, and immediately made pressure with one hand, and assisted him in lying down with the other; but, in the act of lying down, he was seized with faintness, followed by convulsions, which were produced more by the shock the nervous system received, through the injury of the crural nerve, than by the loss of blood. He gradually resuscitated, and when sensibility returned, he experienced an almost intolerable pain in the course of the nerve, more particularly at the lower extremity. This pain, he described as similar to what he felt in the hand, upon contusion of the ulnar nerve, though much more severe. The bleeding was easily and completely controlled by pressure in the track of the wound, immediately over the mouth of the divided vessel; and as he was suffering as much as humanity could bear, the application of the ligature—which of course was deemed necessary—was deferred. Laudanum and brandy were administered, half an ounce of the former and half a pint of the latter, in the course of two or three hours, which barely sufficed to render his pains tolerable. At this time, it was found that the artery was perfectly closed by a coagulum, and as the vehicle had arrived to convey him to his lodgings, (the distance of three or four miles,) I concluded, with the concurrence of Dr. Harrison, to allow him to be moved before tying the artery. Dr. Harrison accompanied him, to make pressure, should it be necessary. No bleeding, however, was produced by the removal. He was still suffering as much as he could well bear, and feeling confident that if adhesive inflammation took place healthily, the artery would close, I concluded to leave it to nature, for the time. A few friends were selected to stay by his side, by turns, with instructions in case of bleeding. Simple water dressings were applied to the wounds. No bleeding occurred, and the wounds healed with little annoyance. The main difficulty was in preserving the limb. Dry heat, frictions, and finally the gentle application of electro-magnetism, were employed, and with evident benefit. Apparently, no circulation existed in the limb; the blood seemed to penetrate the tissues, and on the third day made its way as far as the instep, where it ceased its course. Great difficulty was experienced, also, in effecting the return of the blood, in consequence of the wound of the femoral vein. The injury of the nerve, no doubt, operated upon the nutritive action in the limb. The pain gradually subsided, and sensation, and nutrition are now restored. The foot, of course, sloughed from the point where nutrition ceased, which was at the junction of the tarsal and metatarsal bones. It may be said that this being a gun-shot wound of the artery, it united, which would not have been the case had the wound been an incised one. I admit, that hæmorrhage is more easily arrested in gun-shot wounds, but secondary hæmorrhage is more likely to occur; for the reason, that lymph is not so likely to be thrown out and organized around the mouth of a vessel divided by a ball, as when divided by incision.

CASE 2d —Mr. H. the subject of the former case, received a gun-shot wound in the head, several years since. The ball entered just below the left eye, passed through the antrum, fractured the palate bone, and pterygoid process of the sphenoid, and probably struck against the spine, just below the cuneiform process of the occiput, and fell into the fauces. Mr. H. fell senseless, from the concussion; profuse hæmorrhage followed, but ceased with the syncope. In this state he was conveyed to his room, as dead; he, however, gradually resuscitated, and no further bleeding occurred for the time. Simple applications, I believe, were made to the wound, and very light nourishment allowed. The wound did well, and he gradually rallied until the seventh day, when his friends carried him on board of a boat at Natchez (where the accident occurred), for the purpose of bringing him to New Orleans. From the excitement of moving, or some other cause, an alarming hæmorrhage took place shortly after leaving Natchez, which continued, in spite of every effort made by the physician that accompanied him, until syncope ensued, and another coagulum formed. This secondary clot sufficed to prevent bleeding for 18 or 20 hours, when it either dissolved, or arterial reaction came on, and forced it away. Another bleeding ensued, and terminated in the same manner. The boat was detained, and a third hæmorrhage took place before he arrived in New Orleans. I saw him soon after his arrival, and found him with a pale cadaverous countenance, pulse 140 in a minute, and barely perceptible. It was certain that, in due time, another hæmorrhage would occur, and I, therefore, proposed to tie the carotid artery at once. This was objected to, on the ground that it was too late, and would only add to his sufferings. He, however, rallied under the use of a little ale, and broth, when the point was yielded, and I threw a ligature around the common carotid, by candle-light Some difficulty was experienced; first, from the difficulty of throwing light down into a deep wound, (the patient had a short, thick, muscular neck); and, secondly, from the irregular (though not unfrequent) distribution of the superficial veins. The external jugular and superior thyroid veins united in one trunk, and dipping down, emptied into the internal jugular, crossing the artery exactly at the point where I wished to pass the ligature. I succeeded, however, in opening the common sheath by means of two pairs of forceps. The sheath was seized with one pair, and raised; while with the other, it was seized as near the first as possible, and an opening made in it. By careful management, he gradually recovered without any unpleasant symptom. It is impossible to say what artery was wounded in this case. From the violence of the bleeding, the physician that accompanied him thought it was the internal carotid, but it may have been only the internal maxillary.

This case shows the folly of attempting to arrest secondary bleeding by pressure or plugs. A secondary coagulum in an artery, I believe, never does become organized, but merely obeys physical laws. This case occurred in the same individual, in which the femoral artery united so kindly; and at a time, too, when his system was in a more favorable condition. It is probable, that if from the beginning perfect rest had been maintained, hæmorrhage would not have occurred. The position of the wound, too, was unfavorable, for from the scantiness of soft parts, and from their being held asunder by the surrounding bony structure, it is probable that no lymph was thrown out around the mouth of the vessel; and the whole resistance, to the heart's action, was in the clot in the artery.

Art. VI.—*Remarks on the Treatment of Fevers, accompanying the Quarterly Report made at Fort Gamble, Florida, Sept.*, 1841. By Charles McCormick, Assistant Surgeon, U. S. Army.

Of 167 cases of fever reported, 16 were of the remittent, 119 of the quotidean, and 32 of the tertian type. From this it appears that in 135 of the cases there were daily paroxysms of fever, and I think the strongest peculiarity of character these cases have presented this season, has been the strong tendency they have had to run into, and assume, the remittant type. In fact in many of the cases, it has been extremely difficult to draw a distinct and plain line of demarkation, to say where the one ended, and the other commenced. Some two or three cases assumed the form known as congestive fever.

The three stations appear to have been judiciously located, and are probably as healthy as could have been selected in the neighborhood, with due regard to their military importance. The only causes which operate in producing disease, at these stations, (excepting intemperance alone,) may be considered as general to the territory, such as malarial exhalations, atmospheric vicissitudes, and the exposure to which the troops are constantly subject, day and night.

In regard to the treatment of the fevers I have met with in Florida, arising from these causes, my opinion has undergone a very important change within the last two years, and every day's experience during this period, has tended, in the most ample and satisfactory manner, to convince me that this change has not only been important, but that the practice founded upon it has been judicious and successful in the highest degree. In relation to the intermittent forms of fever, the change was more in the mode of administering, and in the quantities of the remedies given, than in the remedial agents themselves. Some two years since, I was so unsuccessful in arresting the paroxysms of intermittent fever with the sulphate of quinine given in two grain doses every hour, although during the apyrexia as much as 12, 18, or 24 grains had been given, that I laid it by in despair, and resorted to sedative and relaxants, such as opium, camphor, tart. antim. and ipecac pulv. with much success. Still, however, I was not satisfied, and the great reputation the Peruvian bark had so long enjoyed, created doubts as to the propriety of abandoning its use. Soon, therefore, I determined to give it another trial in larger doses; and with this view, I commenced three or four hours before the expected paroxysm, and gave from four to six grains every hour, until it produced its peculiar effect upon the brain—ringing and buzzing in the ears, a sense of stricture across the forehead, and temporary deafness—effects invariably produced in every case where three or four such doses had been given; from this time forward I was constantly successful, nor do I now remember a case in which it failed, when these peculiar effects which it displays on the nervous system were produced. Finding, then, that the enlarged doses had such happy results, I was induced in many cases where the apyrexia was short, to give it in single doses of from 10 to 15, or 20 grains, according to the violence of the disease. Here, then, I saw cases of intermittent fevers that could not be arrested by 15 or 20 grains of the sulphate of quinine, (in fact as I before stated, it daily failed,) given in small and divided doses—yet yielding immediately to the same quantity given by large doses, in a much shorter interval. Again, the small doses seemed, when frequently repeated, in many cases to prove stimulating—flushing the face, producing mental excitement, and head ache; scarcely one of the patients to whom small doses had been given, complained of the ringing, buzzing, or deafness, which was constantly complained of, by those to whom the large doses had been given. There would then appear to be great difference in the effects produced by small and frequently repeated doses of quinine on the system, and those impressions made by large and full doses. The action of opium furnishes what I consider as a close analogy, acting as a stimulant in small doses, and in large ones as a sedative. The analogy holds further; I do not believe the severest pain can modify more strongly the action of opium, and thus render doses beneficial that would under ordinary conditions of the system prove fatal, than that the violence of the fever can, and does modify the action of quinine; thus, during severe and highly dangerous fever, render safe, beneficial and sanatary, a quantity that in ordinary health could not be tolerated. I have given upwards of 300 grains in much less than 24 hours, in an extreme case of congestive fever, that was ushered in by insensibility, speechlessness, inability to swallow, the power of deglutition being entirely lost, cold extremities and surface, pulse feeble and nearly extinct, in fact it was not to be felt; here the first effort to administer the quinine was made, by throwing fifty grains into the rectum. In this case the only effect of the quinine, (which was repeated after the pulse rose, and the surface and extremities became hot in 20 grain doses every hour, for 12 or 13 hours,) which I could perceive was to throw out upon the surface, generally, a warm, free perspiration, and to reduce the pulse from 100 and upwards, to 80 per minute, rendering it soft and compressible, and most undoubtedly, by allaying the fever, enabled the calomel which had been given every hour with the quinine in ten grain doses, to exert its full effect on the secretions; free alvine evacuations were procured, and a speedy and complete recovery followed in a few days.

I am satisfied that the best mode of administering the sulphate of quinine, is to give from 10 to 15 grains as soon as the paroxysm is over; this mode I have found successful. If given before or after the paroxysm, it appears at times to fail; for the paroxysm returns, sometimes without, and at others with a struggle, as the patient has described it to me, between the medicine and the disease. In many cases, it would appear that the disease conquers, and the paroxysm returns; but on examining the pulse, it will be found more bounding, yet softer and more easily compressed. Although the paroxysm has thus returned, it is much moderated and changed in its character, and with, I think, but three exceptions in more than two hundred cases, it has always proved the last paroxysm; the disease having yielded. But to ensure us against such returns, I am confident 15 grains of quinine, given at the end of the paroxysm, and followed by doses of five grains every six or eight hours, until half a drachm is taken in the interval, or even in the twenty-four hours; or by giving in addition to the 15 grains at the end of the paroxysm, 10 or 15 grains an hour or two before the period for the accession of the next paroxysm, will answer. This latter is the plan I have used successfully in the few cases of quartan ague I have met with.

Such I have found to be the most successful mode of using the sulphate of quinine in the treatment of intermittent fevers; and I have never found it to interfere with whatever antiphlogistic measures it has been necessary to use. General and local bleeding should always precede the use of quinine, in every case in which they are indicated. I have found this practice constantly to favor the full impression of the quinine.

In many of the cases of intermittent fever, I have observed a regular recurrence of the disease, in some cases, every seventh, in others every ninth, fourteenth, twenty-first, and twenty-eighth day. In many cases of this kind I have succeeded in breaking up the morbid catenation, and arresting the disease by anticipating the recurrence of the paroxysms for one or two periods, with the sulphate of quinine. In cases of this kind the premonitory stage is very common, and when this makes its attack, the administration of 10 or 15 grains of the sulphate of quinine has invariably proved successful. In this manner, during the past two summers, I have been so fortunate as to free myself from this troublesome complaint.

As I have said at the commencement of the remarks, about two years since I changed my opinion as to the treatment of the remittent forms of fever. At that time, the practice I was pursuing was general and local bleeding as required, free alvine evacuations, the mercurial impression, cold bland drinks, cold affusions and counter-irritants, and I then thought it the most judicious. It however, rarely succeeded in arresting the disease promptly, and no part of this treatment possesses a quick and certain power to arrest the disease, generally, unless in some few cases, venesection, and that only in the forming stage. Here then, generally, was the practice I was pursuing in what are termed remittent fevers, and by far the greater part, indeed nearly all, of the authors whom I had read, and whom I was taught to regard as standard authorities, condemned in the most unqualified terms the use of peruvian bark, and all its preparations, in the treatment of this disease. I gave them implicit belief, and would have deemed it the height of mal-practice to have used it, as it only should be used, boldly, in any such case. Notwithstanding all this reasoning by analogy, an idea that its use might be advantageously extended to the treatment of remittent fevers, in all their varieties, forced itself upon my mind. My attention was first aroused by the wonderful and almost infallible power I had myself witnessed, that this potent remedy possessed over every variety of intermittent fever. Again I reflected that they were the result of the same causes—their anatomical characters, to say the least, very similar—that they had, in very many cases, such a decided tendency to assume the characters of each other—that the differences, in the accession, duration and symptoms of the paroxysms, are in reality but slight. For instance, the diagnosis between them, in many cases, is very difficult from the passing of the one into the other—and in fact, many authors describe them as varieties of the same;

and finally, that they both yield readily and promptly to the same general treatment, except some few cases of the severe forms of each, in which the best devised practice may fail. Here then, by name, are two different diseases, according to some authors; according to others, only a variety of the same type of fever, not in reality differing more from each other in the accession, duration and symptoms of the paroxysms, or in the nature of the intervening periods, than do the varieties of those which are plainly intermittent—there being as great a difference of character, among the varieties of the former, and also among the latter, as between the intermittent and remittent. From this view of the subject, they appear only as varieties of the same disease, and as such I regard them. But admit it otherwise, all who oppose the use of the preparations of peruvian bark in the treatment of remittent fevers, will say the practice I formerly pursued was, as stated above, judicious; I deem it such now, and only regard the addition to it of the timely and free use of the sulphate of quinine as rendering it almost certain of success in this form of fever. The use of it in remittents is as necessary, judicious, and as harmless as it is in the intermittent form. It has been perhaps as much from a different view of the action of the sulphate of quinine upon the system, as from regarding the two forms as different fevers, that the great opposition to its use in remittent fevers has been made. I will endeavor to give a fair and impartial account of its action in many hundred cases. I regard it in large doses as a sedative; certainly all admit it to be the greatest and most powerful anti-periodic or anti-intermittent, known to the profession. I can aver, from long experience, that I have never known it to increase inflammation, and that I have given it freely under all circumstances, and am fully satisfied, from observation, that it not only has no power to retard the cure of inflammation, but that in fact its tendency is to accelerate it; that it will prevent congestions, and inflammations from occurring in fever, by allaying the irritation which excites and causes them. I have given it at all times of the paroxysms with perfect safety, and I have never witnessed any alarming or dangerous effects from its administration under any such circumstances. The result of my experience of the effects and properties of the sulphate of quinine, may be thus stated: 1st. It is a sedative, possessed of peculiar properties, which may be termed anti-periodic, or anti-intermittent. 2d. It has no power to augment inflammation, and may, therefore, be given under all circumstances. 3d. Nor has it any power to prevent the cure of inflammation; but under many circumstances, a direct and positive power to promote it. 4th. It may be given at any period during the paroxysm; (I prefer the decline, or just after it has subsided). 5th. During the course of fever, it accelerates the absorption of whatever remedy may be introduced into the system, such as mercury. This I can only explain, by saying, that it allays the fever, and consequently, withdraws the excitement of the organs, which, as long as it continues, must diminish in a great degree, if it does not entirely stop, secretion and absorption. 6th. It will allay more speedily, and more certainly than any other remedy, the troublesome symptoms, nausea and vomiting, so common in the fevers of this country; and by the same virtues, it will arrest the paroxysm of fever, and thus leave whatever lesion may exist at the point it had arrived at when the fever was arrested, and by thus withdrawing the excitement from the injured organ, will prevent its being hurried beyond this point, and consequently it will much more readily yield to the remedial agents.

Such are the properties of the sulphate of quinine ; and can any one, regarding them as such, hesitate to administer it in fever, as it is evident this remedy may safely be employed, even in cases where local inflammations exist, and that, too, without augmenting them.

In the next place, a view of the nature of fever may in some measure enable us to see the *modus operandi* of the sulphate of quinine, and serve to explain its almost specific power in arresting the paroxysm and progress of fever.

Broussais, in order to explain the power of quinine in arresting intermittent fever, was forced into the absurdity of attributing to a stimulant, (as he regards it), the power constantly of subduing gastro-enteritis, of which, he contended, the fever was symptomatic; and such is likely to be the fate of all who contend that all fevers are symptomatic of local *inflammations* of the different organs.

To endeavor to arrive at the proximate cause of fever, solely by examining the bodies, after death, of those who have died of fever, seems about as hopeless as to examine the wreck a violent tornado has left, in passing over a section of country, in search of the cause that originated the tornado. We may see and know the effects each has produced, and, ever after be able to ascribe these phenomena to the causes producing them; but they reveal nothing to us, in relation to the precise conditions of the body, or of the atmosphere in which they themselves have originated. Dissection cannot, alone, be trusted in seeking the proximate cause of fever. It is doubtless of high importance, in teaching us what organs are prone to suffer, and thus arousing our efforts to protect and relieve them, but when has dissection detected the difference in the brains of those who have died of hypochondriasis, tetanus, or hysteria, or can it distinguish the brain of high intellectual endowment from that of idiotcy?

The train of phenomena constituting, and the action of the remedies most successful in arresting the disease, must doubtless be of vast assistance in enabling us to arrive at just conclusions ; for if they are disregarded, we fail to detect the great and primary link in the train of sequences constituting fever—lesion of the nervous system.

Those who contend that fever is not symptomatic of local inflammations say, "fever is an essential or primary disease. The first appreciable event in the chain of sequences, constituting fever, is functional injury of the nervous system. The only invariable or essential consequence of this affection, is functional derangement of the most important organs of the body, but more especially of the brain, the circulating organs and fluids, the alimentary canal and skin. The changes which have been observed to take place in the blood, and other animal fluids, are, like the local disorders, secondary and not primary; they may be the source of the phenomena remarked in the advanced stage of the disease, *but they are not the source of disease itself in the first instance.*" That functional lesion of the nervous system is the first link in the train of phenomena, is generally admitted, and that this universally follows the impression of the remote cause, must also be admitted. In other words, they stand in relation of cause and effect. The truth, then, is, in my opinion, that the malaria, or other cause of fever, first acts as a morbid irritant upon the nervous system—sometimes more forcibly on the cerebro-spinal, and at others more so on the ganglionic division, causing derangement of function therein; the vascular system throughout the organs, especially the capillaries, becomes involved, and congestions and inflammations are excited, and whatever organs have suffered most from the predisposing and exciting causes, will be most violently affected—there is evidently great disturbance of the nervous and vascular systems, and they evidently act and react on each other, thus producing the phenomena of the paroxysm. The inflammatory congestions that occur in the first paroxysm, are aggravated and increased by every succeeding one—and unless arrested, by suitable means, will, in a great majority of cases, end in organic lesion of some vital organ, and terminate fatally.

Moreover, if from the constant and invariable effect of any remedy in arresting disease, any certain conclusion can be drawn, the almost specific power of the sulphate of quinine, in subduing the paroxysm of fever, must at least be regarded as indicating the nervous system as the *fons et origo* of the cause of fever; for it is manifest, that the whole power and effect of this potent remedy is exerted upon this system; whether or not the remedy be regarded as a sedative, the excitement and derangement produced in this system, by the remote cause, points out its action to be that of a morbid irritant. It cannot consist of inflammation, as quinine possesses no immediate and prompt power to arrest inflammation; nor can it be, as Dr. Cullen supposed, sedative; for, if it acted thus, it would not constantly produce excitement and derangement.

This irritated state of the nervous system, is the *sine qua non*, the original, fundamental, or proximate cause, to which all the other phenomena of fever are successive. It consequently follows, that, if this irritation be allayed, and its return prevented, the phenomena of fever will cease—and such is the fact.

If the fever has been suffered to continue for any length of time, the different organs may, and must, in many cases, be found in various morbid conditions. It must, therefore, be remembered, that removing the cause, can only prevent further injury; and that whatever lesions exist, up to the time of its removal, must remain, and require proper treatment to subdue them. It is like plucking a thorn from a wound; it does not immediately repair the injury it inflicted, but on being withdrawn, ceases to act as an irritant, and leaves the part in a condition most favorable for recovery.

Regarding this, then, as the true pathology of fever, the treatment may be considered to consist mainly in the following indications, viz: *to arrest the fever at its outset*, or as soon thereafter as practicable; to obviate the tendency to inflammatory congestions, especially of the liver, stomach, intestines, and brain, by moderating the febrile reaction; and finally, to remove the local disorders and complications that may have occurred. The remedy, on which I place the greatest reliance, to arrest the progress of the fever, is the sulphate of quinine. To accomplish this object, it should be given boldly, in large doses. General and local bleeding; cathartics, such as the mercurial preparations, castor oil, sulph. magnes. and magnes. calc.; cold drinks, and ablutions, are the means adopted to allay the febrile reaction, and to obviate the tendency to inflammatory congestions; and to combat the local disorders, topical bleeding, counter irritants, and alteratives.

Such is the general outline of the practice I have pursued, for nearly two years past, in the treatment of the fevers I have met with in Florida; and I can safely say, that it has proved successful in every case which has been under my immediate charge, in which it was adopted—amounting to several hundred. I have, at times, prescribed for many patients without having seen them during their illness, and even in every case of this description, under, of course, every disadvantage, it has been fully as successful, with one solitary exception. This was the case of private Campbell, whose death is recorded in this report. He was on duty with a detachment stationed at the Auscilla ferry, near old fort Roger Jones. I was on my way to the place, to have him removed to the hospital at Fort R. Gamble, when his death was reported to me. He died very suddenly, and from the information I received, I believe his death resulted from the form known as "congestive fever," which I have seen in that locality. All the other cases occurring at that station came under my immediate charge, and yielded readily to the treatment.

A few remarks in regard to the mode of using the sulphate of quinine may be necessary. In all cases, strict attention should be given to the state of the vascular system; for, as has been before remarked, general and local bleeding should always precede the exhibition of the sulphate of quinine, in every case in which they may be indicated, inasmuch as this practice will be found constantly to favor the action of the quinine—probably by diminishing excitement, and moderating the reaction of the vascular on the nervous system—with the same view, either before or soon after its administration, an active mercurial cathartic, to procure free alvine evacuations, should be given, to be followed, if necessary, by a combination of sulph. magnesia and magnesia calc., or castor oil. I have concluded, from experience, that the best time for the sulph. quinine to be given, is immediately at the end of, or as a paroxysm subsides—or as near this time as possible. Nature herself would seem to indicate this, for at this time there is generally a calm, a pause in the disease. There is no general rule to determine the quantity of sulphate of quinine that may be required. In ordinary cases I have found from 15 to 25 or 30 grains sufficient. In the severer forms, a larger quantity becomes necessary, and in dangerous cases, such as I cited when endeavoring to show how the severity of fever modified its action, from 10 to 20 grains may be given every hour, in combination with 10 grains of calomel. Here the mercurial impression is an agent whose auxiliary power is required. For those cases commonly called congestive fever, which I had met with previously to having adopted this practice, there seemed to be no remedies on which any reliance could be placed. It has been in the severe and extreme cases of this formidable malady, that I have seen this remedy (sulph. quinine), exhibit to the greatest advantage, the herculean and almost incredible power it possesses over the cause originating the disease—to which it would almost seem to act as an antidote. In cases of this nature, it must be given fearlessly, in large and frequently repeated doses, and persevered in until its effects are manifest.

One of the most annoying and troublesome symptoms in fever of this section of country, is irritability of stomach, accompanied with nausea and vomiting. Hitherto, in a large majority of severe cases, I have had this difficulty to contend with, and in many instances it has resisted every remedy. Under the present mode of treatment, I have had no difficulty in relieving and preventing this symptom.

It is said calomel adds efficacy to every remedy with which it may be combined; for instance it renders squills more diuretic. The same remark applies to sulphate of quinine, in relation to the mercurial preparations. What a happy combination, then, especially in cases of fever, these two remedies must prove; each possessing such remarkable power. I have seen a single dose of 20 grains quinine enable the mercurial preparations to produce their full impression, even to ptyalism, in patients who had resisted every effort that was made to excite it for many days. I think, however, this effect is easily explained. In these cases, the system, laboring under such constant and continued fever, during the whole time, resisted the mercurial impression—the different organs were in such a state of irritated excitement that both secretion and absorption were nearly if not entirely suspended; the quinine, by subduing the fever, allays this excitement, on the removal of which, the functions are resumed, the remedies are taken up, and ptyalism is produced, or in cases where the mercurials have not been pushed so far, the secretions are restored to a healthy standard, without this disagreeable and harrassing accompaniment.

Fort Gamble, W. Florida, Sept. 30, 1841.

ART. VI.—*An Account of the Yellow Fever which prevailed in Woodville, Miss., in the year* 1844. By C. H. STONE, M. D., of Woodville, Miss.

This heretofore very healthy village is supposed to have contained about 800 inhabitants at the time the disease began; nearly 200 went into the country within a week after this period; about 20 had been affected in other places; leaving 600, as near as can be ascertained, liable to it. Of the 600, not more than 5 escaped. I am *certain* of but one. Of the 20, only one was affected, resisting the influence of the poison till a very late period, and having a mild attack. The immunity given by a previous attack seemed as great whether a long or a short period had elapsed, or whether it had been experienced in Charleston, Jamaica, New Orleans, Bayou Sara or Natchez.

The disease began mildly, a great proportion of the cases being mild before the 25th August or 1st September, 40 or 45 days after the occurrence of the first case. As seen about the 10th September it was pronounced by competent judges as malignant as it usually occurs in New Orleans.

The number of deaths in the town and within a mile was about 60; several of these had no physician, which it is equally proper to say was the case with many who *recovered*. It was not only impossible that all the sick could be visited by the physicians, but it frequently happened that there were none in a family well enough to give the others water, as where it began with one member it soon attacked the rest. At one time, there was for nearly two days only one physician, then only two for several days more, able to practise on a fever of which they knew nothing practically, and for a long time not even by what name to call it. They profess to be as competent as other *village* doctors, and if they were unfortunate in their efforts to contend with this (with them) new disease, the same fate might have attended the exertions of more able men.

The circumstances in which this was found chiefly to differ from our ordinary fevers, were the highly inflammatory and continued (not periodical) character; the profuse secretion of bile in most cases, and the inapplicability and often destructive effect of quinine. The differences became more marked as the disease acquired violence, and then it became surprising how this fever could *ever* be confounded with another, or be supposed to have any relationship to bilious fever; at the same time it appears to me to be the only *truly bilious* fever.

During May and June a, diarrhœa of a very severe character, causing several deaths, prevailed here, but not extending into the country. Besides its unusual severity, there was an uncommon liability to salivation, which one of the planters living within seven miles told me, was the case on his place. I use mercurials *moderately* for a practitioner of the South. The salivations were accidentally induced by very small quantities; in one case, 10 grs. of calomel and 1 gr. of opium acting freely in six hours, caused a very sore mouth in twelve hours; in another, 15 grs. hyd. c. creta touched the gums, and so with several more.

Yellow Fever had not been reported at any point in the South-West when the first case occurred here on the 15th of July. But it is not of the origin that I have now to speak; my purpose is to show the form the disease assumed, what course of treatment failed to cure it, also what medicines were destructive, as some guide to other village doctors among whom this fever may make as sudden an inroad as it did here, and finding them as unprepared as we were in knowing what to do, and *what not to do*, may give *them* as much cause for regret as it did us.

The symptoms, I proceed, then, to pourtray. A chill generally preceded, often accompanied by pain in the head, eye-balls, back, and extremities, and lasting a very short time, or several hours. The reaction was violent, the pains increased, irritability of stomach was excessive, in one case, at the beginning, the pulse full, throbbing, firm, bounding, or hard, and not much above the natural volume—beating from 110 to 125. I observed, in severe cases, as I bled to produce an approach to syncope, the pulse became remarkably *slow*, from 125 to 60 or 70; the eyes red, yellow, and suffused with tears, becoming less red and more yellow as the case proceeded, and often, a discharge from the nostrils, especially with children.

The face was bloated and red with some; in others, chiefly those attended with perspiration which (was profuse and of an offensive odor) it was pale and shrunken. Sometimes before, and always after an attack, and not dependent upon the perspiration, a peculiar odor was perceptible which it is not easy to describe, but which to have observed once, is to remember always. I think I could detect the disease by this alone. One of my children, aged 8 years, gave off this odor twelve days before a development of fever, his pulse was now full and remarkably slow, face pale, and prostration extreme; 10 grs. calomel were given; he remained pale and languid, walking about the house for twelve days, and then had a full attack, was bled, took 30 grs. calomel and was soon well.

The tongue did not deviate much from a healthy appearance for 20 or 30 hours, and in some cases not indicating the serious condition of the system, but in the greater number, if severe, becoming dry and red, smooth or cracked and bleeding, or very red and moist, or furred, yellow brown or black.

The thirst, not very great in the beginning, became excessive; vomiting increased as the excitement became greater in the stomach; the matter thrown up being chiefly bile, more often pale than dark yellow, or only mucus or the fluid drunk.

Without much pain, even upon pressure over the epigastrium or liver, there was a full, disagreeable sensation, amounting, in some instances, to a most distressing uneasiness at the stomach; hiccup was common in cases of severity, and the stomach and intestines were oppressed with gas, even in mild cases.

As the pain in the head, eye-balls, &c., began, in one to three days, to subside, the restlessness, before extreme, sometimes lessened, and the patient fell into a dreamy condition, soon, perhaps, to feel quite well, and the pulse to become unnaturally slow. The calm generally came on in four or five days, sometimes later. Some became more restless, delirious, and perfectly unmanageable. The judgment, in others, again, seemed quite unimpaired; or, on the contrary, much brightened, the patient arguing clearly, and obstinately persisting in, and artfully effecting his wishes. Yet nearly everything occurring during the illness was forgotton. One boy, 16 years old, had his memory so impaired, that, two weeks after he had attended the funeral of an acquaintance, he was astonished to hear of his death. The skin, hot and dry in some, was in others, and the most numerous class, bathed in a disagreeable perspiration, becoming cool if the perspiration continued long. The perspiration was yellow in one case, and did not appear to afford relief in any instance. The yellowness of the skin came on in four or five days, sometimes later; in a few instances but little perceptible till after death, and was of a lemon to an orange hue, brownish or greenish, at some points. Ecchymoses appeared almost invariably after death.

The urine was yellow, even before an attack, becoming deeper colored, like ley, porter, or brandy, and tinging the vessel yellow. In proportion as the case was severe, this secretion was suspended, diminished, retained, or acrid and voided with pain, causing an inflammation of, and purulent discharge from the urethra, in two of my patients.

The discharges from the bowels were pale and dark yellow bile; black or green mucus, with or without bile; or serous; sometimes fœtid; at others, inodorous, and at late periods and in dangerous conditions, glutinous.

The blood was not much noticed at the beginning. As drawn from the temporal artery of a child, in strong convulsions, on the 3d day of an attack, which had begun mildly, it was black, and afterwards it was observed to be always so, and the serum yellow.

One child's attack began by a convulsion, followed by coma. Within three days in a few, in others, at later periods, hæmorrhage took place from the mouth, bowels, bladder, leech-bites, scarifications, and blistered surfaces, and from some of these sources an oozing continued after death.

If any remissions took place, they were the temporary effects of remedies, observing no regularity. The fever consisted of one continuous excitement; the pulse maintaining its character, even with improvement of other symptoms, till convalescence set in, or declined just before death, which was, with a few exceptions, preceded by black vomit. Some cases, especially with the children, and at early periods, were so mild as to present no sign of disease except full pulse, sallow skin, copious dark yellow stools, and emaciation, after some days.

The membrane, designated by Dr. De Valletti as a pathognomonic sign, was invariably found, after attention was called to it by him. I saw the same membrane on the gums of a young man, John Dixon, sick in January, 1845, with fever and jaundice, a sequel of an attack of yellow fever he had in the summer; also, last fall, in a negro laboring under pneumonia, living six miles in the country and not exposed during the last summer to Yellow Fever. Very many, laboring under chronic irritations before the epidemic, have been restored to health.

The state of the system, proper for quinine, seemed not to appear; in some of the severe cases it was *supposed* to be indicated after full bleeding, &c., but it caused injury or death.

The mortality, up to a certain period, was great in my practice, and it will appear to be owing more to the want of proper treatment than to the malignancy of the disease. The practice I pursued was to bleed freely, as often as the pulse became full or hard, even three or four bleedings. Local bleedings, largely and often, from the spine, or epigastrium, were never omitted if the case was of any severity. Emetics of ipecacuanha were prescribed in all mild cases, and in such as *appeared* so, at the time of attack.

Calomel, in a purgative dose, of 10 or 15 grains, followed the emetic, and was repeated once or twice, with other purgatives; it was omitted or used with caution as soon as the tongue became red.

Blisters were applied and sinapisms, frictions, not omitting quinine whenever, by the preceding measures, or the progress of the disease, there appeared a proper condition for it; when used, it was in a dose of 5 to 10 grains. Finding a great mortality, I began to use calomel more liberally, and as I did so, I saw its great importance.

With these, and the common adjuvants in the treatment of fever, I endeavored to combat the symptoms as they *presented* themselves, for, having no knowledge of the disease, I could not foretel the varying conditions into which the system was rapidly verging. I could not *anticipate.* The following list will exhibit the result of this practice, which consisted chiefly in liberal bleedings, general and local; the moderate use of calomel, and occasionally the administration of quinine, emetics, &c.

Number of Cases.—Males, 17; females, 6; children, 10; negroes, 18.—Total, 51.

Deaths.—Males, 5; children, 1; negroes, 2.—Total, 8.

I had a fair opportunity of treating all these, except one (Hans) who had been sick two and a half days before he became my patient, and during this time, under the occasional prescription of four doctors and one merchant. He died on the fifth day of his attack.

I have not included in this list the cases of five adults brought into a very alarming condition by the bad practice I adopted in the beginning of their attacks; nor of one (Duncan's) which had been subjected to a red pepper practice; nor of another, (Hemmingway's), which I saw twice in consultation, at an early period, and which, in part by my advice and from the practice pursued by his regular attendant, was in a most hopeless condition, when he was placed under my care; because I am *well satisfied* they would have died but for the liberal use of calomel, and therefore I shall include them in the list of those treated by a very different plan. If they were placed in this list it would exhibit 15 deaths in 58 cases, a frightful mortality when we reflect that among them are included a large proportion of mild cases.

It was only about fifteen days preceding the change of practice that the epidemic began to assume great violence, and all the deaths enumerated, except one, that of the negro Moses, (11th of August,) whose death rapidly followed a full dose of quinine,—were within this period; it had been at its acme about a week, when the practice, of which I shall speak hereafter, was adopted, and when mild cases became rare.

I have the privilege of speaking freely of the practice pursued by myself, and I shall indulge it, in hopes to guard others against quinine and the moderate use or abandonment of calomel, or a reliance upon bleedings; nor shall I, through fear of being charged with presumption, hesitate to speak as positively in favor of the treatment I adopted afterwards. The variety of modes of treatment advocated after so many years experience, confer that privilege on the tyro as well as on the veteran. I admit, I shall insist, that the unfavorable result shown was not from the necessarily fatal character of the fever, but from the error of practice, and if at the same time that I state this, I am entitled to any excuse for being ignorant of the treatment, I, of course, claim it. The disease had not, and was never expected to prevail here; and if so, where, among the discordant opinions respecting its nature and treatment entertained by those having all the experience, was I to look for information?

If bleeding, general and local, can be depended upon as the chief remedies, I should have met with more success, for it was used to a full extent. The pulse being full and firm or hard, I bled repeatedly and always to produce a decided impression, in a recumbent posture; suddenly elevating the patient, when an impression had been made on the pulse, or pain, or the quantity drawn appeared to be as much as the patient's constitution would bear. I then allowed the blood to flow till a faintness was felt; then the pulse, generally from 100 to 125, fell to 60 or 70, in the minute. In bad cases this was almost invariable. This mode of bleeding I continued throughout the epidemic, for I considered it better to produce the effect on the pulse in this way than by a large quantity. I often directed the patient to be elevated several times. In some a large quantity was taken.

Finding such a profusion of bile thrown up, I was led to the exhibition of emetic of ipecacuanha, a grievous mistake, even in the mildest cases, but in severe ones, attended with serious injury. Spontaneous vomiting after a day or two afforded relief; the same act excited by ipecac. fixed an irritation in the stomach, the vomiting became an additional difficulty, and greater in the cases in which it was given than in others of the same degree of violence.

After the emetic, or after the bleeding, I gave a purgative dose of calomel, 10 or 15 grains. This or blue pill, I had an opportunity to repeat once before the redness or dryness of the tongue appeared, and caused me to defer it till the inflamed state of the stomach should be subdued or moderated by more abstraction of blood, or by blisters, &c.

I thought it impossible to act upon the secretions of the liver or bowels with advantage, with such evidence of high excitement. Sometimes the calomel was followed by serous discharges, and that led me to defer it, till by copious leechings and cuppings, I could expect more benefit from mercurials. Finding myself foiled in bringing the organs to a proper secretory point, I gradually began to administer calomel, despite the appearance of the tongue, as I had witnessed the prompt relief from it in cases of less gastro-intestinal excitement, as also, because I remembered, that in *former* times, when I had used calomel freely, I had found it of great value in what seemed nearly similar states of fever. And in my troubles I forgot the damage I had in those days caused by it; thus I began to use this remedy, powerful for good as for evil, in the after stages, a period far from being the most suitable for its administration, but I have the satisfaction of knowing that I saved many valuable lives by it.

The first case of severity I had was the negro Moses, August 8th, who was twice bled largely by Dr. A. C. Holt, within four hours the evening before I saw him, and had the usual purgative given.

I continued the treatment with baths, local bleedings and one dose of calomel. The case presented the usual appearance for the use of quinine about the third day. He took 10 grs.; in 20 minutes his tongue was dry, he became delirious—and in 12 hours he was dead. It was no afterthought that the quinine caused his death, I told Dr. Holt of it at the time.

After this, I still hoped to make it useful; I bled one patient, Mr. Simrall, to faintness, had a large cold enema administered, and gave 10 grains quinine, with decided damage. I had more trouble with that patient's stomach than with any other, except when death was the termination. To one patient I gave quinine with prompt good effect, but he had *simple intermittent*, the only case I saw in the whole summer in Woodville. This person, the Rev. Mr. Craine, had a regular and violent attack of Yellow Fever six weeks after.

I found great difficulty in divesting my mind of the hope that I would, day after day, see my patients in a condition favorable for its use, but that did not arrive. The more nearly they seemed to approach it, the the farther were they in reality from it, except they had been slightly attacked or were well; even then, after recovery, it did no good; it gave no strength, and sometimes oppressed the stomach.

On the 4th of September I visited a negro of Major Traske, seven miles west, who upon my arrival was within two hours of an expected congestive paroxysm; her danger was pressing. The paroxysm was warded off by frictions with fine salt and the sharp points of shucks, applied by a quick light motion; by two doses of quinine of ten grains each, and capsicum tea. As soon as the direction had been given for this, I went to the other quarter, and found a mulatto fellow with the strongest marks our of Woodville fever, as it was then called. The same bloated and almost purple face, turgid veins of the forehead, hard pulse, and excruciating pain of head, eyes, and back. I bled from both arms, raised him suddenly after he had lost a full quantity of blood, and there was found, as soon as he felt faint, *the slow pulse.* I warned Mr. Welsh to avoid quinine, and gave directions for free general and local bleedings, &c. I heard nothing of the case for three days, when I received a letter from Mr. Welsh, from which I was misled to suppose the case had evinced a paroxysmal character, and that I had been mistaken in my first opinion of it. The day after, being the fourth day of the attack, the man was free from fever, with a slow pulse. As I learned afterwards, what had seemed to be remissions was the temporary effects of the depletions; of this Mr. Welsh is satisfied. In accordance with my last instructions Mr. Welsh gave him quinine, five grain doses, during the day, and by night he was in extreme danger; vomiting, pulse small, great restlessness, &c. He continued to get worse through the night, quinine not being left off till late. He rallied by morning; all attempts to *cure* him having been abandoned, and by this happy thought he was saved. He discharged immense quantities of bile from the bowels for a week, and deep yellow urine for two weeks. Mr. Welsh is as certain as I am, that quinine caused all the danger of that day and night.

One physician informs me that he gave quinine in one grain doses, (I could not say more in condemnation of it,) and was pleased with its effect, especially causing a full perspiration in his own case. This perspiration was too *common* in the disease; it is not *probable* that quinine caused it.

In relapses, it was only in three cases, after the *peculiarity* was gone, or very much lessened, that I gave it; and then, in one, I gave fer. cy. quinine, for two days previously, and in one of the others, it was borne with difficulty. I had several dangerous cases of relapse under my care, seven of which were as serious, and three more so, than any original ones I had. I used no quinine except as stated above, and I lost one. If the skin became cooler, and the patient suffered less at one time than another, at irregular intervals, it was more the effects of remedies than a periodical tendency; the pulse was still that of Yellow Fever; full, firm, it indicated the persistance of the fever.

Dr. Rush, who speaks of Yellow Fever as sometimes appearing in the mild form of an *intermittent*, says, "it [the pulse] was nearly as *tense* in the remissions and intermissions of the fever, as it was in the exacerbation." Is it not strange that this able physician, placing the fever in the *blood-vessels*, should speak of an *intermission* of fever, while *such* a pulse continued? He states that bark did not agree. The value of quinine is as an anti-periodic; in Yellow Fever there is *no* periodicity, nor is this the only distinctive character; others exist, which forbid the belief that quinine *can* be a proper remedy.

I need not name the place not *far from this*, where, in 1839, at least one hundred died in one hundred and eighty cases, and where quinine was *the remedy!* Will it be said that *that* epidemic was malignant beyond example? Will it not rather appear that the sick had to contend with two poisons? The same intolerance of quinine was observed by Dr.

Powell, of Pinckneyville, in his cases of yellow fever. He cured all but one, and by calomel.

Dr. A. C. Holt, large, and of vigorous constitution, was attacked with severity, August 24, the head suffering with unusual violence; he was bled freely twice, had at least three hundred and fifty large American leeches applied to the abdomen and arms, (one hundred at one time,) besides free depletion from the head and spine; two or three moderate doses of calomel, producing serous stools, barely tinged yellow. This, with a bath, constituted the chief treatment for five days; it seemed that the only effect gained was to prevent his organs being overwhelmed by the violence of the fever; he began to turn yellow; he laid in a dull, apathetic state, with difficulty was induced to answer, or put out the tongue, which was red though moist; the pulse, which had been of uncommon volume and very strong, was still too full and vigorous; altogether, he presented a most unpromising appearance. I had on the day of his attack just left my bed, where I had lain eight days under his care; in this enfeebled condition, about to lose a valued friend, I found myself in an indescribable state of anxiety. I began to give calomel in ten grain doses, repeated every two hours till he took sixty or seventy grains; when this quantity had been taken, he became better, even before it caused evacuations; he was conscious of a change, and soon began to discharge from the bowels great quantities of dark yellow bile, and entered into convalescence.

Fielding Davis was attacked with only common severity on the 1st of September. I bled, cupped, leeched, &c., and gave but moderate portions of calomel. At length his situation became truly alarming. I gave calomel in the quantity and manner of Dr. Holt's case. The improvement was decided in eighteen or twenty hours, and he was pronounced by an old yellow fever practitioner to need nothing more. I followed this advice, and waited twenty-four hours; gradually he reverted to a condition more dangerous than before; he was considered hopeless; skin cool and moist; pulse small and weak, he was delirious, had tumid abdomen, and extrication of gas, fluids entered his stomach with a gurgling noise, his tongue having a dry red streak down the centre, cracked and bleeding. This appearance of the tongue only gradually disappeared at the end of a week. All, even the veteran, gave him up as dead. I procured as much advice as I could. I began again with calomel in the same manner, and in twelve hours I was satisfied he was responding to it; he gradually improved, his stools became yellow, he was safe.

In this favorable condition he received a fright, had a discharge of bloody mucus, and in other respects was worse; calomel and opii. (10 grains, and 1,) replaced him in his former situation, and he recovered without salivation, and I think with slight, if any, impairment of his constitution.

It was not till the fever had for about a week assumed its utmost violence that it was pronounced to be yellow fever. Why so long in knowing it? I shall not dwell upon that question. I pass it. If pressed, I can only say that those reared with the disease, differ in opinion *when* yellow fever prevails, and mild cases are often considered a *bilious* fever, &c.

Dr. D. Holt, at present a resident here, but at that time living fifteen miles distant, visited Woodville on account of the prevalence of so direful a disease.

He immediately called it by its proper name; he was experienced in the disease. Dr. A. C. Holt and myself sought his advice eagerly, and he gave it as cheerfully. We engrafted his chief remedy upon what our experience had taught us was and must be essential—bleedings, general and local. He advised us to give from sixty to one hundred grains of calomel as soon as the chill was off—to follow it in about eight hours by senna and manna, &c. At first he was not disposed to bleed, not having found it necessary in New Orleans, where he had practised many years.

To him I am indebted for many of the most gratifying periods of my life, and but for him I should, no doubt, have to deplore the continued unfortunate results of my efforts.

The practice I shall now attempt to describe, was also pursued by Dr. A. C. Holt, with some unimportant differences.

Dr. D. Holt, having been accustomed to succeed without bleeding, at first made his dependence on the power of this first dose, and seldom gave any more.

I am not about to advocate the calomel practice of the books, nor of the sliding scale reported by Drs. De Valetti and Logan to the Medico-Chirurgical Society of New Orleans, as the practice generally pursued here; I repudiate them, both equally for the Drs. Holt and myself.

I can show a gratifying result, and I hope to show that the sedative dose of calomel is as imperiously demanded in yellow fever as that of quinine is in malignant intermittents, and that as much, if not more success will be obtained by it. Under the preceding practice, cases of moderate severity were long in tending to health; that process never began in more severe ones, till they had taken calomel freely; this I did not know till I had lost several. The tendency was decidedly to death.

As soon as I began the following, the reverse was the case, a more full and healthy secretion being established in the liver at the onset, it appeared as if a floodgate had been opened, through which the system was relieved, only requiring occasional impediments to be removed, or, as one unfortunate case taught me, demanding that none, especially an untimely purgation, should be exhibited. I can say truly that I would rather contend with yellow fever, as I have seen it, than with the common fever of the South, of equal grade.

The patient generally passed through the chill before being visited; hot foot baths and warm clothing generally sufficed to bring on the excitement quickly.

The first remedy was the lancet. The patient was bled in the manner previously pointed out—once, twice, thrice, or four times, to a period as late as the third day; the condition of the patient, rather than the time he had been sick, being the guide. However mild the case appeared, on the first day, I had cause to regret the omission of bleeding.

Immediately after the first bleeding, from forty to sixty grains of calomel were given. The quantity should be sufficient to be *sedative*, and if one hundred grains should be required for this, then *that* would be the proper quantity.

The attacks of women being milder than those of men, I found forty grains answer; while sixty grains were given to a man, and twenty or twenty-five to children, three grains to an infant *two hours old*.

An enema being now directed, they are left for six or eight hours for the effect of the calomel; to be bled again if the pulse or pain increased very much, or else cupped from the spine.

The effect of bleeding had been of short duration before it was followed by this dose of calomel. Afterwards the quiet and composure were greater, and of longer continuance.

Some patients were sensible of its soothing effects; one not bled, and averse to its use, expressed this, not knowing what he had taken.

It allayed the irritability of stomach, promoted the secretion of bile, if it had been suspended, or moderated it if it had been profuse, rendering it more healthy.

One full evacuation of dark yellow bile was generally produced by it within six or eight hours, and at this time senna and manna, or castor oil, was required to cause four or five operations of almost pure bile, or, mixed with dark mucus.

Bleeding was not required to be repeated as often as it had been before, although the cases were really more severe.

Local bleedings often sufficed to repress the excitement of the stomach and liver, and thus to continue the secretions of the latter organ.

During the first few days they were preferred from the spine, extending to the region of the liver, because of the greater quantity of blood obtained, and on account of the more intimate nervous connection.

The pain of the head sometimes yielded to cupping from this point alone; but if not, the cups were applied to the neck and extended to the whole spine.

The perspiration lessened and the pulse became calmer invariably from this means, though sometimes I bled from the arm immediately after a local bleeding, finding myself deceived in the exigency of the case.

In the later stages, the bleedings were preferred from the epigastrium, or jointly from the spine. They were resorted to whenever the action of the pulse increased, or did not moderate in a reasonable time, after large drops of perspiration on the forehead, alone decided their repetition, and the tongue becoming dry or red, or the secretion of bile deficient during the continuance of the excitement, also rendered their use imperative at whatever period of the case.

If, notwithstanding the local bleedings, the biliary secretion lessened, or became pale, calomel was repeated in doses of 10 to 20 grains. At first, I began its repetition, not waiting for this evidence, but I soon learned that, that was doing *too much*. It induced salivation, which was in no instance serviceable.

Upon the supervention of inflammatory irritation of the gastro-duodenal surfaces, if the appearance of the tongue, and greater thirst, is to be received as evidence of this, I did not the less consider the repetition of calomel necessary and proper, jointly with or after copious local bleedings, provided the secretions of bile became deficient or pale, unless the evidence of inflammation or engorgement of the stomach, duodenum and liver, or the swollen state of the gall ducts was too great to expect the biliary secretion to be promoted or emptied into the intestine. If urged in this condition I saw it *highly injurious*, and more so in small doses than large ones; the former more likely to produce green or mucous stools, or to salivate, and less probably to act upon the liver than one or two doses of 20 grains each.

I made no effort to salivate—on the contrary, I preferred to avoid it as exerting no favorable influence; as not insuring the continuance of the biliary secretions; and in general interfering with the repetition of the mercurial, which still seemed necessary, in some cases, after it was induced. In no instance, in my practice, did a salivation cause any injury: my patients were too well bled; and in a few cases I did not hesitate to give calomel, if, some days after its appearance, I found the liver failing to pour out healthy looking bile; it caused no increase of salivation, and I am satisfied of its occasional propriety in this fever, not in any other.

The more quickly after the chill the sedative dose was given, the greater beneficial influence did it exert on the future course of the disease; but if called at later periods, and finding the inflammatory irritation not intense, I obtained the most satisfactory results from its exhibition, after local bleeding.

Purgatives, except at the beginning, required great caution in their use.

If purgatives, of whatever kind, oil, senna, blue pills, or calomel in small or large doses, caused too strong an impression on the intestines, after the first few days, the liver was sure to have its secretion suspended or materially lessened,—an irritation was fixed in the bowels and the patient made worse.*

An effectual purge of oil or senna had the best effect immediately after the first dose of calomel. The mildest were afterwards selected, their use being most generally superseded by enemata of water, cool or tepid, gruel, or others more purgative.

If the liver was secreting well, there was no difficulty in causing discharges from the bowels; and if it were not, purgatives were not indicated.

Blisters, it was necessary to avoid as long as the pulse would bear local bleeding. If too early applied, as happened sometimes from the impossibility of having the patient leeched or cupped, or from the misapprehension of their power, they did no good, and were a source of much distress. The time being properly chosen they were serviceable, especially in relapses, applied to the abdomen; to the neck and extremities; they did no good.

Sinapisms were serviceable, but more benefit was obtained from frictions with a brush or fine salt in cases attended with full perspiration and cool skin, rendering a local bleeding unnecessary, which the full pulse had appeared to indicate.

Ice and *icewater* were freely allowed and used with great advantage during the highest periods of excitement; after that began to moderate,

* Having conducted one case very much to my satisfaction, an unfortunate error was committed at a late period in the administration of ½ an oz. of oil; it irritated the bowels, the tongue became dry, and the secretion of bile suspended. Instead of relying upon leeches, I deferred them, waiting for the irritation to subside under mucilages, &c., until it was too late. By leeching the epigastrium and abdomen generally, which his pulse would have borne, I might have obviated the evil effect of the oil. But I was astray from the right course, and lost him.

they were injurious, and often complained of. Gum and rice water were the common drinks. Bicarb. soda was very generally directed, affording great relief to the distressing sense of fullness of the stomach. Camphor water was the only stimulant I found any to bear; from it I obtained good effects from prostrate conditions, unattended with red or dry tongues.

Charcoal was found serviceable in a few cases, especially such as were of tedious convalescence.

Anodynes were highly important when much irritability existed, or to restrain the bowels; morphine in solution of soda, or more frequently Dover's powder in a full dose.

If the skin was not dry and hot, it was necessary for the comfort and safety of the patient to keep it well protected. The hæmorrhages from the mouth were unchecked by any means used; in one case cold water appeared serviceable.

Ale and porter were of great service in restoring the strength and tone of the stomach; but brandy and port wine were more valuable for this purpose, during convalescence.

The debility was very great; a person with good pulse and appearance of health, fainted from getting up. Having no experienced nurses, I could have no dependence that the *self-will* of the patient would not prevail against the most positive instructions to remain in bed, not only after the disease was overcome, but before.

This constitutes the outline of the treatment adopted at a time when the disease was at its greatest intensity, and when I had on hand several cases in such a condition, that, but for calomel, I must have lost them.

Calomel is entitled to the credit of their cure, though not used in the mode that I wish to advise, when the treatment is begun at the outset of an attack. The following list includes the number of cases I attended from the period of the former list to the end of the epidemic.

Cases.—Males	12	Females	11	Children	9	Negroes	20	Total	52
Deaths,	" 2	"	00	"	0	"	00	"	2
Relapses,	" 6	"	4	"	8	"	3	"	21
Deaths,	" 1	"	00	"	0	"	00	"	1

The above includes six cases of pregnancy, nearly at full term—three whites and three blacks; three being delivered within two or three days. It also includes the case of a very feeble, consumptive girl, Amanda, whose attack was mild, who was fairly convalescent, as Dr. Proctor knows, and whose death was certainly owing to the faithless nurse allowing her to rise twice from a high bed to evacuate her bowels. Her death speedily followed the exhaustion consequent upon this.

I attended three, with other physicians, one of which was fatal.

The cases of relapse showed as conclusively as the original ones, the necessity of calomel in smaller doses, as a *stimulant* to the liver, and a *purgative*, not as a *sedative*. I gave a fifty grain dose to one that I lost, not, I am satisfied from this cause, but from its intensity, occuring soon after recovery from the first attack, and in consequence of several hours exposure to a mid-day sun. Local bleedings were generally more necessary than calomel, though this was used freely. I treated one case of ten days' continuance with only two doses of calomel, of five grains each, at intervals of several days, relying merely upon cupping over the abdomen, and a blister. I bled none from the arm.

With unimportant exceptions, this was the practice pursued by Dr. A. C. Holt and myself, each with results nearly the same, and if the charge can be justly reiterated, that I *killed* these patients with calomel, how much more justly can it be said, that I lost the others by not giving enough to cure them. That calomel, *improperly* used, can cause death in Yellow Fever is as certain as that quinine can do the same in periodical fevers; but used as I have indicated it, will be as successful.

That bleeding to the extent used, was proper in the highly inflammatory form the fever assumed here, no one will deny, and that such bleeding will be suitable for all other epidemics, of course I do not mean; but that this fever, any where or in any form, can be managed without calomel as successfully as with it, or in the manner advised in the books; that is, indiscriminately through all stages, as successfully as by a first *sedative dose*, and afterwards according as the liver can or cannot be induced to secrete by it, I feel satisfied no one will assert, *if he will try it.*

It has been said, and the idea is constantly held up to view, that Yellow Fever cannot be *cut short.* That it cannot be arrested as speedily as an intermittent, or some remittents, I am well aware; but that an attack, that would otherwise be of long continuance and of urgent danger, can be rendered mild, *so mild that the practitioner will only have the credit of a mild case, I feel satisfied;* and being rendered mild, it will have a less time to run, and in that much it will be *shortened.* The relief must be through the liver—let the action of that organ be well established *early,* while yet it may, and very few cases will run through the desperate stages they otherwise do. It was once thought remittents would run a certain course, that you must wait for the ninth day, &c., and the time is not far back, when few patients rose from their beds for weeks, waiting for *frost* to cure them. Who will be found to assert this now, yet who would succeed better if they gave quinine in one quarter doses, waiting for certain appearances of the tongue, before they even ventured upon that quantity? It will be found that the sedative dose of calomel is as successful in Yellow Fever as the sedative dose of quinine is in remittents or intermittents.

Mr. Loher, aged 27, was attacked, September 14th, and was treated during the first few days by Dr. Brown, his preceptor, by bleeding, cupping, two doses of calomel, of 25 grs. each,, blue pills, &c. On the 6th day he had a pulse, full, firm, and about 120; no pain, flushed face, skin slightly tinged, tongue red and dry at the point, with dark, thick fur at the root; his stools had ceased for 24 hours, having been only slightly tinged yellow from the beginning. He was taking 5 gr. doses of calomel; I advised the discontinuance of all medicine, to depend upon local bleedings freely from the spine and epigastrium, which was adopted and continued for 24 hours. when he refused to lose more blood. During the next 24 hours I declined to advise; he took three doses of charcoal and magnesia, had a blister to the epigastrium, vomited, had hiccough, the tongue more dry and less furred, and pulse still strong. In this aggravated condition I again urged leechings to the epigastrium, full and frequently repeated, and no medicine. During four days he was leeched two or three times every twenty-four hours; the first night with seventy-five American leeches, at three leechings. His stomach would retain only a tea-spoonful of gum water—solution of bicarb. soda, &c.. Mild injections were frequently used and came away untinged.

His skin became deep orange, reddish brown at some parts. His urine was copiously secreted, two gallons were once reported to me in twenty-four hours; he could at almost any visit pass nearly a pint for my inspection, of a dark reddish yellow color, leaving a thick coat of yellow on the vessel in which it remained six hours. The perspiration was slightly yellow.

At the end of four days his tongue had become pale and moist, pulse still of good strength and much slower. I advised calomel at this moment; he took 5 gr. doses every two or three hours, and one ten gr. dose till he had 25 grs ; he was leeched once during this time. A tepid water injection now repassed, tinged yellow, with old fœcal matter, four or five small evacuations, becoming a brighter yellow, followed in twelve hours, making his recovery certain. He continued to improve; he began of his own will and without my knowledge, to drink strong coffee, the tongue reddened and became dry, and the evacuations ceased. He was leeched repeatedly and without benefit, when Dr. B. told me that he had been drinking coffee for three days. He was placed under my exclusive care, had no more coffee, and the leeching now removed the redness and dryness of the tongue; he took ten grs. calomel ; yellow stoods recommenced, and his convalescence again began, and went on without intermission slowly to health.

During 7 days he had no discharge from the bowels; no bile was thrown into them, but it is not so clear that it was not secreted in great quantities, absorbed into the blood, taken up and expelled by the kidneys. It is more probable that these organs will take from the blood bile ready formed; than that they can combine its constituents.

I think his case could not have been brought to a happy issue but for the performance of this office by the kidneys. The action of the kidneys, so truly vicarious, must be regarded as of the same conservative character as a similar action of the skin preserving the brain from a fatal coma in suppression of urine.

I. T. Holt had formerly evinced a strong liability to congestive forms of fever, and his attack differed from any other I saw. He was attacked in the night, and came under the care of Dr. A. C. Holt and myself in the morning. He was throwing up nothing but mucus or water from violent and frequent vomiting, pulse small; he felt as if a cord were drawn tightly across the epigastrium; extremities cool, benumbed, and with a distressing tingling in them; as this increased the pulse became scarcely perceptible, the features were sharpened, and the breathing convulsive. Two moderate bleedings within an hour, frictions, mustard foot baths, ʒ i of calomel, leeches to the stomach, ice, &c., conducted his case to a happy termination, being convalescent in 4 days. One physician urged that he should *not* be bled; another advised 1-2 grain morphine, and another 10 grains quinine, in the condition he is described to have been on the first morning; but we pursued the course stated and the result justified us.

That I gave more calomel than was really necessary I am well aware ; but it was a safe and natural error, with the sad experience of the preceding few weeks before me. If the sedative effect be obtained at the onset it is surprising how much less is required in the after treatment and how much more quickly the liver responds to it.

In other fevers I give it sparingly, my usual dose being from 10 to 15 grains; blue mass being frequently substituted. If the organs are highly excited, I do not use it. I endeavor to remove that condition so unsuitable for their secretions to be promoted. It was this, so proper in the fevers I had before seen, that led me into what proved my error in the commencement of the epidemic. Remittent, intermittent, or congestive fevers are not curable by maintaining a secretory action in one or all the organs. Yellow fever, on the contrary, is always tending to a cure as long as the liver can be induced to act well. They are very different diseases, and amenable to totally different principles of treatment. Although sanguineous irritations do supervene in the gastro-duodenal surfaces, in a great majority of cases; yet it is not essential to constitute yellow fever. Nervous irritation of certain organs, and the alteration of the blood, are alone invariable and sufficient to form the disease. Mild cases in which no appearance of inflammatory irritation is observed, attest this; and in others the irregular and various periods in which this strangely called "localization" takes place, render it more than doubtful, and being true, dissection should show it invariably.

A red, dry tongue and thirst, do not necessarily imply gastritis. They exist in pneumonia and will gradually disappear under the hourly use of from 1 to 3 grains tart. emetic. A gastritis could not bear this, yet such a tongue does, even in the South. Who will say that the bright red tongue of scarlet fever indicates gastritis? Who has not seen it suddenly blanched by a copious bleeding from the lower part of the trachea? The fluid state of the blood causing a stasis in the capillaries of the tongue, and the important part the lungs bear in the formation of the disease, render it highly probable, that the red, dry and bleeding tongue, is an index to these, as well as to the state of the stomach, and therefore is fallacious if viewed as a guide to the *degree* of inflammation in the latter organ alone. Whether this be so or not, calomel *can* be given with happy effect under appearances of the tongue that would entirely preclude it in true gastritis, or that of other fevers.*

The importance of causing and maintaining a full secretion of healthy bile, and its passage from the bowels, will not be denied; and it is as little likely that a person, without preconceived views, will observe the disease in its gradual progress from mild to severe points, without the conviction, that the liver is the organ upon which is thrown the burden of the disease, and that through is *full* and *healthy* secretion only can it be overcome; sooner or later, by nature or art, this must take place. Not undervaluing the importance of *post mortem* examinations—which confirm rather than discredit the view—yet the language of the organs is often a less erring, a more certain guide; and it is by observing diseases

* I gave ℈ ij of calomel to a patient of delicate nervous temperament on the third day of an attack; he had been restless and sleepless for two days and nights, and had a bright, not deep, red tongue, covered with a thin light fur, showing large red spots on its surface. He soon slept well, and was composed for many hours, and at the end of five hours his tongue had lost the fur, and so much of its undue color, that I indulged the deceptive hope of his recovery.

in their mildest form, that the first organ deviating from health can best be determined.

Thus we find, in persons gradually yielding to the poison of yellow fever, the eyes and urine are yellow the skin has lost its fresh color, becoming sallow, the stools are too pale or deficient in bile, when an attack is near at hand.

A patient may be convalescent, the alvine discharges, previously a deep yellow, become pale, the urine is more deeply tinged, and his improvement is at an end; or a person may have recovered, the same changes occur, and the skin previously clear, becomes yellow; he is debilitated sleepless, and in danger of a relapse; in either, the restoration of the secretion of the liver is absolutely necessary, and sufficient to reinstate the health.

The serum of blood drawn during and before an attack, with a great liability to jaundice, and the more frequent, though less marked, biliary derangements, for months after recovery, point to the liver as the all important organ affected in this fever.

The poison of yellow fever, the source of which being neither animal or vegetable decomposition, separate or combined, as little likely the product of too swampy localities, of streets compactly built and thickly peopled, or of any of the commonly bilious circumstances, this poison, *sui generis*, makes an impression on the nervous system, and produces a disease as different from others as plague is from cholera, small-pox from measles, and as little like remittents and intermittents as it is similar to scarlet fever.

It is impossible to suppose that this impression remains general, *if indeed, it be so for a moment*—but on the contrary, speedily "localises itself," to use a phrase applied to the inflammation which *may* take place at a later period.

It is only necessary to state, for the facts to be admitted, that the secretion of the liver or any organ may be *vitiated*, *greatly increased* in quantity, or *entirely suspended* by an irritation *purely nervous*, and that to produce these results a sanguineous irritation is not necessary.

The function of this important organ being impaired, some of the materials it is intended to eliminate remains in the blood, or are combined and absorbed in great quantity and vitiated quality; for, however copious the secretions may be, it is not well-constituted, it is not healthy bile; and the blood thus vitiated impresses in its turn all the tissues unhealthfully.

It is not supposed that the blood has undergone no change till this derangement takes place in the function of the liver, or that the *first* impression has been made upon this organ; it is known that the blood has become dark and less coagulable, emits a peculiar odour, and may have undergone other changes, long before an attack; and which can only result from a derangement in the function of the lungs; but as the serum is also found yellow, it is certain that the liver is implicated at an *early* period.

It is through the respiratory apparatus, of which the liver is an important part, that the poison influences the system, and whether by its absorption and circulation in the blood, or by a direct impression on the nerves, deranging the functions of the organs to which they are distributed, is full of interest and not devoid of practical utility.

The latter is much the most probable, and it is well worthy the attention of those having ability and opportunity to enquire how far this disease may depend, primarily, upon a derangement, an increased or diminished vitality of the filaments of the pneumogastric nerves, distributed as they are in so many important organs—lungs, heart, liver and stomach, not omitting the pharynx; organs playing conspicuous parts in its formation.

Experiments show the blood to become dark, fluid, dissolved, after injuries of these nerves; and that this effect is owing to the impairment of the branch distributed to the lungs is of course plain.

The poison deranges the functions of the lungs, and besides other effects which may result, the usual amount of carbon, one of the elements of bile;—is not removed from the blood;—it accumulates; the liver should perform its functions with increased energy, the secretion of bile should become copious and well constituted, thus supplying the defect in the office of the lungs.

If the liver responds promptly to this call and continues to secrete freely, in proportion as it is healthful, will the attack be milder, and the absorption of the bile from the intestines or gall bladder, which will take place, even if greater quantities are discharged from the stomach and bowels, will prove less dangerous, less depressing, than the element referred to.

The office of the liver failing at the onset, or during the progress, the worst cases are the consequence.

If it be true that the liver must secrete more copiously in consequence of the blood retaining an element of bile injurious to health, will it not show and explain the importance of furnishing the blood with soda, another element, that it (the blood) may go to the liver, with the principles of bile in due proportion, thus insuring a full secretion and proper constitution of this fluid?

And whether the first change in the blood is effected by an absorption of the poison, or a derangement of the vital action of the nerves, might not much benefit be reasonably expected from remedies coming in direct contact with the interior of the lungs? Could not an atmosphere full of life take the place of one of death, and thus in part, or perhaps more fully, gain the benefit of a removal to a pure air?

The peculiar conditions of the pulse, becoming *slow* as an approach to syncope is induced by bleeding at the very onset, and *slow* in the same remarkable manner in the progress of the disease, in the calm, may well deserve to be attributed to an impression on the filaments supplying the heart—in no other manner can it be accounted for.

The contents of the stomach, in the beginning and progress to a certain point, are thrown up, vomited, as in other cases, but when black vomit appears or has existed for some time, it is no longer the same act, the fluids are not ejected by the stomach, its power of contraction is gone, it expands, a paralysis of its branch of the same nerve has taken place. The abdominal muscles at first may contract violently, they become passive, the effort seems centred in the throat, where may be observed a peculiar movement, an index to the approach of black vomit, and by which this fluid is brought into the mouth and spit out.

As in proportion to the virulence of the poison and the power of resistance, will be the intensity of the attack; so also, one branch of this nerve may be supposed to resist a longer or shorter time, to suffer more or less than others, in particular cases and in epidemics; in some again, the vascular, in others, the cerebro-spinal and ganglionic systems may sympathise more or less quickly and profoundly, producing a great variety of grades and forms in a disease, the same in *reality* in one year as another, in any quarter of the globe, giving the same immunity whether incurred here or there, in early infancy or old age, in one form as in another, and perhaps in mildness as in severity.

The poisons which, whatever be their source, float in the atmosphere, causing various diseases, have a specific tendency, each to certain organs or tissues, and *produce specific modifications in their action;* thus, that of small pox tends to the skin, that of scarlet fever to the skin, and mucous surfaces of the mouth, throat and lungs; of cholera, to the stomach and intestines, &c.; while others chiefly impress the nervous system producing periodicity, a distinctive mark of the class, involving or not various organs in an inflammatory action or congestion, sometimes the brain, the liver not *more* frequently than others, and *less* often than the stomach and duodenum, and so on of others.

And yet, although, the liver is charged with a multitude of sins, of many of which it is entirely innocent, it is lost sight of, despite the overwhelming evidence of its profound impairment in yellow fever, which, could the title be *exclusively* appropriated to it, should be called the bilious fever, and thus the liver held responsible *justly* for, at least, one fever, or entitled to the credit of being the organ by and through which it can be most successfully overcome.

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SUPPLEMENT.

As Dr. Kilpatrick has said in his account of the yellow fever, published in the preceding Number, that there are errors in my statement the of origin of the fever in Woodville, I feel it incumbent upon me to reiterate and attest the facts as I stated them.

I fixed the date of Thurber's sickness *during the night* of the 15th, from his own statement, and that of Rev. Mr. Porter, who slept with him that night. I might have claimed the morning of the 16th, as any one can perceive, and no one will believe that Mrs. Lewis would have placed Mr. Porter in so *delectable* a position as sleeping with a sick man. Dr. Brown and Mr. Soher corroborate this date.

Having satisfied myself that Collins' attack had been of yellow fever, by Dr. K's. admission, the appearance of Collins, and his description of his convalescence, I proceeded to the difficult task of fixing the date of his attack, and succeeded, to my entire satisfaction.

Collins asserted that he had received a load of corn from Lewis the *very* day of his attack; that he had been at Lewis' the evening before to engage it; and that Col. Lewis brought me the date of the 16th July, transcribed from the book of the overseer, as the day on which the corn had been delivered. What can be more conclusive than this? Mrs. Lewis' impression cannot weigh against it. Collins did not say he was taken with the *premonitory symptoms* of *fever*, but that he was *taken sick* on the day he received the corn.

Woodville, Jan. 24, 1845.

DR. STONE,

I was taken sick on the 16th July, 1845. I was at Col. Lewis's house, (not inside) on the day before; that is, on the 15th.

[Signed] J. S. Collins.

It is not usual for people to speak of *premonitory symptoms* of yellow fever.

Dr. K. consulted his day-book *before* he gave me the 19th July, as the day he was called to Collins. He ought to have examined *more closely* at that time; he ought to have seen so *plain a circumstance* as a charge on the 18th for medicines which were for Collins own case, as it is to be inferred he means, and which he came for *himself* and *purchased;* so *conspicuous* a thing as that ought to have caught his eye, if it did not *then*, certainly *before* he gave me the note which is appended, in which the word *dates* is italicized by himself. I sent him the manuscript to be read, that if any errors were in it, they might be pointed out, having no purpose to attain but the truth, which, after much trouble, I am certain I have arrived at.

C. H. STONE, M. D.

Dear Sir.—I have with much pleasure examined your manuscript report of the Epidemic Yellow Fever, which prevailed in our village during the last summer and fall; and I take this opportunity, also, of stating that the *dates* of the two cases which you have enumerated as having been treated by me, viz: of Mr. J. S. Collins and servant girl, are entirely correct; and furthermore, I now consider them to have been cases of genuine Yellow Fever.

Respectfully, &c.

[Signed] AND'W R. KILPATRICK.

Woodville, Miss. Jan. 27, 1845.

From the perusal of this note it will appear plain that he is mistaken in saying he told me Collins *had been feeling unwell for a day or two;* the fact being that he informed me he had *been sick* a day or two, or some days, before he was called. It corresponds so well with the documentary evidence furnished by the overseer's book and Collins' statement, that it is out of the question for Dr. K's. subsequent "reflecting on the matter, and collecting attendant circumstances which had been forgotton," to be allowed to alter the date—something more positive is required.

Dr. Brown, who attended Thurber, says that Col. Lewis did *not* nurse him, (any one here would laugh at the idea that he did,) but whenever he saw him there, he had a seat near the window in the cool, airy room, in which Thurber was sick. Also, that Mrs. Lewis and her sister were not in the room at all. That Alfred was *not* the nurse; he never saw him in the room; and that Mack, as I have stated, *was* the nurse.

Dr. B. is a relative of Col. Lewis; knows every member of the family, and *ought* to know what servant nursed his patient.

I applied to Mrs. Lewis for the dates at which she, her children and her servants were taken sick. She could not remember; but after the publication, she told Dr. K. that she found them correct as stated by me, and *wondered* how I had been able to fix them so accurately.

I formerly allowed that Shaw had been visited by Thurber, but Dr. Brown says he did not, as he believes, for on the second day of his sickness, Thurber complained of seeing so few persons; and when asked why his friend Shaw had not visited him, he replied he did not know. Soon after this, Shaw himself was taken, and *could* not have gone. If this be accurate, it leaves no case that can be traced to Thurber till 31st July, fifteen days.

I have the authority of Mrs. Gildart for saying that the girl of Collins', hired by her husband for three months before her attack, had not been allowed to go to her master's house for a long time, at the particular request of Mrs. C. She is certain that for two weeks, perhaps for four, the girl had not been there, or away from her house for an hour, day or or night—of which she can be *certain*, as the girl slept in her room. I fixed the date of her attack on the 2d August, by Dr. K's. own statement, the accuracy of which he does not contest. Looking back two weeks carries us back beyond Sunday the 21st July, and if we take the probability of four weeks, we are forced into a great difficulty to understand how she could imbibe the germs of a disease from a person near whom she had *not been*; or how she could receive the infection from a person two weeks *before* that person was himself sick. Mrs. Gildart is positive about her statement, and therefore the "northwest wind" still blows, indeed it has become a perfect *tornado*.

If the contagion or infection about Thurber's case was so virulent as to have committed the awful havoc that followed, it will be admitted by the advocates of that opinion that the emanations from Shaw's person must have been *nearly*, if not *quite* as much so.—How fared it with those who were in his room? Thus:—Dr. A. C. Holt, his physician, was taken on the 26th August; I attended him two days, and was attacked 16th. Mr. O. Dorsey was more constantly with him than any other gentleman, and had his attack 14th September; Mr. Beach and Mr. Singleton were in his room frequently, and were taken sick *after* the 24th August. Jim, his nurse, did not have fever for sixty days. Although it is true that Mr. Therrell's is a boarding house, yet the room in which Shaw was sick was far from the public part of the house, in another building and up stairs. Except the above named persons, I think Mr. Therrell himself was the only person there, and he was not a yellow fever subject.

Dr. K. has alluded to the families of Col. Gordon, Mr. Posey, Col. Hamilton, and Mr. Simrall, without giving *dates*, which leaves the *impression* that the cases in their families occurred *soon* after Thurber's, and in consequence of visiting Col. Lewis', as he alludes to the habit in villages of intimate visiting, &c. This latter I admit as a general fact, but having no bearing on the present question; because, neither Mr. Posey, sick on the 12th August, Mr. Simrall on the 11th August, Col. Hamilton 16th August, or Mrs. Gordon on the 1st August, had visited Col. Lewis'—I would go farther than Dr. K. and admit that in villages all the *thoughts* and *dreams* of each are known to the others; it is almost characteristic of small towns. It was unnecessary to have given more cases than I did in the neighborhood of Col. Lewis' or any other, and if the dates had been affixed to the cases named by Dr. K., it would not be necessary again to allude to them. Miss M. Hester was taken sick about the 15th August, (authority Mr. Barber ;) Mrs. Kannear on the 19th ; (auth. Dr. Brown,) Miss Higgins, sister of Mrs. Lewis, on the 17th August, (auth. Dr. Brown :) The servant of Mr. Wailes who was sent to nurse Simrall, was taken sick on the 14th August, and on the same day, his wife, who it will not be contended nursed Simrall.—On the 15th three of Wailes' children were taken sick, (authority of Mr. Wailes.) *Long before this* I could name a host of sick in all parts of the town who had *not* been at Col. Lewis'.

It will be seen that *with* dates these cases amount to nothing, but that *without* them, they convey an erroneous impression.

The facts of Mr. McCausland's case are not *important*, but they are *true* as I stated them ; he was my patient and I obtained the information from him and his cousin Douglass West.

Mr. J. H Stanwood returned late, opened a closet, was nauseated, felt unwell and applied to *me* for advice. I prescribed once, December 3rd, and gave some very *mild laxative medicine*, enjoined quiet ; plain food ; and gave, in addition a large dose of *confidence* that he would *not* be sick, and he was not ; he very naturally was uneasy as to what *might* be, and I might have made him, or any one else take his bed. S. B. Leatherman was in Woodville on the 17th of August and was taken sick two days after shaving Soule *after* the 15th September. He remembers that his urine was very yellow two weeks before his attack and his health had been much impaired for some time, (authority S. B. Leatherman.) It is evident that we should consider his attack attributable to his visit to Woodville, making a period of at least thirty days, after exposure, before the development, and not to shaving Soule, who says that Leatherman was so feeble at the time that he trembled while shaving him. It may be well to state that the inflammation of the parotid glands in the cases of Smith and Revercombe was *certainly* the effects of mercurials.—It is not of consequence to notice other errors as they do not bear upon the question at issue, except I cannot avoid entering my protest against the opinion that this *is*, *was*, or *can* be so filthy a village as it is described by Dr. K. 1844 was Dr. K's. first year here and he may have been accustomed to very *cleanly* towns and not have the means of *comparing* this with former years. I have been here since 1832 and have never seen it more cleanly. I must insist upon the accuracy of the description I gave before ; that is, that Woodville is a cleanly village, and in no particular differed that year from former ones. That persons coming from the country and even some living in town observed a disagreeable odor after the Epidemic began, I have heard too often to dispute or think improbable, but that this was the result of animal or vegetable decomposition, I deny, because nothing exists here to produce it. I have admitted that water was held in some cellars this year, *as formerly*. I live next to the house under which water was said to have been ; the occupant of which died and I never knew any odor to proceed from it. If it had any agency, he (the occupant Haas) would have had his attack before the 4th September. That cellar is only one foot deep on the back part and has a *clean clay bottom*. Indeed it is certain that in Woodville and its neighborhood, all the causes to which Y. F. is usually attributed *are to-*

tally absent, and as it must be plain that Thurber had no agency in the creation of the poison, I consider the occurrence of the disease here calculated to show the fallacy of attributing it to causes, which being absent here, cannot be *the* or the *only* sources and constituting another point of interest of as much consequence as the question of its endemic or imported character—and I hope *this* will receive attention. Dr. K. says, I seem "to endow Mr. Thurber with an exclusive patent for Y. F." in which he is mistaken to the extent that I have been tempted to deny that he had yellow fever *at all*, which I could have sustained in some measure by Dr. Brown's first opinion. I am not *certain* that he did not suffer with this fever, but it is a matter of *doubt*, and it behooves those who argue so important a subject on the evidence of *his* case to establish it beyond cavil.

I have made another attempt to obtain from Mr. Thurber such a description of his condition as might throw some light upon his case. I give his letter in answer to my enquiries.

Franklin, June, 7, 1845.

Dear Sir,—Yours of the 30th has this morning come to hand, and I hasten to answer as correctly as possible. 1st.—I believe my skin to have been but little yellow, if any, but cannot possibly remember how it was. 2d.—The urine I believe to have been quite red. and but little of it. 3d.—The operations from my bowels were quite good during convalescence. 4th and 6th—I was confined on the 16th, kept my bed five or six days—kept my room until Thursday the 25th; on the 26th walked to Dr. Brown's and back; 37th, walked to col. Oswald's, and took a ride with him a few miles, &c 28th attended church, 29th went to judge McGehee's and on the 1st, I think, I rode to Natchez; on the 2d took boat to Vicksburg, on the 3d rode on horseback ten miles in the country, preached on the 4th in Gibson's neighborhood, and rode back to town, riding in the heat of the day, and overdoing myself a little, producing a slight chill on the 5th. Dr. McGruder attended me, a slight relapse was soon checked, and on the 7th I left for Cincinatti, where I had another slight chill, which was the last. From there I proceeded on to the lake, my health continually mending until I became fleshy; after spending some two months in Ohio, I returned South, and my health has been ever since *good*.

5th—I never was afflicted with jaundice tc my knowledge. 7th—I do not remember the particulars, but believe the anterior of the sclerotic coat to have been quite yellow during my convalescence.

With these hurried remarks, Dr. Stone, I subscribe myself

Your most obedient,

W. S. THURBER.

I applied to Dr. McGruder, and I give his answer, in which it will be seen that he thinks it scarcely possible that Mr. T. could have had yellow fever so short a time previous. That opinion certainly shows the *mildness* of his case, if no more.

Vicksburg, June 26th, 1845.

Dear Sir:—I received a letter from you asking information with regard to Mr. Thurber, a preacher from Texas, who came under my care last summer. In reply, I would state that I was called to see Mr. Thurber on the evening of the 5th August. I found him in the sweating stage, having just passed through a paroxysm of ordinary intermittent fever. He informed me that he had but recently recovered from an attack of fever at Woodville, and that his present indisposition was attributable to a ride of ten miles through the hot sun, which had fatigued him very much. Upon examination, I found his tongue furred, bowels constipated, and such concomitant symptoms as are usual in an attack of intermittent fever. I prescribed calomel and rhubarb *aa* 12 grains, which not having acted as promptly as desired, was followed by castor oil; this had the effect to produce copious bilious evacuations. I prescribed 12 grains of the sulph. of quinine to be taken after the cathartic, at intervals, divided into doses, so that the whole might be taken before the hour at which the chill came on the previous day; at which time I called, and found he had not taken as much of the quinine as prescribed; he stated as a reason that it produced a great deal of headache when administered to him by the physicians in Woodville, owing to which circumstance he was afraid to take so much. The consequence of his failing to take the quinine as directed was a slight chill and fever about the same hour of the previous day. I renewed the prescription of quinine and insisted upon his adhering closely to my directions, which he did, and expressed his surprise on the next day, when I visited him, that he had not only escaped the chill, but had not experienced the same unpleasant effects from the quinine which it had produced on him in Woodville. He appeared quite well, and left town on the ensuing day, since which time I had not heard of him until I received your letter. I have been thus explicit in stating every thing that came under my observation, and the remedies used, in order that you might be better enabled to draw inferences concerning the case. This case was a very clear one of intermittent fever, and I think it scarcely possible from the symptoms present, that he could have had yellow fever so short a time previous. Whilst I believe that there are too many well attested facts to deny the importability of yellow fever from one place to another, at the same time I am as fully convinced that it is endemic through the whole southern section of this country, and may originate from local causes independant of such communications. To this conclusion I have been irresistably led by the personal observations which I have been enabled to make.

I am very respectfully yours, &c.

A. L. C. MAGRUDER.

Dr. C. H. STONE.

Although I would not express a doubt that Mr. Thurber came from Galveston at the time stated, yet others may wish to know how it happened that his name is not on the register of the New York in the Custom House, as Dr. Logan informed me was the case, last fall. I have thus given all the facts I could obtain about Thurber, and each can judge for himself, though I may be allowed to give my opinion, which is, that it is probable that Thurber had yellow fever, but in a *much milder form* than Shaw; indeed, of this I feel sure. Thurber and Shaw were strangers, in feeble health, and more liable to succumb if suddenly exposed to the poison *already* surrounding us, than those citizens who had been gradually receiving its impression.

This liability is generally admitted, and was well exemplified in cases of persons from the country. Mr. Collins was and had been for some time in feeble health, and less liable to resist the poison than more vigorous persons.

The first cases were mild, and continued so for some time. The disease was prevalent for one month before the attack became violent.

It is useless to say more.

ART. VIII.—*Obstetric Memoranda—Premature Artificial Delivery.*—By A. H. CENAS, Professor of Midwifery in the Medical College of Louisiana.

CASE I. *Induction of Premature Artificial Delivery for the Relief of Obstinate and Exhausting Vomiting.*—On the 25th of October last, I was called to see Mrs. B., a German woman, aged about 35 years, and whom I found laboring under the following symptoms: great muscular prostration, with emaciation and incessant vomiting, aggravated on the injection of the smallest quantity of fluid. She was pregnant with her fifth child, and according to calculation, was then just entering her seventh month. The nausea and vomiting that generally accompany pregnancy commenced with her very early, and gradually increased in severity up to the present time, when her condition was truly pitiable; confined to her bed from extreme debility, and reduced to a state of almost marasmus, she was nevertheless tormented by insatiable thirst, the slightest indulgence of which sufficed to aggravate the vomiting and induced distressing cramps. I learned that she had been for some time past under the prescription of a physician, who had evidently used all the routine of anti-emetics without success, and she now earnestly implored me to relieve her from her wretched condition. Looking upon the case as one of sympathetic irritation of the stomach from uterine distention, and considering the failure of the previous treatment, I made up my mind, at once, that the only plan that held out any prospect of success, was the induction of premature labor—advised by Dr. Lee, and performed several times by himself and others, in similar cases, and with the most successful results. I was not at once permitted to put my plan in execution, from a vague fear of the consequences entertained by the patient herself; but after several days spent in fruitless general efforts, such as leeching and blistering the epigastrium, use of anti-emetic potions, creosote, lime water, &c., I was the 2d November, earnestly entreated to make the trial proposed. Accordingly, in the afternoon, at about six o'clock, after an examination per vaginam, by which I found the uterus low, soft and dilateable, head presenting and fœtus alive, I ruptured the membranes without difficulty, with a small sized gum elastic catheter, armed with its stiles. The liquor amnii dribbled away slowly all that night, and next morning, November 3d, uterine contraction commenced, which on the 4th terminated in the expulsion of a small but healthy and active female fœtus, of about seven months. It is worthy of remark, that not more than an hour after the puncture of the membranes, and before much water could have been lost, the irritability of stomach greatly diminished, and in the course of the night, the patient was able to retain with comparative comfort a considerable quantity of fluid.

From this date the mother recovered rapidly, and is now in the enjoyment of robust health; but the infant died about a fortnight after its birth, more, however, from neglect of the most ordinary precautions than from a want of viability.

CASE II. *Induction of Premature Labour for the Relief of Convulsions.*—Mrs. A., aged about 25 years, of a short and rather stout habit, and in the seventh month of her second pregnancy, fell suddenly into violent convulsions on the morning of the 23d January last, with no other premonitory symptom than a slight pain in the head. Dr. Rushton, the family physician, was in immediate attendance, and commenced forthwith the most active treatment; but finding at about six o'clock, P M., that the convulsions continued unchecked, he requested my assistance. I found the patient struggling in a severe paroxysm, with cold calmmy skin, small frequent pulse, dilated pupils, and slight stertor in breathing. On examination per vaginam, the os uteri was found low, soft and dilatable, and head of child presenting, but no evidence of uterine action was detected. External examination of the abdomen gave to the hand the sensation of an unusual accumulation of water.

After close consideration of the critical condition of the patient, together with the failure of the previous treatment, it was agreed to puncture the ovum and induce, if possible, premature delivery—this was done, as in the above case, with a small sized male catheter; a very large quantity of liquid amnii immediately escaped, and about two hours afterwards uterine contraction became quite active. At nine o'clock, Dr. Stone was added to the consultation, and at eleven as the os uteri was now fully dilated, the head well down on the pelvis, and the pains flagging, the forceps were applied and the labor promptly terminated; but the child, as was expected, was *still-born.* As soon as delivery took place the convulsions ceased, and Mrs. A., under the charge of Dr. Rushton, slowly but completely regained her usual health.

Premature artificial delivery is now an operation of acknowledged value in midwifery. It has been lately resorted to with success, both to the mother and child, in cases of deformity of the pelvis, which formerly necessitated the use of instruments destructive to the one and extremely hazardous to the other. It is valuable, also, in certain grave affections incidental to pregnancy, even when the pelvis is of the natural standard—such as convulsions, obstinate and exhausting vomiting, (in which the life of the patient is in danger from pure inanition,) and accidental and unavoidable hæmorrhage.

The only question which divides the opinion of accoucheurs is the method of performing it. Dr. Hamilton was in favor of detaching the membranes from the vicinity of the neck of the uterus with a probe; but this method is said to be too slow in cases of emergency. Another plan is the sponge tent, by which the os uteri is gradually dilated, and uterine contractions induced; but this too is slow in its results, and by some is declared to have the opposite effect, viz: of irritating the os uteri, and producing rigidity.

Puncturing the membranes is preferred by several distinguished accoucheurs, amongst whom is Dr. Lee; it has the advantage of being simple in practice and speedy in its results; the only instrument necessary, being a small size gum elastic or silver catheter.

There is one important step in the operation not to be neglected; and that is the presence of a good and healthy wet nurse, for the purpose of giving at once to the new-born infant, the nourishment that it is in immediate want of, and which the mother may not be able to supply for some time after delivery. It was the neglect of this precaution that cost the life of the infant in the first of the above cases.

Part Second.

REVIEWS AND NOTICES OF NEW WORKS.

I.—*An Experimental Inquiry into the Functions of the Lacteals and Lymphatics.* By THOMAS FENWICK, Esq., Surgeon, North Shields. From the London Lancet, April, 1845.

This is an interesting paper upon a very interesting subject. The author, after taking notice of the conflicting opinions concerning the functions of the lymphatics, goes on to say: "This state of uncertainty regarding the functions of any portion of the body, would be sufficient excuse for the introduction of a fresh theory upon that subject to the notice of the public ; but when in addition to this consideration, we remember the interest with which the functions of the lymphatics are viewed by the physiologist, and their importance to the practitioner, it must be confessed that the results of any investigations tending to reconcile the conflicting statements of other enquirers, must possess a considerable degree of value. Since the time at which I first introduced my theory on this subject,* subsequent experiments and observations have tended to confirm me in my opinion of its correctness, and finding that the former paper had not attracted the notice of physiologists, I have been induced to add the results of my later investigations, and to bring it more prominently before the profession."

Our author then goes on to state, that "the prevailing theory of the present day, upon the functions of lymphatics, is, that they possess, along with the veins, the power of absorbing all foreign matters presented to them ; but that the lacteals alone have the peculiar property of taking up the nutriment necessary for the support of the system."

After taking notice of the experiments of the Hunters, who attributed the whole process of absorption to the lymphatics ; and of Magendie, who denied that they absorbed at all, the author begins the enquiry, which he treats under six subdivisions. We shall, for the sake of convenience, follow him in this respect, and offer our remarks under each separate head.

1. *The blood-vessels possess a power of absorption.*

Under this head the experiments of Magendie, Delille, Meyer, Sir B. Brodie, Robinson and others, are cited, followed by a criticism on the

* Medical Gazette, July 21, 1843.

experiments of John Hunter, (which we think just,) the author concluding by saying: "We may, I think, without reserve, determine, *that absorption of the fluids by the blood-vessels is an established fact in physiology, which admits of no dispute.*

2. *Do the lacteals possess any power of absorption?*

In answer to this question, the author cites the experiments of a number of physiologists, and relates some of his own. He then adds: "From the above experiments, we observe that the lacteals frequently, indeed I may say generally, contain the same substances as the blood-vessels. It may be inquired, under what circumstances the blood-vessels absorb in preference to the lacteals; or to the absorption of what peculiar fluids do the latter confine themselves. To this no answer can be returned in the present state of our knowledge, as it appears, from direct experiments, that both vessels are frequently found to contain the same substances at the same time, and under the same circumstances."

The following remarks point to the author's own peculiar theory, which we shall speak of presently: "It would appear, then, that if the present theory of absorption by the lacteals, of all substances, be admitted, nature has formed two distinct and different systems of vessels for the same purpose; that she has given to the lacteals a property which is likewise enjoyed by the veins; that, in their formation, she has introduced an anomaly into her work, and departed from her usual simplicity of operations."

We may now mention the author's theory, which is found under the third section—the heading of which contains its germ.

3. *In what manner do the lacteals obtain their contents from the blood-vessels?*

The substance of the theory is contained in the following extract:

Let us imagine that a quantity of food has been digested in the stomach, and having passed into the small intestine, is mixed with the bile and pancreatic fluid, and consequently prepared for absorption. Whilst it is yet in the stomach, that organ being largely supplied with bloodvessels, some portion of the fluid is taken up—a fact easily proved by the detection of many substances in the urine, so immediately after they had been swallowed, as to prevent us from imagining they could yet have entered the intestine. The bloodvessels of the intestine likewise commence to absorb whatever comes within their reach in a state of solution, whether it be albumen, salts, colouring matters, poisons, or chyle; all are attracted, and enter their walls, by virtue of the law we have before so frequently quoted. The first portions so absorbed, in all probability, are carried at once into the general circulation, as I have never observed substances appear in the lacteals immediately after they have been placed in the gut, although it is well known, from the effects of poisons, and the appearance of salts in the urine, that absorption commences almost immediately the fluid comes into contact with the capillaries. But when the bloodvessels become distended with the fluids they have taken up, and as the heart's action is always increased during digestion, and a delay of their current is produced by the same blood having afterwards to pass through another system of capillaries in the liver, the network of vessels in the villi and other parts becomes congested. That this congestion is generally in the intestine, during digestion, any one may easily satisfy himself by opening an animal whilst the process is going on, and comparing the state of the venous circulation with its conditions at other times. Now, were this congestion to increase, all absorption would be soon stopped, and inflammation of the mucous membrane would ensue, as in experiments 32 and 34; but to avoid this the ordinary lymphatic vessels of other parts are here more numerous and of larger size, (the lacteals,) and the matter so absorbed are by the congestion effused into them. To make this more evident, let us imagine a particle of matter has been absorbed by one of the capillaries, situated on the cæcal termination of a lacteal, and being rolled along with the blood globules, it arrives at a part of the network where a temporary obstruction is caused by a cross current, or by the anatomical arrangement of the vessel itself; here its further progress is delayed; it is pressed upon by the vis à tergo; and its onward tendency being checked by the mechanical obstacles before mentioned, it is squeezed through the coats of the vessel along with a proportion of the serum of the blood, and entering the lacteals forms a portion of the chyle.

In other words, Mr. Fenwick supposes that the lacteals do not absorb at all; and that the absorbed matters are first taken up by the red capillaries, and that matters found in the lacteals are pressed into them from the bloodvessels by the congestion of the latter co-acting with the *vis à tergo*, or action of the heart.

This doctrine we can by no means agree to. The author's own experiments, and those of others quoted by him, do not bear him out. An experiment consists of a fact and an inference; the latter being very often mistaken for the former, and this mistake we think our author has committed. In order to prove that the lacteals do not directly absorb, he relates a number of experiments, three of which we quote, as bearing most strongly on the subject:

Ex. 23. I opened the abdomen of a living rabbit, drew out the small intestine which was empty, and injected some warm milk into it; a portion of the gut so filled was isolated by ligature from the adjoining parts, and deprived of its circulation by placing ligatures on its mesenteric vessels. I returned the parts into the abdomen, and after some time found that the lacteals of the isolated gut were empty, whilst those on the adjoining portions were distended with fluid.

Ex. 26. I opened the abdomen of a living rabbit some hours after a full meal, and observing the lacteals of the small intestines distended with white chyle, I isolated a part of the gut in the same manner as in experiment 23, and returned it into the abdomen. On examining the parts a short time afterwards, I found the intestinal lacteals on the isolated portion were empty, whilst those on the mesentery and neighboring parts were still filled with white fluid.

Ex. 34. Having opened the abdomen of an ass, and drawn out a portion of the small intestine, I separated a piece of the tube three feet in length, into three equal portions, by means of tapes, tied with a moderate degree of tightness around the gut. Through holes in the intestine, I poured into the first some warm milk; into the second, a solution of ferro-cyanuret of potassium; and into the third, a solution of starch, deeply colored with orchil. The holes being tied up, the bowels were returned into the abdomen. After the lapse of some time, they were again examined. It was found that the tape ligatures had acted on each portion of the intestine, in the same way as a tourniquet does upon a limb, by congesting the venous system, so that the whole portion of the bowels was of a dark color, but no extravasation of blood could be detected. The lacteals contained neither milk, prussiate of potassa, nor coloring matter, but were distended either with blood or liq. sanguinis.

Now these experiments by no means prove, as the writer supposes, that the lacteals "cannot of themselves absorb." The first simply proves that the lacteals are empty when the *circulation is cut off*. But in this case the *veins are likewise empty*. Through the red capillaries and veins, and through the lymphatics, a continual current is flowing onwards to the heart; and this current is the great agent of conveying absorbed matters into the system. Cut it off, destroy it, and the phenomenon of absorption is reduced to a simple play of endosmose and exosmose between the contents of the intestine and those of the vessels.

Though the lymphatics, as far as we know, have no *direct* communication at their extremities with the blood-vessels, still, it is obvious that they receive their contents from the arteries. If we wanted proof of this, the experiments of Magendie would suffice. He found that by injecting warm water into the veins of animals, so as to produce distention in the blood-vessels, the lymphatics were always highly engorged; moreover, lymph is identical with the liquor sanguinis. The lymphatics must, therefore, receive their contents from the arteries by the fluids permeating the tissues; and every thing that increases congestion in the tissues, or, in other words, causes obstruction to the venous current whilst the arterial remains free, will produce a flux into the lymphatics; and this is all that experiment 34 proves, with this addition, that engorgement or internal pressure on the coats of the vessels will prevent the absorption of substances presented to them—a fact long ago discovered by Magendie.

The author states on one occasion: "I injected a saturated solution of prussiate of potash into the right pleura of a rabbit, and in five minutes opened the abdomen. The lacteals were distended with a clear fluid, and on touching them with a solution of permuriate of iron, their contents assumed a deep blue color." Now it is obvious that the salts of of iron and potash met inside of the vessels. How did the permuriate of iron get there? Certainly *directly* through the tunic of the lacteals, without the necessity of first passing into the blood-vessels.

The fact is, that absorption is performed neither by lymphatics nor veins, except so far as they may be considered cylindrical membranes. These organs are merely the conduits by which the matters absorbed are conveyed into the system. That such is the fact, must be apparent when we reflect that absorption occurs in the egg during incubation, and in the seeds of plants during germination, before any vessels whatever exist; nay, it takes place in any dead organic substance. The membranes of the intestines imbibe, by a physical law, the solution presented; the current within the vessels takes it from the membranes, and it is swept into the torrent of the circulation. Did that current not exist, there would occur, as has already been remarked, a simple play of endosmose and exosmose between the contents of the vessels and contents of the intestine. It is by not paying attention to the existence of this constant current that the subject is involved in obscurity. We are too apt to think that the current exists in the lacteals only during digestion. But this is not so. The current always exists during life, for the lacteals, when they are not carrying chyle, carry lymph.

In the part they respectively bear in the process, the veins, or rather the capillaries, differ from the lacteals only in two respects, they are more numerous, and the current within them is more rapid. Hence absorbed matters are more usually found in the veins than in the lacteals.

That the author should deny a direct entrance of matters into the lacteals surprises us the more, since he himself, when criticising the experiments of John Hunter, tells us, "that late investigations have shown us, that absorption depends not upon any vital attraction of the coats of the vessels, nor upon any elective or galvanic influence possessed by the capillaries, but merely upon a simple physical law, by which a current of fluid absorbs and carries along with it, any stagnant solution which can permeate the porous texture of the vessels." Now there is a current in the lacteals and why should not solutions in contact with their tunics permeate them as well as those of the veins? The law he speaks of unquestionably exists and is general throughout the body—and applies, therefore, to absorption by the skin—by the lungs, etc., as well as to that by the intestinal canal.

As to the chyle globules and the question of their origin, we do not intend here to speak, more than to say, that it is a question in common with the chyle and several other liquids—as blood—milk—pus—and, indeed, all albuminous fluids. As the intestinal membranes possess no visible pores, the globules are undoubtedly formed in the lacteals themselves.

We have sometimes spoken of the *lymphatics*—sometimes of the *lacteals*, but we do not wish to be understood as making a distinction between these vessels except so far as regards locality. The lacteals or chyliferous vessels, as they are sometimes called, are merely the lymphatics of the intestinal canal and mesentery. They have the same structure—the same functions—and that they sometimes carry chyle is a mere accidental circumstance. We regard the whole lymphatic system as an *additamentum* or supplement to the venous system. The latter carries in addition to the *liquor sanguinis* the red globules—the former carries *liquor sanguinis* only. In this opinion we are happy to find that our author concurs, at least in part.

4th. *The power by which the chyle is moved in the lacteals.*

In this section the author mentions the opinion of some physiologists who attribute the motion of the chyle to the suction power of the heart, which he disproves by the following experiment:

Ex. 41. If a lacteal be selected in the mesentery of a living animal, whilst digestion is going on, and a ligature tied round it, the vessel soon becomes greatly distended, its contents being forced inwards, even when their progress is thus obstructed, and any power of suction from the heart prevented.

He also says that he "is not disposed to deny that the method by which the thoracic duct is connected with the jugular vein may not give rise to a suction power," and exemplifies that such may be the case by an experiment which we shall quote presently. But that this suction power, like that of the heart, can only be auxilliary to other powers, is sufficiently proved by the experiment last cited.

But these, he thinks are not the only causes of motion, because in some of his experiments the lacteals had become empty, although the circulation had been arrested.

The causes of their emptiness, we think, may be attributed in the first place to their natural elasticity. The current being cut off, a distending cause was removed and they contracted, just as all other tissues do under the same circumstances. Such contraction would unquestionably propel their contents onwards to a certain extent—for the valves would prevent regurgitation. In the second place, their emptiness is due to a cause indicated in the experiment of Mr. Robinson, above alluded to, and which we shall here quote:

Ex. 42. A piece of the descending aorta of a horse, which formed a flexible yielding pipe, nearly cylindrical in shape, four inches long and one inch in diameter, was fastened horizontally to the lateral opening of the reservoir. Having previously closed all its collateral branches with the exception of one, I fitted into the latter the short arm of an arched glass tube, the long arm of which dipped into a vessel containing colored liquid. The internal orifice of the pipe was somewhat contracted in fastening it to the opening in the reservoir, so that its calibre at this point would be less than of the rest of the pipe. During the passage of the steam through the latter, the colored liquid rose in the glass tube, and on increasing the height of the impelling column, the vessel containing the colored liquid was speedily emptied.

All the particles of a homogeneous liquid attract each other. But a liquid in motion, has a momentum or power which one at rest has not; the latter will therefore be attracted to the former. To apply this law to the matter in hand, it is only necessary to remember that the lacteals, before they reach the thoracic duct, inosculate thousands of times, forming a complete net-work; so that, though the natural current be cut off in some of them, it is still flowing through others with which they are united. The circumstances are, therefore, pretty much the same as in Mr. Robinson's experiment, and the results must be similar.

The author remarks that in one experiment "the lacteal trunks on the mesentery had become greatly distended by the lacteals of the intestine having urged onwards their contents, although both the circulation of blood was stopped, and the suction power of the veins prevented by the ligature passed around them." He also cites an experiment of Cruikshank to the same purport.

But it is plain that in these experiments the circulation was not completely stopped. His own experiments already quoted in this paper invalidate them. Though it may be admitted that the main vessels were tied, the blood must have found its way to the tissues through collateral sources.

We cannot grant to the author that "a power of motion must be allowed to exist in the lacteals themselves, produced either by contraction of the vessels or the action of cilia." The reasons of such a belief he finds in the mechanical obstructions overcome in the glands and in the fact that the chyle continues to flow after death—that is, after the heart has ceased beating and the respiration is stopped. We grant the facts, but not the influence drawn from them.

In our opinion the cause of motion in the contents of the lacteal vessels has been involved in unnecessary difficulty. *The powers by which the chyle is moved in the lacteals, are simply those which move the blood in the veins.* These powers are the action of the heart and the process of nutrition—or in other words, that chemical transmutation that is continually going on between the arterial blood and the molecules of the tissues. That this latter is one of the powers of the circulation there are facts in abundance to prove; but it would be out of place here to enter upon the subject. In a future number we shall take up the subject in detail. Suffice it now to say, it is this process that empties the arteries after death, conveys their contents into the veins, and lymph (which is but a portion of the blood) into the lymphatics. The nutritive process is not always arrested at the moment the heart ceases to beat; the molecular changes between the blood and tissues go on until the arteries are emptied.

To repeat what has been already adverted to in this paper, through the lymphatics as well as through the veins, there is a constant current kept up during life. When any substance in solution comes in contact with the membranes through the capillaries of which this current is passing, the membranes imbibe the fluid—the contents of the capillaries both red and white, take the fluid from the membranes, and it is swept into the vortex of the circulation. The great powers of the circulation are the heart and the nutritive process—the rest, such as the manner of insertion of the thoracic duct into the subclavian vein—the action of muscles on the veins and lymphatics, etc., are merely auxilliary.

5*th. The functions of the Lymphatics.*

Mr. Fenwick, we think, well describes the functions of the lymphatics in the following words:

It is evident that since the lymphatics contain liquor sanguinis, more of that fluid must be effused than the veins and the capillaries participating in their functions are enabled to take up. Now let us imagine a portion of blood propelled through the heart, entering the aorta, and transmitted through various branches, until it reaches the capillary network in some structure of the body. When passing through the terminal branches of the arteries, it will be compressed by a considerable force, the necessary consequence of which is, an effusion of a part of its liquor sanguinis, the greater portion of which effusion will, in all probability, be at once poured out into the lymphatics; but other parts of it may also be forced amongst the cellular tissue. From this, the various textures of the body may be supposed to select the particles of albumen necessary to supply the place of those which have been carried off by the veins in the form of carbonic acid; whilst of the remainder, part will be reabsorbed by the veins and capillaries, and part be again transmitted into the circulation through the medium of the lymphatics, which it has already entered. Let us suppose the portion of the blood, the progress of which we have thus far traced, now to enter the capillary network. In the first part of this progress, in the usual condition of the body, effusion of the liquor sanguinis still continues, as in the arterial branches, until the compression, being relieved by the effusion, the current flowing equally forwards, commences the opposite action, or that of absorption. The capillary in which the blood is contained now becomes more distended by the fluid it has taken up, and may suddenly enter a more rapid stream, or, by other circumstances, the motion of the blood may be obstructed, when a temporary and slight congestion will ensue. Slight though it be, effusion must result, and a part of the liquor sanguinis, along with any small globules the current may have absorbed, is poured out into the lymphatics, until, from the removal of the cause of eversion, the former state of the current is restored, and absorption re-commences. Thus may the same blood continue to absorb and effuse until it reaches the veins, by which it is conveyed to the heart, and mixed in the right auricle with that part of its liquor sanguinis which had been transmitted by the lymphatics. From this view we can easily understand how bile and other fluids, after being absorbed by the blood, had been effused into the lymphatics, in the same manner as the substances absorbed in the intestines appear in the lacteals.

The following is interesting, and contains views, which, at least, are very plausible, and to us new:

Let us go a step further, and suppose erysipelas of the leg sets in where an old ulcer had last existed. In such cases, very generally, one of the first symptoms observed is a tenderness and swelling of the convoluted lymphatics called the inguinal glands. Congestion had occurred in the leg, and effusion into the lymphatics had ensued, which had not been remarked, until from the obstruction to the free flow of the fluid through the gland, the swelling is perceived. But inflammation now appears in the leg itself; the lymphatics can be traced as red lines running up the thigh. Most surgeons are in the habit of saying, under such circumstances, that the lymphatics have absorbed some irritating matters, and have thereby themselves become inflamed. But even granting, for the sake of argument, that lymphatics can absorb, where is this irritating matter to be found? In most cases of this nature, the inflammation appears to depend upon constitutional derangement, or atmospheric influence, and seldom is it that the chance of applying any noxious matter has been afforded. The truth appears to be, that the lymphatics merely contain liquor sanguinis—or if any other ingredient be mixed with it, in all probability it is merely globules of blood effused by the congested vessels—and that they, being distended, act as any other pressure, as, for instance, an abscess, in inflaming the skin immediately over them. But it may be objected to this, that they are not to be traced as red lines in all cases of inflammation. This is true, as in many, the deep lymphatics, and not the superficial, will be congested, and besides, inflammation of the skin is not always produced by the same means in all constitutions: thus, the bite of an insect, or the sting of a wasp, which in one person excites violent inflammation, in another scarcely produces any visible effects.

6*th. The functions of the Lymphatic Glands.*

In this section the author does not advance any new opinion. He thinks "that in the mammalia, the lymphatics are convoluted together in certain convenient situations, near the veins, and being freely supplied with blood vessels, a portion of their contents delayed in the glands, will be absorbed by the capillaries, (which from the ease with which injections pass from the vasa inferentia into the veins) may be considered to be principally absorbent vessels. Now we know that fibrine is of the same chemical composition as albumen, and as, from the researches of some physiologists, an increase of fibrine is found in the lymph, after passing through the glands, it is probable that the whole difference in the contents of the vasa inferentia and the efferent vessels, consists in the greater inspissation of the lymph contained in the latter, the more watery parts of the fluid having been re-absorbed by the venous capillaries."

We must now take leave of this subject with the remark, that perhaps in all physiology there is not one more interesting. Upon a knowledge of the true principles of absorption depends more than upon any thing else a correct view of the higher functions of nutrition and secretion.—The strange facts discovered by Dutrochet, threw the first light over these obscure phenomena, and it is with pride we can mention that none have advanced the subject more than our own countrymen, Drs. Mitchell, Faust, and Rogers. J. H.

II.—*Medical Jurisprudence;* By Alfred Taylor, Lecturer on Medical Jurisprudence and Chemistry in Guy's Hospital. Edited, with Notes and Additions, by R. E. Griffith, M. D., &c. Philadelphia, Lea & Blanchard. 8v. pp. 539.

Within a few years past many valuable works have appeared on this interesting and complex branch of knowledge; but notwithstanding this nothing could be more *apropos* than the appearance of the present treatise. Some of those heretofore published, have been on special departments, and others, written with a view to render them comprehensive, have been diffuse and cumbrous, and have to some extent defeated their ends, by often treating of matters beyond the legitimate limits of medical jurisprudence, and on which a medical witness is not expected to give evidence.

The eminent attainments of Mr. Taylor, and the high reputation he so deservedly enjoys as a medical jurist, as well as the acknowledged merit of his former works on this subject, led us to expect, in the present, a work of pre-eminent excellence. We have not been disappointed; for he has really established strong claims to the gratitude of both the medical and legal professions, by producing a treatise so admirably adapted to their wants. Every topic upon which a medical witness is likely to be interrogated, every thing that a coroner or lawyer can make available in criminal or other medico-legal questions, is treated of in a clear and definite manner. The style of the work is plain, simple, intelligible and concise; and while the author avoids every thing like unnecessary expansion, he at the same time avoids, with equal care, the degree of condensation that would be incompatible with a lucid exposition of his subject.

The work, as it came from Mr. Taylor's hands, intended for the use of the English public, was not well adapted to the use of American professional readers, in consequence of the circumstance, that the laws of Great Britain define and view some crimes in a manner different from those of some of our States. This defect, however, has been in a great measure remedied by the very excellent commentaries and annotations thrown in by Dr. Griffith, the American editor. One of the most interesting features of the work, consists in the numerous and well selected cases which illustrate the principles inculcated, and it is not possible to describe the cases in language more concise and clear than that which is used.

He starts out by very appropriately defining medical jurisprudence, as "that science which teaches the application of every branch of knowledge to the purposes of law." The claims of this science to especial study are based upon two grounds: 1st. "That the subjects of which it treats are of practical importance to society." 2d. "That they are not included in other branches of medical education." We cannot but concur with the author, in respect to the importance of making this branch a speciality; more particularly in large cities and thickly peopled districts, where crime is of frequent occurrence, and often of difficult detection.—No one can doubt that in this country, where so little attention is generally paid to the minutiæ of primary and preliminary examinations, there are numerous cases of escape, without even a suspicion of crime. Neither can it be doubted that criminal convictions would be much more frequent if coroners' examinations were made by, or under the direction of persons whose senses, as well as intellect, were trained to the detection of the traces of crime, under all the difficulties and intricacies which she generally throws around her path. We cannot but believe that one of the most important measures to insure the safe administration of justice, to render the punishment of crime more certain, and at the same time to protect the innocent against false accusations, would be to render the office of coroner a medical one, and thereby to hold out inducements to members of that profession, to give a more especial attention to the study of those branches of knowledge which bear most directly upon the forensic department of medicine. No one who has had occasion to observe the progress and result of criminal trials, could have failed to observe, how often the conviction depends upon facts and circumstances observed and noted by the coroner; and how frequently those escape, of whose guilt the law would never have entertained a doubt, were it not for the absence of a kind of observations which can only be made by those who know exactly what is to be observed and noted in each particular case. How often, in this country, do we see wise-acre justices of the peace hold coroners' inquests, in cases which never could be rendered comprehensible to them, even if Orfila himself were to explain it to them. Sometimes, it may be, that he calls a physician to his aid, who is equally skilled with himself in interpreting what may accidentally come under their notice; and the two, acting with beautiful concert, either dismiss the case, or lay the matter before a higher tribunal, in such a form as to lead to the condemnation of the innocent; or to render it impossible for the learned judge to make head or tail of the case. With whom does the fault rest? Certainly not with the physician; for what inducement is held out to him to spend his money and time in the purchase and perusal of works, which would only qualify him for the performance of arduous and disagreeable duties, when the ignorant, as is now the case, are to reap the profits? Who will fully prepare himself for the performance of duties, which he will be called upon to perform only a few times in the course of his professional career, and for which the magistrates really receive the important fees, while the physician, if paid at all, receives a pittance too contemptible to mention.

In the section of the work in reference to poisoning, our author follows very much the arrangement, &c. adopted by Christisson. He makes some interesting remarks upon the imperfection of the usual definition of a poison:

"A poison," he remarks is commonly defined to be a substance, which, when administered in small quantities, is capable of acting deleteriously on the body. It is obvious that this definition is too restricted for the purposes of medical jurisprudence. It would, if admitted, exclude a very large class of substances, the poisonous properties of which cannot be disputed."

He illustrates his position by relating many deaths and trials which have been consequent upon the administration of articles which would not come within this definition. As for example, a trial for feloniously killing, by the administration of sulphate of magnesia; others for employing the sulphate of iron, &c. He also considers pounded glass, pins, pieces of sponge, &c., as capable of producing death, by inducing inflammation, &c., merely by mechanical action; but notwithstanding, such cases would not be considered as cases of poisoning. The American editor suggests the following definition, as one that will cover all cases that can arise, viz: "A poison is a substance, which, when taken internally, or applied externally to the surface of the body, is capable of destroying life, without acting in a purely mechanical manner.

That portion of the work in reference to wounds, is an interesting and complete summary of every thing that is known of the matters under consideration.

In this edition of the work, the author devotes a chapter to the kinds of circumstantial evidence which may be made available in relation to the detection of crime. The chapter on circumstantial evidence respecting wounds, is especially interesting. Our author regards this kind of evidence of the highest consequence, more especially in exciting suspicion and directing our investigations. In this country, too little consequence is generally attached to the bearing of the circumstances which a keen and practised scrutiny will frequently detect on the persons of the dead, or things by which they are surrounded when discovered; and which have frequently, under a skillful direction, fixed the criminality almost as definitely as an eye-witness could have done.

"Circumstantial evidence," says Mr. Taylor, "is allowed to prevail in the conviction of an offender, not because it is necessary and politic that it should be resorted to, but because it is in its own nature capable of producing the highest degree of moral certainty in its application. Fortunately for the interests of society, crimes, especially those of great enormity and violence, can rarely be committed without affording vestiges by which the offender may be traced and ascertained. The very measures which he adopts for his security, not unfrequently turn out to be the most cogent arguments of guilt. On the other hand, it is to be recollected that this species of evidence requires the utmost degree of caution and vigilance in its application.

"There are many cases on record in which the observance of slight and unexpected circumstances, by medical men, has led to the detection of offenders. In the life of Sir Astley Cooper it is stated, that when called to see Mr. Blight, of Deptford, who was wounded by a pistol shot, in 1806, he inferred from an examination of the localities, that the shot must have been fired by a left-handed man. The only left-handed person near the premises was a Mr. Patch, a particular friend of the deceased, who was not in the least suspected. This man was tried and convicted of the crime, and made a confession of his guilt before execution."

In the perception of these important, but often accidental, and apparently trivial circumstances, there is a vast difference in the natural capacities of different men; but education, or practice, has likewise a most important influence. A person, in order to make examinations of this kind to the best effect, should be educated in the highest degree, both in intellect and the powers of perception. Every one knows the effect of habit and use, in enabling those who pursue certain modes of existence, readily to detect marks, and circumstances, with the observation of which they are familiar, and which are imperceptible to the unpractised senses of others. This is frequently observable among wild and otherwise uncultured people, who from the observation of things only perceptible to their keen and practised senses, arrive at conclusions with a degree of certainty which is altogether unconceivable to the uninitiated. Medico-legal examinations, in order to furnish satisfactory circumstantial, as well as other kinds of evidence, should be conducted with a nice attention to the minutest particulars of locality and other circumstances of the suspected crime, and should be made under the direction of a cultivated, quick, and practised intellect.

The remarks of the author on the means of distinguishing the cause and mode of death, and on the reasons for suspecting murder or suicide in particular cases, are very interesting. His chapters on infanticide, cannot be be read without satisfaction and instruction; and the same remarks apply, likewise, to those of most of the remaining subjects on which he treats, for we cannot fail to observe that he treats his subjects as a master, who is perfectly familiar with them in all their bearings.

It is, however, a matter of regret, that all the chapters of this work are not so satisfactory as we could desire; and some of them appear to us decidedly poor. It may, perhaps, arise from an unsettled state of opinion among medical men, upon the subjects, or from a desire felt by the author to present them merely in their present legal position, and from the dread of innovation; but whatever may have been the cause, certain it is, that some parts of the work leaves a feeling of dissatisfaction on the mind of the reader. Take, for example, the chapter on rape, and though it may be allowed to be a tolerable abstract of matters relative to the subject, it does not convey the kind of knowledge which is required at the present day, to constitute a respectable medical witness; much less to qualify a person as a medical jurist. We hoped that a man of Mr. Taylor's high attainments and reputation, would certainly have exposed and taken a stand against the palpable and preposterous absurdities, which constitute the basis of the laws of most countries, respecting this crime, which we cannot but regard as one of the most grave that can come under the notice of the medical jurist. The laws of most countries base the definition of this crime on either rupture of the hymen or on penetration; and in almost all, penetration is regarded as an essential feature of the crime. Now this definition, is not only in the highest degree arbitrary, but decidedly unjust, and the punishments are administered without reference to the real nature of the offence, and without any precise relation to the real injury inflicted. Laws are too frequently constructed so as to leave entirely out of view, injuries which affect only the moral and social relations of individuals; and the laws respecting this offence certainly pay little regard to these points. There are numerous cases of recent date on record, which show conclusively that fecundation and pregnancy may take place in cases when even a slight degree of penetration of the male organ was impossible; in consequence of atrasia more or less complete of the vagina, or of almost entire occlusion of the vulva. Colombat* relates a case from Arnaud, in which the labia were so completely adherent, that the vulva only presented too little orifices, one through which the menses escaped, and the other was opposite to the meatus urinarius. Notwithstanding the almost complete occlusion of the vulva, she married and became pregnant. When labour came on, the cohering parts were divided with scissors. In a recent number of the Gazette des Hopitaux, there is a still more striking case, in which pregnancy took place, notwithstanding that there was extreme narrowing of the vagina, and only a minute external orifice, and in which there was no possibility of the semen having been more than merely applied to the small orifice, and it must then have been carried up the genital canals by some force or influence residing in the parts. In such cases as these, no violation could come within the definition of rape, since perforation was impossi-

* Diseases of Females; Am. edit. p. 77.

ble; there was no rupture of the hymen, and in fact nothing that could come with the present legal acceptation of rape, even though they might have become pregnant in consequence of the seminal fluid being brought or thrown against the little external orifice, in forced cohabitation.—The truth is, that recent facts and researches would seem to show, that when the seminal fluid is placed in contact with the external orifice of the vagina, it may be carried, by some unknown action of the parts, up the vagina to the uterus and from thence by the fallopian tubes to the ovaries, and thus induce pregnancy. This might take place, even without contact of the male organ, by the ejaculation of the fluid, or by the contact of the clothes, which may have been smeared with it, in the struggle of resistance to the attempt. Be this as it may, there is no doubt that contact of the male organ to an impenetrable orifice, accompanied by ejaculation of the fluid, is all sufficient to induce fecundation, and should therefore be regarded as coming under the head of "carnal knowledge," as spoken of in law.

The definition of rape, however, is not the only point that we would regard as defective in the laws and enactments relative to this crime.—The punishments, based, as they are, on arbitrary distinctions, are far from commensurate with the various grades of the injury inflicted upon the person, character, morals, and standing of the injured individual.—The only really just mode of measuring penalties, is to keep, always, in view the effects which are likely to result; and to punish rather for the aggregate result, than for the primary action. In cases of rape, the primary act is perhaps a physical one, penetration, but the injury inflicted is not limited to this; and we may have resulting from it, one or more conditions which materially injure the individual, and of which the various combinations might serve as a measure to the enormity of the offence.—These conditions are as follows, viz: 1st. Physical violence may be inflicted, which may injure the genitals or other parts of the body. 2nd. Venereal diseases may be communicated. 3rd. Pregnancy may be induced. 4th. The woman's social relations and character are necessarily affected, and a degree of infamy always attaches itself to her; for society though it may pity her, seems to forget that she may be entirely blameless, and views her infamy almost in the same light as if it had originated in a crime of her own. 5th. The moral effect on the individual herself is of a nature deserving of serious consideration. After all, this would seem to be the worst feature of the crime; for the individual can never recover from the sense of degradation, with which a crime so abominable must always impress a pure and virtuous mind.—The current of her thoughts are thrown into new channels, to assume, too often a turbulent course; erratic feelings may be created, and thinking herself branded with an indelible stain of infamy, and regarding herself as an outcast from the society of her pure associates, she may avoid them, to seek associates, and attempt to hide her shame, amongst those to whose condition she feels herself degraded. Thus may she be driven by the stings of a wounded spirit, from one thing to another, to disappear at last beneath the surface of the immense sea of prostitution—a victim, not of her own acts, for she was perhaps entirely innocent, but of an abominable crime, which from some of the senseless technicalities of the law, was perhaps allowed to go unpunished

Why, we would ask, is the crime of rape punished at all? certainly not for the mere circumstance of penetration, or rupture of a membrane; for these would be circumstances of little moment, were it not for the fact that they necessarily imply a violation of the rights, feelings, or moral and social relations of the woman. It is then for these effects that the crime should be punished. But who will maintain the principle that violence may not be done to the feelings, morals, or standing of a woman, unless penetration or rupture of the hymen has been effected. If then the worst and essential features of the crime may be produced without penetration, we cannot see the justice of insisting upon this as an absolute requisite to the commission of the offence.

We are strong advocates for the supremacy of the law; but we would like to see our legislators adapt their enactments to the existing state of science and opinion; for nothing tends so much to undermine the respect for the laws, as an impression that they are not improved so as to harmonize them with the progress of general knowledge.

Mr. Taylor's work is, however, fully deserving the confidence of the medical and legal public; for the faults above mentioned seem to be incidental to the subject rather than to the work, and we know of no work upon these matters against which very much the same kind of objections may not be urged. The truth is that this work is adapted to so large a class of readers, and so well calculated to interest even unprofessional men, that more good may be expected from its publication than from any that has yet appeared. W. M. C.

III—*The Anatomy and Diseases of the Urinary and Sexual Organs: containing the Anatomy of the Bladder and of the Urethra, and the treatment of the obstructions to which these passages are liable.* By G. J. Guthrie, F. R. S., Surgeon to the Royal Westminster Opthalmic Hospital, &c. From the third London edition. Philadelphia: Lea & Blanchard, 1845. pp. 150.

The diseases of the genito-urinary organs, admitted to be of frequent occurrence, exercise a controlling influence over the moral as well as physical well-being of man. Directly connected with the most important of the animal faculties, reproduction, it is no wonder, that they should be often overtaxed, and consequently seriously deranged. Accordingly, as might be expected, the surgeon is frequently called upon to treat the various diseases of the reproductive organs. including also the urinary apparatus. With actual structural or mere functional diseases of these organs we not unfrequently find associated a morbid irritability of the brain perverting the moral faculties, rendering the unfortunate subject a misanthrope, disqualifying him for all mental efforts, embittering his life, and casting a shadow of moral darkness over his fortune. The sympathetic connection between the brain and the genital organs is strikingly illustrated in the arts and stratagems resorted to, to bring about the union of the sexes. Indeed, this influence of a purely passionate nature upon the most noble organ of the body, is absolutely requisite to accomplish the great work of reproduction.

Independant of the serious effects of the diseases of the genito-urinary organs upon the general system, they call up a hideous train of morbid impressions, which not unfrequently conduct the unfortunate sufferer to the verge of despair, and sometimes urge him to suicide. Who ever saw a sensible or rational man with a stricture of the urethra, or a spermorrhœa, who did not harass his medical attendants for a speedy cure ; the mind under such circumstances is constantly dwelling upon the most painful subjects, such as impotency, and its attendant evils.

In all highly civilized communities, where the appetite is freely indulged and the passions gratified because the means are constantly at hand, the diseases of the genito-urinary organs are both frequent and complicated.

Add to this that the genital organs have more than one function to perform in man, this must render them much more liable to derangement and disease ; for we are taught to believe, and observation confirms the teaching, that the liability of any organ to functional or structural change is just in proportion to the plurality of offices it is called upon to execute. Here, then, in addition to the other causes already mentioned, we have another, ever acting to claim the attention of the surgeon. In many countries the treatment of the diseases of these organs form a special department. In France, M. Amusat, Civiale, and others, pay particular attention to the maladies of the genito-urinary apparatus; and in other parts of the world, many distinguished surgeons have written treatises upon this department of surgical practice. In consequence, much important light has been shed upon the nature and treatment of these, in some measure, special diseases. Amongst this number, may be classed the excellent work before us, by Mr. Guthrie, who has long enjoyed a high reputation as a practical surgeon. It is to be regretted that mankind in general is so little acquainted with the causes, nature and best mode of treatment of this class of diseases ; for this very ignorance lulls their apprehensions, or induces them to rely upon inadequate means. Great and lasting injury is frequently inflicted upon men laboring under the various affections of the genital organs, by a mode of treatment, not founded upon a correct knowledge of the anatomy of the parts, much less clear and distinct ideas of their peculiar diseases ; hence, the patient generally fails to invoke the healing art, until every nostrum known to quackery has been fully tested, and found unavailing. He now presents himself at a consultation, distracted with fear and wavering with doubt, as to the curability of his case. If the honest surgeon does not give him positive assurance of a speedy delivery out of all his troubles, he becomes impatient, and flies with eager joy to " some manufacturer of specifics," whose ignorance of medicine is only excelled by his unblushing assurance. Quackery has found more dupes, and done more mischief in this, than any other department of practice. Men, even of intelligence, daily consult the lowest order of quacks for these diseases ; which, of all others, demand a more thorough acquaintance with the structure and functions of the parts implicated than, perhaps any others in the entire catalogue.

The first chapter of his neat little work is on the " structure of the bladder ;" and no writer has given a more clear and satisfactory description of the anatomy of this organ than Mr. Guthrie. Each muscular fibre, every faciculus, is traced and distinctly described; in some respects, new views are advanced, and others that have long been entertained by the profession, are demonstrated to be erroneous. Next in order, follows a clear and concise description of the "structure of the urethra," which is the best, we think, to be found in the same number of pages on record.

The subject of "strictures," both "spasmodic and permanent," is the third chapter. Spasmodic stricture is liable to seize those who, when laboring under a gleet or gonorrhœa, commit some excess in stimulating beverages. They are seized with a desire to pass the urine frequently; it flows in a small stream, after much effort; finally, as the irritation about the neck of the bladder becomes considerable, spasmodic action in the muscles surrounding these parts arrests entirely the stream of urine. The distress increases hourly; the patient implores the physician for relief, which may be afforded by rest, the warm bath, hot fomentations, and a decided anodyne, preceded by a bleeding when called for. In cases when the irritation is not excessive, and the spasm not too violent, the catheter may at once be passed into the bladder, and instantaneous relief procured.

The proximate cause of spasmodic stricture is irritation, which prevents that consentaneous action between the bladder and the muscles surrounding the urethra, necessary for the expulsion of the urine. In order that the urine may flow, the contraction of the muscular fibres of the bladder must be attended with a relaxation of those situated at the opening into the urethra; in other words, irregular muscular contraction may be assumed as the cause of spasm.

The treatment of spasmodic stricture is illustrated by some apposite cases. Indeed, the therapeutics should be such as are generally adapted to allay spasmodic irritation in any organ of the body.

For the management of "permanent stricture," Mr. Guthrie seems to prefer, when the passage is not completely obstructed, gradual dilatation by means of bougies. M. Amusat, denies, however, that the track of the urethra is ever entirely impervious; for he declares that, although the urine can not be made to flow, yet tepid fluids can, under these circumstances, be forced into the bladder without pain or difficulty.

When dilatation fails, Mr. G. resorts to caustic, and only in the last extremity, to the cutting stylet. Pressure is well known to be a powerful means of destroying by absorption many, and, indeed, almost all kinds of morbid growths. In obstructions of the urethra, this principle has been made available with signal advantage.

It may be effected by carrying a suitable-sized bougie down upon the stricture, and then confining it for a few hours; then it is withdrawn, and again applied at proper intervals, as before.

This work is entirely practical; the reader is introduced to the case under treatment; he sees Mr. Guthrie's masterly manipulations; he follows his movements, and subscribes to the principles on which he founds his views as to the best mode of treatment.

Mr. Guthrie's experience is immense, and this book contains that experience on the diseases of the genito-urinary organs, in a few words. There are many diseases to which these organs are liable, that have not been mentioned in the volume before us. This, however, is the "first" part of the work. The "second," which is to follow with the least possible delay, will treat of the chronic complaints, of the prostate, the diseases of the bladder, calculous affections and the various operations for stone. The book may be found at J. B. Steel's, 14, Camp street. A. H.

IV.—*The Dispensatory of the United States.* By George B. Wood, M. D., Professor of Materia Medica and Pharmacy in the University of Pennsylvania, &c., and Franklin Bache, M. D., Professor of Chemistry in Jefferson Medical College of Philadelphia, &c. Sixth edition carefully revised, pp. 1368. Grigg & Elliot, Philadelphia: 1845.

When the fifth edition of this very valuable work was issued it afforded general satisfaction, from the circumstance that great pains had been taken in bringing the materials up to the state of science at the time of its publication. Every medical man will no doubt look to the present edition with the expectation of finding similar advances, but will probably be in some measure disappointed, as it is hardly more than a reprint of the last edition, having the same number of pages, and the same matter in almost every particular. It is true a few compounds and preparations have been brought in, which should have been in the last edition, as some of them have been long in use; *the solution of the iodide of arsenic and mercury*, for example, which has been in use since 1839, is now brought into the appendix of the work. Edition after edition of this work is brought out, and some of the most interesting and useful articles of the Materia Medica are perseveringly omitted. What the reason of this is, it is difficult to conceive; the learned authors must have some good reason for it; and we cannot attribute it to want of knowledge of those substances, for they are familiar to a large proportion of the intelligent practitioners of this country. Take, for example, two of the salts of *quinia*, the *iodide* and the *ferrocyanide*. It is true that a passing mention is made of the latter at page 239, under cinchona, but nothing is said of its peculiar properties, and the authors seem to rest satisfied with accepting the assertion of Pelouze that this salt is only pure "quinia mixed with a little prussian blue;" an idea that is certainly erroneous. The *iodide of quinia* seems not to be mentioned, and this is not easily accounted for, if, as we cannot doubt, the authors were acquainted with this salt and its valuable powers. It may be that here, where every disease so often presents the conditions which call for the administration of quinine or its compounds, more attention may have been paid to the peculiar combinations especially adapted to particular conditions and complications of disease. However it may be, the preparations above mentioned are in common use, and are each found to be admirably suited to cases in which other preparations of quinine cannot be used to advantage.

In a work like the one under consideration, the wants of the profession should, as far as possible be supplied. Take the iodide of quinia for example; this substance does not keep well, and should be prepared a short time before being used. It is easily made; the means of its preparation are in the power of almost every practitioner in the country; but in what work are we to look for the directions for its preparation, if they are omitted from the elaborate work of Wood and Bache. The same remarks might be made in relation to several other substances, but these are sufficient. It is true that these substances are not officinal, but are certainly of so much value as to deserve a share of notice in some part or other of a work like this.

Such omissions, however, weigh as nothing in estimating the value of such a work as the one under consideration, which as a text book, and as a convenient book of reference for the physician, is decidedly the best ever published in this or any other country. As its value is well known and appreciated by physicians, it would be useless to dwell further upon its merits. One of the strongest recommendations of this work is in its almost universal adoption as a text book in all the medical schools in the United States, and it may be safely asserted, that to the student who wishes to obtain a correct and practical knowledge of the materia medica in use in this country, it is better than all the others taken together.

W. M. C.

V.—*A Treatise upon the Diseases and Hygiene of the Organs of the Voice.* By Colombat de L'Isere, Chevalier of the Royal order of the Legion of Honor, M. D. &c., &c. Translated by J. F. W. Lane, M. D. 12mo. pp. 220. Boston, 1845.

This excellent work of Colombat de L'Isére, has just been translated, and issued from the American press. The book is neatly printed, but the wood-cuts by which it is illustrated are no better than those which mar the pages of most of the American republications; they are so bad, indeed, that they would rather have the effect of confusing, than of illustrating the subject, and a natural conclusion might be deduced, that a book could not be worth reading, if it did not deserve something better in the way of engravings. The work, however, is one of sterling merit. The position of the distinguished author, as the founder and head of the Orthophonic Institution in Paris, has afforded him an opportunity of observing and studying almost every variety and grade of modification to which the voice and its organs are subject. The fact, too, that he has made a speciality of the diseases of females, and has studied the laws of sympathy in all their relations, has enabled him to do more than merely to point out the lesions and affections incident to the organs of voice themselves, but likewise to enlighten us in respect to the interesting subject of the sympathies and relations by which those organs and their functions are linked with other and distant parts of the economy.

The author first describes the mechanism of the vocal apparatus, the nature of articulate and musical sounds, and the means by which they are produced and modified.

He then proceeds to consider the diseases of the vocal system which are connected with structural lesions of the organs themselves, and the connexion or relations of these lesions with diseases of other parts of the system.

Finally, he gives an excellent view of the affections and modifications of the voice which are purely sympathetic or relative. This is, perhaps, the most interesting portion of the work, for here the author labors to elucidate all those curious relations which are established by nervous and sometimes obscure connections. He points the bearing and value of these phenomenas as means of diagnosis and prognosis. He thinks that he establishes the principle that the voice is not a mere physical phenomenon, but a result modified by the vitality of the organs which produce it.

"The voice" he says, "is not a simple vibration, but it is living, and is animated like the organs that produce it. The voice being the sonorous expression of our feelings, it must necessarily change with those which it expresses; it must then be modified by the diseases which influence the vitality of the whole system, or which react sympathetically upon the vocal organs."

The following is the classification of the affections of the voice adopted by the author:

First Species.—*Idiopathic cases of aphony and dysphony, arising from a physiological, anatomical, or traumatic lesion of the vocal organs.*

These may be caused by the inflammations of the larynx, by those of the trachea, bronchi, isthmus of the fauces, tonsils, uvula, veil of the palate; by laryngeal phthisis, œdema of the glottis, thickening of the pharyngo-laryngean mucous membrane, atony and paralysis of the muscles of the pharynx and larynx, spasm of these organs; by falling down of the uvula, by its division with that of the veil of the palate and of the palatine bones; finally, by wounds or contusions of the larynx and trachea, or an opening situated below the glottis; finally, by the section or lesion of the laryngeal and pneumo-gastric nerves, etc., etc.

Second Species.—*Aphony and dysphony symptomatic of certain diseases which affect the whole economy.*

These may be caused by adynamic and ataxic fevers; by some worm affections; by pulmonary phthisis; by aneurism of the aorta which then compresses the left recurrent nerve; by lesions of the spinal marrow, excessive swelling of the stomach; by apoplexy, hemiplegia, anemia, general weakness, convulsions, epilepsy, hysteria, catalepsy, chorea, insanity, cholera, frenzy; acute moral affections, such as fear, anger, joy, etc.; finally, by the abuse of ardent spirits, and the introduction into the economy of some poisonous or narcotic substances, etc.

Third Species.—*Sympathetic aphony and dysphony, depending upon the reaction which results from a pathological condition of certain organs more or less remote, and having no immediate relation with the vocal apparatus.*

These may be caused by a falling down or an enlargement of the womb; by the presence of a polypus in the cavity of this organ; by ulcerations situated about its neck; by the state of pregnancy; by amenorrhea; by dysmenorrhea; the sudden suppression or the commencement of the menses; by the swelling or inflammation of both testicles; by chronic hepatitis; a derangement of the system of the vena porta, or atony of the primæ viæ; finally, by the suppression or diminution of a natural or artificial discharge, by perspiration too long continued or suddenly stopped, especially about the feet and cutaneous surface.

Fourth Species.—*Specific aphony and dysphony, resultng from a primitive or consecutive remote affection, which has been conveyed to the vocal organs.*

These may be caused by the venereal, scrofula, scurvy, arthritis, rheumatism, gout, psoriasis, herpes; by nearly all the exanthematous affections; finally, by the ill-managed employment of the preparations of iodine and of mercury, which sometimes give rise to swellings and ulcerations of a peculiar nature.

Of the treatment it will hardly be necessary to speak, as it is based upon general principles, and in each particular case depends upon the affection being connected with local lesions, or merely symtomatic or sympathetic.

The remarks at the end of the volume on the subject of the hygiene of the voice are interesting, and worthy of perusal, as there are many curious facts respecting these matters which are not generally known.

W. M. C.

SOUTHERN MEDICAL SOCIETIES.

We mentioned in our last number that we had received from a correspondent an account of the organization of the Alabama Medical Society. We regret very much that the letter has been lost, and it is now out of our power to publish it, as we had intended. We may state, from memory, that this is a State Society, located at Selma. It possesses the power of granting license to practise physic, and also of conferring degress. Its regular sessions are, we think, monthly and annual, and it will bestow prizes for the best medical essays. Our correspondent did not give us the names of the present officers. The society has been established some three or four years, and is in a prosperous condition.

The West Feliciana Medical Society.

We are indebted to Dr. S. A. Jones, Corresponding Secretary, for a copy of "The Rules and Regulations," recently adopted for the organization and governance of this new Society. There is embodied in the rules a code of *medical ethics*, which is highly commendable. This department is assigned to the special charge of "A Committee of Honor," who, among other duties, are required "to interpose their best endeavors to cause a friendly reconciliation, whenever apprised of any misunderstanding that may unfortunately occur between members of the Society." This is a somewhat new, but we think a very praiseworthy feature in the constitution. No means should be neglected to promote harmony and good feeling, both among members of societies and of professions. A code of ethics is calculated to generate an *esprit du corps* that is every way beneficial. We had the pleasure, some time ago, of reading an introductory address delivered before this association by Dr. P. B. McKelvy, which was creditable to the author, as well for the liberal sentiments, as the professional attainments displayed. The West Feliciana Medical Society is to meet in St. Francisville every three months. The officers of the Society are :

P. B. McKELVY, M. D., President.
THOS. BEAUMONT, M. D., Vice President.
D. B. GORHAM, M. D., Treasurer.
SAMUEL A. JONES, M. D., Cor. Secretary.
G. W. PRUNELL, M. D., Rec. Secretary.

W. G. Austin, M. D.
Wm. Ingalls, M. D.
Thos. P. Linton, M. D.
} Committee of Honor.

We heartily wish success to this new Medical Society and hope to receive from it many valuable communications.

We are now aware of six Medical Societies in our vicinity, all in successful operation, viz: 2 in New Orleans, 1 in Mobile, 1 in Natchez, 1 in Selma, Ala., and 1 in St Francisville, La. We wish that the physicians of every county and parish throughout the South, would form themselves into similar associations; nothing is better calculated to unite, strengthen, and promote the best interests of the Profession.

NATIONAL MEDICAL CONVENTION.

The New York State Medical Society, at its last annual meeting, adopted resolutions recommending a "National Convention of Delegates from the Medical Societies and Colleges in the whole Union, to convene in New York on the first Tuesday in May, 1846, for the purpose of adopting some concerted action for elevating the standard of Medical Education in the United States."

We look upon this as one of the most important moves ever proposed in regard to the Profession in this country, and we sincerely hope the call will be responded to throughout the Union. To elevate the standard of Medical Education, is certainly an object of paramount importance in a country whose population is increasing as rapidly as ours, and where so many young men are disposed to enter the Profession, having no adequate conception of its requirements, its troubles, and its responsibilities. Let as many enter as choose, but for humanity's sake, let them qualify themselves; the ensignia of the Profession should be conferred alone upon those who do. We must think that a Convention of liberal and enlightened Physicians from all parts of the country will be attended with the most beneficial results.

Part Third.

EXCERPTA.

1.—*Observations upon the Employment of Compression in Aneurism, with some statistical details.* By O'B. Bellingham, M. D., F. R. C. S. I., *one of the Surgeons of St. Vincent's Hospital.* (*Dublin Journal.*) — The subject of aneurism, since the time of Hunter, has always been one of extreme interest to surgeons, manifested by the numerous attempts which have been made to modify, or to simplify the Hunterian operation. Thus, to assist union by the first intention after the operation, the two ends of the ligature were formerly sometimes cut off; or the ligature, after having been applied for a certain length of time, was removed, and the lips of the wound then brought together; or the ligature itself was discarded, and a *presse artère* applied instead to the denuded artery, and retained for a longer or shorter period, according to circumstances. But as these different proceedings were occasionally followed by unfortunate results, they came ultimately to be, in a great measure, discontinued, and surgeons were content to follow the Hunterian operation, adopting only such improvements as time had sanctioned. Nevertheless, the operation of placing a ligature upon a large artery was always one of considerable anxiety to the surgeon, seeing that even the most skillfully performed operations were now and then followed by secondary hæmorrhage, by phlebitis, or by gangrene; any treatment, consequently, which promised to do away with those risks, was a great step in advance.

When I first brought the subject of compression in aneurism before the Surgical Society of Ireland, only three cases had occurred in which this proceeding had been employed; that number has now been increased to twelve. Upon that occasion I observed: "The application of well-regulated pressure in the treatment of popliteal aneurism, cannot but be looked upon as a most important improvement in surgery. The operation of tying the femoral artery is perhaps the least successful of that on any of the larger arteries; and when three cases have occurred in succession, in three different hospitals, within a short period, it is not too much to expect that the necessity for performing this operation will in future be much diminished. This result, however, must depend upon the trial of compression in a larger number of cases; though its success in these offers great encouragement to surgeons to attempt it, inasmuch as the difficulties which hitherto surrounded it, in the imperfect construction of an instrument for the purpose, have been in a great measure overcome; and the correct theory of the mode of action of compression, and the amount of pressure required for the success of its application, have been nearly established.

That I was not then over-sanguine, has been shown by the subsequent results of this mode of treatment; and the success which has attended the treatment of aneurism by compression, may be judged of by the following list of cures which have been effected since its introduction by Dr. Hutton, in November, 1842. The cases are arranged as nearly as possible in the order of their occurrence.

Cases of Popliteal and Femoral Aneurism cured by Compression between November, 1842, *and February*, 1845.

1. Dr. Hutton. Richmond Hospital, Dublin. Popliteal aneurism.
2. Dr. Cusack. Steeven's Hospital, Dublin. Popliteal aneurism.
3. Dr. Bellingham. St. Vincent's Hospital, Dublin. Popliteal aneurism.
4. Mr. Liston. University College Hospital. Femoral aneurism.
5. Dr. Harrison. Jervis-street Hospital, Dublin. Popliteal aneurism.
6. Mr. Liston. University College Hospital. Femoral aneurism.
7. Dr. Bellingham. St. Vincent's Hospital, Dublin. Femoral aneurism.
8. Dr. Kirby. Jervis-street Hospital, Dublin. Popliteal aneurism.
9. Dr. Allan. Royal Naval Hospital, Haslar. Popliteal aneurism.
10. Mr. Greatrex, Assist. Surgeon Coldstream Guards. Popliteal aneurism.
11. Dr. Cusack. Private patient, Dublin. Popliteal aneurism.
12. Dr. Porter. Meath Hospital, Dublin. Popliteal aneurism.

Eight of these twelve cases were treated in Dublin; and in all the cure has been permanent. The aneurismal tumor in a few instances was of very large size, and in a few the operation by ligature would very probably have failed, owing to the diseased condition of the vessel, or some other cause.

It will be observed, from the histories of the cases which have been published, that the femoral artery could be traced after the cure to near the sac of the aneurism; proving that the artery is never obliterated at the point compressed Upon a former occasion* I endeavored to shew that such an amount of pressure as would obliterate the artery is never necessary; and that a cure would be more certainly and more quickly brought about, by allowing a feeble current to pass through the sac of the aneurism, than by completely checking the circulation in the vessel. As this principle appears to have been established by the results of the cases which have occurred in this country since, I shall now merely quote what I then said upon the subject.

"When it was considered absolutely necessary for the success of compression, that such an amount of pressure should be applied, as was almost certain to occasion sloughing of the part, and very certain to occasion intense pain and suffering to the patient; and when, in addition, this was to be prolonged through five successive nights and days, we can readily understand why patients refused to submit to it, and we can easily account for the disrepute into which the practice fell, and for the unwillingness of surgeons to adopt this treatment in preference to the simple operation of placing a ligature upon the femoral artery. It would appear, however, that it is not at all essential the circulation through the vessel leading to the aneurism should be completely checked, but rather the contrary: it may, perhaps, be advantageous at first for a short period, by which the collateral circulation will be more certainly established. But the result of this case, if it does no more, establishes the fact, that a partial current through an aneurismal sac, will lead to the deposition of fibrine in its interior, and cause it, within a few hours, to be filled and obstructed, so as no longer to permit of the passage of blood through it. Pressure, so as altogether to obstruct the circulatien in an artery, must necessarily be slower in curing an aneurism, as it must in some measure act by causing obliteration of the vessel at the part to which the pressure has been applied; whereas a partial current through the sac enables the fibrine to be readily entangled in the parietes of the sac in the first instance, and this goes on increasing, until it becomes filled; the collateral branches having been previously enlarged, the circulation is readily carried on through them."

It is deserving also of remark, that, in the cases which have been detailed in full, an enlargement of the articular arteries about the knee coincided almost with the cessation of pulsation in the tumour. This increase in size of the anastomosing vessels, showing that the collateral circulation is becoming established, is obviously a very favorable sign; and, if it occurs early during the treatment, we may look for a speedy cure, as it indicates the filling-up of the aneurismal sac.

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* Dublin Journal, vol. xxiii., p. 465.

The principal improvement which has taken place in the treatment of aneurism by compression, consists in the mode of applying the pressure; that is, instead of employing a single instrument, we employ two or three if necessary; these are placed upon the artery leading to the aneurismal sac, and when the pressure of one becomes painful, it is relaxed, the other having been previously tightened, and by thus alternating the pressure, we can keep up continued cempression for any length of time. By this means the principal obstacle in the way of the employment of pressure has been removed; the patient can apply it with comparitively little inconvenience to himself; time will not be lost owing to the parts becoming painful or excoriated from the pressure of the pad of the instrument; and as the pressure need not be interrupted for any length of time, the duration of the treatment will be necessarily considerably abridged.

Some of the success of the improved method of applying pressure must, however, be referred to the improvement of the instrument used. That which I employed (made by Mr. Millikin, of Grafton-street), is a modification of a carpenter's clamp, which was invented by a patient under Dr. Harrison's care for popliteal aneurism whom I had the opportunity of seeing several times, both while under treatment and after a cure had been effected. It consists of an arc of steel covered with leather, at one extremity of which is an oblong padded splint, the other extremity terminates in a nut, containing a quick screw, to which a pad similar to that of the tourniquet is attached. The principle of this instrument is exceedingly simple, so much so, that the patient can regulate its application himself, and it can be made of every size, so as to compress any vessel within the reach of compression. It appears to be a much superior instrument to that which was employed in the cases treated in the London hospitals, the application of which cannot be maintained for any length of time, without occasioning severe pain.

Advantages of Compression over the Ligature.—I propose now to enumerate some of the advantages which compression appears to possess over the ligature in the treatment of external aneurism.

In the first place, the employment of pressure is not attended by the slightest risk to the patient. If this applied to the operation by ligature (leaving out of consideration the horror many patients have of the surgeon's knife), it might not constitute a very stringent argument with some, for deviating from what are considered established usages; but when the facts are so much the reverse, when even the most carefully performed operation for aneurism of a large artery is liable to be followed by fatal results, and when this is due, not to the increase of the disease, but to the operation performed for its relief, a mode of treatment which is exempt from all danger has obvious advantages on the score of humanity; and when this mode of treatment has proved successful in every case in which it has been carried out since its introduction, it must constitute a powerful argument in its favour over the ligature.

Again, pressure is applicable to certain cases of aneurism to which the ligature is not, as well as to some cases in which the operation by ligature would be likely to be followed by unfavorable results. For instance, when an aneurism has attained a very large size, the long-continued pressure of the tumor must act injuriously upon the collateral circulation, compressing the veins, perhaps obliterating the arteries in its vicinity, and causing œdema of the limb below. If a ligature under such circumstances is applied, the extremity is very likely to be attacked by gangrene. This cannot happen in the treatment of aneurism by compression, which acts slowly and gradually, and can be interrupted at any time. Indeed, it appears to me, that pressure would probably succeed more quickly in curing a large than a small aneurism; inasmuch as the lining of the sac of a large aneurismal tumor is generally rougher and more irregular than that of a small one; it will therefore more readily entangle the fibrine of the blood which is allowed to flow through it: moreover, in several of the examples of aneurism cured by compression, which have been published, the tumour was of a large size.

Again, when an aneurism has attained a large size, if its contents are principally fluid, and its parietes are much thinned, inflammation or suppuration of the sac very commonly follow the application of the ligature, which may bring the patient's life into danger, and, at best, must render the recovery very tedious. This has never occurred yet after the compression, and such a result is evidently much less likely to follow it. Indeed, Mr. Cusack's last case of popliteal aneurism cured by compression, is an example in point, the tumor was of large size, the circumference of the limb at its seat being five and a half inches greater than on the opposite side; its parietes were so much thinned that "great apprehensions were entertained lest they should give way;" the limb was likewise œdematous; aud yet every thing proceeded as favorably as could have been desired, and the cure was completed within a shorter period than in several other cases which have been related. Mr. Liston's second case of femoral aneurism cured by compression, is also a good example; here the aneurism is stated to have been no less than sixteen inches in circumference.

Again, aneurism not unfrequently occurs in individuals in whom the coats of the artery, between the tumor and the heart, are so much diseased, that the vessel, instead of taking on the adhesive inflammation after the application of the ligature, ulcerates; or the ligature cuts its way through; or aneurism may occur in subjects laboring under valvular, or other disease of the heart. In such cases the operation by ligature is contra-indicated, and would almost necessarily fail; whereas pressure may be applied with the same prospect of succes as in subjects in whom the heart and arteries are perfectly healthy. Indeed, in one of the earliest cases of popliteal aneurism treated by compression, since its re-introduction by Dr. Hutton, the patient was not considered a favorable subject for operation.

Pressure is applicable to cases of the aneurismal diathesis, and when more than one aneurism exists at the same time; cases in which the operation by ligature is likewise contra-indicated, as well as to cases of spontaneous aneurism occurring in individuals of intemperate habits, or of broken-down constitution, in which the surgeon, with great reluctance, would perform any operation. A few cases have been related in which the operation by ligature failed in consequence of some irregular distribution of the artery above the aneurism. Now, in such cases, compression promises to be equally effectual as in any other. Again, cases occasionally occur, where the patient has so much horror of a surgical operation, as to refuse to submit to it, although made acquainted with the risk of delay. Such individuals will gladly embrace any means by which they may be relieved from the necessity of undergoing an operation, and will cheerfully submit to any other method of treatment which promises a chance of cure. Indeed, it may be said to have been this accidental circumstance which led to the recent re-introduction of compression in the treatment of aneurism.

Lastly, if pressure should fail to cure an aneurism (which, from the results hitherto observed, is very unlikely), its employment will not preclude the subsequent operation by ligature; but, by retarding the increase of the aneurism, and assisting in the establishment of the collateral circulation, it would tend rather to render the chances of the operation by ligature more favorable.

Objections to the treatment of Aneurism by Pressure answered.—I propose now to make some observations upon the objections which have been put forward against this method of treating aneurism, since its re-introduction in Dublin, and shall endeavor to reply to them.

It has been urged as an objection to the treatment of aneurism by pressure, that the arteries are few in number to which this mode of treatment is applicable; but what is really the fact? The artery, above all others, in which aneurism is most frequent, after the aorta, is the popliteal and next in frequency come the femoral and the brachial. Lisfranc has given a table of 179 cases of aneurism (exclusive of those of the aorta) collected from various works, and of this number the popliteal artery was engaged in fifty-nine instances, while the carotid was engaged 17 times, the subclavian 16, and the external iliac only 5 times. But even this may be much below the average, few cases, comparatively, of operations for popliteal aneurism have been published (owing to its frequency,) unless there happened to have been some peculiarity in the case; whereas, most of the operations of the iliac, subclavian, and carotid arteries, have been brought before the profession, on account of the infrequency of the disease in those vessels. It must be recollected, also, that aneurism of the subclavian or carotid arteries, near their origin, and of the common iliac, or inominata, which do not admit of the application of compression, do not admit, either, of the employment of the ligature. It surely, therefore, ought not to be urged against this method, that, because aneurism occurs in arteries beyond its reach, we should refuse to apply it to vessels to which it is adapted; or that the practice should be denounced, because it is not applicable to every vessel.

It has been objected to this method of treating aneurism, that the pulsation is likely to return, in consequence of the artery not being obliterated at the point to which the pressure is applied; and that the patient therefore cannot be considered safe from a relapse for a considerable period. Now, in my mind, a case of aneurism treated by pressure upon the artery above it, and according to the rules laid down, it is much less likely to be followed by a return of the pulsation than one treated by the ligature, and for these reasous: The manner in which pressure brings about the cure of aneurism, appears to be very nearly that by which Nature, under the most favorable circumstances, effects a spontaneous cure. The fibrine of the blood is entangled by the lining membrane of the aneurismal sac, successive depositions occur until the sac is completely filled, the tumour becomes solid. and all pulsation ceases. The sac no longer permitting the passage of blood through it, the collateral branches become enlarged, and the circulation is carried on by them. The tumour then gradually diminishes in size owing to the absorption of its contents, and the gradual contraction of the sac, and, finally, it disappears. On the other hand, when a ligature is applied to an artery, as, for instance, to the femoral, for popliteal aneurism, the current of blood into the sac is at once intercepted; after a time, however, the blood finds its way into it by the collateral branches; now, if an anastomosis of large vessels exists between the branches of the artery above the ligature, and those between it and the aneurism, a strong current of blood will come to pass through the sac, and the pulsation will return, which cannot happen in the former case, for the reasons stated. The sac of the aneurism likewise, after the application of the ligature, not being necessarily filled by solid fibrine, but by a coagulum which may be more or less loose, pulsation is more likely to return, as the sac must contract considerably before the patient can be considered safe from a relapse; and this, from the inelastic nature of the parietes of the sac, must require, sometimes, a long time to be accomplished.

It has been also urged as an objection to this mode of treating aneurism, that it is more tedious and more painful than the method by ligature. That it is less tedious, sometimes, several of the cases which have been published prove; indeed, in one of the last cases cured by compression, the pulsation in the aneurism ceased in a few days after the application of the two instrument; in some of the others the cure was also rapid: and if, in a few others in which this mode of treatment was adopted. a longer time elapsed, it depended probably upon the imperfection of the instrument, the irritability of the patient, or upon two compressors not having been employed together. With respect to the treatment by compression being more painful than the operation of placing a ligature on the vessel, including the subsequent dressings, until the ligature separates, and the wound is healed, this might have been an argument against the method, when so great a degree of pressure was supposed to be necessary, as would obliterate the vessel at the part to which the instrument was applied; but the fact is, the application of the compressor (according to the rules laid down now), really relieves the pain which the aneurismal swelling occasions; after it has been applied, however, for a certain time, the pressure does cause pain, but the patient then can relax it, after having tightened the other instrument, and so continue to compress different points of the vessel for any length of time.

It has been also urged, that the period which has intervened since the re-introduction of this method of treating aneurism is too short to allow us to conclude that the cures will be permanent. I do not know the exact length of time which it is considered necessary should elapse before a cure in such a case can be pronounced permanent: two of the cases of aneurism treated by compression in this city, have remained well for upwards of two years, and two others for nearly the same period, and in none of the remaining cases has there been any tendency to, or appearance of, a relapse. Now supposing, for argument sake, that the aneurism should return,—the same thing has occurred after the application of the ligature; and if there should be a relapse, would not pressure be as applicable then as in the first instance? and would not its employment be much more certain and safe than the application of the ligature a second time?

The last objection to the plan of treating aneurism by compression, or rather, I should say, the last objector to this method, is Mr. Syme, of Edinburgh. His arguments against it are, however, almost confined to a few assertions, the value of which may be appreciated by the following quotations from his paper on that subject.

"The femoral artery may be tied with so much ease, so little suffering, and such perfect safety, that the laborious, distressing, and tedious procedure, which has lately been brought again into notice by a surgeon of Dublin, will probably soon return to the obscurity in which it has, very properly, been allowed to slumber.

"For my own part, having tied the femoral artery thirteen times for aneurism, and never having met with the lightest sympton of an unpleasant nature from the operation, I shall certainly not deviate from the line of practice hitherto pursued."

I consider it only fair to Mr. Syme to place his observations upon record here, because a surgeon who has tied the femoral artery thirteen times for aneurism, must necessarily be looked upon as a great authority upon the point. However, as several surgeons in Dublin, whose experience is somewhat more extensive, and who have tied the femoral artery rather more frequently than thirteen times for aneurism, have not hesitated to "deviate from the line of practice hitherto pursued," and have employed pressure successfully in the treatment of aneurism, I shall merely remark, that, if Mr. Syme (after the numerous cases of the successful application of pressure which have been published), applies a ligature to the femoral artery in a fourteenth case of popliteal aneurism, without previously trying the effects of pressure, and it should prove a fatal one, I shall not envy his reflections.

I think, then, from what has preceded, we are warranted in concluding—

1st. That the arteries to which pressure is applicable, being far more frequently the subject of spontaneous aneurism than those to which it is inapplicable, compression promises to supersede the ligature in the great majority of cases.

2nd. Pressure has several obvious advantages over the ligature, being applicable to a considerable number of cases in which the ligature is contra-indicated, or inadmissible

3rd. The treatment of aneurism by compression does not involve the slightest risk; and even if it should fail, its employment not only does not preclude the subsequent operation by ligature, but renders the chances of the operation by ligature more favorable.

4th. Such an amount of pressure is never necessary as will cause inflammation and adhesion of the opposed surfaces of the vessel at the point compressed.

5th. Compression should not be carried even so far as completely to intercept the circulation in the artery at the point compressed; the consolidation of the aneurism will be more certainly and more quickly brought about, and with less inconvenience to the patient, by allowing a feeble current of blood to pass through the sac of the aneurism.

6th. Compression by means of two or more instruments, one of which is alternately relaxed, is much more effectual than by any single instrument.

7th. Compression, according to the rules laid down here, is neither very tedious nor very painful, and can be maintained, in a great measure, by the patient himself.

8th. An aneurism cured by compression of the artery above the tumour, according to this method, is much less likely to return than were the ligature had been employed.

I have thus endeavored, as concisely as possible, to enumerate the advantages which compression appears to possess over the ligature in the treatment of aneurism; and I have endeavored also, as briefly as the nature of the subject would permit, to reply to the objections which have been urged against this method since its re-introduction in this city. But I regret to be obliged to add, that some of the objections to this plan of treatment appear to me to spring, not from an anxiety to develope the truth, but have their origin in a less praiseworthy source. Upon this point, nowever, I shall merely say, that if a mode of treating aneurism, which promises to supersede the Hunterian operation in a large number of cases, had been introduced, and proved to be not only practicable, but easy of application, by a surgeon of a London or Edinburgh hospital, we should have heard fewer of these objections.

2.—*Abstract of the Memoir on Endosmose, by* M. M. Matteuci *and* A. Cima. Having been charged with the delivery of a course of lectures on the physico-chemical phenomena of living bodies,* I was led into a minute study of the phenomenon of endosmose. Notwithstanding the numerous experiments made by the distinguished physiologist who discovered this beautiful phenomenon, we must admit that its application to physiology is not more advanced than at the time Dutrochet made the discovery. They were generally limited to variations of the merely physical circumstances of the phenomena, such as the density of the two fluids, their nature, temperature, &c.; we have directed our researches with reference to the physiological relations, and have, consequently, experimented upon different animal membranes; we have principally attended to the disposition of the membrane relatively to the two liquids. We have employed the membranes of living animals, or taken from those recently killed, and have compared the results with those obtained with those which had been dried or altered by putrefaction.

These researches were long and minute, and it was only by varying and repeating the experiments, that we have arrived at a few constant and interesting results.

The instruments used differ in no respect from the endosmometres of Dutrochet. However, we would remark that in all of our experiments we had two endosmometres in operation, the tubes of which were of precisely the same calibre, divided into millemetres, and of which the internal diameter was three millemetres.† In a crystal vessel, large enough to contain the two instruments, we placed a kind of support, upon which was solidly fastened a metalic plate, pierced with numerous small holes. Upon this plate the two endosmometres were placed, and, to prevent their changing place, they were loaded with a large plate of lead, having two holes in it to fit the necks of the instruments. The membranes in the two instruments were placed in an inverse order to each other; so that in using skin, for example, it was so placed, that in one its external surface presented towards the interior of the instrument, whilst in the other, the internal surface of the skin presented in that direction.

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* The French translation of these lectures is about to be published by M. M. Fortin and Masson.

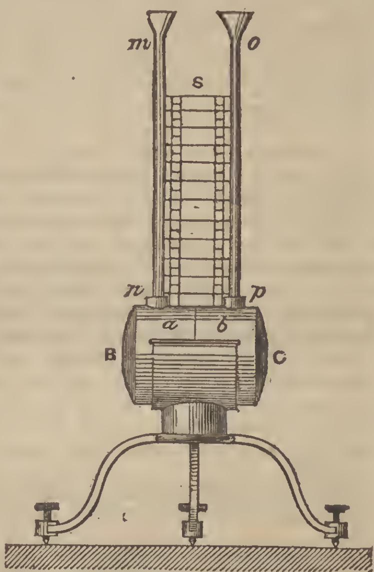
† About a line and a half.

All our experiments were made at temperatures of from 53° to 59° Fht. In the greater number of cases they were of two hours duration, and we repeated them more than once. We were careful to employ in the two endosmometres, with which the two comparative experiments were made, two portions of membrane, of equal thickness, taken from the same animal, and from similar portions on opposite sides of the body or organ. The liquids used, besides spring water, were the following, of which we give their density, in degrees of Beaume's areometer:

Solution of sugar,	19°
Solution of white of egg,	4
Solution of gum arabic,	5
Alcohol,	34

These liquids were generally placed within the instrument, the water being placed exteriorly.

On some particulars occasions, of which we will speak hereafter, we employed two vessels to place the endosmometres separately; we have employed another instrument, of which the following is the description:



B and C are two cylindrical brass reservoirs which unite by fitting nicely one into the other. The receiver B, has at the end which faces the receiver C, a plate, pierced with small holes, upon which was applied the membrane on which we desired to experiment. C has likewise a similar perforated bottom, which accurately embraces the membrane when the two receivers, B and C, are united. By this arrangement, the membrane will not yield to the greatest pressure that the most dense of the liquids in one of the recepients may exercise upon it; *m n* and *o p* are two tubes of equal calibre; which communicate respectively with the two reservoirs, B and C. When the instrument is used, the most dense of the liquids is introduced into the reservoir B, so as to fill the tube *m n* to a certain height; then the reservoir C is filled with water, by plunging it into a bucket-full, and it is then united under water to the reservoir B; the two reservoirs are fastened together by a screw, so as to prevent any of the liquid contained in C from escaping. The instrument is then levelled, and the tubes filled so that the liquids stand at zero of the scale in each tube.

The instrument shows, simultaneously, the elevation and depression of the two liquids, thus doubling the amount of the resulting phenomena, and consequently rendering the effect more apparent.

We will commence by stating the results obtained, by employing, as membranes, the skin of the frog, the torpedo, the eel, and the liquids indicated below. Our earliest experiments exhibited clearly the marked effect which the relative positions of the membrane and fluid exerted on the phenomena, and it was this discovery that led us to try the same experiments with the urinary bladder and stomach of different animals.

By a little care entire skins may be obtained, and rid of all the subcutaneous cellular tissue. Thus prepared, and by removing the portions pierced by the cutaneous appendages, they constitute membranes proper for these experiments.

Two endosmometres were supplied with a solution of gum arabic of equal density, and for membranes, the skin of the torpedo, but arranged with the surfaces in an inverse order in the two. In one instrument the external surface of the skin was turned towards the interior of the instrument, and in this the liquid rose in the tube thirty millimetres; whilst in the other, the position of the skin being inverse, the liquid rose to only sixteen millimetres. When with a similar arrangement of the membranes, a solution of sugar was used instead of the gum-water, the difference was still more striking. Similar results were likewise obtained with a solution of albumen.

The results obtained with the skin of the frog, were similar in kind to those with that of the torpedo. The same remarks apply to the results obtained with the eel skin; but in this case the process does not take place so soon as in the others, and the freshness of the skin is of more consequence.

When alcohol and water are the liquids used with either of the above three skins, the current always takes place from the water towards the alcohol; but the position most favorable to it is not the same for all of them. Thus, in the frog-skin the current is most energetic when the external surface is turned towards the water, and the internal towards the alcohol; whilst with the skins of the eel and torpedo, the current is strongest when it takes place from the internal towards the external surface of the skin. The same kind of results, and the same degree of intensity were observed, whether the skin was taken from one part of the body or another; and in the torpedo, whether taken from over the electric organ, or from other portions of the body.

The following table exhibits the comparative intensity of the force, when these three skins were used with different liquids:

Solution of Sugar,	Skin of torpedo,	100	millimetres.
	frog,	25	"
	eel,	15	"
Solution of Albumen,	torpedo,	30	"
	frog,	15	"
	eel,	8	"
Solution of Gum Arabic,	torpedo,	120	"
	frog,	22	"
	eel,	6	"
Alcohol,	torpedo,	35	"
	frog,	80	"
	eel,	55	"

The next series of experiments were made by using for the membrane the stomachs of lambs, dogs, cats, and the gizzard of fowls; from which the muscular tissue was removed as far as possible, so as to leave only the mucous membranes. The following table represents the direction in which the endosmose, from water towards the liquid used takes place most energetically through these membranes:

Stomach of lamb.	Solution of sugar, from the internal towards the external.
	Solution of white of eggs, from the external towards the internal.
	Solution of gum arabic, nearly equal in both directions.
	Alcohol, from internal to external.
Stomach of the dog.	Solution of sugar, from the external towards the internal.
	Solution of gum arabic, " " "
	Solution of albumen, nearly the same in both directions.
	Alcohol, from the internal towards the external.
Cat.	Solution of sugar, from the external towards the internal.
	Solution of gum arabic, " " "
	Solution of albumen, nearly the same in both directions.
	Alcohol, from the internal towards the external.
Mucous membrane of chicken gizzard.	Solution of sugar, from the external towards the internal.
	Solution of gum arabic, nearly equal in both directions.
	Solution of albumen, " " "

In all these cases the endosmose is directed from the water towards the other liquid used; but an exception is found when we employ the mucous membrane of a chicken's gizzard, with water and alcohol; when the direction of the endosmose is reversed, and takes place from the alcohol towards the water, and is more energetic from the internal towards the external surface of the membrane.

When the mucous membrane of the urinary bladders of the cow or hog were used, the same kind of results were obtained, but they were less energetic generally, and more irregular.

In all these cases it was found that the results were much diminished by using membranes which were not recently taken from the animals, or when dried or beginning to putrify; and there is an intimate relation between the phenomenon of endosmose and the physiological state of the membrane.

The following are the most important conclusions arrived at by these researches:

1st. That the membrane used to separate the two liquids exercises a great influence, both upon the direction and intensity of the current of endosmose.

2nd. The intensity of the endosmose is modified in general, by the direction of the surface of the membrane relatively to the two liquids.

3rd. The direction most favorable for endosmose through skins, is generally from the internal towards the external surface, with the exception of the skin of a frog, in which endosmose is most energetic from the external towards the internal surface.

4th. The direction favorable to endosmose through stomachs and urinary bladders, varies much more than in skins, in relation to the liquids employed.

5th. The phenomenon of endosmose is connected with the physiological state of the membranes.

6th. In using membranes dried or partially putrid, we no longer observe those differences which depend upon the relative direction of their surfaces; or endosmose may, even, not take place at all.

In order to explain the most singular result of our experiments, and the conclusions we have deduced therefrom, it is necessary to take a different view of endosmose from that hitherto taken. The increase of the volume of liquid in the instrument is regarded by M. Dutrochet as resulting from the difference between the *strong inward current and the weak outward current;* and the amount of this augmentation is equal to the excess of one current over the other. Experiments were necessary to ascertain what was the true explanation of the observed phenomenon; and not only to prove the existence of exosmose but to measure it as we had the endosmose.

Two endosmometres were used with the frog or eel skin; in one the internal surface of the skin was turned towards the interior of the instrument, and in the other, in the inverse order. A solution of salt, of known density, was introduced into the instruments, which were then plunged into separate glass vessels containing a quantity of distilled water equal to that of the salt water in the instrument. After allowing the instruments to stand some hours, it was found that the endosmose through the skins, from the water towards the solution of salt, was more energetic when the internal surface of the skin presented towards the water; that is, it took place most readily from the internal to the external surface. When the density of the salt-water in the instruments, and of the distilled water in the external vessels, was tried, it was found that in *the endosmometre in which the augmentation of the volume of the salt-water was greatest, its density was also greater than in the other*, *and vice versa; that in the vessel in which the diminution of volume of the distilled water was most considerable, a quantity of the solution of salt was found to be mixed with it, which had been thrown out by exosmose; but that this quantity of the saline solution mixed with the water in this vessel was less than that mixed with the water in the vessel in which the volume of distilled water had undergone a less diminution.*

These results are by no means explained by admitting that the rise in the water, and the increase of the liquid of the two endosmometres are due solely to the difference of the two currents endosmose and exosmose. For in this case the liquid in the endosmometre in which the rise is greatest, ought to have a less density than in the other in which the rise is less; while on the contrary, we are forced to admit that the current of endosmose has been equal, or nearly so, in the two cases, whatever was the disposition of the membrane; whilst the current of exosmose has been strongest in the apparatus in which the rise was least.

We are far from believing that we have exhausted this subject, which we hope to be able to take it up again; nevertheless, it is shown by this memoir that the nature of the membrane, its state and physiological function exercises a great influence upon the phenomenon of endosmose, and shows its importance in the explanation of certain functions of the living body.

3.—Paris, May 2nd, 1845.—*Weekly Review of Clinics.*—M. Rostan had lately in his wards, a woman recently delivered, attacked with *phlegmasia alba dolens*, and we were much pleased to hear him express a very decided opinion upon the nature of this disease. According to him, there is the best evidence, that the affection is due to the interruption of the current of blood, consequent upon the formation of coagula in the veins. This opinion does not belong exclusively to the professor, for many other physicians, as Bouillaud and Velpeau, in France, have endeavored to establish it.

It is strange that an affection so well characterized by its symptoms, should only have been specially described at the beginning of the 18th century; first by Mauriceau, who viewed it only as incident to the puerperal state, whilst it is likewise occasionally met with under other circumstances. Puzos and Levret, followed the errors of their predecessor, and moreover, framed an hypothesis respecting its etiology, which was—that the affection depended upon *effused milk*, a *milky deposit*, induced by a metastasis of the milk to the part.

A work published in the Medical Gazette by M. Bouchut, entitled *a Memoir on Phlegmasia Alba Dolens*, is well worthy of the attention of physicians. This work is based on exact and extended observation; the opinions of authors are discussed, the results well deduced, and it leaves in the mind precise notions respecting the subject of the work.

In forty cases, taken from the works of Robt. Lee, Davis, Velpeau, Laennec, &c., the anatomical lesion observed, was obliteration of the veins. In one exceptional case observed by Velpeau, the veins were likewise affected, and contained pus. "It appears to me impossible," says M. Bouchut, "in view of these facts, not to attribute phlegmasia alba dolens to obliteration of the veins."

It is generally supposed that this disease only attacks the lower extremities.—This is an error, as Gardien, Bouillaud, and others, have seen the upper extremities affected. Laennec reports a remarkable case, which shows that even the eye may be attacked by it; and discovered that it was due to the formation of clots in the opthalmic vein.

Numerous instructive cases are related of its occurrence in various parts, in both men and women. In one case, both of the lower extremities and the right arm were affected, and the left pulmonary veins were likewise found obliterated; it consequently follows that we must admit a *phlegmasia alba dolens* of the lungs. In another case, the *vena porta* and *mesenteric* veins were obliterated. "This lesion, says M. Bouchut, prevented the passage of the blood along its proper channels, and caused it to transude into the tissues around, through the venous parietes. Below each of the valvulæ conniventes, there was a small, black, elongated, and soft coagulum. The mucous membrane was coated with reddish villosities; the sub-mucous tissue infiltrated by a sanguinolent, plastic liquid, in which were points of coagulated blood, three lines in dimension.

It is true that *phlegmasia alba dolens*, usually attacks the lower extremities; and the left more frequently than the right.

It has been remarked that this disease may terminate fatally. According to M. Bouchut, "this is not the case when the disease is uncomplicated; unless, indeed, the disease is very much extended, so as to cause death by producing obstacles to the circulation through the lungs or heart. In the generality of other cases of death, attributed to this disease, it is really due to other causes;

they are poisoned by the puerperal purulent diathesis, characterized by alterations of the symphyses, abcesses of the pelvis, and purulent transformations of the viscera;" the *phlegmasia* being purely accessory.

The suppuration of the symphyses sometimes observed coëxisting with the *phlegmasia*, induced the supposition that there might exist some relation between them, but these are mere coincidences.

It has been already remarked that this affection is not confined to the puerperal state, but is also observed under other circumstances, and this is an important fact. Robt. Lee, Andral, Cruveilhier, Livois, Trousseau, Bouchut, &c., have observed in the course of hectic fever, a painful variety of œdema exactly similar, in its symptoms, progress and anatomical lesions, to the child-birth variety; which cannot therefore be regarded as a special disease.

This circumstance which would, apparently, only serve to render the subject more obscure, is, to the contrary, that which chiefly elucidates it. What, indeed, is the state of the blood in cachectic persons, in the last stages of exhaustion, and presenting the painful œdema, independently of the puerperal state? There is a *marked diminution of the globules, chlorosis*, that is to say, a *relative* augmentation of the fibrine, and increased coagulability of the blood. In phthisical cachexia, as in the puerperal state, there is alike an excess of the fibrine, and of the coagulability of the blood; but in the former, the excess is only *relative*, and due to the diminution of the globules, the fibrine remaining the same; whilst in the latter, there is simultaneously a diminution of the globules, and augmentation of the fibrine; that is to say, an increase, both *relative* and *absolute*, of the fibrine.

Thence, it follows that *phlegmasia alba dolens* is essentially a disease of the blood, but in which we should not overlook the influence of some occasional causes, such as the presence of fœtal head on the pelvic veins. This local circumstance is in fact of great importance, as it alone, explains the relative frequency of this affection in the left leg, after child-birth. Admitting an excessive coagulability of the blood, it is very easy to understand the formation of coagula in veins compressed above.

The name, *phlegmasia alba dolens*, is bad in two respects; first, by its generally received acceptation, which conveys the idea that it is exclusively a child-bed disease; and secondly, the adjective *alba* does not apply in all cases; as M. Bouchut has correctly remarked, that the parts are cyanose when the subcutaneous plexus of veins are much developed, to supply the place of the obliterated central veins. M. Bouchut suggests the name of *spontaneous obliteration of the veins:* but it is defective, as no change of the kind is spontaneous; and also in failing to distinguish it from other forms of œdema connected with obliteration of the veins, which are not all painful.—*Gazette des Hopitaux.*

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4.—*The Intra-uterine Causes of Death amongst Premature Children, and their Treatment.* By J. Y. Simpson, M. D., Prof. of Midwifery in the University of Edinburgh.—The pathological causes that lead to this unhappy result, sometimes in a succession of pregnancies in the same female, are, as far as I have yet investigated them, principally *three* in number, namely.—

First, Peritonitis in the Fœtus, as in the child lying before us. And one remark applies to this and to the other causes,—namely, that in consonance with a curious law in intra-uterine pathology, the same morbid conditions of the fœtus and its appendages are apt to recur to the same woman in successive utero-gestations. When the child dies of peritonitis, the placenta has always a whitish, washed, or bleached appearance, as if it were drained of all red blood, but has no morbid change or deposit in its structure.

The other two causes of the successive deaths of premature children are referable to morbid conditions of the placenta itself, and consist of:

Secondly, Inflammatory Induration and Degeneration of the Placenta, such as you see in several specimens placed upon the table. In this morbid affection, the inflammatory action is generally confined to a limited portion, or a few lobules of the organ, whilst the others are left sound and free. In some cases, however, we find it invading the whole surface of the placenta,—an observation which you will see confirmed by the state of the organ in many cases such as this, which I now show you, where there has been born a secondary fœtus, along with one at the full time,—the secondary fœtus, as it is called, being merely a fœtus which had been destroyed as early as probably the fourth or fifth month, by the morbid alteration which had occurred in the structure of the placenta, or portion of the placenta belonging to it. In inflammatory induration, the morbid deposit and change seem generally to stretch, as shown in this preparation of Mr. Goodsir's, from the maternal surface of the placenta towards the fœtal. The decidual membrane covering the exterior surface of the placenta is sometimes, as here, much thickened by the inflammatory deposit,

The other morbid condition of the placenta,—if we may call it morbid,—which leads sometimes to the successive death of children in the same mother, is:

Thirdly, Hypertrophy of the Placenta.—When the placenta presents this condition, the organ is greatly enlarged: the divisions between its lobules and maternal surface are very marked and very deep; and the edge of the placenta seems as it were almost to turn over to a certain degree the boundary of the fœtal surface.

No event is liable to produce more domestic distress and unhappiness than the loss of a succession of children from the intra-uterine causes I have just mentioned. How then may we most easily make out the diagnosis of them; and what treatment will be most successful in averting the evil to which they so generally lead?

The *diagnosis* of the pathological cause of the death of the fœtus, in one or two successful pregnancies, can only be made out, with any precision, by having an opportndity of examining the body of one of the fœtuses and its placenta. In doing so, we may be enabled to observe which of the three causes I have mentioned is the source of the calamity, and to direct our treatment accordingly.

Some years ago, when I was engaged in the investigation of peritonitis in the fœtus, I more than once asked myself the question, in what good could such an inquiry result? I felt utterly sceptical as to its being of any benefit except in satisfying pathological curiosity. But often when we enter on a subject of pathological study, we really know not to what ultimate results it may lead, and never ought to condemn or eschew any morbid investigation because we do not immediately see any practical advantage to which it may tend. I have latterly become convinced that the study of peritonitis in the fœtus may be made of no small practical utility in the following respect:

In describing it, in the paper to which I have already referred you in the *Edinburgh Medical and Surgical Journal*, I have, in discussing the exciting causes of the disease, stated, that in some cases the mother, as in the woman Anderson, has been exposed to bodily injury, &c., and after mentioning other probable morbific circumstances, have added, that it appeared to me highly probable, from the investigations I had then made on this point, that a great proportion of those children of syphilitic mothers that die in the latter months of pregnancy, may be shown to have perished under attacks of peritoneal inflammation; and further observations have led me to conclude that the evidences of peritonitis, in several successive children of the same mother, is a pretty certain test of one or the other of the parents, especially the mother, being tainted with syphilis.

The practical deduction in the way of *treatment* from this observation in the way of *diagnosis* is evident. It is, I believe, in these cases of successive premature labors, where the child perishes of *peritonitis*, and in these cases only, that mercury and other non-syphilitic modes of treatment are alone useful, though these modes of treatment have been supposed to apply to *all* instances where there is the unfortunate habit of losing the infant when in the last months of utero-gestation.

For the treatment of the cases in which the child dies in consequence of disease, not in its own structures, but in the economy and structure of the *placenta*, I belive that totally different principles ought to be pursued; and in a number of instances now, I have had the good fortune to see, in my own practice, these means of treatment followed by the most happy and successful results.

You may easily understand the principles on which I have proceeded in these last affections, if in the first instance, you recollect that the two functions which the placenta appears to perform in the fœtal economy, are those of nutrition and respiration; or probably we should more properly say, that this organ is the medium of these two functions between the mother and infant.

When the placenta becomes diseased, it can destroy the infant only, (seeing there is no morbid lesion in the fœtus itself,) by the imperfect manner in which one or both of these functions is performed. Such children, however, as we find in cases of diseased placenta, would not appear to perish for want of nutrition, because in many instances we find them not more lean and atrophied than healthy children sometimes are at the time of birth; and on opening their bodies you have often abundance of adipose matter. I believe for my own part that they die generally rather from the diseased placenta not being able to act sufficiently, as a *respiratory medium* between them and the mother, and that the infant in consequence dies from the morbid condition of the placenta, in the same manner as we should die if our lungs were densely studded with tubercular deposits, or extensively destroyed by inflammatory action.

Now the question is—with such an imperfect placenta, (or imperfect fœtal lungs, in other words,) what means can we possibly adopt in order to make this diseased placenta serve as a respiratory organ to the infant for a very few weeks longer; the question being in general one only of a few weeks,—that is to say, if we could preserve the child's life during that period from the action of the deleterious influence of which I speak, we could save the child until it was fit to take on an extra-uterine existence.

I have generally, in cases in which, from the history of the previous pregnancies, I knew the tendency to be to *chronic inflammation and induration of the placenta*, attempted to prevent the inflammatory action which produces induration from going to any considerable extent by leeching from time to time, particularly at those periods when the woman would have had her catamenia present, providing she were not in the family way, because it is, I believe, at these periods that she runs most danger,—there being, during pregnancy, in many females a monthly *molimen* of blood in these parts, though there be no monthly discharge. But though we may moderate the inflammatory effusions in this way, we can seldom, I believe, prevent. Hence our objects is to make as much as possible of the diseased placenta efficient as a respiratory organ, or rather to make the respiratory change in the remaining healthy part as active and intense as may be.

To understand how this may be done, consider for a moment how the fœtus does respire or breathe. Its type of respiration, as I have described it to you at other times, is like that of fishes. The blood of the fish is sent into the vessels of the gills in order to undergo the respiratory change which is there effected through the oxygen contained in the surrounding water. The blood of the fœtus is sent into the tufts or terminal branches of the fœtal placenta, (its gills, other words,) in order to be there exposed to the oxygen contained in the maternal blood, by which these tufts are washed in the cavernous structure of the placenta.

The respiration of the human fœtus is, I say, like that of a fish, then, with this difference, that the blood in the gills of the fish is arterialized by the *water* in which these gill are freely immersed, whilst the blood in the placental tufts of the fœtus is arterialized by the *maternal blood* in which these tufts are freely immersed. We can influence the vitality of the fish by the quantity of oxygen in the water applied to its gills. I believe we may do the same with the fœtus, by changing the quality of the maternal blood applied to its tufts.

Then comes the question, by what measures could we render the maternal blood as highly an oxygenized medium as possible, in order that, when it is applied to the fœtal placental tufts, it may make up by the quality or intensity of the respiratory change, which it there produces, for that loss of quantity which is a necessary consequence of a portion of these placental tufts being already destroyed by disease?

I have attempted to do this, and in a number of cases, as I have already stated, apparently with perfect success, by keeping the patients constantly on small doses of alkaline salts, such as chlorate of potass., nitrate of potass., bicarbonate of soda, &c., given several times a day, on an empty stomach, exactly as Dr. Stevens, some years ago, proposed to do for the restoration and arterialization of the unarterialized blood in fever patients.

You are aware that the addition of alkaline salts to the blood in this way appears to promote greatly—I had almost said to impart—arterial changes and properties, and that in a way which physiologists and chemists have not yet been able satisfactorily to explain. If you cover a coagulum of newly drawn venous blood, with a thin layer of water, the surface of the blood continues to retain its black color. If you add alkaline salts to the intervening layer of water, the air will very speedily act through this medium so as to render the clot of a red arterial color.

Patients have repeatedly averred to me, that the use of the salts I have spoken of, has a perceptible influence on the strength of the motions of the fœtus, —or, in other words, on its muscular power and vigor for the time being; but the observation is liable to so many fallacies on the part of the parent, that, probably, we should not build much upon it. But if these salts act in the manner which I suppose, on the maternal blood, the fœtus, under their use, is just placed in a better and purer atmosphere, (to use language applied to extra-uterine life,) and in this better atmosphere is capable of living for a few weeks longer than it otherwise would have done. I think it might be a matter of some chemical importance to inquire, what special salts would probably be of most use in rendering the mother's blood as highly an arterializing medium as possible, and if the use of iron in any form would increase its power in this respect. The subject is quite open for inquiry, and one in regard to which I know not any very accurate existing data.

I have stated to you that *hypertrophy of the placenta* seems to occur successively in different pregnancies in the same woman, and sometimes to be a cause of the death of the infant in the last weeks. One of the preparations on the table is a specimen of this diseased condition, and the patient from whom it was taken had produced six or seven dead-born premature children. Mr. Goodsir has directed his attention particularly to this effect of hypertrophied placenta, and similar observations have been made by some continental accoucheurs.—It is difficult to say how the hypertrophy of the placenta destroys the functions of the organ, for in the specimen before you there was no special lesion in the body of the child itself. Sometimes, however, the child presents dropsical effusions, such as anasarca and ascites. Probably the mutual compression and impaction of the different lobules and parts of the hypertrophied placenta on each other, are such as to diminish and destroy its action as a respiratory organ, and to impede the circulation through its vast collection of vessels. Under these circumstances, the alkaline salts might also be of use in the way of lengthening, for a time, the intra-uterine life of the infant.

In all the three series of cases which I have adverted to, that is to say,—in cases where children of the same mother have died successively of peritonitis before birth,—where a series of children have been lost from inflammatory induration of the placenta,—and where the hypertrophy of the placenta has acted in the same way,—in all of these, I say, I believe that the *induction of premature labor* about the seventh or eighth month ought to be a point of treatment held in view, and frequently had recourse to. I look upon this remark as especially holding good with regard to the placental cases; and that obstetric authors should add (what no one of them, so far as I know, mentions) the diseased states of the placenta to which I have alluded, as a *cause* for the induction of premature labor, when they have recurred several times upon the same mother.

and produced death of the child but a few days previous to its birth. Out of three cases of diseased placenta which have been under my care since the commencement of the present year, in two I induced premature labor successfully, as regards both mother and child, one of the patients having previously lost six, and the other three children. I had thoughts of allowing the third to go on to the full period, but fortunately, natural premature labor come on about the eighth month, and a living child was born. The placenta was so destroyed by inflammatory induration in this last case, that I am sure it could not have served as a lung for the child for a much longer period. Nature here pointed out strongly, and effected, by her own efforts, what ought to be done by art in similar instances.

In this and other instances where premature labor is required, the introduction of a sponge tent into the orifice of the uterus is by far the simplest and the safest means. In one of the two cases which I have just mentioned, a tent was put in at 12 at noon, and a larger one about 8 in the evening. Pains came on in an hour or two afterwards, at a time when the os uteri was *already* dilated by the action of the tents, and the first stage of labor, as it were, *half completed* before labor pains had yet begun. In 13 hours in all, after the introduction of the tent, a living child was born. It is seldom, however, that uterine contractions occur so speedily after the commencement of these or other measures for their induction.

5.—*Hopital de la Charité.—M. Velpeau—Hydatiform Tumor of the wrist—(tumeur en bissac de Dupuytren,)—Compression by Straps,—Puncture; Iodine Injection; Cure.*

Pouraine (Louise,) domestic, aged 28 years, entered the Charité 6th March, 1844,—a woman of good constitution and habitual good health.

Eleven years since, in going up the steps of a cellar, carrying a basket of wine, she fell but sustained herself by the left hand, the back of it resting on the ground. A few days afterwards a tumor appeared as large as a filbert above the anterior ligament of the carpus, near the cubital edge of the fore-arm. About two months later a second small tumor appeared near the ball of the thumb. The patient experienced acute pains. Baths were prescribed without advantage. She then consulted Dupuytren, who prescribed leeches and flying blisters. She did not pursue this treatment.

During 7 or 8 years, the tumors remained nearly stationary, but about three years since, another appeared in the palm of the hand and from that time they all commenced to increase. The patient engaged in hard work in the field.—When she remained quiet some time the tumours diminished.

Annoyed by the disease, she submitted to an incision which was made in the inner side of the highest of the tumours; there issued a spoonful of yellowish-white grains, compared by the patient to grains of pearl barley—these grains were viscid and surrounded by a sort of jelly. The incision healed up quickly, but the tumor diminished but little in volume. Finally she came to Paris, and the tumours troubling her much, she decided upon entering the hospital.

On the 10th March, the following condition was observed: there exists on the left fore-arm and hand, a multilocular tumor; the annular ligament of the carpus divided it in into two distinct parts. The skin unchanged in color; there was neither heat, nor pain; the tumors fluctuated; their base is large; they raised the tendons of the palmaris longus and brevis, so that these tendons divided the upper tumor into two others, internal and external. The latter is about as large as a filbert; the internal of the size of a pigeon's egg and limited externally by the tendon of the palmaris brevis, externally by that of the flexor carpi ulnaris which it pushes outwardly, and below by the annular ligament.

The upper tumour seemed to have its seat in the sheath of the flexors; it communicates with that of the palm of the hand, by passing under the annular ligament which binds them down. The cavity of the palm is effaced by the palmar tumour, which reaches from the annular ligament to near the metacarpo-phalangian articulation. The movements of the hand are limited in consequence of the size of the tumour and the passage of the muscles over it.

When the prominences are pressed, a peculiar crepitus is felt, which gives the idea of grains passing from under the finger,

The 11th March, Velpeau punctured the most prominent part with a hydrocele trocar and there issued half a tumbler full of yellowish grains resembling rice. These grains, to the naked eye, presented the appearance of little cysts containing a gelatiniform matter. When the envelope of these cysts were torn by a needle under a magnifying glass they were seen to contain a tremulous opaline jelly. An iodine injection was thrown in through the puncture.

The 12th, the patient suffered during the night, and could not sleep; the tumour has returned to its ordinary size; it is painful; the fingers are flexed, and can not be extended without sharp pains. Compresses with solution of acetas plumbi; repose.

13th. The pains are somewhat less, the patient has slept a few hours; the tumour has increased.

14th. The size of the tumour increased; and is greater than before the injection; it is red; lancinating pains are felt; but it is not painful to the touch.

The volume of the tumour now began to diminish. On the 30th March the little tumour situated outside of the palmaris brevis is not larger than a hickory-nut; that situated within is harder and more lobed (*bosselée*) than before the injection, but does not appear to have diminished. That of the ball of the thumb is reduced in size to one half; it no longer communicates with that of the wrist, nor even with that of the opposite edge of the hand. It appears encysted, fluctuation is more manifest, but no crepitation could be perceived. The tumour on the inner side of the palm is much as it was before the injection; it still communicates with the large one of the carpus. Frictions with mercurial ointment.

11th April. Compression was made by means of adhesive straps on all the tumours; they were removed on the 18th, and the mercurial frictions were continued till the 30th, when the compression was again resorted to until the 8th of May.

9th May. The patient went out in the following condition: there remains on the carpus only a small lump belonging to the internal aponeurosis. In the hand the tissues are a little thickened, on the ball of the thumb there is a small hard compact lump. The motions of flexion are easy.

About the 15th, the patient came to show her hand, all parts of the tumours much diminished. The mercurial frictions were continued from time to time.

This case viewed in relation to the usual prognostic, of the "*tumour en bissac.*' of Dupuytren, should encourage surgeons to try the iodine injections in such kind of cases.—*Pajot.*—(*Gazette des Hopitaux.*)

Part Fourth.

MEDICAL INTELLIGENCE.

(FOREIGN.)

1.—*Nutrition and Secretion.*—The following interesting extracts are taken from the July number of the Medico-Chirurgical Review, for the present year. They are extracted by the Review from a Physiological Essay on the Thymus Gland. By John Simon, F. R. S., Fellow of the Royal College of Surgeons, &c.

"Before advancing to the consideration of what is peculiar to the thymus, he proceeds to demonstrate the characteristics of the class to which it belongs; and while justifying the constitution of this gland, he endeavors to illustrate the points of similarity and of difference, by which the organs included in it are related to the true glands; the points in short, by virtue of which the functions of these bodies may be considered as forming part of the general problem of secretion. After alluding to the well known views of Müller as to the nature of a gland, and to the researches of Purkinje, Valentin, Henle, and Schwann, on its essential anatomy and organic means of secretion, Mr. Simon gives his own views on this difficult subject.

"The organic means for secretion, as illustrated in the anatomical structure of true glands, may be stated to consist in an arrangement for the growth of deciduous cells, in close relation with blood-vessels on the one hand, and with an evacuant channel on the other.

"All the chief acts of the vital economy are carried on by the medium and instrumentality of cells: in evidence of which fact it will be sufficient to cite the process of growth in its various modifications,—whether as seen in the development of the embryo, or in the repairing of injured textures, or in the organization of morbid products. Further, by the proportion in which these microscopical elements (or their rudiments, or their reliques) are present, we are enabled to measure the functional activity of any particular organ; their plenteousness and constancy are direct indications of abundant organic change—of life active in the part."

"In examining under the microscope a thin section of any one of the true glands, we find its bulk mainly consisting of cells, or their rudiments, in the closest possible aggregation; nowhere in the adult body do we find greater evidence of nutritive activity than in such a specimen; it is as obviously a growing structure as if it had formed part of an embryo. And when, after contemplating the important functions discharged by cells in other organs of the body, we turn to consider the use and object of their extreme preponderance in glandular structure, we are impelled to believe their essential connection with the processes here effected. Every analogy leads us to anticipate that here, or elsewhere, they should be the media of organic change,—that their growth should inseparably identify itself with the manifestation of whatsoever specific materials it may be the function of their particular gland to eliminate from the system.

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"In the liver, it is quite certain that the bile is for a period contained within the cavity of the cells. Henle, in confirmation of Hallmann's observation, states that he has seen these corpuscles of a yellowish tint, and ascribes that hue to the coloring matter of their secretion. But the natural appearance of the liver-cell is liable to certain exaggerations, which, though they originate in disease, are yet serviceable additions to the knowledge derived from healthy structure. For instance: the frequency of enlargement of the liver in cases of pulmonary disease, and its disposition to compensate for defective function at the lung by increased activity of its own secretion, had long been known; Louis had directed attention to the remarkable number of instances in which its fatty degeneration is found coincident with phthisis; and it was a natural step in pathological reasoning to suppose,—as the two organs have a certain analogy of chemical action,—that this morbid development of fat in the liver might have reference to the superabundance of carbon in the system, which the diseased lung could no longer eliminate,—that it might be a sort of vicarious action. It is now known that the actual seat of this fatty deposit is within the cells of the gland, that there is in each cell a morbid increase of the oily matter, which it should naturally contain; and it is argued, with great probability, that these minute elements, so surcharged with their compound of carbon, are but the ordinary channels of health, and that the fat is but an exaggerated secretion of the gland, here surprised in its very transit from the system.

"Still more conclusive is the evidence furnished by the following fact, as it relates to changes which have a merely mechanical origin. In cirrhosis, the essential primary disease is an inflammatory action, under the influence of which a quantity of coagulable lymph is poured into the interstices of the vessels and ducts; and, as this product of inflammation becomes organised, it contracts very closely, and surrounds with a dense capsule various isolated portions of the hepatic substance, or forms tough septa and constrictions within the liver and on its surface. By the condensation of this adventitious material, and by its pressure on the normal elements of the gland, there are produced various secondary results, which depend on mechanical obstruction; for example, partial atrophy of some spots with apparent hypertrophy of others; or again, jaundice, or ascites. Under these circumstances (probably where the strangulation has especially told on some small duct, and has obstructed or obliterated it) we find certain circumscribed masses of the gland colored with the deepest yellow, from intense biliary congestion. These parts will invariably present the microscopic appearance to which I wish to refer; namely, the ultimate cells are identified as the seat of this deep ochrous coloring,—they are gorged with bile. And, as this phenomenon cannot be ascribed to the physical cause of imbibition, (for it always prevails first and chiefly in the interior of the cell, and about its nucleus,) there appears no other possible explanation of its occurrence, than the theory here defended,—that the secretion of glands first manifest themselves in connexion with cells, or with their germs,—that the secretory process in glands is one with the cell-growth of their parenchyma.

"In certain other glands I have likewise discovered a circumstance, which appears even more conclusive than that last mentioned, as to the point in question; viz., the occurrence within a nucleated cell-membrane of solid saline materials, corresponding to those of the secretion. In the urine of fishes, for example, and in the thyroid section of many animals, microscopical crystals frequently occur in considerable quantities; and, even where the crystalline form is incomplete, the same peculiar product may be recognised by its dioptric qualities, as its minute masses float in the fluid. In several instances, while examining the kindneys or thyroid gland with the microscope, I have noticed that certain of the cells (unquestionable cells, with complete membranes and distinct dotted nuclei) have been distinguished from those in their vicinity, by the fact of their contents possessing the same refractive properties as the floating particles of the secretion; often, too, I have succeeded in distinctly recognising a crystalline arrangement in these inorganic contents of a cell." P. 69.

"It appears that in the development of secretory cells, there are the following steps: First, the formation of the nuclei; Secondly, the deposition of material around them; which step seems the first evidence of their peculiar function; Thirdly, the isolation of this material by the growth of a membrane about it,—in other words, the completion of a cell, which has now all its elements—nucleus, membrane, and contents; Fourthly, a stage of apparent quiescence, during which the specific contents of the cell are probably either increased in quantity, or brought to greater concentration; a stage, in one word, of ripening; Fifthly, the falling of the cell with its contained material, in the from of excretion.

"Now, in certain cases (and these bring us very near to the habitual state of the glands without ducts,) it seems that the third stage of this process is absent, that no cell-membrane is formed, that the nucleus, with the material developed round it, constitutes the sole physical evidence of activity in the part. Indeed, in all glands this stage appears far less complete than in other organs of the body; in most it seems an exception, rather than the rule, to find the cell-membrane perfectly and definitely formed; the liver is the chief—if not the only—instance to the contrary. Moreover, where we are able to trace the products of secretion actually within a cell (as in the above-quoted instance of cirrhosis of the liver) we find them either exclusively, or at least with a very marked predominance, accumulated in that portion which corresponds to the nucleus: as though this corpuscle were the true centre of attraction, and the cell-membrane only the boundary, or passive recipient, of the matter to be excreted.

"These considerations and—still more forcibly—various illustrations which may be gathered from the history of the suppurative process and from other pathological phenomena, lead us to the following conclusions: viz. (1) that the cell-membrane—whether perhaps it exert any specific vital influence on the matter with which it has contact, or merely serve for the mechanical isolation of its contents—must be viewed, at least in the secretory process, as a secondary and inessential formation: (2) that its existence, in nutrition generally, bears relation to the rate and perfectness of the cell-growth, and to the permanence of the organic combination effected in that process; that it will not occur where the particular nutritive act is ill-supported by the economy, where it is either absolutely or proportionally accelerated, or where (as in instances presently to be specified) the peculiar functions which have their centre in the nucleus, are exerted for a short time only, and for temporary purposes: (3) that (in respect of secreting organs particularly) the nucleus—from its constancy, from the priority, of its formation, and from the peculiar arrangement which the secreted matters assume in reference to it—must be considered as the characteristic and essential part of the apparatus, not requiring the completion of a cell in order to the performance of its functions; and (4,) that the act of secretion, though essentially homologous with ordinary molecular nutrition, is peculiarly prone, in various cases and for various reasons, to exhibit its process of cell-growth in a low, and as it were, aborted form.

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"If we examine, with a magnifying power of 300-400 diameters, the structure of a true gland, e. g., of the pancreas or kidney, we find that its assimilating cells are included within a simple continuous tunic; and, as we extend our inquiry, we learn that varieties in the arrangement of this membrane determine the several shapes affected in the ultimate structure of glands. Thus, whether we examine the botryoid vesicles of the salivary glands, the bulging tubules of the stomach, the simple follicles of the intestine, or the long windings and uniform calibre of the urinary and spermatic canals, we observe the outlines of each various form to be marked and limited by the homogeneous membrane referred to,

"It is not peculiar to the true glands, but belongs equally to those without ducts; although it has been almost overlooked in their structure, and never correctly described. Nor is it even distinctive of the generic anatomy of glands; for a tissue identical with it is seen bounding the fasciculi of voluntary muscle. and the tubules of the nervous substance, as likewise occurring in various other situations. In the mucous and vascular systems it has been named *tunica propria*, or *basement tissue;* in the structure of muscle, *sarcolemma;* in that of nerves, *tubular membrane* or *neurilemma;* but in each case, its anatomical relations are the same; it is, in each, the *barrier between nutrient vessels and the products of nutrition*,—a barrier that serves to support and to circumscribe the latter, but yet affords ready transit to the materials of constant renovation supplied by the former. Its functions and physical characters are strictly identical in all its various positions, whether it be seen as the single tunic of a capillary blood-vessel, or as bounding the ultimate structure of muscle, nerve, or gland; I have therefore ventured to apply to it a name which suits it equally in all these relations, and have, throughout this essay, spoken of it as the LIMITARY MEMBRANE.

"It extends, with more or less development, into every organ with deciduous cells, and constitutes in all (with certain rare but striking exceptions) a definite, but permeable, wall between the capillary blood-vessels on the one hand, and the assimilative cells, or cytoblasts, on the other. On its one surface, is the slow and equable circulation of blood through the finest net-work of capillaries; on its other, there advances the constant function of cell-growth, as I have above described it; while, intermediately, its own delicate tissue imbibes and is traversed by the liquor sanguinis, furnishing materials for the secretory process." P. 72.

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"In arriving at this point—the specific nutrition of gland-cells—we are as far as mere Anatomy, or *Statical Physiology*, can conduct us; yet how distant still from understanding the actual nature, the actual mystery, of the process! Doubtlessly it is a great generalisation—one that fills the mind of the physiologist with a sense of beautiful harmony—to find, as has been found, that the nutritive and secretory processes are essentially one; their organic instruments alike; their traceable steps parallel. But let not this brilliant discovery be misunderstood, or misapplied; the law developed in it is morphetical, and this merely. Whether we are occupied with the large, or with the little; whether we collect the flowing secretions from divided ureters and gall-ducts, or—armed with all optical resources, and standing on the very confines of the visible world—view the first molecules of bile and of the lithates, as they gather in their respective cells; equally in either case the pursuit is mere anatomy; equally in either case we are in the domain of form and phenomenon, not in that of power and law. With respect to secretion, the microscope has only shewn us in ultimate detail what for ages had been known in its broader features; it has been revealed of the molecules that which was known of the masses. Physiological difficulties are not contingent on size: 'Why does the liver-cell contain oil-globules and yellow matter, rather than urea and the lithates?' is a riddle involving the same speculations and requiring the same answer, as that plainer question of our forefathers, 'Why does the liver secrete bile rather than urine?' For the solution of these doubts, it is in vain that we scrutinise the lifeless molecules and mechanism of the dissected body. The glands are too essentially alike for us to venture on ascribing to their fancied diversities of affinity and filtration the manifoldness of their several products." P. 77.

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"1. The *phenomenon of secretion* essentially consists in the manifestation of particular products around organic centres or nuclei, which develop themselves in the effused plasma of the blood, and which, from their actual or potential relation to cells, are called cytoblasts.

"2. The *cytoblasts* bear no varying relation to the nature of the particular secretion, but are in all instances of substantially the same composition as the liquor sanguinis from which they are engendered, and of which they may be considered a mere solidification.

"3. The *materials of secretion*, if solid, arrange themselves in almost impalpable molecules around the several cytoblasts; if fluid, they constitute a common medium, in which the cytoblasts float.

"4. The *completion of a cell*, for the isolation of so much of the secreted product as is collected round each cytoblast, is a very frequent secondary process. In the true glands it is very frequent; in those without ducts exceptional. The steps of its development, and the circumstances regulating its existence, may be illustrated seriatim: viz.—

"α. In the Malpighian glandules of the spleen,—where the secretion is fluid and can but rarely be detected by the microscope in a molecular form, where the nutrition fluctuates from hour to hour, and where the assimilative affinities are therefore exerted with equal intensity only for the shortest periods,—a cell rarely, if ever, exists.

"β. In the Thymus,—where also during infancy, the secretion is fluid, and where (as I shall presently further illustrate) the assimilative acts probably vary in intensity over short and frequent periods, the persistence of cytoblasts without cells seems at first sight equally regular. And such is actually the case during the time of the gland's most active function; but when it becomes comparatively quiescent or when (as in many reptiles and some mammalia) it assumes the characters of a permanent organ, it will be found that its cytoblasts have undergone their complete development, and become nuclei of the fat-cells which are formed within the limitary membrane.

"γ. In the tubes of the supra-renal glands, where the product is solid, there is constant opportunity of observing that transitional stage, in which the secreted matters are closely aggregated in a molecular from around the several cytoblasts; here, too, the completion of a cell is frequent.

"δ. In the vesicles of the thyroid gland, owing to the fluidity of the secretion, no intermediate stage of cell-growth can be seen; but cells, taking the characteristic cytoblasts of the organ for their nucli, are often developed, and may be seen to contain a fluid of the same nature as that wherein they float.

"ε. Where the secretion of mucous surfaces is hurried by irritation, the bulk of it is found to consist of floating cytoblasts, which would else have been nuclei of epithelial cells; and the suppurative process, which is a similar abortion of cell-growth, indicates the operation of like influences in its origin.

"ζ. In the true glands, the formation of a complete cell is at once recognised as the regular type for secretory nutrition: The exceptions are very frequent, yet they admit of ready explanation on principles here stated; they are fallings short of the mature process, by the omission only of that which is in-essential; they are instances of unfinished development, under circumstances of local excitement or general ill-supply.

"5. The *vascular apparatus* is nearly identical in all organs of secretion; its main requisite being a distribution of blood in such delicate vessels, as may readily permit the transudation of its fluid ingredients.

"6. In most instances the cytoblasts are formed, and the particular products manifested on the surface of a *limitary membrane*, which divides them from immediate contact with the capillaries: but this tissue is absent under circumstances which exclude the possibility of its being essential to the process of secretion. It seems entirely subservient to the mechanical office of support: it is found in the true glands to be prolonged into the excretory apparatus, in those without ducts to be folded into closed tubes or vesicles; so as in the latter case to retain, in the former to conduct, the secretion.

"7. A distinction, implied in this differing arrangement of the limitary tissue, divides the glands into the two classes, which I have compared and contrasted. The specific difference of the smaller group consists in their not excreting from the body those materials, which are shed into them from the blood. *Their secretion is into closed cavities.*

"8. The glands without ducts *vary in activity* according to varying circumstances: sometimes their cavities are distended, sometimes nearly empty. In the latter case, that, which they had withdrawn from the blood, must again have passed into it.

"9. The last fact strikingly distinguishes them from the Wolffian bodies, as shewing that their sequestration of peculiar materials from the blood is only for *temporary or occasional* purposes.

"10. This metabolical action in regard of the circulation—this alternate withdrawing and rendering up again of certain peculiar materials, may be called the *common function* of the class; they belong to the offices of vegetative life, and are accessory to the circulatory system; they act on the blood occasionally, in respect of its chemical constitution." P. 85.

2.—*Clinical Examination of the Urine..*—The chemical examination of the urine is generally considered as a matter of so much difficulty, that the physician is deterred from making any attempt to ascertain by this means the nature of the morbid alterations, in this fluid, which are evident to the naked eye, and too frequently goes blindly to work, giving medicines which are really often contra-indicated by the very signs upon which he relied for his crude diagnosis. Simplification of the processes was therefore a desideratum, and we make no apology for extracting the following, from a review of Dr. Golding Bird's work on urinary deposits, their diagnosis, &c., in the April number of the Medico-Chirurgical Review:

"We may premise that the urine examined should be an average specimen of that passed in the preceding twenty-four hours, or at least that resulting from the first act of emission after a night's rest."

"It is necessary to recognise three distinct varieties of the of urinary secretion in every case under investigation: Firstly that passed some little time after drinking freely of fluids, generally pale, and of low specific gravity, (1.003—1.009) *urina potus.* Secondly, that secreted after the digestion of a full meal, varying much in physical characters and of considerable density, (1.020—1.028 or even 1.030,) *urina chyli vel cibi.* Thirdly, that secreted from the blood independently of the immediate stimulus of food and drink, as that passed after a night's rest, *urina sanguinis;* this is usually of average density, (1.015—1.025,) and presents in perfection the essential characters of urine."

"*Urine without any visible deposit.*—A piece of litmus paper should be immersed in the urine, which if acid, it will change the blue color of the paper to red. Should no change occur, a piece of reddened litmus paper must be dipped in, and if the secretion be alkaline, its blue color will be restored; but if no change occur, the urine is neutral.

"Some of the urine should then be gently heated in a polished metallic spoon over a candle, or what is preferable, in a test-tube over a spirit lamp, and if a white deposit occurs, albumen or earthy phosphates are present; the former, if a drop of nitric acid does not re-dissolve the deposit; the latter, if it does.

"If the urine be highly colored, and undergoes no change by boiling, the coloring matters of bile, BLOOD, or purpurine are present. To determine which, pour a thin layer of the urine on the back of a white plate, and allow a few drops of nitric acid to fall in the centre; an immediate and rapidly ending play of colors, from green to red, will occur if bile, but no such change if purpurine alone, exists. Should the highly colored urine alter in color or transparency by heat, the presence of blood must be suspected.

"If the addition of nitric acid to deep red urine, unaffected by heat, produces a brown deposit, an excess of uric acid exists. If the urine be pale, immerse the grivimeter, and if the specific gravity be below 1.012, an excess of water exists in the urine, but if above 1.025, the presence of sugar, or excess of urea is indicated; to determine which, place a few drops in a watch-glass, and an equal quantity of nitric acid, and allow the glass to float on some cold water; crystallization of nitrate of urea will occur in two or three minutes, if the latter exists in excess. Should this change not occur, the urine must be examined specially for sugar, which, it must be remembered, may exist in small quantities, without raising the specific gravity of the fluid.

Should the urine be alkaline, add a drop of nitric acid; if a white deposit occurs, albumen is present; if a brisk effervescence follows the addition of the acid, the urea has been converted into carbonate of ammonia.

"*Urine depositing a visible sediment.*—If the deposit is flocculent, easily diffused on agitation, and scanty, not disappearing on the addition of nitric acid, it is chiefly made up of healthy mucus, epithelium, or in women, an admixture of leucorrhœal discharge.

"If the deposit is ropy and apparently viscid, add a drop of nitric acid; if it wholly or partly dissolves, it is composed of phosphates; if but slightly affected, of mucus. If the deposit falls like a creamy layer to the bottom of the vessel, the supernatant urine being coagulable by heat, it consists of pus.

"If the deposit is white, it consists of urate of ammonia, phosphates, or cystine; the first disappears on heating the urine, the second on the addition of a drop of diluted nitric acid, whilst the third dissolves in ammonia, and the urine generally evolves an odour of sweet briar.

"If the deposit be colored, it consists of red particles of blood, uric acid, or urate of ammonia, stained with purpurine. If the first, the urine becomes opake by heat; if the second, the deposit is in visible crystals; if the third, the deposit is amorphous, and dissolves on heating the fluid.

"Oxalate of lime is often present, diffused through urine, without forming a visible deposit; if this be suspected, a drop of the urine examined microscopically will detect the characteristic crystals.

"Much of the little time required for the investigation thus sketched out, may be saved by remembering the following facts:

"If the deposit be white, and the urine acid, it in the great majority of cases consists of urate of ammonia; but should it not disappear by heat, it is phosphatic:

"If a deposit be of any color inclining to yellow, drab, pink, ot red, it is almost sure to be urate of ammonia, unless visibly crystalline, in which case it consists of uric acid.

"The only apparatus and re-agents required for these investigations at the bedside are:

"A gravimeter, made small enough to float in an ounce of fluid.

"Red and blue litmus paper.

"A test-tube and watch-glass.

"Nitric acid.

"All these are readily arranged in a little case, and can thus be always at the convenience of the practitioner. For the microscopic examination of the urine, a vertical instrument on a firm tripod stand, and large ring-stage, provided with a good half-inch achromatic object glass, is alone required.

"The following table briefly points out the best mode for the analytical examination of the saline deposits, either by chemical tests or the misroscope. The latter mode of investigation is infinitely preferable to all others, both for accuracy and economy of time, but is of course not readily available in the sick room.

"*A.—A table for discovering the nature of saline deposits by chemical re-agents.*

1.	Deposit, white - - - - -	2.
	——— colored - - - - -	5.
2.	——— dissolves by heat - - -	Urate of ammonia.
	——— insoluble by heat - - -	3
3.	——— soluble in liquor ammoniæ -	Cystine.
	——— insoluble in liquor ammoniæ -	4.
4.	——— soluble in acetic acid - -	Earthy phosphates.
	——— insoluble in acetic acid - -	Oxalate of lime.
5.	——— visibly crystalline - - -	Uric acid.
	——— amorphous - - - -	6.

6. readily soluble by heat - - - -	Urates.
——— slowly dissolves by heat - -	——— stained by purpurine.

B.—" *Table for determining the nature of saline deposits by the microscope.*

1. Deposit, white - - - - -	2.
——— colored, - - - - -	5.
2. ——— an amorphous powder, - -	Insoluble by heat—Phosphate of lime. Soluble by heat—Urate of ammonia.
——— in defined crystals - - -	3
3. ——— in prismatic crystals - - -	Triple phosphate
——— in octohedral or tabular crystals -	4.
4. ——— in octohedra - - - -	Oxalate of lime.
——— in simple or compound tables -	Cystine.
5. ——— in transparent crystals, - -	Uric acid.
——— amorphous, or in spherical masses,	Urates of ammonia or soda."

"On these directions we have to observe that they contain one very remarkable (typographical ?) error. Dr. Bird states that, "if the urine be very highly colored, and undergoes no change by boiling, the coloring matters of bile, *blood*, or purpurine are present." The statement is correct as regards bile and purpurine, but not with respect to blood; for it is well khown, as Dr. Bird himself subsequently states, that bloody urine alters both in color and transparency by heat. Moreover, the headings,of these two tables are incorrect; for Dr. Bird cannot mean to assert that either *Uric acid* or *Cystine* are 'saline' substances."

Uric Acid, Urate of Ammonia, and Urate of Soda..—The first of these, viz: *Uric Acid*, invariably occurs in a crystalline form, though under some considerable variety of shapes, all referable, however, to modifications of the rhombic prism, which may be assumed to be the normal crystalline form of this acid. The crystals are never colorless, but always present some tint of yellow or red. They do not dissolve when heated in the urine, but are soluble in liquor potassæ as well as in nitric acid; and the nitric solution yields by evaporation, a pink residue constituting the *murexid* of Liebig, the *purpurate of ammonia* of Prout.

Urate of Ammonia is amorphous, and may be white or colored (yellow, red, pink, or purplish.) It does not appear in the urine until this liquid is cooled, and instantly disappers on the application of heat.

Urate of Soda occurs in gout, and in the urine of persons laboring under fever who have been treated with carbonate of soda. It does not disappear so readily as urate of ammonia, on the application of heat. When examined by the microscope it is found to consist of mixed yellowish masses provided with projecting, generally curved processes.

Uric Oxide, called by its discoverer, Dr. Marcet, *xanthic oxide*, is a very rare kind of calculus, which, in nearly all the recorded cases, has occurred in children. Its formula is $C_5 N_2 H_2 O_2$. Concretions formed of it resemble those of uric acid, but their sections are characterized by a well marked salmon or rather cinnamon tint. Uric oxide is distinguished from uric acid by its insolubility in a solution of carbonate of potash; by its dissolving in nitric acid with little or no effervescence; by its nitric solution having on evaporation a yellowish residue; by its not being precipitated from its solution in strong sulphuric acid by the addition of water; and by some other less important characteristics. No crystalline arrangement of its parts can be detected by the microscope.

Purpurine, called by Simon *uro-erethrine*,, is one of the coloring matters of the healthy urine. It must not be confounded with *murexid* (the purpurate of ammonia of Prout.) On account of its solubility in water, it never occurs as a deposit unless urate of ammonia be present, when this salt, in precipitating from urine, carries with it the great mass of purpurine, and in consequence acquires a more or less deep carmine tint. The deposit, which is amorphous, yields up its purpurine to alcohol. This would distinguish it from blood, with which it has sometimes been confounded. Moreover, alcohol is without action on murexid. The presence of purpurine in urine is best detected by adding a little hydrochloric acid to some of the urine previously warmed; a color varying from lilac to purple, according to the quantity of coloring matter present, immediately occurs.

On evaporating urine containing purpurine to the consistence of an extract, and digesting it in alcohol, a fine purple tincture is obtained, the intensity of the tint being rather heightened by acids and diminished by alkalies.

Cystine, termed by its discoverer, Dr. Wollaston, *cystic oxide*. is distinguished from all other urinary concretions by the sulphur it contains, and which amounts to about 26 per cent. The formula for cystine is $C^6 H^6 NO^4 S^2$.

"When an ammoniacal solution of cystine is allowed to evaporate spontaneously on a piece of glass, it leaves crystals in the form of six-sided laminæ. These are probably exceedingly short hexagonal prisms. When the evaporation is slowly and carefully performed, these laminæ are transparent; but in general they are crystalized in a confused and irregular manner in the centre, the margins only being perfectly transparent. When examined by polarized light, these crystals, when sufficiently thin, present a beautiful series of tints, which are not observed when thick, on account of a high refracting power of cystine.

"When cystine occurs as a sediment, it is always crystalized, never under any circumstances being amorphous. Among the crystals, a few regular six-sided laminæ are often seen, but the great mass are composed of a large number of superposed plates, so that the compound crystals thus produced appear multangular, as if sharply crenate at the margin; and the whole surface is traversed by lines, which are really the edges of separate crystals. They thus resemble little white rosettes, when viewed by reflected light. These compound crystals always appear darker in the centre than at the circumference, which is sometimes quite transparent. Prisms of the triple phosphate are often mixed with the cystine, but on the addition of a few drops acetic acid, they readily dissolve, leaving the rosettes of cystine unaffected."

Oxalate of lime, (*oxaluria.*)—To examine urine for the purpose of detecting the existence of the salt under consideration, allow a portion passed a few hours after a meal to repose in a glass vessel. Decant the upper 6-7ths of the urine, pour a portion of the remainder into a watch glass, and gently warm it over a lamp; in a few seconds the heat will have rendered the fluid specifically lighter, and induced the deposition of crystals of oxalate, if any were present: this may be hastened by gently moving the glass, so as to give the fluid a rotary motion, which will collect the oxalate at the bottom of the capsule. The application of warmth serves, also, to remove the obscurity arising from the presence of urate of ammonia, which is readily dissolved by exposing urine containing it to a gentle heat. Having allowed the urine to repose for a minute or two, remove the greater portion of the fluid with a pipette, aud replace it by distilled water. A white powder, often of a glistening appearance, will now become visible, and this, under a low magnifying power, as by placing the capsule under a microscope furnished with a half-inch object-glass, will be found to consist of crystals of oxalate of lime in beautifully formed transparent octohedra, with sharply-defined edges, and angles.

"The crystals of the oxalate, when collected in the manner above directed in a watch-glass, are unaltered by boiling either in acetic acid or solution of potass. In nitric acid they readily dissolve without effervescence. The solution may be very readily watched under the microscope. When the oxalate is allowed to dry on a plate of glass, and then examined, each crystal presents a very curious appearance, resembling two concentric cubes, with their angles and sides opposed, the inner one transparent, and the outer black, so that each resembles a translucent cube set in a black frame. This is best observed under a half-inch object-glass; as with a higher power this appearance is lost.

"In a few few cases the oxalate is met with in very remarkable crystals, shaped like dumb-bells, or rather like two kidneys with their concavities opposed, and sometimes so closely approximating as to appear circular, the surfaces being finely striated. These crystals are produced, in all probability, by a prolific arrangement of minute acicular crystals."

Earthy Phosphates.—Deposits of earthy phosphates are always white, unless colored with blood. They are soluble in hydrochloric acid and are insoluble in ammonia or liquor potassæ. Nor are they soluble in the urine by heat. Phosphate of lime occurs as an opaque amorphous powder. The triple phosphate (phosphate of magnesia and ammonia,) forms a crystalline deposit called *white gravel*. The neutral triple phosphate (HO, NH^4O, MgO, P^2O5) occurs in prisms, stellæ, and penniform crystals. The prisms are well defined, with sharp and well defined angles and edges. The basic triple phosphate (NH^4O, $2\ MgO$, P^2O_5) occurs in stellar and foliaceous crystals.

Non-crystallizable Organic Deposits—Elements of Blood.—Serum of the blood may occur in the urine either alone, as in Bright's disease, and in the anasarca which results from scarlatina,—or it may be accompanied by the red particles, as in cases of fungus hæmatodes of the kidneys. When the quantity of blood effused is large, it coagulates and forms masses like pieces of currant-jelly. Urine containing albumen becomes opaque by heat, as well as on the addition of nitric acid. The deposit stained by heat should not disappear on the addition of a few drops of nitric acid. This distinguishes albumen from the earthy deposits sometimes obtained by boiling urine. Pariset's test for the detection of hæmatosine is the following:—"Boil the urine and filter it. Brown coagula of hæmatosine and albumen will be left on the filter; pour on these liquor potassæ, and if hæmatosine be present, a greenish solution will pass through, from which hydrochloric acid will precipitate white coagula of protien."

Purulent Deposits.—The pus-globule is a nucleated cell larger than a blood disc, and floats in an albuminous liquor (*liquor puris*) not spontaneously coagulable.

Mucus.—Mucous particles are not distinguishable from the pus-globule, with which probably they are identical.

"Where mucus and pus essentially differ, it is not in the nature of the particles, but in the fluid secreted with them, and in which they float; the *liquor puris* being albuminous and coagulable by heat, the *liquor muci* not being affected by it."

Organic Globules.—Dr Bird describes two of these. The *large* organic globule resembles the pus or mucous globule, but is unaccompanied with the characteristic albuminous fluid or pus, and with the glairy fluid of mucus. It has been found in every case of ardor urinæ. The *small* organic globule contains no traces of nucleus or traces of granulation. It is very rare.

Epithelium.—Epithelium-cells occur abundantly when deposites of oxalate of lime exist in the urine. They are regularly oval or irregularly angular flattened cells, and contain a well marked central nucleus.

Milk.—Dr. Bird remarks that no satisfactory case is recorded, by any observer of credit, in which milk has been discovered in the urine. All the cases of milk-like urine, where no fraud has existed, are instances of phosphatic, purulent, or fatty urine. The pellicle which is found on the urine of pregnant, and probably also of suckling women, and which has been called *kiestein*, consists, according to Dr. Bird, of fat, (butter?) numerous crystals of triple phosphate, and an animal matter allied to, or identical with, casein, and which Dr. Stark has proposed to call *gravedine*.

Fatty Matter.—All the genuine specimens of fatty urine that have occured to Dr. Bird have been opaque, like diluted milk, and, in the majority of instances, have spontaneously gelatinized like so much blancmange, on cooling. To these the term *chylous urine* has been applied by Dr. Prout. By agitating the fresh urine with an equal bulk of ether in a tube, the fat is dissolved, and by repose a yellow ethereal solution of it will float on the top of the urine, which, by thus losing the fat, becomes nearly transparent. If the solution be decanted, and suffered to evaporate spontaneously in a watch-glass, solid butter-like fat, having a rancid odour, is obtained. The urine also contains albumen [fibrin ?] in a spontaneously coagulable form. It occurs in patients who are disposed to obesity.

Spermatozoa.—If any of the spermatic secretion be present in urine, it subsides, by repose, to the bottom of the vessel, and may be mistaken for mucus; but the microscope detects in it spermatozoa.

Sugar.—Dr. Bird gives several tests for sugar in urine:

"1. *Trommer's Test.*—Add to the suspected urine in a large test-tube just enough of a solution of sulphate of copper, to communicate a faint blue tint. A slight deposit of phosphate of copper generally falls. Liquor potassæ must then be added in great excess; a precipitate of hydrated oxide of copper first falls, which re-dissolves in the excess of alkali, if sugar be present; forming a blue solution like ammoniuret of copper. On gently heating the mixture to ebullition, a deposit of red sub-oxide of copper falls if sugar be present.

"2. *Capezzuoli's Test.*—Add a few grains of blue hydrated oxide of copper to urine contained in a conical glass vessel, and render the whole alkaline by the addition of liquor potassæ. If sugar be present, the fluid assumes a reddish color, and in a few hours the edge of the deposit of oxide assumes a yellow color which gradually extends through the mass, from the reduction of the oxide to a metallic state (suboxide ?)

"3. *Moore's Test.*—This very easily applied test was lately proposed by Mr. Moore, of the Queen's Hospital, Birmingham, and depends for its action on the conversion of the colorless diabetic (grape) sugar into brown melassic (or perhaps sacchulmic) acid under the influence of a caustic alkali. Place in a test-tube about two drams of the suspected urine, and add hearly half its bulk of liquor potassæ. Heat the whole over a spirit-lamp, and allow actual ebullition to continue for a minute or two; the previously pale urine will become of an orange-brown, or even bistre tint, according to the proportion of sugar present. The test appears to be remarkably free from sources of fallacy, as boiling with liquor potassæ rather tends to bleach non-saccharine urine than to deepen its color."

The reviewer remarks that experiments have led him to place but little confidence in Trommer's test, as he has obtained the same results with it with healthy urine, or when urate of ammonia was dissolved in warm water, as it is said to produce with diabetic urine containing sugar.

3.—*Of the use of Stimulants in Inflammatory Diseases of the Lungs in Children.* By Dr Posner, of Berlin.—In inflammation of the lungs of infants, the author admits a stasis of the blood in the capillary vessels, and from this circumstance he argues that we ought to remove the obstacles to the circulation of the blood. He first obtains this effect by the employment of sanguineous emissions, and the use of those medicines calculated to augment the fluidity of the blood. It is, however, requisite to allow to the circulation sufficient energy to carry out of the lungs the products of inflammation. If we carry the anti-phlogistic treatment too far, we incur the serious risk of inducing an asthenic inflammation. According to our author, a period may arrive when a stimulating treatment may be indicated. M. Posner, on this subject, observes, that the pneumonia of infants differ from those of adults. In the latter, we may bleed freely with advantage, and there is but little danger of inducing exhaustion; but in the case of children we must exercise more discretion; we must not push depletion too far, otherwise we sacrifice the life of the little patient. Under the influence of too abundant bleedings, they become pale and almost livid; their lips assume a bluish appearance; the face hippocratic; the pulse rapid; respiration hurried; the cough less frequent, but paroxysmal. Auscultation reveals the râles characteristic of hepatization. If we persist in the anti-phlogistic system of treatment, we have symptoms of adynamia, and ataxia, convulsions, coma, and death; not from the effects of the disease, but from the treatment.

If, on the contrary, we decide promptly upon the employment of stimulants, the patient may yet be saved. In such cases, our author extols wine, which the little patients now swallow with instinctive avidity. It should be given, at first, in small quantities, but persevered in regularly. It will soon impart a better expression to the face, diminish the frequency of pulse, and the respiration, and induce a quiet and refreshing sleep. In a few hours, under this course of treatment, Mr. Posner has seen the râles of an hepatization changed into those of a simple catarrh. After the persistance of the antiaphlogistic treatment, and the use of calomel and stibiated tartar, for about two days, then our author thinks we may appeal to stimulants, and the cure will be certain and rapid. Before we resort to wine we may try the polygala, and the ammoniacal preparation. Many cases are cited by Posner to confirm the views above developed.

Remarks.—The observations of the Prussian physician are, in our opinion, highly judicious, and deserve consideration. The advocates for depletion see nothing but inflammatory engorgement of the viscera, and believe the debility the result of a sthenic condition of the system; whereas, those who dread the lancet, see, on the contrary, nothing but asthenia, adynamia, and their fatal consequencs. To both, we would say, bleed to fulfil certain indications, and stimulate when the symptoms seem to call for it. Exclusivism in medicine is a disgrace to the science, and should not be tolerated. Let practical medicine aspire to something more than a thing of fashion—to be patronized into notice to-day, and frowned down to-morrow.—[*N. O. Edrs.*]

4.—*Cases of Epilepsy, treated with success.* By M. Anglade, M. D., of Rouen.—The first case mentioned by our author, was a young girl, aged 22 years, of a bilioso-sanguine temperament, who had been laboring under epilepsy for six years, and on whom six or eight of his confrères had exhausted all the resources of our art. After an attentive examination, M. Anglade came to the conclusion that the epilepsy was excited by the violent pains, caused by six or eight carous teeth. In this opinion he was confirmed by the fact that on tapping gently with a small mallet, the canine tooth of the right side, a paroxysm of epilepsy was instantaneously developed. After affording such temporery relief as is usual in such attacks, M. Anglade proceeded to extract the eight carous teeth. Since their extraction, (which was in 1837, to the present, 1844,) she has not experienced a single attack of epilepsy; but, on the contrary, has been in the enjoyment of uninterrupted good health.

The second case was the daughter of M. Nadol, of Cazes, near Rouen, aged 18 years, of a nervo-sanguine temperament, affected with epileptic fits for ten years. In March, 1838, M. Anglade undertook the treatment of this case, not, however, until she had tested the skill of some dozen medical practitioners. In the examination to which our author subjected this female, he learned that she experienced a great degree of hardness or tension of the hairy scalp after an epileptic paroxysm. He immediately made two deep incisions, one inch from each other, down upon the scalp, extending antero-posteriorly from the hair in front, down to the occipital protuberance behind; from these incisions about two pounds of blood were obtained. These incisions were dressed with lint, spread with digestive ointment, for about fifteen days. Since the operation, March 4th, 1838, the patient has remained perfectly healthy. Since, she has married, and had robust and healthy children.

Case the third was a man, aged 36 years, of a bilioso-sanguine temperament, who had been affected with epilepsy for three years, when he applied to M. Anglade, in December, 1838. On this patient, he practiced a semi-lunar incision, six lines above the ear, beginning about the middle of the temporal, and ending at the junction of the parietal with the occipital. The bistuory was carried down to the bone, and the hæmorrhage was so great, in consequence, that M. Anglade was compelled to use the actual cautery to arrest it. He dressed the wound as above, and the patient was permanently cured.—*Jour. des Connais. Med. et Chirg.*

5.—*Causes of Death in Capital Operations.*—At the session of the Academy of Sciences, Paris, for February 10th, 1845, M. Ballard, Chief Surgeon of the Military Hospital at Besançon, read a memoir entitled:—*Practical considerations upon capital operations and upon the means of avoiding, in a great measure, their dangers and accidents.* The author does not think diet has such great influence in the mortality following operations; he thinks it better to feed too much rather than too little. M. Ballard has studied in succession the causes of death in those who have been operated upon, and the result has exceeded his expectations; for he cites 28 amputations, 20 of the abdominal extremities, of which eleven were upon the thigh, without a failure in a single instance; that is without a single death before complete cicatrization.

The first cause of death in those operated on, is the fear of the operation and the dread of the moment when it is to be done. We should then leave in ignorance, even the most courageous patients on these two points.

The second cause of mortality is the pain. When the first has been avoided, it is seldom that the operation proves fatal; but when the two are combined, the danger is much greater.

We should then endeavor to destroy or diminish sensibility by moderate compression, over the principal nervous trunks; but experience has also convinced M. Ballard that narcotics used in exciting doses, for two or three days before the operation, may fulfil this indication perfectly. Three, four, or five *centigrammes* (5-10 to 7-10 of a grain.) or even more of the hydrochlorate of morphia in a potion of 120 grammes, given in spoonful doses, daily, between meals, and at night, suffice to produce a sedative effect upon the nervous system, which will obviate any serious shock which might otherwise be communicated by an operation.

The third, and the most frequent cause of death, is traumatic fever. When once developed, it is difficult to control; but this may be done by keeping down the heat and pain, which are, in M. B's. opinion, its first elements. This object may be accomplished by the constant application of cold lotions to the part, rest, &c.—*Journ. des Connaiss. Med. et Chirgurg.*

6.—*Toxicology—A simple process for the detection of Arsenic.*—M. Bevan takes a glass tube of small calibre, which is half filled with nitric acid, diluted with four times its weight of water. A small plate of copper well cleaned is then introduced into the tube and immersed to one half its length in the fluid; a layer of melted lard is then poured on the top of the liquid, which hardening, forms a diaphragm, which separates the immersed portion of the plate from that which projects above the surface of the fluid. The fluid supposed to contain arsenic is then poured into the tube and remains above the diaphragm. If arsenic be present, in 6 hours it becomes precipitated from the liquid and deposited on the metalic plate.

By this process the hundredth of a grain of arsenic can be detected.

A zinc rod similarly placed gives the same results.—(*Journal de Chimie Med.*)

One advantage of this mode is that the arsenic may be detected in fluids containing a large quantity of organic matter in solution.

7.—*A new process for identifying spots of blood.*—M. Persoz has suggested hypo-chlorous acid as a proper reagent for the identification of spots of blood, in cases in which, on account of their age, or other unknown circumstances, they no longer yield their colouring matter to water, either pure, or holding potassa in solution. This property of hypo-chlorous acid, has recently been employed for the first time, by M. M. Orfila and Cottereau, in medico-legal analysis, on the occasion of a trial for assassination.

The shirt worn by the deceased was sent to them, together with an over-shirt taken upon the accused, and a knife carried by the accused on the day on which the crime was committed; they were requested to ascertain whether the spots on the articles of clothing, and blade of the knife, were produced by blood; and if so, whether the blood on the articles belonging to the accused, was the same as that on the shirt of the deceased.

The over-shirt of blue linen appeared to have been long used. On both the front and back there were numerous stains, some of which seemed to be due to dried blood.

The knife blade was covered with rust on both sides, and presented numerous spots, easily distinguishable from the rust, by their deeper color, which were tear-shaped, and appeared to be due to some liquid which had been projected in drops.

The following trials were made upon the over-shirt of the accused.

1st. Four pieces were cut out, on which there were small spots; these were macerated with distilled water for four days, when the water became turbid, and of a dirty yellowish color; but became transparent when filtered, still retaining its yellowish color.

This liquid did not restore the blue color of turnsol paper which had been reddened by an acid.

Heated to ebullition, a flocculent cloud was produced, indicating the presence of an albuminous matter. The addition of caustic potash quickly restored its transparency, and no difference of color was perceptible, whether viewed by refraction or reflection.

Liquid chlorine rendered it slightly greenish, and afterwards colorless.

Nitric and sulphuric acid, and galls, hardly rendered it turbid.

Ammonia and ferro-cyanide of potassium produced no change.

These reactions might justify the suspicion, but not a decision that the spots were produced by blood.

2d. Two other pieces were taken, having spots of a grayish brown color upon them, supposed to have been produced by blood imperfectly washed out, and these were treated with distilled water in the same manner as before. In four days the water became turbid and yellowish, and by filtering became limpid, without losing its color.

This liquid gave the same results as the preceding, but rather more decided.

3d. These results not being entirely satisfactory, and knowing that from several circumstances the coloring matter of blood stains sometimes was not readily taken up by water, they thought advisable to try the effects of hypo-chlorous acid, which possesses the power of removing from tissues all stains, excepting those of blood, of which it only deepens the color, and renders them brownish-black.

Hypo-chlorous acid was prepared expressly for the occasion, as it quickly loses its character. It was first applied to a part of the over-shirt where there was no stain; in a few instants the blue color disappeared, and left the tissue white. It was then applied to the spots, all of which resisted its action, and became blackish-brown. The grayish-brown spots also resisted its action; and it was consequently established that the stains were produced by blood, which, in the spots having a grayish-brown color, had been partly washed out.

The stains on the knife blade yielded nothing to distilled water. Were dissolved almost completely in hydro-chloric acid, forming a chloride of iron. Hypochlorous gave no result.

M. M. Orfila and Cottereau concluded, 1st., that many of the stains on the over-shirt were produced by blood; 2d., that nothing showed the spots on the knife blade to have been due to blood.

M. Persoz, in a letter relative to the use of chlorous acid for the identification of blood-stains, says:

"It often happens that blood-stains lose their power of dissolving in water, and in such case we cannot avail ourselves of the method recommended in books, for their identification. I had occasion in 1836, to use the hypo-chlorous acid* of Ballard to identify blood-stains upon an over-shirt, upon which there were likewise many wine stains. This acid destroys all stains excepting those of iron-rust and of blood."

In order to ascertain whether stains which resist its action, are those of rust, or blood, it is necessary to treat them with a mixture of hydro-chloric acid, and chloride of tin, which mixture whitens rust-stains, but does not affect these produced by blood.—(*Journal de Chimie Med'cale.*)

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8.—*Asparagine extracted from the vicia sativa.*—M. Menici, of Pisa, says that asparagine may be obtained in large quantities, from the *vicia savita* (the vetch) etiolated by growing in the shade.

He has observed that the exclusion of light causes the transformation of the starch and some other saccharine principles into aspragine.—(*Jour. de Chimie Med.*)

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9.—*Valerianate of Quinia.*—Dr. Devay in an essay on the Valerianate of Quinia, published in the Gazette Medicale, recommends it as equal to the sulphate in its antiperiodic effects, and much superior in its nervo-sthenic properties.

Preparation.—To a concentrated alcoholic solution of quinine, add a slight excess of valerianic acid. Dilute the solution with twice its volume of distilled water, and evaporate in a stove, with a temperature not above 120° Faht. During the evaporation handsome crystals of the valerianate are formed, arranged singly or in groups. Dry the salt in the open air.

The valerianate of quinia may also be obtained by decomposing sulphate of quinine, with the valerianate of lime or baryta, both being in solution in dilute alcohol.

Chemical and Physical Characters.—The Prince of Canino, who was the first to prepare this salt, ascertained it to be composed of valerianic acid, 1 equiv; quinia, 1 equiv., and water, 2 equiv. of which one constitutes its water of crystallization. The crystalline form seems to be variable, sometimes hexahedral or octohedral and at others forming rhomboidal tables, or agglomerated in light, silkey masses.

The odour slightly resembles that of valerianic acid; and the taste bitter. It dissolves readily in water, but alcohol and olive oil are better solvents. It is decomposed by the mineral, and most of the organic acids. It loses one equiv. of water when heated to 90°, and then melts like a resin.

Dose and mode of administration.—Being readily decomposed it is recommended to administer it in a simple mixture, as with gum mucilage. Three and a half ounces of the mucilage of gum arabic, will dissolve 8 grains of the salt. A grain may be given at a dose.

One of the great advantages of this salt is supposed to be its ready solubility in olive oil, in which form it may be applied endermically over the region of the spleen. The form recommended is:

℞ Olive oil, ℥ ii;

Valerianate of Quinine, grs. xvi.—dissolve.

M. Devay reports many cases; some of which were severe and complicated intermittents, which the sulphate had failed to relieve, and which yielded when the valerianate was used. From 1½ to 6 grains, constituted the per diem quantity in the various cases.

From the experiments made, M. Devay concludes, 1st. Valerianate of quinine is superior as an antiperiodic to the sulphate in consequence of its nervosthenic properties, and because it acts in smaller doses.

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* The hypo-chlorous acid employed in this process, is the crude product of the action of chlorine on the oxyde of mercury in suspension in water.

2nd. Given alone, it is equivalent to cinchona combined with the *nevritiques.*

3rd. In the worst fevers (the malignant ataxic) it is thought to act most beneficially, by its specific properties.

10.—*Digitalin.* *—After the discovery of digitalin by M. Leroyer, of Geneva, other chemists came to the conclusion that it was merely a compound of chlorophylle, resin, fatty matter and some earthy salts. Now recent investigations, however, seem to show that it may, with propriety, be considered as a peculiar vegetable principal endowed with many of the properties of the plant from which it is obtained.

According to M. Nativelle, digitalis contains: 1, digitalin in combination with tannic acid; 2, a crystallizable substance; 3, an aromatic principle; 4, a crystallizable resinous matter; 5, a fixed oil; 6, sugar; 7, a red coloring matter, soluble in water; 8, chlorophylle; 9, extractive; 10, albumen; 11, salts with vegetable acids; 12, salts with inorganic acids.

Extraction of Digitalin.—Take 2 pounds of the dried leaves of digitalis, coarsely powdered, moistened, and place in a displacement apparatus; treat it with cold water; a concentrated solution is thus obtained, and the digitalis is exhausted. The solution thus obtained is precipitated immediately by the sub-acetate of lead; the liquid then filtered, passes limpid and nearly colorless. To this liquid, carbonate of soda is added as long as there is any precipitate; filter again, and precipitate any salts of soda that may remain in the liquid by the addition of phosphate of ammonia. Filter again, and try the filtered liquor with an excess of pure tannin; an abundant precipitate is thrown down which is to be collected on a filter, and while still moist, mixed with one fifth its weight of pulverized oxyde of lead (litharge.) The soft paste thus obtained is placed between sheets of unsized paper; dried by heated air, and then powdered. This powder is then placed in the displacement apparatus, and exhausted by means of concentrated alcohol. This alcoholic solution is to be purified by means of animal charcoal, filtered, and evaporated. For a residue, a granular yellowish mass is obtained, which when washed with a little distilled water, dried, and re-dissolved in boiling alcohol, this solution by evaporation, deposits the digitalin in a mamelonated granular form on the sides of the vessel.

When dried, the digitalin should be washed with boiling concentrated ether, which separates a whitish crystalline matter, and traces of a green and other coloring matters.

Physical and chemical characters. Digitalin obtained by this process, is in the form of a white, inodorous powder; with an excessively bitter taste, and provokes violent sneezing when disseminated in the air; hardly soluble in cold, but more so in boiling water. † Soluble in every proportion in alcohol, nearly insoluble in ether.

When entirely neutral it combines neither with acids or bases.

Concentrated sulphuric acid dissolves it, producing a deep red color.

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* This article was drawn up principally from the memoirs of Nativelle, Homolle and Quevienne, published in the Journal de Chimie Medicale, for February and March, 1845.

† When obtained by a method recommended by M. Nativelle, digitalin is soluble in all proportions in water.

This method is to take the precipitate obtained by an excess of pure tannin, as described above; for each 300 grains of this humid tannate, previously mixed with a little tepid water, add 10 drops of ammonia: the solution quickly takes place when it is shaken; dilute with 2 pounds of water; add an excess of the solution of neutral acetate of lead; filter; if the liquid obtained is not entirely colorless, add a small quantity of the sub-acetate of lead, and pass a current of carbonic acid gas. Then add a small quantity of sulphate of ammonia, separate the precipitate of lead by filtering, saturate the liquor with sulphate of ammonia; and in a short time the digitalin separates in white flocculi.

The digitalin thus obtained is perfectly white, and of great purity.

Concentrated nitric acid dissolves it, depriving it of its bitterness, and making a yellowish solution.

Hydrochloric acid colors it green.

Caustic soda and ammonia render it yellowish brown.

Physiological and Therapeutic effects.—Its action on the denuded dermis was found to be so severe as to render its endermic application unadvisable.—Given internally, it always produces a notable diminution in the frequency of the pulse.

It was found that not more than 1-10 or 1-12 of a grain a day, could be borne, even when divided into 4 or 5 doses and taken at intervals.

It generally increased the appetite, and produced dragging sensations about the stomach, intestinal pains, flatulence, and constipation. The diuretic effect was by no means uniform, though sometimes very great. It sometimes produced head-ache, giddiness, a general feeling of weakness, and heat of hands and feet.

Dose and mode of Administration.—M. M. Homolle and Quevienne ascertained experimentally that about 1-16 of a grain (4 *millegrammes*) of Digitalin, was equivalent to about 8 grains of best digitalis in powder.

It has been used in pills, syrup, and mixture, so compounded as to give 1-50 of a grain, and repeat from four to six times per diem.

11.—*Valerianate of Zinc.*—During the past year experiments have been multiplied, respecting the value of the valerianate of zinc as a therapeutic agent, and many favorable reports are recorded in the European Medical Journals, particularly those of France. This article was recommended at first, principally by the experiments of Prince Lucien Buonaparte. The well established efficacy of both of the constituents in numerous morbid states of the nervous system, gave great confidence in the virtues of the compound, and this confidence has, in some degree at least, been confirmed by the trials that have been made of it.

Preparation.—The mode of preparation is as follows: Take valerianic acid, any quantity, introduce it into a glass vessel, add the hydrated protoxide of zinc, slowly to saturation; heat promptly the combination; add distilled water, sufficient to dissolve all the valerianate of zinc, filter and pour the solution thus obtained into a porcelain evaporating dish, add a small quantity of protoxide of zinc, to take up all the valerianic acid. By evaporating slowly, a crust of valerianate of zinc forms on the surface, which is removed as it forms, dried, and preserved in close stoppered bottles.

Appearance.—The valerianate of zinc is a whitish salt, with sometimes a pearly lustre; nearly odourless, or having somewhat the smell of valerian; of a nauseous and styptic taste.

Therapeutic uses.—It has been employed with decided advantage, or complete relief, in many obstinate cases of hysteria, chorea, and epilepsy; in many cases of neuralgias of various kinds. It is said to have palliated the pains, and agitation, accompanying, and depending on, cancer of the womb.

Dose.—A grain may be given at a dose, and gradually increased to two grains, two or three times a day.

The solution may be rendered pleasant by using orange flower or mint water, a little sweetened.

12.—*Remarks on M. Maisonneuve's Case of Guinea-worm (Filaria Medinensis).*—In October, 1843, M. Maisonneuve received into the Hopital St. Antoine, a man named Ede, just from Senegal, where he had been a soldier two years; he had a small tumour on the dorsum of the left foot, near the ankle joint, accompanied with intolerable itching. An incision was made into it, and a white filament, being observed, was drawn out about nine inches, when it broke. Another tumour rose near the upper head of the fibula. From this tumour a kind of flexuous, hard, subcutaneous cord extended round the anterior part of the leg, and lost itself in the calf. Two incisions were made along its course, and the entire worm extracted; it was two feet long, about the size of a crow's quill, and looked like the *vas deferens*. The *milky fluid contained in the worm, as also the purulent matter from the abcesses, presented, under the microscope, myriads of small cylindrical worms with pointed tails.*

M. Maisonneuve thinks this case interesting, as it establishes a point in natural history, which has been much doubted, viz: the existence of filaria as a distinct species, capable of propagating whilst in the animal economy.

Now this case is certainly interesting, and so are the points in natural history, which it establishes; but we are very far from owing these discoveries to M. Maisonneuve, as they are due by long priority to the distinguished Jacobson, as will be seen by a perusal of the following extract from the *Annales des Sciences Naturelles*, 2d series, vol. 1, page 320. Indeed no one can fail to remark the striking resemblance between the two cases.

"At the session of March 17th (1833), M. De Blainville communicated to the Academy of Sciences, a letter from M. Jacobson, containing a detail of very curious observations upon the Guinea-worm (*Dragonneau*). This savant received into his hospital an Arab, who had a tumour near the external malleolus, which was recognised as produced by the Guinea-worm, which, after several fruitless attempts, was extracted by the ordinary method. A second tumour having manifested itself at the other malleolus, an incision was made, and the knife divided a part of the worm longitudinally; *there issued from the opening a purulent matter, which, examined with the microscope, presented a multitude of small, elongated, filiform worms, with their heads a little larger, and a short tail, much more slender than the body.* The same phenomena were exhibited by the fluid, taken from other parts of the cavity occupied by the worm."

The conclusion deduced was, that the animal propagated while in the animal economy.

13.—*Female Medical Faculty, at Cairo, in Egypt.*—Islamism has hitherto thrown almost insurmountable obstacles in the way of the rational treatment of diseases, particularly those of women. The principal difficulty seems to be the jealous observances of the harem, which does not permit the approach of men, even when disease renders medical aid necessary. Méhémet Ali, desiring to introduce some medical system in adaptation with existing institutions, ordered the purchase of a number of female negro slaves, to be instructed in the medical sciences. One among these, named Fatmé, particularly distinguished herself, and after several years study under the direction of the celebrated Clot-bey, she was promoted to the place of chief physician to the female department in the Hospital of Esbeckia. With this clinical service is connected a maternity hospital, where young female negro slaves are instructed in the obstetric art, and in the treatment of the diseases of women. Great advances have already been made in Egypt by this system, and vaccination, which is practised by these women is rapidly progressing.

Fatmé is spoken of as possessing high attainments, not only in the medical but in other departments of learning. The daughter of the viceroy of Egypt was so much pleased and astonished at her acquirements, that she bestowed rich presents on her, with the title of Effendi, or *learned*. She is consequently called Fatmé Effendi.—(*Gaz. des Hopitaux.*)

AMERICAN MEDICAL INTELLIGENCE.

Gastrotomy, successfully performed by Dr. I. E. Manlove, of Tennessee, in a desperate case of obstructed bowels.

The following interesting case was reported to the Tennessee State Medical Society, and published in the Boston Medical and Surgical Journal for August, 1845:

On the 7th of July, 1844, I was called to see Alfred, a colored boy, aged 17 years. He complained of some general uneasiness of the abdomen, was laboring under febrile excitement, pulse, 110. Learned that he did not recollect having a passage from the bowels in 12 or 15 days. On the 4th had walked several miles to a barbecue, and probably had indulged freely in eating. He had taken Epsom salts and castor oil; also several enemata had been administered by his master. I bled him "*ad deliquium animi*," gave him a general warm bath, and directed four grains of calomel and $\frac{1}{2}$ grain of opium every four hours, until three portions should be taken, to be followed by castor oil and spirits turpentine.

8th—Medicine had been all taken; no evacuation of the bowels; had vomited once, throwing up the medicine. Pulse 120. Bled him, administered a stimulating enema, and directed calomel and opium as on the previous day. Visited him again in the afternoon. Condition found to be the same; no evacuation. Spent the night with him, and made every effort I could to procure evacuation of the bowels, but they all proved ineffectual. Vomited several times during the night. Pulse 120 and feeble. Abdomen tympanitic.

9th—Dr. Ford was called in consultation. His condition remained the same, except that all the symptoms were now growing more and more alarming, with the certainty that death must speedily ensue without relief. Flexible tubes were introduced as far as possible into the intestines, and stimulating articles were thrown up so as literally to fill the lower bowels. These were all soon thrown off without any appearance of fœces. About 60 grains of tart. antimony were dissolved in water and introduced at two injections, with little or no influence on the general system. An emetic also of ipecac. was administered: emesis was readily produced, but no alteration in the symptoms. Being now night, it was thought advisable to wait on the means which had been used till morning.

10th—Abdomen enormously distended; difficulty of breathing; extremities cold; pulse very feeble and quick; countenance anxious; no evacuation. Gastrotomy was considered the only possible means of even prolonging his life; and although the operation promised but little benefit, yet the certainty of death without it, justified us, in our estimation at least, in undertaking its performance. An incision was made in the median line, commencing about two inches below the umbilicus, and extending down towards the pubis four or five inches. The peritoneum and bowel along the lower half of the incision had formed a most intimate adhesion, and in cutting through the former an opening of about one fourth of an inch in extent was made into the latter. From the opening there proceeded large quantities of flatus and liquid fœces, as well as the oil and turpentine which had been taken. On further examination it was discovered that the intestines were united to the peritoneum by extensive adhesions at various points within reach of the finger and probe. The wound was closed by sutures and adhesive straps, except the opening into the intestine.—The amendment in all the symptoms in one hour was astonishing; the extremities became warm, and the pulse slower and fuller, and during the morning he was able to fan himself, the weather being excessively warm. On the next day his appetite was good, and he continued to improve and to discharge the contents of the bowels through the artificial anus until the 17th day after the operation, when the bowels acted naturally, the opening having nearly closed.

It will be proper to state that about six months before his present illness, the boy received an injury from the falling of a piece of timber on the abdomen. The hurt caused him to keep his bed several weeks, and hence, no doubt, the adhesions which were discovered in the operation. The boy is now well (nine months after the operation,) and is present for the inspection of the members of the Society.

Dr. Manlove is of opinion that in this case there was mechanical obstruction, caused by adhesions formed between the peritoneum and the bowels, the result of previous violence. He believes that cases of obstructed bowels, which resist all the other resources of our art, not only justify, but loudly call for the interference of surgery. He thinks that the dangers of peritoneal inflammation have been over-estimated from the earliest times. He remarks:

If the chief dread in gastrotomy, in such cases, be peritonitis, it may be removed somewhat by the reflection that the disease is combated successfully every day by the well established means which we all have at hand. And if it supervene after an operation, may not the same means be successful? And when we consider the importance of the operation, as affording the only chance of recovery to the patient, we may surely conclude that it is the smaller of the two evils, and justifiable in its adoption. We have but little doubt that experience will show that this operation may be performed with as little danger to the patient, and with as great prospect of relief, as that of lithotomy, and many others equally important. It is apparent, however, that the success of the operation depends much on the time of its performance. If delayed until disorganization of the intestinal tube is established, failure will await us almost inevitably. If performed sufficiently early, we would expect to be successful oftener than otherwise.

In support of these views, as far as one instance will go, we will here mention a case which may possibly be brought before the Society in another form. Our respected friend, Dr. Wilson, of this county, was called to attend, in conjunction with several others, a negro man who was supposed to be laboring under intussusception. All remedies had been used to procure evacuation of the bowels which ingenuity could suggest, but in vain; and the patient was reduced to the last point of life. Gastrotomy was determined to be the only means of affording relief. It was performed by Dr. W., and the intestines drawn out of the cavity of the abdomen until the point of obstruction was arrived at. About one inch of the ileum was found to be invaginated; and the attempt to relieve the intussuscepted portion, discovered the fact that adhesions had been formed between the receiving and received parts of the intestine. The adhesions were dissected loose, and the bowels returned into the abdomen. Natural passages immediately took place, and the patient was rapidly restored to perfect health. It is not unreasonable to suppose the chances of recovery would have been much enhanced, had the operation been performed before the adhesions were established.

As for incised wounds of the intestines, Professor Gross' interesting experiments show that they heal most kindly. Dr. Manlove's case was creditable alike to himself and to the Western Surgery, for it is evident that without that bold operation, the patient must soon have died.

NEW-ORLEANS, SEPTEMBER 1, 1845.

UNION OF MEDICAL JOURNALS.

Having united the interests of the New-Orleans Medical Journal, and of the projected Louisiana Medical and Surgical Journal, we owe some explanation to our respective subscribers. We have taken this measure with the belief that the general interests of science and of the profession may be fully served by one periodical, which, by the concentration of our materials and labor, we hope to render fully adequate to the demands of an enlightened public, and highly satisfactory to all of our subscribers. To those of our mutual friends who are subscribers for both Journals, the union of the two will be advantageous in an economical point of view.

In adopting a name for our Journal, we have been actuated by motives which we hope our friends will appreciate. We have deemed it expedient to select one, which indicating the union of the two, may at the same time perpetuate and serve as a continuation, to the one which has already been some time in existence in this city. For this reason, we have made as little alteration in the title as is compatible with the amalgamation of the two.

In consequence of this arrangement we have made the year of the new Journal to begin with the July number of the New-Orleans Medical Journal, already issued, which will be supplied to those subscribers who have not already received it.

At the end of the year, a new title page will be furnished, to take the place of that which stands before the first number of this volume.

Situated as we are in the commercial and social centre of a vast and populous region, the diseases of which are in many respects peculiar and interesting, we pledge ourselves to use every means in our power to render our Journal the medium through which the Medical gentlemen of this region may communicate their interesting observations, researches, and discoveries, to the profession at large.

We further pledge ourselves to sustain our work as an independent and impartial organ, in which we desire that the whole profession, with its various sectional and other interests may be fairly represented.

With these views we invite all members of the profession to lend us their aid, and to co-operate with us in our enterprize.

W. M. CARPENTER.
E. D. FENNER.
J. HARRISON.
A. HESTER.

September 1st, 1845.

HEALTH OF NEW ORLEANS.

Our city continues in the enjoyment of excellent health. We doubt whether any other, of like population, is more blessed in this respect. Intermittent and scarlet fevers are the most common diseases, but even these prevail to a very limited extent. Scarlet fever is confined chiefly to children, and certainly continues unusually late, especially when we consider the extreme warmth of the season. There have been a great many deaths from *coup de soleil*, or sun stroke; we heard of as many as eight in a single day. The Board of Health published some advice upon the subject, and recommended the public authorities and citizens generally to suspend work in the sun, for a few hours in the heat of the day. This was attended to for some time, and the result was beneficial.

The Weather.—This has been one of the warmest summers ever experienced in this city. The thermometer is variously reported on some of the hottest days. One of our city papers (the Picayune) states it to have risen as high as 98°. It was the same in the office of the St. Charles Exchange; and by comparison, these thermometers agreed with each other. The exposure of the thermometer at the St. Charles would certainly appear to be a fair one. At other places, on the same day, it was noted as only 96°; whilst with our correspondent, Mr. Lillie, who is as careful as it is possible to be, the thermometer on the same day only rose to 92¾°. We are aware that many thermometers are incorrect, but Mr. Lillie's have been carefully tested, and having ourselves compared one of his with that of the St. Charles, we found but a slight difference. The discrepancy then must depend upon the difference of locality. We refer the reader to Mr. Lillie's Meteorological Table, and observations—they are worthy of particular attention. The nights have been uniformly warm. In our last number we stated that there had been a great deal of rain in May and the early part of June, but that it was dry and sultry when we went to press. Since that time we have had frequent showers, and, upon several occasions, very heavy falls of rain.

The River.—We stated in our last number that the Mississippi had been up, but was then pretty low, exposing an offensive *batture*, that had been long covered with water. Since then it has risen very high; within a few feet of last year. It remained up but a short time, and is again very low. The Mississippi bottom has not been extensively inundated, as it was last year.

We hear of no sickness on the Gulf, or in the West Indies. The schooner Water Witch arrived here, from Vera Cruz, on the 14th of August. She reports no sickness at the city. The *vomito* had prevailed there for a short time, but was then extinct. There are still various surmises afloat respecting the prospect of an epidemic; many wise and experienced ones being confident that we shall yet have it, whilst others begin to express the opinion that we shall not only escape this year, but even altogether in future. Time alone must prove the truth or fallacy of these impressions.

We have no yellow fever as yet, but there is still sufficient time for a dreadful visitation. There were but five cases of this disease in August of last year, and only *four deaths*. By reference to a table published in the first number of this journal, which shows the date of the *first* and *last* cases of yellow fever in each year, for a period extending from 1822 to 1844, at the Charity Hospital, it will be seen that whenever an extensive epidemic has prevailed, it has generally commenced earlier in the the season than this. To cite a few instances:—in 1833, first case July 17th; 1837, July 13th; 1839, July, 23d; 1841, August 2d; 1843, July 10th. In 1835 it commenced a little later, August 24th; but in 1829, a great deal earlier, viz: May 23d. These are the most remarkable epidemic seasons within the period stated.

It is certainly not *impossible* for yellow fever to be banished from New Orleans; and we are anxious to encourage such a belief, under the hope that if generally entertained, it will cause the adoption of such measures as would be calculated to accomplish so desirable an object. As we have remarked upon a previous occasion, when we take a view of our peculiar locality, and the numerous apparent causes of disease to be met with from the centre to the suburbs of our city, adding to this, our exposure to the influence of an almost tropical sun about six months of the year, we must confess there is much greater occasion for surprise that we have so little sickness, than that we have what we do. Nothing has ever yet been done in this city solely with the view of improving its health. All our improvements (and they are great and expensive) have been effected for the purposes of commercial advantage and social convenience. In a hygienic point of view, we have been *incidentally* benefited; but the truth is, the three grand *desiderata* go hand in hand, and are to be accomplished by the same system of measures;—let us have good wharves and well paved streets—let the city be abundantly supplied with water and kept clean—let all vacant lots be elevated, and the adjacent swamps drained, and we shall then attain commercial advantage, social convenience, and last, though not least, *good health.*

We have often heard it remarked, that the yellow fever of late years is not so malignant as it was formerly. This is an amelioration not to be overlooked; and when our citizens begin seriously to consider the possibility of evading it altogether, we may expect to see such steps taken as will accomplish the object. Our liberal profession, renowned as it has ever been for philanthropy, will gladly co-operate in *this glorious work.*

HEALTH OF THE COUNTRY.

Having determined to go to press earlier in future, and to issue our Journal on the first day of the month, we are not able in this number to give returns from but a few of our correspondents on the subject of *the Health of the Country.* We have to request our correspondents to write us so that their letters will *be certain to reach us by the 20th of the month preceding publication*—otherwise we shall not be able to report them.

Shreeveport, July 12th.—Our correspondent writes, "This place, with the vast country around, has never been so blessed with health as at present. During the spring months we had a few cases of Cholera Morbus, Diarrhœa, and Pneumonia; nearly all easily combated. Since the 1st of June, we have had a few cases of bilious, intermittent, and congestive fevers—all as yet mild, but marked by unusual irregularity of paroxysm. Mild purgatives, quinine in liberal doses during the intermission, and occasionally morphia, are all the remedies requisite."

Montgomery Ala., Aug. 15th.—Our correspondent writes—"We have had during the last month or six weeks, a very few extremely mild cases of remittent fever; but at this time I do not think there is a single case in town. With the above exception there has scarcely been a vestige of disease of an acute character among us. Perhaps in no previous year have we had at so late a period in the season, so small an amount of our usual summer diseases. The spring and summer have been unusually dry—while at the same time we have had perhaps more than the usual amount of high winds, and thunder and lightning."

Matagorda, Texas, Aug. 10th.—A letter just received from a professional friend, states—"Our little village continues to enjoy incomparable health."

Vicksburg, August 19.—"The city and vicinity have been more healthy than I have ever known them at a similar period of the year. Indeed, we enjoy an almost total exemption from disease of every kind; the few cases that are seen, being chiefly intermittents of a mild type."

Our position affords us extraordinary facilities for hearing quickly from an extensive region of country, and the result is, that a summer *so universally healthy throughout the South-West*, has never been experienced before. All localities, river or sea port and interior, hill and valley, are alike healthy. This is very extraordinary, for the differences of these localities are very great; and generally heretofore, a season favorable to one set, was apt to prove disastrous to another. This, however, may be sadly changed before the warm season is over. No one can tell "what a day may bring forth."

HOSPITAL REPORTS.

Charity Hospital.—As we have determined to go to press earlier in future, we shall not be able to report the month immediately preceding publication. The following is the

Monthly Report of Patients in the New-Orleans Charity Hospital for July, 1845.

MAIN BUILDING.

Admitted,—Males, 536; Females, 89; Total, 625.
Discharged,—Males, 435; Females, 65; Total, 500.
Died,—Males, 57; Females, 5; Total, 62.
Réceived in *articulo mortis*, about 30.
Remaining on the 1st of August, 63.

LUNATIC ASYLUM.

Admitted,—Males, 16; Females, 12; Total, 28.
Discharged,—Males, 19; Females, 8; Total, 27.
Died,—Males, 5; Females, 3, Total, 8.
Remaining on the 1st August, 50.

MEDICAL WARDS.

SERVICE OF DR. T. M. LOGAN.—(WARDS NOS. 5 AND 6.)

We extract the following from this gentleman's notes as most worthy of particular notice.

Engorgement and Ulceration of the neck of the Uterus.—Cure.—Johanna M********, ætat. 27, native of Ireland, of a nervo-sanguineous temperament, was received into the Hospital on the 22nd May, 1845, complaining of most of the pains and inconveniences attendant upon organic uterine disease. Upon minute inquiry into her condition, it was ascertained that she had always enjoyed good health—had borne several children, and that her menstruation continued regular. About seven months previously she aborted of a four month's fœtus, from which she dates the commencement of her present affection. Although the catamenia recur in the usual periodical manner, during the interval a perceptible leucorrheal discharge persists, together with a tenderness upon pressure over the whole abdomen, particularly over the region of the ovaries, sometimes the pain being most sensibly felt on one side, and sometimes on the other. The *toucher* conveyed the sensation of great heat in the superior part of the vagina, and a large soft cervix uteri was readily felt, while little or no pain was experienced. Specular examination disclosed a congested and swollen condition of the cervix, which was of a dark red color, also extensive ulceration of both lips of the os tincæ, and a mucoso-purulent discharge proceeding from the uterine orifice. The pressure of the speculum gave but slight pain, and an oozing of blood from the ulcerated part followed its application. As no leeches were to be had, narcotic poultices were perseveringly used over the whole abdomen; the bowels were kept free by means of enemata and occasional laxatives, and the diseased part was kept clean by means of injections of infusio ulmi, which answered the purpose of a topical bath. At the same time the patient was put upon half diet, and ℈ij of the hydriodate of potash administered every 24 hours in a decoction of sarsaparilla. As soon as the inflammation would permit, the whole surface of the ulcer, as well as the cervix uteri and upper part of the vagina were freely cauterized with the nitrate of silver, which was repeated at intervals of 3 to 5 days, according to the exigencies of the hysterical symptoms. These latter were always relieved by morphia and the suspension of the cauterization. By the 23rd June, the ulcerated parts had almost entirely healed, and it was satisfactorily ascertained, by the mucus passing from the interior of the uterus being perfectly transparent, that its internal surface was not inflamed.—The patient now became impatient to be set at liberty, and although she was warned of the consequences, insisted upon being discharged.

On the 1st July, she returned in the same, or rather, a worse condition than upon her first admission on 22nd May. Leeches were now abundant and half a dozen at a time were repeatedly applied per vaginam upon the inflamed parts. Instead of the topical use of the lunar caustic, the nitrate of mercury in the following proportions was resorted to: One drachm of the proto-nitrate was dissolved in ℥i of nitric acid, and of this solution ʒ i was dissolved in ʒ ij of water and then applied. The patient experienced but little pain or inconvenience therefrom and the ulceration rapidly healed. Injections with alum and the sulphate of zinc were next resorted to, with complete success, in order to reduce the chronic engorgement of the cervix, and the patient is now in a state of convalescence

Induration and Schirrus of the Mamma.—Cure.—Ellen Y****, a poor deformed female, ætat. 26, of a lymphatic temperament, with a congenital absence of the right arm, was in the frequent habit of entering into ward 5, appropriated to invalid women and children, to be treated for some disorder brought on by her own imprudence or by inebriety.—On one occasion, 4th June, 1845, she entered the hospital to be treated for an ulcer on the sacrum, which had been occasioned by a fall or blow. At the same time it was discovered that her left breast was in a state of indolent engorgement with several small ulcerations around the nipple. Although the patient did not complain much of her breast on admission, still it soon became the seat of throbbing and shooting pains. She had vague and irregular rigors, the skin became dry, and a fever of an intermittent character supervened. Narcotic and emollient poultices were applied to the breast, and occasional doses of syrup of morphia, and solution of quinine during the apyrexia, were perseveringly prescribed. No suppuration of any consequence resulted, and the engorgement soon terminated in an indurated condition of the gland with most of the characteristics of schirrus. The chain of glands towards the axilla became successively indurated and painful. Severe pains shot down the arm of the affected side—indeed, extended like rheumatism all over the body, especially to the back and sacrum, and it was confidently anticipated by many, that extirpation by the knife must eventually be had recourse to. The following recipe, viz: ferri iodid. ʒ ij—alcohol et aqua a. a. ℥ ij;—was ordered to be taken by tea spoonsful morning, noon and night, and a free generous diet, with porter, was allowed. The condition of the patient soon began to improve under this treatment, and after eight weeks persisting therewith, she is now almost entirely restored to health.

Service of Dr. E. D. Fenner.

Of the cases alluded to in my last report as being still under treatment, I will remark, that the man with *general paralysis* is still in the Hospital and has improved considerably. The same course of treatment has been kept up, viz: *tonics, strychnia, frictions, and a generous regimen.* I generally order two of strychnia to be made into twenty-four pills; one of which he takes two or three times a day, until they are all used. I then omit it for four or five days. It sometimes produces such twitching and jerking of the muscle that it has to be discontinued before the two grains are taken. He is now perfectly comfortable—he has gained flesh and strength since last report, but his limbs are still feeble.

Of the *two cases of dropsy,* one has been discharged, cured; the other being greatly improved, asked for his discharge also, but returned in a few days with increased swelling, and is still under treatment.

Salicine.—I have now tried this remedy in twenty-two cases, but have desisted for the present, on account of the expense of the article in the large doses requisite. I shall report the result of my observations at a future time.

Several very interesting cases have been noticed in this service, but we can only make room for the following.

**Hepatic Abscess, with extensive Ulceration of the Intestines—Death—Autopsy.*—P. B. an athletic Irish laborer, aged 35, entered the Hospital, ward 10, June 19th.

Previous history of the Case.—P. B. had been in America 10 years—employed for six years past ditching on the plantations along the Mississippi river—has been very much exposed—says he has often worked for days in succession in mud and water waist deep, generally drinking about a pint of whiskey daily. Usually enjoyed fine health and was robust. About the 1st of June, 1845, he was attacked with dysentery, having frequent bloody mucous discharges, tenesmus, pain, and vomiting. All these symptoms were relieved by laudanum and one or two doses of salts. Soon after this he felt an uneasiness and fullness at the pit of the stomach. These symptoms increased upon him and he came to the Charity Hospital as above stated.

Existing State.—Looks rather pale—says he has lost 30 or 40 pounds of flesh—pulse about 100, and small—tongue natural—bowels and bladder free; there is a distinct tumour in the epigastric centre, just below the ensiform cartilage, about the size of a man's fist. It is oval shaped, firm to the touch, and somewhat tender. It is circumscribed in its limits, and a distinct pulsation may be discerned as he lies upon his back; it gives him pain in any other position except the right side. He sighs and brings deep inspirations frequently, and complains of oppression at the præcordia, especially when lying on the back.

The case was at once diagnosed *hepatitis, with tendency to abscess;* and although there was not much prospect of terminating it by *resolution,* it was at least deemed worthy of trial.

Treatment.—Cups over tumour to ℥ x. Calomel and quinine, a a. gr. i ter die.

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* This Report was read before the Medico-Chirurgical Society of Louisiana, on the 20th of August, and ordered to be filed among its archives.

June 20th.—Feels better; rested well; less uneasiness in the epigastric region. Breathes better, lies on back or left side, is cool and sweating, pulse 90; tumour smaller and less tender.

Treatment—To repeat cups and continue medicine.

June 21st.—Symptoms all better; skin cool and moist; bowels open; slight appetite; pulsation very distinct in tumour, but evidently proceeding from the aorta underneath.

Treatment.—Emplast. Vesicat. 6 by 8 *over tumour; continue calomel and quinine night and morning.*

June 22nd.—Says he feels better; rested well; pulse 90; no thirst; no pain; but better appetite.

Treatment.—Continue medicines—dress blister with merc. unguent.

June 23rd.—Comfortable, but did not rest well; skin cool; pulse 100; more appetite; mouth beginning to get sore. *To stop the calomel, continue the mercurial ointment, take compound decoc. sars.*

June 25th.—Much the same; mouth quite sore, but not much saliva; tumour softer; bowels rather costive; urine scanty.

Treatment.—Purg. enema, quin. grs. 2½ ter die. Re-apply blister.

June 26th.—Patient pale and nervous; pulse small and 100; mouth very sore, very little saliva; blister caused great irritation; one stool; urine scanty and high colored; no appetite; no thirst.

Treatment.—To continue quinine; porter; full diet.

June 27th.—Complains of debility; did not rest well; sweated profusely but did not recognise any chill or fever; costive; slight thirst; skin cool and moist; tongue furred; pulse not quite so frequent; tumour tense and elastic, more pointed; blistered surface very sore.

Treatment.—Purg. enema; kreosote wash to mouth; continue quinine, porter, &c.

June 28th and 29th.—Much the same; *continue treatment, with poultice to tumour.*

June 30th.—Patient feels and looks better, but pulse still frequent; some appetite; mouth better; tumour prominent and pointed; fluctuation evident; feels a constant burning sensation in it. With the concurrence of my friend Dr. Slade, I now determined to adopt the suggestion of Dr. Graves, of Dublin, i. e. to make an incision through the abdominal parietes to the peritoneum, with the view to cause adhesion of the liver, and to prevent the escape of the pus into the peritoneal sac. I made an incision about an inch and a half long, and as deep as I thought it prudent to go. This was filled with lint and a soft poultice applied over it. *The quinine, porter, &c., were continued.*

July 1st—Same: continue treatment.

July 2d—Was disturbed, and did not rest well. The tumour being now prominent and quite soft, I determined to open it. The patient was placed on his right side, and inclined forward as much as possible. The lancet was then introduced into the centre of the former cut, to the depth of nearly an inch, when a thin yellow matter began to flow. It became thicker as it progressed, and after a discharge of about six ounces, it ceased—the tumour appeared to be only about half evacuated, but I did not encourage the flow any further at the time. *A poultice was applied, the other treatment continued, and an anodyne at night, if necessary.*

July 3d—Patient feels much better. Took syrup of morphia at eight o'clock, and slept all night. The opening is firmly closed, requiring the lancet to re-open it. The discharge was trivial, although the director was moved about freely in the cavity. A tent was now inserted in the opening, and the treatment continued.

July 4th—Rested pretty well. When the tent was withdrawn there was a discharge of about four ounces of thin pus, mixed with clots and lumps. Bowels too loose.

Treatment.—Insert tent, with compresses on each side—a tea-spoonful of paragoric after every two or three loose stools.

July 5th—Feels better; rested well; bowels checked; tumour smaller; discharged about an ounce. *To continue treatment.*

July 7th—Patient troubled with diarrhœa in the night. Tumour discharged half an ounce of reddish pus.

Treatment—Continue tent and compress—bathe feet and sponge body with nitro-muriatic mixture—infus. quassiæ and chamomile instead of the quinine—chalk mixture for diarrhœa.

July 8th—Bad night; very feeble; bowels very loose; no pain, fever or thirst; poor appetite; abscess discharged about an ounce of reddish pus, somewhat fœtid.

Continue treatment, with the addition of anodyne enemas p. r. n.; a little French brandy and elixir vitriol drink.

July 12th—Patient declining rapidly; bowel complaint returns upon him the latter part of every night; he is very much debilitated and has colliquative sweats; the abscess discharged this morning about ℥ iv. of a mixed pus, having a sickening odor.

Treatment—Injected a solution of chlorid. sodæ into the abscess; other treatment continued.

I now left the city for a period of ten days, and my friend, Dr. J. B. Henderson, took charge of my wards. By particular request, Dr. Slade visited Patrick with Dr. H., and has kindly furnished me the following notes. The chloride injection was repeated on several days.

July 17th—Patient slept badly; diarrhœa troublesome; skin moist; pulse 110. Just before the physicians' visit, *a gush of bile* came out from the abscess, and continued to flow gradually for an hour; quantity discharged ℥iss.

Treatment—Dover's powder morning and night; solution quinine; porter. &c.

15th.—Slept none; diarrhœa worse; skin sodden; pulse 110; discharge of bile from abscess ceased; purulent discharge still continues; urinates naturally. *Treatment, anodyne enemas, paragoric, Dover's powder, porter.*

16.—Appears better; rested pretty well; diarrhœa still; very slight discharge from abscess; no bile; pulse 100. *Treatment continued.*

17.—Much the same as yesterday. *Treatment, pills of acetat. plumb. and opium. Other medicine continued.*

18.—No better; various anodynes and astringents were now tried from day to day, but to no use.

July 24th.—I returned to the city and took charge of my patient again. I had not expected to find him alive. He was extremely low, feeble and emaciated. The abscess in the liver appeared to be almost healed; the cavity nearly obliterated, and the discharge in twenty-four hours amounting to only a few drops of white pus. All efforts to check the dysentery (for such it should now be termed, from the bloody discharges and tenesmus attending,) had proved unavailing; he was now having from six to ten of these discharges a day. No appetite; pulse small and frequent; skin bathed in clammy sweat; distressing thirst; sleeps the fore part of the night, but the dysentery always seizes him before day. From this time he lingered out a miserable existence, suffering daily an intolerable agony with his bowels, in spite of every remedy that could be suggested for his relief. It would be useless to detail farther the progress of the case. He died on the morning of the 29th July.

Autopsy, about six hours after Death.—Present: Drs. Slade, Henderson and others.

Body, very much emaciated. *Chest*—Heart and lungs perfectly sound.

Abdomen—On raising up the abdominal parietes, very firm adhesions were discovered between these, the omentum majus, and the left lobe of the liver. The omentum was turned upwards from its usual position before the intestines, and extended as high as the diaphragm; forming an intermediate attachment between the liver and abdominal parietes. This attachment was as *firm as cartilage*—about an inch in extent each way, and having in its centre an opening as large as a goose-quill, leading from without to the interior of abscess.

Liver—The abscess was situated in the centre of the left lobe—it contained about a table-spoonful of thick healthy pus; the internal surface was covered with granulations, and the adjacent tissue condensed, as might be expected, from the closing of a large cavity. Indeed, the hepatic abscess might be considered as *having healed beyond all danger*; the patient would certainly have recovered, but for more serious lesions in other parts. The right lobe was considerably hypertrophied, and its texture very firm. The gall bladder was full of ropy bottle-green bile; the small ducts throughout the lobe contained yellow bile. Several of these ducts could be traced to the abscess, where they were closed.

The Spleen was considerably enlarged, and its texture very firm.

The Stomach contained some green bilious matters; its mucous membrane was very slightly reddened, thick and rather soft. That of the duodenum was much the same. From the duodenum down the small intestine, three-fourths its extent, was to be seen *an immense quantity of yellow bile*, deeply staining the mucous membrane. The whole membrane appeared thickened, and the *valvulæ conniventes* were very prominent. A few red spots were seen, from which a little blood oozed, as is sometimes seen in black vomit cases. The bilious stain ceased abruptly some four or five feet from the cœcum; here evidences of inflammation and ulceration began to appear, and to increase throughout the whole extent of the large intestine. The ulceration of the colon and rectum was general, and very deep; in one place so near through the intestine, that adhesion had taken place between it and the side of the pelvis. The colon and rectum contained a small quantity of bloody mucous. *Here we find the cause of death.*

The mesenteric gland were considerably enlarged; *the Kidneys and Bladder* were normal; *the Head* was not examined.

Remarks.—This man was first taken sick with dysentery, which a dose or two of salts and laudanum seemed to relieve. Hepatitis then became the prominent disease; but as soon as this terminated in suppuration, and the patient seemed like recovering, the dysentery returned with fatal violence. He surely would have recovered from the hepatic abscess alone. The case is interesting in many respects, but *especially in regard to the discharge of bile from the abscess.*

Dr. Stokes, in his Lectures states, as "*a very curious fact*, that it has been found impossible to salivate persons laboring under hepatic abscess, so that the presence of matter or not, in the liver, may be determined by the circumstance of the patient being susceptible or not of the full effect of mercury." In this observation he says he has the concurrence of Mr. Annesly and other distinguished writers. Our case is *directly at variance*—the mouth was not only *easily* and *quickly* affected, but soon became so sore as to require remedies to mitigate the suffering.

Hepatic Abscess is by no means rare in the wards of the Charity Hospital.—We have met with quite a number of cases within the last four years, generally terminating fatally. There is a case in the hospital at this time, (service of Dr. Mercier) almost precisely similar to the one just reported, so far as the liver is concerned. The abscess was in the left lobe—pointed at the epigastrium—was opened externally—discharged pus for 15 or 20 days, and is now well. In this case the hepatic ascess was not complicated with any other affection.

Our subject had possessed a herculean constitution, yet he could not stand working in mud and water up to his waist, and drinking from a pint to a quart of whiskey, for years.

Case of Neuralgia of the Head—good effects of iodide of potass, internally, and croton oil, externally.—cure. J. S., a stout Irish laborer, aged 28, been living about New Orleans 11 years; has been much exposed for three years past, acting as fireman in the city; occasionally subject to rheumatism. Was seized, on the evening of the 8th of June, with violent pain in the head, chiefly on the left side, extending from the left eye to the occiput. He had been doing extraordinary labor for two or three days previous, and drinking hard. The attack was not accompanied by chill or fever. The following morning he was easy, and went to work again; but at the same hour in the evening, the pain returned with equal violence—and so he went on from day to day, well enough to work in the morning, but suffered great pain every evening; *unaccompanied by chill or fever.* He applied cold water to the head during the paroxysms, but took no medicine.

July 17th. Being unable to work any longer, he entered the Charity Hospital. Dr. Henderson, who was attending to my service at the time, gave him quinine freely during the mornings, had him cupped freely in the evening; afterwards applied a blister to the nucha, and Granville's lotion to the temple and side of the head—but all without any relief.

When I returned, on the 24th of July, I found the patient suffering as much as ever. Observing the regular periodicity of the pain, I wished to witness the effects of quinine, and prescribed it *in scruple doses* during the intermission, and allowed him to drink porter freely, full diet, hot mustard, pediluvia, &c.

He got much worse—became furious during the paroxysm, and had to be cupped and have cold napkins applied to the head.

I then gave him a combination of rubigo ferri and pulv. valerian for a few days. He got no better.

I then prescribed iodid. potass. ʒ ii; decoc. sars. ℥ xii M.—dose ℥ii morning, noon and night, the side of the head to be shaven and rubbed with croton oil, two or three times a day. The nurse misunderstood the directions and gave the whole mixture at three doses. The next day I found him better—the paroxysm had been much lighter. I continued to give him daily iodid. potass. ℈ ij; decoc. sars. ℥ xii. On the second day the croton oil began to inflame the skin considerably, and to cause great local irritation. As this cutaneous inflammation progressed, the neuralgia diminished, and on the third day he had nothing to complain of except the soreness produced by the oil. This was quickly relieved by sweet oil. He mended apace—appetite returned—and he was discharged, cured, on the 8th August.

Remarks.—I have upon several cases witnessed the admirable virtues of these remedies, (iodid. potass. in large doses, and croton oil externally applied.) Indeed, I have never known them fail to cure the most violent and obstinate neuralgias.

SURGICAL WARDS.

Service of Dr. A. Hester.

Case of Rupture of the Urethra—Retention of Urine—Extravasation of Blood—Re-establishment of the Urethrine Canal.—An Irishman, of short stature and robust frame, aged 35 years, fell from the wheelhouse of a steamer, astride the edge of a narrow piece of timber. The force of the blow was expended upon the perineum, producing great contusion of all the parts between the scrotum and rectum. For some distance down the internal face of each thigh, the parts were much contused, and of a dark livid appearance. Three or four days after this accident, the man reached the city, and came directly to the Hospital for relief. This was on the 28th of May. On examination, we found the entire perineum greatly bruised, swollen, tender, and of a very dark color; the scrotum was also seriously involved, black, and the seat of considerable extravasation.—Since the accident he had passed his urine in small quantities at a time, mixed with blood. Without advice, had taken senna and salts. Pulse but little excited; some febrile movement; trifling accumulation of urine.

A catheter was introduced, as we then supposed, into the bladder, and abont eight ounces of a bloody fluid were drawn off, with some pain to the patient. Ordered a mercurial cathartic, and warm hip-baths every three hours; also, hot fomentations in the intervals of the baths. This was on the second day after his admission, and on the 30th May, the third day, no urine had been discharged. Pulse rather excited; perineum more tense; hot and painful; bladder distended. Another attempt was made to introduce the catheter, but without success. In passing the instrument along the urethra, we could feel distinctly the point at which it seemed to pass through the rent. This was its membranous portion. When the instrument was withdrawn, it was found blocked up with shreds of half coagulated blood. It was not until now that we felt convinced that the urethra was completely ruptured. Nevertheless, to sooth excitement and allay irritation, we prescribed warm prolonged hip-baths, nauseating draughts, and emollient cataplasms to the perineum.—By 12 M., of the same day, the accumulation of urine was so great as to cause the patient some uneasiness; the bladder mounted up to the umbilicus; the pulse was quick and the skin feverish; legs constantly flexed upon the pelvis and drawn upwards; countenance rather flushed and anxious. The house surgeon, in the mean time, endeavored to relieve the bladder by catheterism, but failed. We now determined to puncture the perineum, which was tense and protruding; evacuate the extravasated blood, and search for the prostatic end of the urethra, and then push the catheter, if possible, home into the bladder. The catheter was first introduced as far as it could be carried as a guide; then a lancet was pushed into the most prominent part of the perineum by the house surgeon, just over the end of the sound. On withdrawing the lancet, a full stream of dark fluid blood escaped. This ceased suddenly; the wound was enlarged by a bistoury, the finger introduced, and a large quantity of coagulated blood removed.

After clearing and sponging away the extravasated blood, repeated efforts were made to carry the catheter on into the bladder. In this we failed, not being able to find the prostatic end of the urethra, amid such contusion, extravasation, and displacement of the parts. We examined the bladder per anum, and found it pressing upon the rectum. Finding it impossible to reach the bladder "*per vias naturales*," and the sufferings of the patient growing hourly more intense, we decided to puncture the bladder above the pubis, as advised by Druit, Cooper and others, in such cases. Happily for us, and fortunately for the poor patient, whilst removing the hair of the *pubis* with a razor, the man suddenly exclaimed: "I want to make water," and immediately the immensely distended bladder was seen to contract, and a bold stream of dark, thick and turbid urine, escaped from the wound made in the perineum. About ten or twelve ounces were discharged, to the great relief of both the patient and ourselves. Of course the operation of puncturing the bladder above the pubis, was abandoned at once. A warm bath, to be followed by a decided anodyne, (which is a much better diuretic, by the by, than many of the remedies classed as such,) were ordered. At the end of three hours, another copious flow of urine took place as at first, followed by still greater relief. From this time, the urine was discharged through the perineum; the extravasated blood prevented the infiltration of urine, to any serious extent, in the surrounding tissues. For some days he complained of pain and soreness above the pubis; hot fomentations, lavements, and an occasional anodyne, dissipated these symptoms, and the case was left to nature. On the 6th of June, he passed, for the first time, a portion of urine through the penis; the greater portion, however, still escaped through the wound in the perineum.

The quantity of urine evacuated *per vias nat.* gradually increased, whilst that through the wound, in the same proportion diminished.

The 24th June, the patient was permitted to promenade the wards; he had no difficulty in retaining his water; the wound gradually cicatrized, and by the 26th, the fistula was not larger than a crow-quill.—From time to time, in the progress of the case, an effort was made to carry the catheter into the bladder; but from the slight tortuosity of the passage and the tenderness of the parts, we gave it up, unwilling to push matters when nature seemed so competent herself for the task. About the 1st of August, some of the inmates of the Hospital, with some little effort, passed the catheter; since which time, the stream of urine has been larger. In a short time we anticipate a perfect cure.

We regret that we cannot make room for any thing more from the Surgical Wards. Owing to pressing engagements, Dr. Mercier has not been able to make out his observations in time for publication.

We may remark that his case of Ligature of the left Sub-clavian Artery, mentioned in our last, has proved *perfectly successful*—the patient has been long since *discharged, cured.*

MORTALITY OF NEW ORLEANS.

List of DEATHS and DISEASES in the City of New Orleans, during the months of July and August, 1845, *viz:*

July.—Yellow Fever,* 1; Bilious Remittent Fever, 3; Pernicious Intermittent

* Questionable.

Fever, 5; Congestive Fever, 5; Typhus Fever, 2; Typhoid Fever, 5; Injury of the Brain, 1; Concussion of the Brain, 1; Apoplexy, 1; Congestion of the Brain, 14; Cerebritis, 3; Meningitis, 27; Dementia, 1; Angina Maligna, 3; Croup, 2; Bronchitis, 5; Pertussis, 2; Apoplexy of the Lungs, 2; Pneumonia, 7; Phthisis Pulmonalis, 27; Pleuritis, 1; Hydrothorax, 1; Disease of the Heart, 1; Hypertrophy of the Heart, 1; Gastritis, 6; Cancer of Stomach, 1; Gastro-Duodenitis, 1; Gastro-Hepatitis, 1; Gastro-Enteritis, 17; Enteritis, 9; Diarrhœa, 9; Cholera Morbus, 4; Indigestion, 1; Entero-Colitis, 1; Dysentery, 15; Worms, 2; Congestion of Liver, 1; Hepatitis, 2; Hepatic Abscess, 2; Peritonitis, 2; Ascites; Icterus, 1; Metritis, 1; Uterine Hæmorrhage, 1; Recto-Vaginal Fistula, 1; Parturition, 3; Retention of Urine, 1; Dislocation of Cervical Vertebra, 1; Spina Bifida, 1; Tetanus, 4; Trismus Nascentium, 11; Convulsions, 8; Delirium Tremens, 4; Insolation, 36; Paralysis, 2; Scarlatina, 13; Erysipelas, 1; Anthrax, 1; General Dropsy, 3; General Debility, 1; Rubeola, 2; Dentition, 8; Marasmus, 4; Scrofula, 1; Intemperance, 5; Syphilis, 1; Empoisonment by Opium, 1; Unspecified Accident, 2; Explosion of Steam Boiler, 3; Scald, 8; Old Age, 4; Still Born, 20; Drowned, 16; Unknown, 23. TOTAL, 382.

AUGUST, FROM 1ST TO 15TH.—Pernicious Intermittent Fever, 3; Bilious Remittent Fever, 3; Typhoid Fever, 4; Congestive Fever, 3; Hectic Fever, 1; Angina Maligna, 1; Hæmoptysis, 1; Phthisis Pulmonalis, 8; Pneumonia, 2; Bronchitis, 4; Croup, 1; Pulmonary Congestion, 1; Pleuritis, 1; Asphyxia, 1; Disease of the Heart, 1; Aneurism of Aorta, 1; Gastro-Enteritis, 6; Enteritis, 1; Diarrhœa, 2; Dysentery, 7; Tabes Mesenterica, 1; Hepatitis, 2; Peritonitis, 5; Ascites, 1; Icterus, 1; Procidentia Uteri, 1; Extravasation of Urine into the Cellular Tissues, 1; Invagination of Intestines, 1; Parturition, 1; Fracture of Cranium, 1; Ramollissement of Brain and Spinal Marrow, 1; Cerebritis, 2; Meningitis, 4; Luxation of Cervical Vertebra, 1, Trismus Nascentium, 5; Tetanus, 2; Delirium Tremens, 1; Convulsions, 1; Scarlatina, 3; Rubeola, 1; Dentition, 4; Gangrene, 1; Intemperance, 2; Drowned, 2; Anasarca, 1; Murdered, 1; Still Born, 8; Unknown, 12. TOTAL, 119.

ABSTRACT OF A METEOROLOGICAL JOURNAL FOR 1845.

BY D. T. LILLIE, AT THE CITY OF NEW-ORLEANS

Lat. 29° 57′ Lon. 90° 7′ west of Greenwich.

1845. weekly.	Thermometer.			Barometer.			PREVAILING WINDS.	Force of wind in ratio 1 to 10.	RAINY DAYS.	QUANT. OF RAIN.	
	MAX.	MIN.	RANGE.	MAX.	MIN.	RANGE.				INCHES.	THOUSANDS.
7 July	88.5	73.	15.5	30.22	30.00	0.22	S.W.	$2\frac{1}{4}$	5	0	918
14	90.	73.5	16.5	30.22	29.98	0.24	N.	$2\frac{3}{4}$	3	0	472
21	90.	74.	16.0	30.26	30.15	0.11	W.	$1\frac{3}{4}$	1	0	145
28	92.7	78.	14.7	30.16	30.05	0.11	S.W.	$1\frac{3}{4}$	1	0	057
4 Aug.	90.	75.5	12.5	30.21	29.98	0.23	N.	2	1	0	291
11	89.7	72.5	17.2	30.21	30.09	0.12	S.W.	$2\frac{1}{2}$	4	2	172

REMARKS.—The Barometer used for these Observations is located at an elevation of 19 feet above the level of the ocean, and is suspended clear of the wall of the building.

The Rain Guage is graduated to the thousandth part of an inch, and the receiver is elevated 40 feet from the ground.

The Thermometer is not attached to the Barometer; is placed in a fair exposure, and has been fully tested. Regular hours of observation, 8 A. M., 2 P. M., and 8 P. M.

The hottest day of the season was on the 23d July, during which day the mercury, as observed, was, at 8 A. M. 87°; 2 P. M. 92° 5', and at 8 P. M. 88°. Meximum of the 24 hours 92° 7'; minimum 86°. Several of the daily papers noticing the temperature of that day, gave the maximum from 92° 7', even up to 98°; these last quotations, however, must have been incorrect, and the error owing either to an improperly graduated scale, unsuitable exposure, or carelessness of observation; perhaps a little of each. The Thermometer at the St. Charles was cited as a criterion, but this has since been compared with one fully tested, and found to be 1° 7' too high when at 90°. The Thermometer used at the City Bank (a fair exposure) gave the maximum on that day at 92°, but on comparison has been found 1° too low when at 90°.

The Thermometer used for the above table is placed free from any wall or object which can retain or emit heat, exposed to a free circulation of air, and in a position excluded from any reflected heat; was canstantly watched during the day, and was carefully tested previous and subsequent to that day. The same Thermometer, under similar exposure, (excepting being on the second floor,) indicated a maximum temperature of 95° on the 18th of July, 1841, and again on the 21st of July, 1842, indicated a maximum temperature of 93° 5'; but the mean temperature of the 23d July, 1845, much exceeded either of those days, and the mean temperature of the week ending on the 27th July, was greater than any I have recorded for the last six years, and probably equal to any ever experienced in Louisiana.

ERRATA.

We are requested by our esteemed correspondent, Dr. Kilpatrick, to point out the following typographical errors in his paper on yellow fever, published in our last number. We very much regret that errors of this kind have been so numerous in our past numbers, and hope they will be avoided in future.

Page 53, 16 lines from the top, read "to be called *in*," for "called *it*."

Page 45, eight lines from the top, the three sentences following should be on p. 46, closing the list of organic derangements.

Page 45, line 16th from the top, for "lasting from three *or* four days," read "three *to* four days."

Page 47, six lines from bottom, for "life *strings*," read "*springs*."

Page 49, 26 lines from top, for "seldom that *ever*," read "*even*."

Page 50, eight lines from bottom, for "night preceding *the*," read "*he*."

Page 51, 18 lines from bottom, for "*Thirrell's*," read "*Therrell's*."

Page 52, 14 lines from top, for "*discrimination*," read "*dissemination*;" and last line, for "*thise*," "*is the*."

Page 53, first line, for "*nursery*," read "*nursing*;" same page, 18 lines from bottom, for "*Argus*." read "*Ayres*;" same page, 14 lines from bottom, for "*will*," read "*wall*."

The 4th name on the list p. 55 is "*T. Land*," and not "*F. Laud*."

mus Gland, imperial 8vo, with lithographic plates,
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Graham's Elements of Chemistry,
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Johnston's Chemistry,
Webster's do.

☞The above comprehends but a part of my stock of Medical Books to which I am daily receiving additions from all the new publications that appears from the press. On the next page is a Catalogue of works for general reading, to which I would respectfully call attention.

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THE NEW ORLEANS

MEDICAL AND SURGICAL JOURNAL.

NOVEMBER, 1845.

Part First.

ORIGINAL COMMUNICATIONS.

Art. I.—*An Account of the Epidemic Erysipelas; with Cases.* By R. G. Wharton, M. D., of Grand Gulf, Mississippi.

During the months of April and May, 1844, and the first quarter of the present year, the town of Grand Gulf suffered very much from an epidemic, which has been described very properly by Drs. Hall and Dexter in the January No. (1844,) of the American Journal of Medical Sciences, as an erysipelatous fever; the same disease has, since that time, prevailed very extensively in isolated localities throughout most parts of the United States, and even now, is sweeping with unabated violence over many parts of this State. It has assumed in no place a more malignant form than it did in this town; and I am, consequently, enabled to give a description from observation of a form of disease, which whether we regard its frightful appearance, or, in many cases, its intractableness to all remedial means, is unsurpassed by cholera. The name of *black tongue*, accorded to it in the newspapers, was well calculated to inspire terror at its approach, and though unappropriate and vague, that very vagueness rendered it more fearful.

It attacks in so many different forms, and assumes such a variety of appearances, that it is impossible to give any description which will apply to all cases. The first cases assumed the form of acute laryngitis of the most obstinate kind, and several proved fatal in spite of the most active depletion by the lancet, frequent emetics of tartar emetic, warm baths, and blisters to the throat. At the time, I did not suspect this inflammation to be of the erysipelatous kind, but in a short time I became convinced that it was. These cases occurred in March, 1844. Early in April, several were suddenly seized with violent fever, swelling and slight redness of the parotid glands, headache, drowsiness almost approaching to stupor, severe pain in the ear and pains in the limbs. In the course of 36 to 48 hours, the swelling and inflammation extended from the parotid to the larynx, producing all the symptoms of acute laryngitis, such as great difficulty and pain in deglutition, which was performed with a convulsive effort, and with a gurgling sound; small quick pulse, tenderness on pressure of the larynx, anxious countenance, &c.

After these symptoms had continued for two or three days, or had been somewhat relieved, a slight swelling and great tenderness and redness might be seen in some part of the face, generally about the ear or in the ear—sometimes about the nose; the throat symptoms, though much slighter, still harrassed the patient: there was a constant hawking up of a tough mucus, and a difficulty of deglutition different from that first experienced, and now depending apparently on a paralysis of the muscles of the throat. The uvula and velum were of a dark purple, and very much swollen; also the tonsils in many cases; the pulse rose very much in frequency as the swelling of the face progressed; and the drowsiness or stupor also kept pace with it. The swelling extended rapidly, and involved, in most cases, the whole of the face, ears, forehead, and, in a few cases, the whole scalp, sometimes extending down the breast or back. In these cases there was total blindness for five or six days from the swollen state of the face and eyelids; and the patient generally lay either drowsy, with a muttering delirium, or stupid; feet and hands cool or cold; head hot; with great pulsation of the carotids. The pulse is now very weak and rapid, and it is with the utmost difficulty, that the patient takes any medicine or water. In favorable cases the swelling gradually subsides, first on that side of the face on which it commenced; a copious secretion of purulent matter forms under the cuticle; the eyes open; fever gradually abates; and in the course of eleven to fifteen days the patient is convalescent. It may prove fatal in the early stage, (if not actively and promptly treated,) from suffocation. Laryngeal symptoms in the swelling even may subside, and matter may be freely secreted, yet there is an absolute impossibilility of swallowing, from the paralysis of the muscles of deglutition; nervous symptoms supervene—tremors; inability of urinating, and the patient dies about the tenth day, sometimes as late as the fifteenth.

This was the form the first cases assumed; but soon after others occurred, in which the first symptoms were precisely similar to those above detailed; there was violent fever; pain and swelling of the parotids. Sometimes, however, these glands were unaffected, the disease attacking the muscles and tendons on the side of the neck, causing the most excruciating pain, swelling, and tenderness on pressure; there was soreness of the throat, pain and difficulty of deglutition, the fauces were of a dark or livid color, the tonsils were enlarged, and patches of ulceration might be observed, or a tough mucus, which looked like ulcers. In these cases there were severe pains felt in different parts of the body, generally on top of one or both shoulders, shooting up the sides of the neck; sometimes there was violent pain in the feet and legs. Numbness of limbs was a very general symptom. Though these cases constituted a much milder form of the disease than the first, the pains were often excruciating, the fever very high, with a pulse always frequent, sometimes strong and full.

Sometimes, without any external inflammation, the disease attacked the lungs, producing the most malignant form of pneumonia; sometimes the stomach and bowels, producing the most intractible form of gastritis or enteritis. We consider these to be cases of erysipelatous inflammation, because they appeared at the same time, in the same families, some members of which were attacked in this manner, others with the other forms of the disease; and because these are symptoms common to nearly every form of it. I had one patient who was attacked in the usual manner, with chill and high fever, violent pain in the side of the neck, with soreness, pain, and difficulty of deglutition, pains in the limbs. He was relieved in the course of four or five days; when, from imprudent exposure to the sun, he relapsed, and the disease attacked the lungs, stomach and bowels; producing pneumonia with gastro-enteritis, which nothing could relieve. In the relapse there was severe pain in the neck, and sore throat. Again, in other cases, after suffering for two or three days in a slight degree with fever, pain in the neck, and sore throat, the patient was attacked in the stomach with nausea and vomiting, and other symptoms of gastritis which was, as far as I have seen and heard, always fatal. When the viscera, lungs, stomach, or bowels were the seat of the disease, I have never yet seen a case recover. It generally attacked these organs when the patient was in general bad health, was addicted to habits of intemperance, or dyspeptic; always attacking the weakest organ. In these visceral cases there was a degree of malignancy which I have never before witnessed in any disease. In the course of a very few hours the countenance is entirely changed and collapsed; the pulse very weak and not very frequent; the prostration of strength astonishing. You may bleed—which, however, must be cautiously done; cup, bathe and blister; and though you reduce the patient by these means as much as you dare, he still complains that the pain is only partially relieved, and the disease proceeds with more or less rapidity to a fatal termination, while the physician can scarcely even palliate symptoms.

I had a number of slight cases characterized by chill, fever, pains and numbness in the arms and legs, pain and soreness in the side of the neck, slight soreness of the throat. These were generally relieved in the course of four or five days; the patient, however, was left in a very weak and languid condition for several weeks.

The tongue, in most cases, where the stomach was not particularly affected, was not much changed from its natural state; sometimes it was dry and glazed, and in a few cases it was covered from the beginning, with a heavy coat of dark fur.

I saw several cases where the violence of the disease was concentrated in the muscles of the neck, causing the most exquisite tenderness and swelling, which often, in the course of two or three hours, was as large as a hen's egg; this swelling and tenderness would remain stationary for several days, and then disappear, or disperse, by extending in the form of erysipelas of the skin around the neck.

In all forms of the disease there was a remarkable tendency to relapse. Often, when the patient was apparently out of danger, the slighest imprudence was sufficient to produce a relapse of the most formidable kind.

The question whether or not it be contagious, was much agitated on its first appearance here; sufficient proof, however, has since that time accumulated to convince the most sceptical that under certain circumstance it is contagious. It may be taken by inoculation; several cases of this kind have occurred in this county. One of them was that of a physician, who lost his life in consequence. When proper cleanliness and ventilation are neglected, it is very apt to be communicated to the nurses and attendants. This is not peculiar, however, to this disease, but is common to it with many others not usually considered very contagious. By observing the precautions of cleanliness and free ventilation, every member of one family, the head of which had a most violent attack, escaped entirely; whilst in another family, where these wholesome safeguards were neglected, the disease attacked every member, several of them more than once. I have thought that those cases where the throat was deeply affected were most contagious; probably from the fact that the breath of the patient is then more highly charged with the morbific miasm.

Prognosis. Those cases where the viscera are attacked, are, so far as I have seen or heard, always fatal, and especially if our remedies produce only a partial relief. While the pulse is weak and compressible the prognosis is unfavorable. Next in the order of malignancy are those in which there is deep seated disease, swelling and ulceration of the throat, while the face is swollen at the same time. In these cases, if the mind remains clear; if our remedies, especially emetics, are borne well, and if the pulse, though small and rapid, does not give way; and if the nerves are not much affected; though most dreadful, we may entertain a reasonable hope of recovery. The intemperate, and those delicate constitutions whose viscera are unsound, and those of advanced age, are those most likely to succumb under an attack.

Treatment. From the description which has been given of the various forms which this disease assumes, it will at once be readily understood that considerable modification is required in the remedies in consequence of this variety; and that, like all other disease, it must be treated according to the indications in each particular case. When the erysipelas attacked the face and head; and when the throat was at the same time affected, as was the case when the disease first appeared; if the pulse was full and strong, as it was in nearly every instance, free and early depletion with the lancet was indispensable. I had to bleed most of those who suffered under this form of the disease twice, with most happy effect. A few cases occurred in this form in old and debilitated, or intemperate subjects who could not bear the lancet. The next remedy, and a most important one it is, is the free exhibition of antimonial emetics; the laryngeal symptoms here were most urgent, and nothing has such a controlling power over them as tartar emetic. Besides relieving the patient of the symptoms of suffocation, which were most distressing and alarming, the frequent exhibition of emetics had a most happy effect in keeping down the arterial excitement, which ran high; it equalized the circulation, and removed the stupor caused by so great a determination of blood to the head. This emetic, or nauseating course, I had to pursue in some instances for three, four, or even five days.

The throat required some especial remedies; and when the velum and uvula were swollen, as it was in many cases very much from the commencement; free and deep incisions were necessary to relieve the patient of the difficulty of breathing and of deglutition; and after this had been done, or without it, when the parts were not much swollen, but very sore and ulcerated, a strong solution of argent. nitrat. (ʒ i to ℥ i water,) as recommended by Tissot in chronic laryngitis, was applied to the throat by means of a mop or sponge fastened to the end of a whalebone, being careful to make the patient take a little salt and water in case any of the solution might be swallowed. I saw several cases where the uvula and velum were so enormously swollen as almost to impede the entrance of air into the larynx. By free and deep incisions a large quantity of purulent matter was discharged, to the great relief of the patient.

Many of these cases were most distressing to witness; the patient lay drowsy or perfectly stupid, snoring, his face enormously swollen, and of a dark purple color, almost black, so that it would be impossible for his friends to recognize him; eyes closed, a constant sense of suffocation, extreme difficulty (sometimes, for seven or eight hours, an actual impossibility) of swallowing, and it was necessary to raise him to a sitting posture every time he attempted to swallow. This motion almost exhausted him. The pulse is weak and rapid, and altogether the case appears desperate. After remaining in this apparently hopeless condition from 24 to 48 hours, the throat appears a little better; the patient can, by great exertion, take a little water; and now a state of collapse or great debility occurs, and by the cautious exhibition of carb. ammon. and quinine, the pulse becomes firmer, more steady, the swelling of the face rapidly subsides, and with the improvement in swallowing there is a return of appetite.

The convalescence is most tedious; desquamation of the cuticle takes place; large collections of pus form under the cuticle, as well as in the deep seated cellular substance, requiring deep incisions to the bone to discharge it. For a long time the patients looked badly; the skin was red and claret colored, like that of a patient recovering from the small pox.

In those forms of the disease where the internal organs were attacked, though the indications were plain, unfortunately medicine afforded only a temporary relief. The pulse in these cases was weak and soft; and though the severe pain seemed to require venesection, great caution was necessary in its use, on account of the prostration of the general system. I bled, however, a majority of the patients attacked with this form of the disease, and some of them two or three times, and yet I could attribute only a very temporary amelioration of symptoms to it.

Cupping was resorted to, and carried as far as could be borne; and this, too, afforded only a partial relief.

Hot mustard baths was the remedy on which I placed the greatest reliance, both on the general principle of a strong revulsion, and especially because it appeared to be the only remedy which afforded more than a very slight mitigation of the patients' sufferings. I usually ordered it from three to six times in the 24 hours, and continued it each time, as long as the patient could bear it.

Large blisters were applied over the seat of the pain, and in most cases it was difficult to get them to draw; like all the other remedies, however, they afforded little, or only a very partial and temporary relief.

Calomel and opium, in large doses, were used in addition to the revulsive means; and this, too, instead of doing good in many cases, evidently aggravated the symptoms, introducing irritation in the bowels, and great distress; and thus, in spite of every rational resource, we were destined to see our patients, in the midst of the most horrid tortures, snatched away by the relentless hand of the scourger.

In some of these cases, however, towards the close of the disease the patients sunk into a state of quiet delirium, unconscious of every thing; and with little suffering, breathed their last; the sensibilities having been previously exhausted by the violence of the pains. In one case that I saw, the force of the disease was concentrated on the feet and legs, producing the most excruciating pains, as severe as an attack of the gout, extending up the body; with great restlessness, fever, full and strong pulse. In this case I bled freely; and at the suggestion of my friend, Dr. Wilson, of Port Gibson, (who saw many of these cases with me, and whose practical suggestions in all cases of disease are characterized by a discriminating tact rarely met with,) had the patient's feet and legs immersed in a hot lime bath, made by dissolving one gallon of lime in four gallons of hot water. This had a most happy effect in relieving the pains, and, together with other means, which were indicated, soon restored the patient to health. There were a number of cases comparatively mild where the violence of the disease was spent on the face and head, producing fever and pain of the face, which was so much swollen that the person could not be recognised, nor see at all for several days. The patients were very drowsy—constantly in a state of muttering delirium—the feet and legs disposed to be very cold, with great heat of head. In these cases the throat was unaffected; consequently there was no difficulty of deglutition; the pulse, though frequent, being sufficiently firm; there was, besides, no malignancy. A very simple course of treatment was sufficient for these. Mild saline purgatives, the constant application of cold cloths to the head, and hot mustard baths to the feet and legs, wherever there was much delirium or stupor; these were the means which being persevered in for six to ten days, would always bring the case to a favorable issue. A mild anodyne at night was sometimes required in the latter stages of these cases.

There were a number of other cases in which the patients complained of severe pains in the sides of the neck, shooting down the arms; soreness of throat, with considerable difficulty of deglutition; fever, severe headache, numbness of the limbs, and a great depression of spirits, attributable in part to the alarm occasioned by the prevalence of the epidemic. The stomach was generally irritable; the tongue red; pulse weak, and rather frequent. All these cases were successfully treated by hot mustard baths, and copious draughts of a strong hot infusion of eupatorium perfoliatum, which produced a copious perspiration, which was kept up for two or three days, to the great relief of the patient.

I have said nothing as yet about the local treatment for the erysipelas when it attacks the surface, being well convinced from large experience that no local applications have any great effect in the worst cases, and in the milder cases they are not much required. I have, however, in nearly every case endeavored to arrest the progress of the inflammation by surrounding it with blisters, or cauterising the skin around with argent. nit. In many cases the disease was arrested by the lines drawn around it; but in the bad cases where such effects were most desired, they did no good—the inflammation extending as rapidly as if they had not been applied. I have covered the face with mercurial ointment, and have applied nearly every kind of lotion, and from the result of this experience have as much faith in cold water as in any other one remedy. Still it is well to try a variety of them, as there are some patients who receive more benefit from certain lotions than others. For this purpose we may use solutions of sal. ammon., plumb. acet., a mixture of equal parts of alcohol and ether, &c.

Case 1st.—April 6th, 1844, called to see Mr. T. A. Applegate, who was the first one attacked with the erysipelas of the surface. He was taken down the day previous with high fever, swelling of the parotid glands, inability to protrude the tongue. He was very restless—severe headache—tender on pressure over præcordia—drowsy, and rather stupid. Bled to ℥ xvi, cupped over the stomach, and ordered warm bath. 9. P. M.—Bath produced perspiration, but the fever continued high. 7th. Rather better this morning; still considerable fever; gave mass. hyd. grs. xii. At 7 P. M. not much better; complains of general fulness about the head; ordered blister to nape of neck. Was called up at one o'clock, A. M.—found him laboring under acute laryngitis, the inflammation having extended from the parotids, which were still very much swollen and inflamed. There was considerable difficulty of deglutition, which was performed with a convulsive effort, and with a gurgling noise; there was also tenderness on pressure of the top of the larynx. Used frictions of Granville's liniment to the throat, without benefit. At 4 A. M. he was much worse; gave emetic of tartar and ipecac., which afforded considerable though temporary relief. 8th, at 6 A. M., in a very bad condition; countenance frowning; extreme pain in swallowing; pulse quick and tense; frequent watery discharges from the bowels. Bled to ℥xvi; ordered laudanum enema, and applied emollient poultices of onions, tansy and rue, as hot as they could be borne, changing them every fifteen minutes. The poultices had a most happy effect in relieving the urgency of the laryngeal symptoms; much better than blisters to the throat, which I now never use in such cases. At 12 M. still improving, can swallow without that convulsive effort so characteristic of laryngitis. At 3 P. M. pulse 100, soft; free perspiration—gave 10 grains Dover's powder at bedtime. 9th. Slept well—pulse 90, soft; feels much better, still there is considerable pain in swallowing; has much thirst. Ordered 1-3 grain tartar. antim. every two hours, and the hot poultices to be continued to the throat. Visit at 9 P. M. He had vomited two or three times during the day; spits frequently a tough mucus which is secreted in large quantities; pulse 100, quiet; had two operations; ordered 6 grains Dover's powder; discontinued the poultices. 10th. Passed a tolerable night. At 3 P. M., he was rather drowsy and stupid; pulse 110. Ordered the solution of tartar every two hours; this reduced the pulse to 104 by 9 P. M. and produced free secretion from the throat, which appears to be almost entirely relieved. There is still, however, a great determination to the head with considerable discharge from the nostrils. He complains of tenderness and swelling of the left side of the face, in front of the ears. 11th. Face still swelling on the left side. Ordered the tartar water during the day; at night gave hyd. chlorid. mit. grs. vi.; pulv. jalap gr. iv. to relieve the bowels. 12th. The face very much swollen this morning; pulse 110; medicine acted well on the bowels, with some relief; scarified the velum and uvula, which were very much swollen, and impeded the passage of air. I now became aware of the necessity of arresting, if possible, the extension of the erysipelas, and for this purpose surrounded the inflamed parts with strips of emplast. episp., about 1 1-2 inches broad, and covered the face with unguent hydrar. At 7 o'clock, A. M., no better; pulse 116; gave hyd. chlorid. mit. grs. viij. 13th. Medicine taken last night operated once; pulse 110; blisters drew well; face still more swollen; quiet, disposed to sleep. 3 P. M., pulse 120; still drowsy, intelligent when aroused; great difficulty of swallowing from apparent immobility or paralysis of the muscles of mouth and throat. Ordered carb. amm. of which he took two doses without any good effects. 14th. Passed a tolerable night; pulse during the last night varied from 120 to 112, changing every half hour; it is now 112. Ordered 2 grains quinine every three hours; this had a fine effect; under its use the pulse became firmer and more steady. 15th. Much better; pulse 108; continued quinine; the swelling is now abating; swallows much better. Ordered light broths, with a little wine whey. 16th. Improving rapidly; pulse 88; continued quinine every two hours. 17th. Pulse 82; quinine, broths, wine whey; at bedtime ordered a dose of laudanum to quiet the bowels. He was now rapidly recovering, and in the course of four or five days was able to sit up. Large quantities of purulent matter formed on the cheek bone and between the upper eyelids, which had to be opened with a lancet.

Case 2d.—The following case has some peculiarities, worthy of notice. It is the only one that I saw where the tongue was so much affected. June 5th, at eleven A. M., called to see W. Gott, a boy aged sixteen, who had returned from school about two hours before, complaining very much of his tongue, which he said felt very sore, and was swelling rapidly. Found him with a frequent pulse, rather hot skin, complaining of his tongue as well as a pain on each side of neck, the surface of which looked a little red, and was painfully sensitive to the slightest touch. The tip of the tongue for an inch was very much swollen, and there were several white specks on it. Ordered warm mustard baths, blisters to the painful surface of the neck, red pepper tea as a drink, and as a gargle or mouth wash. At four P. M. he was much worse, his tongue enormously swollen, so that his jaws could not be closed, and he could not speak; great difficulty of swallowing, mind stupid, does not complain of pain in the tongue. Pulse 120, and very small; great heat of skin. Ordered mustard bath again—in two hours bled to ℥ xvi, cupped on back of neck and behind the ears, to relieve the determination to the head. I requested Dr. Wilson, of Port Gibson, to meet me; we applied a strong solution of argent. nit. to the fauces, which appeared ulcerated and dark coloured, and scarified deeply under the tongue. At midnight ordered another bath, and and gave strong infusion of serpentaria and chamomile flowers. 6th.—Somewhat better this morning; head clearer; pulse 120, and rather small; continued the infusion. Swelling of the tongue subsided, so that he could articulate. Throat better; repeated the caustic, and gave a cathartic last night of sulph. magnes. which operated well. There was not much change during the day—at midnight was sent for in haste, and found him suffering from intense pain of the throat, so that every breath caused him to cry out; pulse 120, weak and quick, skin moderately hot—mind not clear. Ordered him to be put in a mustard bath for twenty or thirty minutes, and a large blister to be applied to the abdomen. 7th.—Somewhat easier, though not relieved of the pain. Countenance bad—sleeps with eyes half open—rather delirious; pulse 130, weak and irritable. Continue the chamomile and serpentaria with eupatorium. Towards evening became much worse; the pain left the stomach and bowels, and attacked the throat, so that every inspiration caused him to cry out as if he had been stabbed. The pain was deep seated, and changed its seat constantly—first on the right, then on the left side; pulse 140, weak and quick; countenance more cadaverous. Again used the hot bath, as it was the only remedy which seemed to afford even a temporary respite to his agony. The skin having relaxed, added 3 grains quinine to the other medicines. 8th.—Pulse intermitting; delirious all night; skin cool and relaxed; talks coherently when roused; pain still severe, though much lighter; pulse 140 to 150; gave the quinine in large doses, but as it did no good, and he was sinking fast, discontinued every thing. He expired about day, on the morning of the 9th. This boy was apparently of a good constitution. His family had suffered very much from the erysipelas for six or seven weeks, and his was the last case that occurred during the last year. The attack appears to have been brought on by his bathing in a muddy pond of water during the heat of the day.

CASE 3d.—April 26th, 1844. Visited Mr. Calahan, who was suffering with acute pains in the feet and legs, arms, stomach, and bowels. Pulse 85; stomach tender on pressure; tongue clear and rather red; no headache. As he had taken some cathartic pills over night which operated harshly, ordered nothing but a hot bath. At 7 P. M. the bath had not relieved the pains, which were very severe in the abdomen. Cupped very extensively. 27th. Passed a very restless night; the pains are very severe in the hollow of both feet, and in both big toes, one spot being particularly painful. The pain extends from this up the legs and thighs; pulse 80, and rather full; skin hot; very restless. Bled to ℥xxiv; put him into a hot bath. 11 A. M. The bath relieved him for one or two hours; as the pain in the abdomen is severe, cupped again, and gave the following: ℞ mass. hyd. grs. vi., extract conii. grs. iij. M.; ordered the bath again, at 1 P. M. At 2 1-2 P. M., more quiet, and easier than he had been; dozes occasionally; pulse 80, and softer. Says the pains are much slighter, though still severe. Ordered the same dose at 4 P. M. At 6 P. M. complains very much of the pain in his feet and legs, which is very excruciating; has a most distressing nausea and vomiting. Requested Dr. Wilson to see him. We had his feet and legs immersed in a hot lime bath several times during the night, and applied a large blister to the abdomen. The bath relieved the pain very much; the nausea was partially relieved by the blister. 28th. Slept very little, skin hot; pulse 85; complains still of nausea. Gave sulph. morph. gr. i, and as soon as the stomach is composed, ordered hot infusion of eupatorium perfoliatum and a general warm bath. These means relieved him very much. At 8 P. M. complained of inability of urinating—relieved by sp. æth. nit. and emollient fomentations over the bladder. 29th. Slept rather badly, feverish, some pain in the abdomen. Another bath, with the infusion of eupatorium. This again relieved him, and in a day or two he was convalescent. This case differed very much from the ordinary forms of the disease; still I class it with them as it occurred at the same time, and in a family where several others were then suffering with the more common forms of the disease. I have seldom seen a patient suffer more than did this one from the pain in the feet and legs, and I feel confident that without prompt treatment he would have sunk under it.

Art. 11.—*On the Beneficial Effects of Free Bleeding, in a case of Puerperal Convulsions.* By Samuel A. Jones, M. D., of Bayou Sara, La.

The embarrassment unavoidably experienced in describing a case of puerperal convulsions, will be readily conceived, when it is considered that many of the ablest writers of the medical profession, actuated more by motives of philanthropy than sordid interest, have heretofore given it that deliberate consideration, which excludes, in a great measure, any comment from one of limited experience. This embarrassment will be more apparent, when it is admitted that nothing *new* is offered as regards the *depleting* mode of treating this truly formidable complaint. Indeed, the only object in preparing this article for publication, is by reiteration to impress upon the mind of the young practitioner, *the importance of the use of the lancet, the lancet alone, and nothing but the lancet*, as the radical curative agency to be relied upon.

Of course, in every case of puerperal convulsions, symptoms will arise, requiring the judicious administration of appropriate remedies. But, in any given number of cases, however great the similarity of symptoms in each, there is such a want of sameness in the whole, as to make it impossible to adopt an universal rule of practice; the intelligent physician, however, will readily pursue the proper course.

Without further preface, I will now proceed to describe as briefly as possible, the case of Mrs.——, a resident of this place, aged about 25 years, From notes carefully kept during my attendance, I find, I was hurriedly called to see her on Monday, 24th Dec'r, 1844; her husband, who came for me, expressing a fear that his wife would die before I could get to his house. On my arrival, I found my patient in convulsions of that character appropriately denominated by some writers as the *epileptic* form of puerperal convulsions. I immediately bled her; permitting the blood to flow without reference to quantity, until the pulse softened, and the convulsive paroxysm subsided. She lost, probably, at this first bleeding, ℥ xxx. Her consciousness having returned, I commenced my enquiries from herself, as well as from her attendants; and ascertained that she had been attacked with labor pains during the preceding night, but their violence having subsided, her midwife, (an ignorant colored woman,) had left her, saying "her time had'nt come." I further ascertained that she had suffered two convulsive paroxysms before I arrived, which, added to the one under which I found her laboring, made *three*, that had occurred within the space of an hour—intervals of repose only lasting some ten or fifteen minutes. Hence, my gratification on perceiving that for nearly two hours after being bled, there was no return of the convulsions; and I took the precaution, during this time, to make a thorough examination, the opportunity not having offered itself before

I discovered, in the first place, there was a total absence of labor pains; 2d. the child's face was inclined towards the pelvis; 3d. the pulse full and strong, but not quick or hard; 4th. the pupils of the eye very much dilated, and an expression of wonder (not pain) in the countenance. The remaining symptoms were of minor importance, for those enumerated seemed to call for immediate action; nor was I permitted long to hesitate, for at this time, say 2 o'clock, a slight labor pain was felt, which was immediatly followed by another convulsion. The treatment adopted was as follows, viz: cold applications to the head, stimulating frictions to the extremities, and *venesection* again, to the extent of ℥xxx. The convulsions lasted, probably, ten minutes, then gradually subsided, leaving my patient very much exhausted. The symptoms remaining the same as before this last paroxysm, I concluded there was no time to be lost, and that to render assistance at all, it was indispensable to do it during this interval of repose. To change the position of the child—to promote the action of the uterus, and to equalize the circulation, seemed so clearly indicated, that I did not feel a doubt as to the propriety of the course to be pursued. There was no difficulty experienced in altering the position of the child, so as to direct its face towards the left acetabulum; and while engaged in the performance of this duty with my right hand, constant friction over the abdomen was made with the left, frequently dipping the hand in a basin of cold water brought to the bedside for that purpose; and I had the satisfaction soon to perceive, that not only were the pains returning, but the child's head was rapidly descending, with every probability of the infant's immediate expulsion. In this expectation, however, I was disappointed, for the parturient pains, at no time sufficiently strong to expel the child, again began to subside, and ultimately ceased altogether. Cold applications were continued to the head, and sinapisms, substituted in place of the stimulating frictions, to the extremities.

It now became a questionable matter with me whether or not the ergot should be administered. I could not recollect any authority, as a precedent for such a practice in puerperal convulsions; but here was my patient, composed it was true for the moment, but in lingering or unavailing labor—the os tincæ fully dilated—the external parts of generation amply relaxed, and a return of the convulsions every instant expected. I came to the conclusion, that all the circumstances considered, no means should be left untried to induce the return of labor pains; that the delivery must take place, or the mother, as well as her infant, would die. The evidences of her being composed, however, (for she was apparently asleep,) and the hope I entertained of a salutary influence, through the agency of rest, after so much exhaustion consequent upon the convulsions, induced me to defer the exhibition of the ergot, until about 6 o'clock.

I now gave, mixed in half a tumbler of water, pulv. secal. cornut. grs. xx, and in ten or fifteen minutes the pains returned; but my anxiety for my patient may be imagined, when I say the convulsions returned also. It seemed, indeed, that each labor pain was but the precursor of a convulsive paroxysm; and during this horrid condition, (for who can say that has ever witnessed it, that it is not truly horrible,) the tongue was protruded, and badly bitten—blood and froth issued from the mouth—stertorous breathing superseded quiet respiration, and a frightful distortion of the features completed a spectacle of human suffering, sufficient to appal the calmest beholder. But, as is the case, while performing an operation, or while in the dissecting room, with scalpel in hand, seriously pursuing our anatomical investigations, we almost forget we are manipulating our fellow man; so, in the effort I was making at this critical moment to effect immediate delivery, I was surprised that my sympathies were scarcely excited; and I had the pleasure of perceiving, in about half an hour after the administration of the ergot, that my exertions were crowned with success; that is, success as for as the birth of a fine healthy child was concerned, together with the discharge of the placenta. But now, indeed, came on the crisis as regarded the mother; for as soon as one convulsion subsided, another began—no interval of repose was enjoyed by the suffering parent; a remission (not intermission,) between the paroxysms, seemed only to mark the fact of a succession of them. The countenance continued flushed, the pulse excited, indicative of cerebral congestion, notwithstanding the previous copious bleedings. I immediately opened one of the temporal arteries, and also permitted the blood to flow at the same time from the arm in which I had previously bled her, (the bandage having accidentally come off,) to what extent or quantity, I had no means of accurately estimating, but do not think less than ℥ xx, before the softening of the pulse admonished me to arrest it. The cold applications to the head, and the sinapisms to the extremities were continued—but the convulsions, unfortunately for the poor woman, continued likewise, each paroxysm rendering it questionable whether or not she was expiring.

At this stage of the case I asked for a consultation, and, at my suggestion, Dr. P. B. McKelvey, of St. Francisville, was called in.

On the arrival of Dr. McKelvey, at 11 o'clock, P. M., he was made fully acquainted with all the peculiarities of the case, and the treatment that had been pursued by me. This remark is now made, with the view of directing particular attention to the suggestion of the doctor, after he had examined the patient, viz: the repetition of venesection, notwithstanding I had already bled to the extent of ℥ lxxx, believing as I sincerely do, that if the suggestion had not been acted upon, the woman would have died.

The Symptoms, as ascertained in consultation, were, a quick, full pulse, stertorous respiration, loss of the power of deglutition; tongue protruded, swollen and badly bitten; insensibility; pupils of the eyes dilated; convulsive paroxysms, returning at intervals of about ten minutes, each of which lasted probably fifteen minutes.

Treatment.—Immediate venesection, during a convulsion, permitting the blood to flow, until the impression produced on the system was such, as materially to lessen the violence of the paroxysm*—directed the ad-

* The assistant who held the arm of the patient while I was bleeding her, was overpowered by the convulsive action going on, and the consequence was an unintensional plunging of the lancet into the arm, causing a very large orifice. This occurrence, I am inclined to believe, though not intended, proved to be highly beneficial, because I do not think the patient could have survived the loss of much blood at the

ministration of stimulating enemas, and the topical treatment, previously adopted, to be continued.

December 25th. 9 o'clock, A. M. *Symptoms.*—Pulse quick, but soft; respiration not so stertorous; convulsions occurred during the night, at intervals of an hour; deglutition not restored, tongue protruded; pupils still dilated; insensibility.

TREATMENT.—Blisters were substituted for the sinapisms on the extremities, and one applied to the head; enemas had not been given; again ordered them, and an emollient poultice to the abdomen, confined by a bandage.

5 o'clock, P. M.—Convulsions had ceased entirely at 11 o'clock; signs of returning consciousness; pupils still dilated; less embarrassment in respiration; deglutition not yet restored; tongue still protruded; pulse same as in the morning; frequent dejections of fœces, mixed with dark colored mucus.

TREATMENT.—Directed the patient to be kept quiet, and sub. mur. hyd. grs. x, to be given her as soon as she could swallow.

December 26th. 9 o'clock A. M. Respiration natural; tongue within the cavity of the mouth; deglutition restored; had taken the calomel; frequent alvine dejections; appears to be conscious at times; pupils still dilated; much exhausted.

TREATMENT.—The same as last evening, with instructions to dress blisters, &c., and to give a few spoonfuls of water and gruel occasionally.

4 o'clock P. M. Pulse less frequent; speaks when spoken to: seems conscious of her situation at times; complains of headache; pupils still dilated; lochia has made its appearance; countenance more natural; no secretion of milk.

TREATMENT.—Directed to be kept quiet, and to be indulged in the moderate use of light diet.

December 27th. 9 o'clock A. M. Entirely conscious: notices, and for the first time seems pleased with her child; countenance sprightly; pulse natural; bowels regular; appetite good; indeed, the only complaint she makes, is, of weakness, soreness, a slight headache, and the uncomfortable situation in which she was lying.

From this time, the favorable progress of this case, was not characterized by any peculiarity from ordinary cases of accouchement, excepting in one particular feature, viz: the feet and ankles became œdematous *during confinement.* To use the patient's own language, "they had never swelled before to my [her] certain knowledge." And on being closely interrogated in relation to them, during the period of utero-gestation, she said she was very sure they were never swelled up to the first attack of labor pains.

Without speculating on the probable cause of this œdema, I will now hasten to a conclusion, by merely remarking that considerable difficulty was experienced in healing the blisters on the ankles, in consequence, no doubt, of their œdematous condition, and, perhaps, a temporary torpid action of the general system, produced by extreme suffering.

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time, and the rapid flow obviated the necessity of a large quantity of blood to produce the desired effect.

There were about xvi oz. of blood drawn at this time; making altogether, in the course of twelve hours, ninety-six ounces.

Art. III.—*On the Use and Action of the Sulph. Quinine in Large Doses.* By Charles McCormick, Assistant Surgeon U. S. Army.

I have used the sulphate of quinine in large doses, say from four grains to thirty grains and upwards, constantly, for about four years. I have nearly always used the sulphate of quinine simply diffused in cold water as a vehicle. The average dose I usually employ in intermittents, is from ten to twenty grains at bed-time, and a similar quantity the following morning, or three or four hours previous to the period of the expected paroxysm. In remittent and continued fevers, from twenty to forty grains is the average dose in which I administer it, to be repeated every 12 or 24 hours, or oftener, if necessary, until the fever is arrested.

I have employed the sulphate of quinine in the treatment of all kinds of fever; intermittent, remittent, and continued. I have used it in large doses in acute and chronic rheumatism, in neuralgia, in tetanus, dysentery, diarrhœa, cholera morbus, cholera infantum, asthma, icterus, measles, and scarlet fever. I have seen evident and decided benefit from its use in every stage of fever (whether idiopathic or symptomatic), and in rheumatism and neuralgia. I have derived great benefit from it combined with opium in the early stages of dysentery, diarrhœa and cholera. I have given it in large doses, under all circumstances, in the prodromic, the cold, the hot, and sweating stages of fever, and during the intermissions and remissions; in simple uncomplicated fever, and in fevers complicated with various local inflammations of the head, chest, and abdomen; and have never witnessed any injurious effects or unpleasant symptoms *when given in large doses.*

I am fully satisfied from long use of the sulphate of quinine in free and large doses, that I have had fewer diseases of the bowels supervening and ensuing on fever, and that under many circumstances these very affections are relieved by the timely administration of this remedy. A remarkable case of dysentery entirely relieved by this remedy I reported in September, 1841.

Notwithstanding that the propriety of administering the sulphate of quinine in very large doses is a mooted question throughout the medical world, it is far less a novelty in medicine than its exhibition during the exacerbations of intermittent, remittent, and continued forms of fever; or, in other words, when the powers of life are in an exalted condition. We may prescribe any number of grains at a dose, and yet regard the remedy as a tonic; but surely no person would venture to administer it to a patient laboring under high febrile excitement, if he regarded the remedy as belonging to the class of excitants.

An extensive experience in practice, of nearly six years, has satisfied me beyond a possibility of doubt, that it may not only be advantageously given in very large doses, but also (and what I regard as a far more important fact,) that it can be given without fear of any injurious effects, or unpleasant symptoms, in any state of the system whatever, *even during the highest febrile excitement.*

This single fact, for fact it is, is conclusive evidence that this medicine cannot be regarded as belonging to the class of excitants, since they are altogether inadmissible *when the powers of life are in an exalted condition.*

The same experience has taught me that it has no power to augment, or increase inflammation, or retard its cure; and again, that it may with *safety* be given in large doses, *at all times*, and *under all circumstances*; and *that it possesses the power to arrest fever*, and, as a consequence, when administered in the early stage, to obviate and prevent the congestions and inflammations that occur during the progress of fever.

That these statements are facts, any persons can satisfy themselves by administering this remedy in any number of cases, where the powers of life are in an exalted condition, in large doses, and closely watching its effects.

Let it, then, be given in large doses, in any inflammatory disease whatever, at all times, and under all circumstances, and it will not be found to augment, or increase local inflammations; but, on the contrary, to accelerate their cure. Given in large doses boldly and freely, during the high febrile excitement of idiopathic, and arthritic fevers, it will be found to lessen the force of the pulse, diminish the heat and dryness of the skin, relieve restlessness, and throw out on the surface, a general, warm, and free perspiration; in short, in a few hours (generally from one, to three or four,) to cut short the paroxysms and arrest the further progress of fever; generally acting sensibly on the skin, occasionally on the secretions from the bowels; producing free alvine dejections; acting also on the kidnies and bladder, by causing a free secretion and discharge of urine; and sometimes, without any other sensible effect, the fever is seen to subside under the influence of this potent drug.

The quantity necessary to produce these effects may vary in different cases; generally a dose of thirty grains will be sufficient. Where venesection is required, let the sulphate be given immediately after.

Stimulants possess the power of exciting, and sedatives that of diminishing the animal energies. Tonics are classed as permanent stimuli. If, then, a remedy may not only be *safely*, but *advantageously* administered to patients laboring under high fever, acute rheumatism, and fevers with *various* local inflammations, it surely must be admitted that it does not possess the power of exciting the animal energies, and is not, therefore, a stimulant, or tonic; for, as previously stated, such remedies are altogether inadmissible where the powers of life are in an exalted condition. And as, on the other hand, it is found to lessen the force of the pulse, heat and dryness of the skin, allay restlessness, and to throw out on the surface a general, warm, and free perspiration, if given at a time when the powers of life are in an exalted condition; its action more closely resembles that ascribed to the class of antiphlogistics. But it possesses other powers than those of an antiphlogistic, and to these its apparent action as such is to be attributed. When given in large doses I have seen it allay pain, inordinate muscular action, and restlessness. In neuralgia, odontalgia, and acute rheumatism, it allays all pain as speedily as opium; and it often acts not only as an anodyne, but as a soporific. In spasmodic asthma I have seen it afford relief frequently; in one case of traumatic tetanus I gave it to allay spasmodic action, with more effect than opium; and in one case of agonizing pain, brought on by the crown of a large jaw tooth having been broken off in a violent effort to extract it, this remedy gave immediate relief in a dose of about ten or fifteen grains, when a large dose of tinct. of opium had failed.

Here, then, are further properties I have found this remedy to possess, more analagous to the effects of opium, (which is admitted to be a sedative,) than any other remedy we have. But it differs from opium in the greater permanence of its effects, and in not possessing any narcotic properties; and consequently, it may be given in almost unlimited doses. I have given three quarters of an ounce (360 grains) to a patient in the space of twelve hours, with the most beneficial results. Like opium, too, its action is almost immediate upon the brain and nerves. In two or three hours after a large dose, it almost invariably induces a sense of fullness in the head; stricture across the forehead, or slight pain; ringing and buzzing in the ears; and partial temporary deafness. Sometimes a peculiar muscular debility follows its administration.

I have found opium and quinine to have a similar action when given in congestive fever, particularly in the cold stage; these are the remedies I especially rely on in this disease. Like quinine, opium when given in a full dose, seldom fails to arrest the paroxysm of an intermittent, (this I noticed in a report made in 1839,) or to moderate the fever, when given in the hot stage. Drs. Lind, Trotter and others, speak of the beneficial effects of opium, when given in the hot stage. Like opium, too, the action of quinine is highly favored by bloodletting when required. In short, it possesses the power, in large doses, of diminishing the animal energies, and is consequently a sedative.

These are essentially the same facts stated in my report in September or October, 1844. To favor the view I have taken of this remedy, I will add the following extracts from Professor Liebig's work on Animal Chemistry:

"88. With respect to the action of the other nitrogenized vegetable principles, such as quinine, or the alkaloids of opium, &c., which manifests itself, not in the processes of secretion, but in phenomena of another kind, physiologists and pathologists entertain no doubt, that it is exerted chiefly on the brain and nerves. This action is commonly said to be dynamic,—that is, it accelerates, or retards, or alters in some way, the phenomena of motion in animal life. If we reflect, that this action is exerted by substances which are material, tangible, and ponderable; that they disappear in the organism; that a double dose acts more powerfully than a single one; that after a time a fresh dose must be given, if we wish to produce the action a second time; all these considerations, viewed chemically, permit only one form of explanation; the supposition, namely, that these compounds, by means of their elements, take a share in the formation of new, or the transformation of existing brain or nervous matter.

"However strange the idea may, at first sight, appear, that the alkaloids of opium or of cinchona bark, the elements of codeine, morphia, quinine, &c, may be converted into constituents of brain and nervous matter, into organs of vital energy, from which the organic motions of the body derive their origin; that these substances form a constituent of that matter, by the removal of which the seat of intellectual life, of sensation. and of consciousness, is annihilated: it is, nevertheless, certain, that all these forms of power and activity are most closely dependant, not only on the existence, but also on a certain quality of the substance of the brain, spinal marrow, and nerves; insomuch, that all the manifestations of the life or vital energy of these modifications of nervous matter, which are recognized as the phenomena of motion, sensation, or feeling, assume another form as soon as their composition is altered. The animal organism has produced the brain and nerves out of compounds furnished to it by vegetables; and it is the constituents of the food of the animal, which, in consequence of a series of changes, have assumed the properties and the structure which we find in the brain and nerves.

"89. If it must be admitted as an undeniable truth, that the substance of the brain and nerves is produced from the elements of vegetable albumen, fibrine, and caseine, either alone, or with the aid of the elements of non-azotised food, or of the fat formed from the latter, there is nothing absurd in the opinion, that other constituents of vegetables, intermediate in composition between the fats and the compounds of proteine, may be applied in the organism to the same purpose.

"91. Brain and nervous matter is, at all events, formed in a manner similar to that in which bile is produced; either by the separation of a highly nitrogenized compound from the elements of blood, or by the combination of a nitrogenized product of the vital process with a non-azotised compound (probably, a fatty body.) All that has been said in the preceding pages on the various possible ways by which the bile might be supposed to be formed, all the conclusions which we attained in regard to the co-operation of azotized and non-azotized elements of food, may be applied with equal justice and equal probability to the formation and production of the nervous substance.

"We must not forget, that, in whatever light we may view the vital operations, the production of nervous matter from blood presupposes a change in the composition and qualities of the constituents of blood. That such a change occurs is as certain as that the existence of the nervous matter cannot be denied. In this sense, we must assume, that from a compound of proteine may be formed a first, second, third, &c., product, before a certain number of its elements can become constituents of the nervous matter; and it must be considered as quite certain, that a product of the vital process in a plant, introduced into the blood, will, if its composition be adapted to this purpose, supply the place of the first, second, or third product of the alteration of the compound of proteine. Indeed, it cannot be considered merely accidental, that the composition of the most active remedies, namely, the vegetable alkaloids, cannot be shown to be related to that of any constituent of the body, except only the substance of the nerves and brain. All of these contain a certain quantity of nitrogen, and, in regard to their composition, they are intermediate between the compounds of proteine and the fats.

"92. In contradistinction to the chemical character, we find that the substance of the brain exhibits the characters of an acid. It contains far more oxygen than the organic bases or alkaloids. We observe that quinine and cinchonine, morphia and codeine, strychnia and brucia, which are, respectively, so nearly alike in composition, if they do not produce absolutely the same effect, yet resemble each other in their action more than those which differ more widely in composition. We find, that their energy of action diminishes, as the amount of oxygen they contain increases, (as in the case of narcotine,) and that, strictly speaking, no one of them can be entirely replaced by another. There cannot be a more decisive proof of the nature of their action than this last fact; it must stand in the closest relation to their composition. If these compounds, in point of fact, are capable of taking a share in the formation or in the alteration of the qualities of the brain and nervous matter, their action on the healthy as well as the diseased organism admits of a surprisingly simple explanation. If we are not tempted to deny, that the chief constituent of soup may be applied to a purpose corresponding to its composition in the human body, or that the organic constituent of bones may be so employed in the body of the dog; although that substance (gelatine in both cases) is absolutely incapable of yielding blood; if, therefore, nitrogenized compounds, totally different from the compounds of proteine, may be employed for purposes corresponding to their composition; we may thence conclude that a product of vegetable life, also different from proteine, but similar to a constituent of the animal body, may be employed by the organism in the same way and for the same purpose as the natural product, originally formed by the vital energy of the animal organs, and that, indeed, from a vegetable substance.

"93. * * * It is singular that we find medicinal agencies all dependent on certain matters, which differ in composition; and if, by the introduction of a substance, certain abnormal conditions are rendered normal, it will be impossible to reject the opinion, that this phenomenon depends on a change in the composition of the diseased organism, a change in which the elements of the remedies take a share; a share similar to that which the vegetable elements of the food have taken in the formation of fat, of membranes, of the saliva, of the seminal fluid, &c, &c.

"94. Thus, as we may say, in a certain sense, of caffeine, or theine and asparagine, &c., as well as of the non-azotized elements of food, that they are food for the liver, since they contain the elements, by the presence of which that organ is enabled to perform its functions, so we may consider these nitrogenized compounds, so remarkable for their action on the brain and on the substance of the organs of motion, as elements of food for the organs as yet unknown, which are destined for the metamorphosis of the constituents of the blood into nervous substance and brain. Such organs, there must be in the animal body, and if, in the diseased state, an abnormal process of production or transformation of the constituents of cerebral and nervous matter has been established; if, in the organs intended for this purpose, the power of forming that matter out of the constituents of blood, or the power of resisting an abnormal degree of activity in its decomposition or transformation, has been diminished; then, in a chemical sense, there is no objection to the opinion, that substances of a composition analogous to that of nervous and cerebral matter, and, consequently, adapted to form that matter, may be employed, instead of the substances produced from the blood, either to furnish the necessary resistance, or to restore the normal condition."

It would hence appear, that the sulphate of quinine in its medical virtues and chemical composition, is very analogous to opium; that, like opium, too, along with its sedative, it possesses stimulating properties; exciting the motion of the blood, as all will admit who have used it as a stimulant or tonic; increasing the frequency of the pulse, although, at the same time, diminishing its force. Now as regards its exhibition in the tonic or atonic states of the system, it has been shown that it may be safely given in the former, although heretofore, its administration in such cases has been regarded as at variance with sound principles of practice. The use of both opium and quinine has been sanctioned by the profession at large in the low forms of fever, and they have been most liberally used in the atonic states of the system, i. e. when the powers of life were in a depressed condition.

This apparent inconsistency of giving these remedies in such opposite conditions of the system, is satisfactorily explained, when we recollect that, as sedatives, they both act directly on the nervous power, diminishing the sensibility, irritability, and mobility of the system; thus allaying pain, inordinate action, and restlessness; and hence they may be used in a great variety of diseases. Now, a remedy possessing the power of diminishing sensibility and allaying inordinate action, when given in large doses, may yet (as opium) act as a stimulant in small and frequently repeated doses. In the former case, their action is highly beneficial when the powers of life are in an exalted condition—in the latter, proving useful to sustain the system and obtund sensibility.

But, whatever may be its action, I can safely aver that I have never seen any injurious effects or unpleasant symptoms supervene, in any state of the system; and I have given it in every possible condition, and have so constantly witnessed such surprising remedial effects follow its exhibition, as to justify me in regarding it almost as an antidote to the cause originating fever; and as with antidotes, the amount that will be required, will be in exact ratio to the quantity of poison (malaria) imbibed. At the Hopital des Enfants at Paris, M. Guersant employed the sulphate of quinine, after amputating both lower extremities, for the traumatic fever, and with effect. (See London Lancet, from Gazette des Hopitaux, March 14; also, Philadelphia Examiner, Aug. 5, 1843.) I have myself seen it allay febrile action in pneumonia.

Professor Liebig says:—"It is singular that we find medicinal agencies all dependant on certain matters, which differ in composition; and if, *by the introduction of a substance, certain abnormal conditions are rendered normal, it will be impossible to reject the opinion, that this phenomenon depends on a change in the constituents of the diseased organism, a change in which the elements take a share;* a share similar," &c. Now the cause of fever acting on the human organization, may cause therein the waste of some element which is supplied in the quinine when given; and as quinine and the cause of fever, both act immediately upon the nervous system, it must be in that part of the organization we are to search for the explanation of fever.

It would therefore seem, that the proximate cause of fever consists in some modification of the nervous system, by malaria creating a changed change of structure or function, and preternatural waste therein, as is seen to occur in other tissues during the progress of fever, and which are only remedied by the exhibition of articles containing the appropriate elements for the formation of such tissues; for instance, as gelatine is supposed to act in convalescence in restoring the cellular tissue, cartilage, &c.; so quinine may be supposed to act as food supplying waste or change in the organism, produced by fever in the nervous tissue.

I have suggested that it is to the nervous system we are to look to find the proximate cause of fever; inasmuch as the *modus operandi* of the *great febrifuge* is direct on the brain and nerves; and inasmuch as, in in virtue of its composition, it may be termed food for the brain and nerves; as caffeine and theine are so called for the liver; and further, inasmuch as the action of the malaria is direct upon the brain and nerves.

All, then, that has been said in relation to the sulphate of quinine, would tend to confirm this opinion, and an examination of the nature of fever will, I think, have a like tendency.

"That, derangement of the nervous system constitutes the initial link in the chain of morbid actions, which occur in the development of fever, cannot be doubted. The mental and muscular languor, rigours, pains in the back, limbs, and joints, irritability and fickleness of temper, confusion and dullness of the intellectual powers, morbid perceptions of temperature, which so universally usher in febrile diseases, afford unequivocal evidence of general derangement of the nervous system;" or, in other words, they indicate lesion in the function of innervation. Now as this function is admitted to preside over those of calorification, secretion, circulation and absorption, we cannot conceive it to suffer disorder or derangement of function without the others being immediately and invariably more or less implicated; and such is the fact; for in fever, we constantly see alteration of the animal temperature, and general disorder of the functions. Again, as long as this function of innervation is carried on in a healthy manner, or preserves its integrity, those dependant on it, or over which it presides, will also maintain their integrity. This is a state of health. Fear, grief, joy—in short, all the passions, act by the impression they make upon the brain and nerves; and each has been known to cause immediate death. The cause of fever has been also known, in the severe congestive forms, to deprive the patient instantly of all sensation and motion, and at times to cause immediate and almost instantaneous death. How can any other view of the pathology of fever than the nervous, explain such phenomena? Surely, death in such cases could not have been the result of inflammation, as this occupies time to run its course. The rush of blood to the cheeks, as seen in blushing, erection of the penis, and determination of blood to the head in anger, are examples of congestion occurring suddenly, and almost instantly, from the nervous influence. And no appearance in the early stage of fever is more universal than these irregular distributions of the blood, evidently arising from the same cause, viz; nervous influence. Symptomatic fever, or fever dependent on local inflammation, presents the same phenomena as idiopathic; as *ubi irritatio ibi affluxus* is a maxim, it will be admitted, that even in inflammation, the nerves are first implicated, and the action of the vascular system secondary. Certainly, if the sensibility and irritability of the part be not altered, there will be no inflammation, and they exist only where the brain and nerves are present. But in inflammations, as in fever, the functions of the vascular system are deranged, and consequently that of innervation primarily, as it presides over the others. Now the cause of fever, (malaria) acts through the impression made upon the organs concerned in innervation; probably by the malaria being taken into the current of the circulation in the lungs. However this may be, it is evident that its first effects are manifested in the nervous system, and it produces impressions on this system, more or less lasting, and more or less injurious, according to its amount and concentration. And as with poison, corrosive sublimate for instance, its injurious effects cease with the antidote; although, should the poison have excited inflammation previous to the exhibition of the antidote, this latter will have no effect in curing the inflammation; only acting upon the poison and arresting its effects, leaving whatever inflammation may have been excited up to the time of its action. But, as suggested, in all inflammation whatever, the function of innervation is more or less implicated, and consequently quinine, opium, and all sedative remedies have a direct tendency to favor its cure; and such I have found to be the fact. Fever, in most cases, yields only after the lapse of a very considerable time to antiphlogistic remedies; but when to these, are added quinine and opium, it yields readily, (generally in a few hours,) in nearly all cases; certainly in all cases I have ever seen, when given in the earlier stages.

In idiopathic fever, there is a great proneness to congestions and inflammations as long as the fever is suffered to run its course, and when these occur they greatly modify it. Now pneumonia and cases of traumatic fever differ from the idiopathic, and, although presenting the same great general phenomena, are yet different in their condition and course. In idiopathic fever, the malaria produces lesion in the function of innervation; in pneumonia and traumatic fever, the organic lesion is present from the commencement; in other words, the difference between idiopathic and symptomatic fever is, that in the former the cause acts on the general nervous system, and all the inflammations that occur are predominantly modified by the peculiar impression made on this system, (nerves and brain;) and in the latter, the cause acts locally on the sensibility or irritability of the part, causing local inflammation, inducing a state most closely resembling the former, by the local impression being reflected upon the general system; yet differing in this, that, in the former, the nervous, and in the latter, the inflammatory condition predominates; and hence it is, that idiopathic epidemic fevers are found one year to be congestive, and another inflammatory, accordingly as the function of innervation is more or less deeply implicated, and such in a great measure is the cause of the difference observed in the North and the South. The nervous system in the latter suffers most; and we know that in those cases inducing sudden death, the function of innervation is most deeply implicated; and that in synocha, the vascular system is predominantly deranged; and in typhus the nervous; *and that, in all of them, lesion of innervation, is the primary condition, without which, fever cannot exist.* "It is in the regular relation of the nervous and vascular systems, and of their functions, that life and health consist; it is from derangement of their harmony, that disease and death result."

The result, then, may be comprised thus: that, inasmuch, as the functions of calorification, secretion, circulation, and absorption, are all under the influence and control of the function of innervation, (or, in other words, under the control of the brain and nerves), it follows, that when disorder exists in this last function, the others all become more or less implicated in direct ratio with such disorder. As also in the chain of sequences constituting fever, all these functions are prominently and constantly disordered—as the primary link is lesion of function in the nervous system—as all the other phenomena observed in fever are subsequent to this—as the effects of malaria are first manifested on the nervous system—it would seem that the true seat of the proximate cause of fever is in this system. And when we recollect or add to this, that the action of the sulphate of quinine is admitted to be direct upon the brain and nerves—that it is a sedative possessing the power to cut short the paroxysm, and arrest the further progress of fever—the conclusion is inevitable, that the proximate cause of fever must consist in some modification of the brain and nerves. Now, dissections fail to trace the peculiar condition of the brain in epilepsy, &c., and yet no one doubts its nervous origin. The difficulty and impossibility of demonstrating the peculiar condition of the brain in those who die of nervous diseases, explains the great difficulty, and probably lifts the veil, that has enveloped the subject of fever. There is one thing certain: all the dissections that have been made of those who have died of fever, have only served to make us acquainted with the results of pathological phenomena that *occur during the course of fever; but have not revealed the slightest glimpse of its intimate nature or primary and essential cause.* But let all this be as it may; since I have adapted my practice to such views of fever, endeavoring not only to counteract the morbid state of the vascular system by antiphlogistics, but, on the first indication, to remedy that of the nervous also, I have not had a single opportunity to make a post mortem examination of the body of a fever patient; for in my practice, and it has probably been as extensive as falls to the lot of one man, not one case has proved fatal. I have tried all modes of practice, and neither mercury, nor the bleeding and refrigerant plan, will arrest a single severe case. The following plan has proved successful in all subjects, from infants to women during the last month of pregnancy.

In treating fever, then, as well as inflammation, our success will be infinitely greatest when proper attention is paid to the *two great systems implicated, and remedies appropriate to the peculiar state of each system are exhibited.* It has been seen that these two great systems are the nervous and the vascular. In the former, derangement and excitement would seem to shew the action of the cause to be that of a morbid irritant; it may hence be inferred that as the sensibility and irritability are increased to a great degree, the action of remedies possessing the power of diminishing and allaying them, viz., sedatives, should be first invoked to quiet and regulate the nervous system; and with a view to counteract and restore the condition of the vascular system, general and local bleeding, cathartics and refrigerants may be necessary, Now, it may be said that in congestive fever, where the patient is in a state of insensibility and immobility, the cause of the fever has not acted as an irritant, but that the impression it has made, is one of sedation; that this is not so, is seen in the fact that sedatives, such as opium, camphor, and quinine, used in large doses, are the most certain remedies we can employ to restore reaction. It is true a patient in such a state of fever, often bears a strong resemblance to a person laboring under the effects of an over dose of opium; but that the two states or conditions are totally different, must be admitted from the evidence of the effects of the remedy; in the latter, case death would inevitably result from a full, large dose of opium. In the former, it is the surest means of restoring the patient to health. Now, as sedatives, cathartics, &c., do not possess exactly the same virtues, or act in precisely the same manner, the sulphate of quinine is the sedative I employ in fever, as before stated. It possesses the advantage of opium in not possessing narcotic properties, and can therefore be safely given to whatever extent may be necessary.

The first indication in the treatment, is, to cut short the paroxysm and arrest the further progress of the fever. And with this view from twenty to thirty grains of the sulph. quinine is the remedy I administer at first. As has been stated, bloodletting favors the action of quinine, whenever it is required, and this is the only condition I regard previous to the exhibition of the remedy. Wherever violent local determinations are present, especially of the head, bleed to the relief of the symptoms, and immediately give the quinine simply diffused in half a wine glass full of cool water; where there is much nausea and vomiting, combine with it from thirty to sixty drops tinct. opii. If the remedy cannot be retained in the stomach, it must be given in two or three times the quantity, by the rectum. I have generally given from 40 to 60 and 80 grains in this way, dissolved in as little water as possible, by means of sulphuric acid, and mixed with a solution of starch, not to exceed two or three ounces. As this is the remedy to rely on to arrest the fever, it must be administered either by the stomach, skin, or rectum. Should diarrhœa or dysentery be present, give with the quinine forty or sixty drops of laudanum, or from ten to fifteen grains of Dover's powder. Should it be otherwise, any laxative will answer; castor oil, or magnesia, or the latter combined with cream of tartar or epsom salts, will in mild cases be found sufficient; but in all severe forms of fever I employ one or two free mercurial cathartics in the commencement of the disease, and if necessary follow up their use with gamboge, and even croton oil, according to the effect desired and the difficulty of producing it. For instance, in a severe case of congestive fever I have only succeeded in inducing one or two alvine evacuations, after having given, in a few hours, several twenty gr. doses of calomel, fifteen grains of gamboge, and sixteen drops of good croton oil, in addition to eight drops of croton oil I had regarded as bad, because when given to this same patient in a dose of four drops, and repeated once, it had failed; after these had acted on the bowels the patient recovered his senses; and the day following, when the function of the brain was free, a single dose of castor oil operated freely three or four times.

The indications are these: 1st, to moderate the febrile reaction; 2d, to arrest the fever; 3d, to obviate and remedy the various local congestions and inflammations that occur. To fulfil the first, bleeding, refrigerants and laxatives are required; for the second, quinine and opium; and the third, local bleeding, and counter-irritants, but in a great majority of cases the quinine, with mild laxatives and cooling drinks, will be all sufficient. In congestive forms, nothing is to be depended on but opium, camphor, and quinine, to induce reaction; and the latter, in full doses often repeated, can alone be trusted to prevent the recurrence of the fever—mercurials and cathartics, to move the bowels, if necessary. Give, during the cold stage,

℞ Camphor ʒ ii.

Alcohol q. s. to make a thin paste, to which add magnes. calc. ʒ i.; rub well together and add gradually water to ℥ viii.,

Give a table-spoonful every half hour, or oftener, with a few drops of laudanum; or give free doses of opium at first, and follow with the camphor mixture, and at the same time, quinine in 40 to 60 gr. doses, or larger if necessary; the quantity it will require cannot be told: give it during the paroxysm, at least in two grain doses every hour until the fever subsides. Blisters may be applied during the cold stage, but they rarely do good until reaction is established, when they make their full impression, and serve to keep up irritation of the extremities, and thus have a tendency to equalize the circulation.

The quinine, (at times), appears to fail in arresting the paroxysm of fever: but in many cases, after two ten, or two fifteen grain doses had been given, the fever appeared to have maintained its course, and I should have repeated it until the fever stopped, only the remedy was precious and scarce; I, therefore, in all such cases withheld the third dose (after having given ten grains at the end of the paroxysm, and ten grains, three or four hours before,) and in a great majority of cases no more was required. This should be remembered, as it has a great tendency to prevent any person who is not fully aware of it, from coming to a correct conclusion in regard to the action of the sulphate quinine; but let another dose, and another, be given if any doubt exists.

Many of these fevers occur every 7th, 14th, 21st, and 28th day, with as much regularity as a quotidian or tertian, and sometimes assume the former, and at others the latter type. They should be anticipated for one or two periods of recurrence by sulph. quinine 10 grains, given the night before the expected attack, and 10 grains the morning of the attack; this I have found sufficient; if costiveness prevail give laxatives; if diarrhœa or dysentery, opium is the remedy.

May not the injurious effects, stated by several physicians of Paris, to have resulted from the use of quinine, be either attributed to a coincidence of fatal symptoms, at, or near the time the remedy was exhibited, or else to the sulphuric acid given with each dose? They exhibited it in the form of complete solution, and with each drachm dose thus taken, the patient received, in addition to the sulphate of quinine, at least fifteen drops of sulphuric acid; and if this dose were but seldom repeated, it seems to me more reasonable to attribute the injury done, to the sulphuric acid; because all admit, and must have seen great injury from its imprudent use. Whereas few have seen any injurious effects resulting from taking the *simple sulphate of quinine* in large doses *simply diffused in cold water*. For the clear solution of sulphate of quinine in water, it requires one drop of oil of vitriol to each three or four grains of sulphate of quinine. The solution when made, is decidedly acid, and the acid is not taken up by the sulphate to form a bi-sulphate. In my opinion the clear solution of the sulphate of quinine is, in fact, a solution of sulphate of quinine in diluted sulphuric acid. And I can readily believe it may prove highly irritating to the stomach, and even much more mischievous.

Art. IV.—*A few Observations on the use of Large Doses of Quinine in the Treatment of Bilious Remittent Fevers.* By W. J. Tuck, M. D., of Memphis, Tenn.

Messrs. Editors:—In compliance with a promise made to you some time since, to give you an account of some of the diseases incident to our city, and the mode of treatment, I hasten, amid numerous interruptions, to redeem my pledge to the best of my ability. In doing so I prefer the epistolary mode, as ideas, I believe, are thus expressed with more freedom and ease, and are generally more intelligible to the reader.

And may I not be here permitted to remark, that the subject of fever is *one* of the most, if not *the most, important and interesting* that can engage the attention of the physician, and that, he who can suggest *an idea or remedy*, which would tend to relieve the violence or fatality of this disease, under its various modifications, would confer a benefit upon his species, which would entitle him to the gratitude of all mankind and all posterity. It is estimated by Dr. McIntosh, that *four-fifths* of the deaths that have occurred in the world, have been occasioned by fever. How infinitely important, then, is this subject to the physician, upon whose skill, to a considerable extent, hangs the lives of thousands of his fellow creatures.

Situated, as the city of Memphis is, in the midst of the great valley of the Mississippi, in latitude 36°, in a newly settled region of country, and settled by people from every part of the world, it could not but be expected, that fever in various forms should prevail here more or less; and as this city has been growing so rapidly for the last few years; its population so rapidly increasing, (and there is every prospect of a continuation of this increase and prosperity, from the location of the naval depot at this point, as well as from the great natural advantages of the situation,) any account of the diseases most prevalent in this city, would probably be interesting to the physicians of the South and South-West. Should time and opportunity permit, it would afford me pleasure to con. tribute to your interesting journal, from time to time, a brief account of the diseases most prevalent in this city, and the most successful mode of treatment. As the season is now approaching when our city is visited, to some extent, with the bilious remittent form of fever, I have thought that it would be appropriate to allude briefly to this disease, and more especially with the view of pointing out the success which has been attained here in the treatment of this form of fever, by the use of quinine; in illustration of which, a few cases, occurring in my own practice, will be presented.

For a number of years past, I presume, it is well known to all the reading physicians of the South, that quinine has been employed to a very great extent, in the treatment of remittent and congestive fevers; and by many physicians, this medicine is regarded as *indispensable* in the treatment of these diseases in the South. Who was the first to adopt this mode of treatment, as far as I am acquainted, has not been ascertained; but, from the success which has attended it, its author, were he known, should be entitled to great praise. The probability is, that whoever first adopted the treatment, was led to do so upon correct and philosophical principles; for, by a very natural process of reasoning, it might be deduced, that, if the bark and quinine were so successful in arresting the most violent forms of congestive or malignant intermittent fever, as occurred in the practice of Lind, Senac, or Bailly, it might be equally successful in the treatment of the *remittent form*, which is produced by the same causes, and which, in some of its forms, may be regarded as congestive fever; for the word *congestive*, according to the best authors, is merely considered a *condition*, and is associated with intermittent, remittent, and typhus fevers, respectively, and gives them a marked character.

The mode of treatment of remittent fevers by large doses of quinine, was not alluded to, so far as I can remember, during an attendance on two courses of lectures, in Philadelphia, in the winters of 1839, and 1840, and the summer of the latter year; and the first time I ever became aware of it, was through the conversation of a young gentleman from Alabama, who was my room-mate, and who had determined to write a thesis upon the success of this mode of treatment, (as he had been convinced of its correctness from the success attending the practice); from which I attempted to dissuade him, as his views were so contradictory to what I had been taught, and believed to be the opinions of the professors; and I feared that such ultra notions might occasion his rejection. By my own preceptor, a distinguished physician of Virginia, I had been taught, (and this, so far as I was acquainted, was the opinion of the most eminent professors in the country,) that the smallest quantity of quinine would be almost sudden death, if administered in a case of remittent fever. Whether the young gentleman alluded to presented his thesis, I am not aware, as he was not a candidate for graduation until a year after I left. Removing to the South-West in 1840, I had an opportunity of meeting with some distinguished physicians, of enlarged experience, and who, for a number of years, had employed quinine in large doses, in the treatment of bilious remittent fevers, with the most signal success; but my prejudices were so strong, from early education, against such a course, that I could scarcely be made to believe it, until I had an opportunity of witnessing the success with my own eyes during the following summer. In a conversation with Dr. Thomas Fearn, a very distinguished physician of Huntsville, Ala., several years since, he informed me that he was the first person, so far as he was aware, who introduced the use of large doses of quinine in the treatment of a very fatal form of fever, which prevailed in Huntsville, some fifteen years since. He informed me that the disease was producing the most destructive ravages, and was fatal in almost every case. No remedies seemed to be of any avail, but rather did harm, and when any member of a family was seized by the fever, he was regarded as already dead. The doctor mentioned, that he did not recollect what particular circumstances led him to adopt the use of large doses of quinine: but while anxiously casting in his mind to ascertain some more successful plan of treatment to arrest the progress such a fatal disease, he fell upon the use of large doses of quinine, and the first case in which the treatment was employed, recovered. It was tried with like success in the second case—it was adopted by the other physicians; and, from that time, the disease was arrested without any difficulty. In connexion with this statement of my own, permit me to extract the following from Dr. Dunglison's Work on New Remedies;

"A case of severe remittent has been detailed by Dr. Thomas Fearn, in which he gave, at one dose, three teaspoonfuls, weighing thirty-two grains. At the end of an hour, there was a diminution of the frequency of the pulse; the inevitable effect of large doses of quinine when its operation is favorable." Dr. Fearn remarks, that his usual practice in remittent fever, has been to give three doses of twenty grains each, with the interval of an hour between.

But, however this treatment by large doses of quinine originated, or whoever may have been the author, it must be gratifying to every friend of humanity, that so useful a discovery has been made, and that it is now becoming the established practice among the most intelligent physicians of the South; and that where, formerly, death swept over the land with a resistless tide, destroying thousands in his career, we are now able to arrest his destructive march, aud almost insure a speedy return of health where, previously, even a hope to live would have been looked upon as folly.

Having briefly alluded to the history of the introduction (so far as I have been able to inform myself,) of the use of quinine in large doses, in the treatment of bilious remittent fever; and of the reasons by which I was led to adopt the same opinions; I will proceed to give a brief account of the success I have met with in the treatment of this disease in Memphis and vicinity, during the summers and autumn of 1842, 1843, and 1844, and specify a few cases, of which I made very hasty and imperfect notes. My practice during these seasons, has of course been limited, compared with that of older physicians, but, during these three seasons, I think that I have attended, upon an average, from twenty to thirty cases, each year, of remittent fever, and I can state with certainty, that not one of them has died, where the quinine has been used freely, and that, only two deaths of the whole number occurred; one, a well marked case of sporadic yellow fever, in which I was afraid to use the quinine, but now believe that if I had done so in the early period of the attack, the man might have been saved; the other, a negro girl, who was brought here very low, with the disease so much advanced, that medicine could produce no effect. Very few cases have occurred in which convalescence did not commence in the course of three or four days, and in a number of cases, the patient would be attending to his business in a week from the time of the attack. Some of the cases would be denominated congestive fever; but, correctly speaking, should not be called so; since, as has been stated before, congestion only implies a certain condition of disease, and is alike incident to intermittent, remittent, and continued fevers, as well as many other diseases. The cases which I have noted have been done in a very hasty and cursory manner, and will only indicate that quinine has been relied upon as the prominent remedy; although in every case, other means were employed, such as bloodletting, cupping, aperients, &c., as circumstances indicated.

CASE 1.—P. B., boy, about fifteen years of age; sanguine temperament. Was taken with the usual symptoms of fever on the 26th of June. saw him on the morning of the 28th; had been very sick; said he had taken some pills which operated very freely. Had considerable fever when I saw him; skin dry and hot, with pain in the head. Quinine was freely employed for three days in quantities of about 15 grains a-day.

Cupping, mild aperients, and warm baths were also employed, but the quinine was relied upon as the prominent remedy. On the fourth day from the time I saw him, all unfavorable symptoms were relieved. On the seventh day he was able to start home, at Holly Springs, some fifty miles distant. There was a high exascerbation of the fever, in this case, in the afternoon, which was entirely checked on the third evening after using the quinine.

Case 2.—Mrs. J., between 30 and 40 years of age; had been very sick for a week when I was called to see her. Saw her in the morning; pulse very feeble; skin cold, relaxed, and clammy; her extremities cold; I thought her condition very unfavorable; was told she had high fever every afternoon, and was sometimes delirious. Left her quinine to take very freely; did not expect to find her alive next day; directed, also, the use of stimulants. Next morning found her much improved; continued the quinine, with mild aperients, and in a few days she was able to leave her bed, and in a fair way for recovery.

Case 3.—P. T., a man about twenty years of age, very fleshy. Attacked with a severe chill on July 5th, seized with another on the 7th, when I was called in; high reaction occurred; had taken a large dose of ipecac, which he had procured from the apothecary, and which occasioned great nausea and severe vomiting of large quantities of bile; pulse frequent and full; skin very hot; used bloodletting; and after checking the vomiting by appropriate treatment, gave, in the evening, a mild aperient, and a few grains of Dover's powder. By night, the fever was much abated, and the skin moist. The following morning commenced the use of quinine; gave twelve grains—next morning there was no return of fever. The patient continued to convalesce; and by the continued use of the quinine, *proper regimen*, a dose or so of blue mass and Seidleitz powders, he recovered.

I shall only report one other case, which occurred last summer. On Saturday evening, 24th of August, I was called to see Mr. H———, living fifteen miles in the country. Patient, of a delicate constitution; had suffered much from indisposition during the previous portion of the summer, from the imprudent use of medicines. For several days previously, represented as having a paroxysm every morning; fever continuing during the day; remission at night. When I saw him on Saturday evening, near night, his skin was cool, particularly the extremities; and occasionally cold and clammy perspiration appeared on the forehead; he frequently complained of much pain in the stomach; the tongue did not indicate inflammation, yet there was great pain on slight pressure immediately over the *scrobiculus cordis*. With the view of preventing the paroxysm from recurring on the following morning, as I believed the recurrence would probably prove fatal, I gave him during the night at least thirty or forty grains of quinine, until his ears were affected; and applied stimulating applications to the extremities; using also stimulants internally, when the condition of the pulse required them. The patient passed a restless night, talking and muttering most of the time, but towards morning there was a manifest improvement. I neglected to mention, that cups were applied to the epigastrium; and that opiates were also administered, which seemed to have a good effect, and the patient slept some about daylight. About the time at which his paroxysm usually occurred in the morning, he appeared much better; reaction had taken place; skin pleasantly warm; his stomach much relieved and pulse about 75. Being compelled to return to Tennessee, I left directions for the quinine to be continued, with mild cathartics. I heard from the patient on the following Wednesday; he was much improved; no return of fever, and no complaint except debility. Prescribed the bark as a tonic.

A number of other cases, where the effects of quinine were equally conspicuous, might be alluded to; but perhaps the above may be sufficient, as I seek brevity; and besides, even such imperfect notes as are given above, were not taken.

The conclusions, then, to which I have been inevitably conducted from my own observations and from the testimony of gentlemen of correct powers of observation, and much more enlarged experience than my own, is, that quinine in free doses, should be relied upon as the prominent remedy, not only in intermittent fever, but in the bilious remittent form, and, in what are called the congestive fevers of the South. Other remedies, I believe, in most cases are important, according to the constitution of the patient, the violence of the disease, and other indications which may be presented, which remedies will present themselves to the mind of every intelligent and observing physician; but no medicine can be compared to the quinine in free doses, for breaking up that train of morbid actions going on in the nervous system, and which, in its own time, brings round a recurrence of a paroxysm so often fatal.

In regard to the theory of the modus operandi of quinine in the treatment of fevers, different opinions have been advanced. My opinion, from the first time I was convinced of the efficacy it possesses, has been, that it acts primarily and directly upon the nervous system, and indirectly, through the medium of this action, upon the sanguineous system. Aware, that there must always be a proper and healthy balance between the nervous and the sanguineous system; and that quinine exerted a specific influence upon the nervous system; the inference was very natural, that quinine produced its salutary effects in arresting the progress of fever, by restoring the impaired energy of the nerves, and thus controlled the circulatory system, and brought about that harmonious balance between these two systems which is so essential to health. In accordance with this theory, and in proof of its correctness, I have, in various instances, witnessed the effects of large doses of quinine in subduing the frequency of the pulse, and relieving severe pains in the head.

In the case of a young gentleman of this town, last summer, the frequency of the pulse diminished from about 140 to 106, in the course of a night, by administering quinine freely. And another remarkable case occurred also last summer, in the person of a lawyer of this place, who had a severe attack of fever. He had a paroxysm and high fever, every afternoon, for some ten or twelve days; the physician in attendance not thinking it appropriate to use quinine in his case. Other physicians were called in, and in our consultation, it was determined that quinine should be exhibited every hour, until thirty grains were taken. There was no return of the paroxysm that evening, or comparatively very slight; and speedy convalescence ensued by a continuance of the quinine in small doses. Instead, then, of fearing to give quinine when there is febrile excitement, hot skin, frequent pulse, headache, &c., as we are taught by the writers on Materia Medica; and looking upon it as a stimulant and irritant, we are forced to the conclusion that this medicine, in large doses, acts as a sedative. And in corroboration of this conclusion, we find the following statement in Stokes' and Bell's practice, which work, however, I had never read until my own observations and experience had led me to the same conviction. The author says, "A large dose acts at once, or very soon, on the nervous system; and by diffusing the sedative influence throughout all its parts, it completely allays irritation, and induces general tranquility of the functions." And here, in this connexion, permit me to make another extract from the excellent work just alluded to, to prove more satisfactorily the sedative effects of large doses of this medicine, and to show that irritation, which is so much dreaded by many, is not produced, even by the most enormous doses which have sometimes been taken by mistake. This work states an instance of a patient who took by mistake a box of pills containing sixty grains of quinine, and with no other inconvenience than a singing in the ears.

I will not protract this letter by any remarks upon this interesting subject. My object has been to give you a plain and brief account of my own experience and observations in the treatment of fever by large doses of quinine; and, by the statement of facts, together with the corroboration of the evidence of others more experienced than myself, to satisfy those members of the profession who are sceptical on this point; that, quinine is by far the best febrifuge we have in the Materia Medica; also, that there is no danger in employing it in what are styled large doses, and that consequently it may be relied upon with more certainty than any other medicine, in the treatment of fever. We are satisfied, that there are many physicians the South-west who still hold on to the old notions about quinine; who give it only in grain doses, and then only in intermittent fever; and who would not for the world, give a grain of it in remittent fever, especially when *the fever is on;* and it is such notions as these that we wish to combat, and to prove by facts and irrefragable evidence, that these notions are entirely incorrect. We believe, that if other gentlemen would write, and every physician who employs large doses of quinine in the treatment of fever, would record and publish the results of his observation and experience, an array of facts and evidence would be presented, which would convince every physician in the country of the truth of our arguments and the correctness of our practice.

ART. V.—*A Case of Intermittent Fever, attended with a periodical Hæmmorrhage from the Kydneys.* Reported by C. GLIDDEN YOUNG, M. D., Greenwood, La.

Vinkler Harper, aged 8 years, an exceedingly interesting little boy, of precocious intellect, the son of a wealthy and respectable planter of this parish, in October last, had chills, attended with urgent symptoms of cerebral excitement, which disappeared after the operation of a cathartic; which removed from the first passages a large quantity of fruit seed.

He then took quinine, and recovered. After this attack, the mucous membrane of the stomach and bowels was left in a tender, and irritable state; and, notwithstanding the fever had left him, and there was no return of chill, and the little boy was running about, and had a good appetitite; the skin remained harsh, and dry, the countenance sallow, the tongue too red, and the sclerotica presented that peculiar, bluish appearance which is so remarkably characteristic of affections of the spleen in miasmatic districts. Under these circumstances, our further attention did not seem to be needed, and was not required. In the latter part of November of the same year, (1843,) we were informed that our little patient had had a return of chills, that he had taken a dose of calomel, followed by a dose of castor oil and oil of turpentine, and that during the febrile excitement, next subsequent to taking the oil and turpentine, he had hæmmorrhage from the kidneys. We prescribed tinct. opii iij gtt; to be repeated if necessary; warm fomentations to the lumbar region; mucilaginous drinks, and quinine to arrest the chills. The next day he was up, and continued well untill the 11th of December, when he had another chill with return of the hæmmorrhage, and on the 13th we were called to see him.

We were informed that he had had no fever on the 12th, and that he had appeared quite well.

The paroxysm came on this morning (13th) at 11 o'clock A. M. and was declining on our arrival. We found him sitting up in the bed, fretting for something to eat; and he had just been indulged with a few oysters. The tongue was a little furred; the bowels open; the skin harsh, dry, and sallow; pulse 120, feeble; abdomen tympanitic; liver and spleen both enlarged. We were shown a vessel containing about three pints of bloody urine, which he had just voided, and were told that was not half he had passed during the present paroxysm. The urine and blood were so intimately combined, that no estimate could be formed of their relative proportions. Nitric acid and heat both threw down copious precipitates of coagula. Both testicles retracted; he complained of numbness down both thighs, and pain in the lumbar regions on both sides, which was not much, if at all, aggravated by pressure. He had passed only healthy urine during the intermission.

We knew hæmorrhage from the bowels was not generally a dangerous symptoms in our fevers; that, that from the kidneys in this case, subsided when the fever was arrested, in November, and we hoped it would do so again. But the case was unique; the quantity of blood discharged during each paroxysm alarming; and the general state of the system was so vitiated that we doubted whether quinine would control the chills. We accordingly gave a guarded prognosis, and prescribed hyd. chlorid. mit. grs. v; pulv. ipec. comp. grs. iii. to be repeated in six hours. Sp. ether. nitros. ℥ f.; tinct. opii xl. gtt. M.; to take 20 drops every four hours. Cupping over the lumbar region, followed by a warm poultice. To be kept warm in bed, and drink freely of hot teas.

December 14th. Found my little patient with but slight fever; quite restless and fretful. Medicine produced three small mucoid evacuations; no pain in the lumbar region; urine natural; no numbness in the thighs; testicles not retracted; abdomen tender on pressure; slight eruption of urticaria on the breast and legs. Applied four cups to the abdomen; gave hyd. chorid. mit. grs. iii., creta prept. grs. iv., pulv. ipec.

comp. grs. iii., to be repeated every four hours until he has taken four doses. Continue spts. nit. without the laudanum.

15th. Saw our little patient early in the morning. Had some fever; slight pain in the head; tongue more coated, and not so red; tenderness in the abdomen subsided; evacuations bilious; urine natural; skin jaundiced; eruption disappeared. Gave one grain of quinine, after which fever soon rose without any well marked chill. Fever was higher than it had been during any previous paroxysm, attended with slight delirium, and the most copious discharge of bloody urine. Pain in the lumbar region, retraction of the testicles, and numbness down the thighs, all returned. We made use of cold applications to the head and gave ½ gr. tart. emet. The febrile excitement and heat of the skin partially declined, but the stomach became very irritable, and we had in consequence to discontinue its use.

16th. He slept but little during the night, throwing up every half hour, or hour, large quantities of grass green matter from the stomach. His bowels, too, became affected with watery discharges during the latter part of the night. At 8 o'clock A. M. we cupped over the region of the stomach and liver, and gave him grs. x hyd. chlorid. mit., and grs. iv pulv. ipec. comp., and applied a blister to the sacrum. We visited our patient again in the evening and spent the night with him. The general aspect of the case appeared somewhat improved, and he had voided healthy urine about 4 o'clock P. M. The other symptoms referable to the kidneys had not subsided as in the previous intermissions; indeed, to-day there was only a remission. The bowels had been moved several times and the stomach was still irritable. We gave him hyd. sub. muriat, grs. iv, sulph. morph. gr. ⅛, pulv. camphor grs. ii, and put him in a warm bath. In the latter part of the night we endeavoured to keep the stomach quiet with opium and camphor, and the effervescing draught; and at 5 o'clock A. M. commenced giving quinine in doses of two grs. every two hours. Almost contrary to our expectations the quinine was borne remarkably well. Under its influence, combined with that of the opium, he rested, and slept naturally; the pulse became slower, and fuller, and the skin moist. At 9 o'clock, the dose was increased to three grains and another opiate combined with it. In a few minutes he fell into a quiet sleep, and so continued until half past ten o'clock, when we awoke him to administer another dose. He immediately asked for water, and in the same hurried breath called for the chamber; the features of his face were contracted, his countenance anxious in the highest degree, the natural expression of his "soft black eye," which had been so beautiful in health, was changed to a painfully wild and vacant stare. We knew this paroxysm would be the last act in the scene, and the curtain of death would soon fall, and turning round to see who was in the room to witness with us the sad catastrophe, we caught his father's anxious countenance resting full upon us—"is there no hope?"—his looks inquired, and our answer sent a world of miseries to his heart—all is over.

Just before his death he voided a large quantity of bloody urine; and this singular feature of the case—the periodical hæmorrhage from the kidneys, coming on with the paroxysm, subsiding as it declined, disappearing entirely during the intermissions; while the severity of the exascerbations indicated in every instance the quantity of blood discharged—is, so far as we know, unique. The hæmorrhage undoubtedly resulted from extreme venous congestion of the kidneys; and the periodicity of the pathological condition of this organ was distinctly marked. To a greater or less extent a similar condition of the extreme veins, and even the larger trunks of other organs, or of the system generally, always obtains in that fatal form of fever, which has been denominated in some parts of our country, in the emphatic language of the South and West, "*the battle axe of Death.*" And the particular organ upon which the congestion concentrates, is, for the most part plainly marked by a vice of function, and other indications, which it is not our province at present to enumerate. This is a task we may undertake another time.

ART. VI.—*On the Treatment of Ulcers, and certain Cutaneous Affections.* By E. H. KELLY, M. D., of Mobile, Ala.

There is no class of diseases which may be so justly termed "*opprobrium medicorum,*" as that which embraces almost every variety of ulcer, and of cutaneous disease. Thompson says, speaking of ulcers, that out of twenty surgeons, not more than one can be found, who can treat ill-conditioned sores or ulcers, the consequence of wounds necessarily inflicted by themselves, in their operations. Can this be attributed to prejudice and disgust for such loathsome affections; or does it arise from the adverse and complicated distinctions of nosologists; the discrepancy of remedial agents; or, more probably, from the want of a correct knowledge of their pathology? As I have encountered the usual difficulties, and have been much disappointed, in the treatment of such cases, by the routine practice of ointments, lotions, bandages, &c.; and as I have, on the other hand, been very successful in effecting cures, in some remarkable instances, by the application of a certain compound powder, I take great pleasure in now laying before the profession the result of my experience, and the means I have employed.

Having witnessed the surpassing efficacy of wheat flour, as an application, in three cases in which the persons were very badly scalded, some years since, by the bursting of the boilers of the steamboat Walker, I was led by inference to adopt a plan somewhat similar, and based on the same principles, for other breaches of surface and cutaneous affections.

My first case was B——, a young merchant of this city, who had been afflicted for about six months with psoriasis of the back of the hand and between the fingers, which had resisted every remedy in the hands of other practitioners. Greasy, escharotic, and other applications, conjoined with the internal use of sarsaparilla, &c., had been used in vain. It now occurred to me, that if I could produce an artificial crust over the disease that would absorb the acrid discharge, and at the same time protect the tender cuticle beneath, I would succeed in producing a healthy and durable dermoid surface. I directed him accordingly to discharge any fluid that might collect—to bathe his hands with acetic acid, and to follow this up by the following application, which was to be powdered on the surface, *ad libitum:*

℞ Oxymur. Hydrarg. ℈ i.
Lapidis Calaminaris ʒ i.
Marantæ Arundinaceæ opt. ℥ i.

Misce et tere diligenter ut redactius sit in pulverum sublillissimum.

Besides the local application, I directed such constitutional treatment as was adapted to the case, and had the satisfaction of seeing, as the result of my remedies, the perfect cure of my patient in about a month.

My next case was Nicholas M———, a barkeeper, who was afflicted with a disease of the feet, which, on examination, corresponded much with Sir Everard Home's "fungated ulcer" of the sole of the foot and toes. The metatarsal bones and phalanges were denuded of integuments in some places. My patient had used a variety of remedies for more than a year, without any mitigation of his sufferings; and he was now hopeless of a cure being effected. I directed him to use the same formula as above; preceded, however, by bathing the ulcerated parts with a solution of argent. nitrat.; and to take internally fluid extract of sarsaparilla, for constitutional effect. The powder was dusted over the ulcerated surfaces; a scab immediately formed; all pain ceased; granulation was effected; and my patient discharged, perfectly cured, in little more than one month.

On his representation, M———, a barber, consulted me for the same disease, which had annoyed him for about four months, and had made already considerable depredation. He had used various remedies administered by others, but with no avail. The same course was pursued with him for three weeks, with the like happy result.

The above cases were under treatment in 1842–3, and in no instance has the disease returned, or any constitutional bad effect resulted from this method of cure. I have since cured several species of cutaneous disease, as *sycosis*, *herpes*, *&c.*, by this process; and have not yet failed, in the application of it, in any variety of ulcer which I have encountered. The following I will particularise as another evidence of success:

V———, a countryman, applied to me in November last for medical aid, on account of a syphilitic ulcer of the thigh, which was as large as a dollar, and of the depth of an inch. The same plan was adopted; a scab immediately formed; my patient could attend to his occupation; and, notwithstanding this ulcer had resisted all treatment for six months, in other hands, it entirely healed up in less than five weeks, under this mode. Besides the above mentioned, I have discharged, recently, two cases of ulceration about the ancle joint, in which situation most experienced medical men will agree with Sir E. Home, that ulcerations are extremely intractable. In both these instances, the patients were cured in the space of two weeks.

The most remarkable case, however, of the efficacy of this plan of treatment is one which I had the pleasure of discharging this week, cured. Mrs. C———, had been much afflicted with scaly tetter of the hand, for three years. During this period, to use her own words, she had tried various physicians, not excepting the noted (Thompsonian) McLean, formerly of this city. She had used a variety of applications, and taken at least a *barrel of infusion of sarsaparilla*, all with no happy effects, or alleviation of her troubles. I directed her to use the powder in the same manner as in the case of B——, (above described) and to take internally, eight drops of Fowler's mineral solution three times a day, &c. By these means, the disease disappeared in five weeks; and a sound and healthy dermoid texture is now to be observed. What is the *rationale* of the reparation of ulcerated parts? Home and Hunter tell us that it consists in the formation of small red points and eminences, which are termed granulations. That an exudation of coagulable lymph is to be regarded as the first step in the process; that these granulations are supplied with bloodvessels and nerves from the adjacent parts; that these new substances have the same power, *i. e.*, to secrete pus; and that they contract, and are finally covered over with cuticular substance, by which, further secretion of pus is prevented.

We will now take under consideration the indications to be fulfilled in the cure of ulcers; and here we find no settled policy—some recommending greasy, emollient, or applications in the form of vapor; others condemning them *in toto;* and but few evincing a correct knowledge of the pathology and treatment of this class of disease.

The following are the prominent indications to be fulfilled:

1st. The promotion of a healthy secretion of pus; for Thompson tells us, that he has never seen granulations without pus.

2d. To confine and prevent evaporation of matter, so as to retain a moist and warm atmosphere. According to Thompson, a local increase of temperature of two or three degrees, is always necessary to granulation.

3d. To preclude the contact of air and light, (two stimulants;) for the same author says, that ulcers sometimes show a tendency to gangrene, from unknown states of the weather; and ulcers are apt to change their character from vicissitudes of the air.

4th. To protect granulations, and sometimes to repress, without irritation, their excessive growth; diminish serous and puriform discharges, and give support to the ulcer; but this growth must be kept back by only such resistance as they are able to overcome: otherwise the absorbents will remove the granulations.

5th. To promote the formation of scab or cuticular covering.

We see the above indications carried out in Sir E. Home's application of dry lint; which, he says, is to protect the granulations, absorb, retain, and prevent evaporation of matter. So, also, he used powdered rhubarb, *i. e.* to repress granulations, and form skin. Thompson says, Baynton's plan of using adhesive strips, and Whately's process of bandaging, act on similar principles. With like views, Dr. Physick applied his favorite cicatrizer—simple cerate and British oil; Sir E. Home, his alcohol and various innocuous powders; Harness or Thompson, the grated root of the cassava, (a fecula) in weak sphacelating ulcers of seamen, &c.

With all these rules before us, it is surprising how little regard is paid the *lex naturæ* in the cure of these diseases. Does this arise from ignorance, or inadvertence, or nosological errors? That there are some general principles wanting in the cure of these affections, is evident from the fact that very few ulcers will continue to heal under the usual treatment, beyond a certain time, without a change of remedies; and from the multitude of discrepant ones applied by different practitioners, all tending to the same end, but without knowing the why or wherefore. I must here observe, that I do not lay any claim to the discovery of "a new method" in the treatment of ulcers. The treatment by the formation of an artificial scab, is as old as the days of Celsus, of which any of your readers may satisfy himself by referring to his work, "*De Re. Med., Lib.* V, *cap.* IX, *quæ crustas ulceribus inducunt.*" I only wish to attract attention to the modification of an old method, which I have used, and to the efficacy of which I can testify. At the same time, I am perfectly aware, that, by bestowing unworthy and extravagant praise on a remedy, we in reality do but detract from its reputation, and run the risk of banishing it from practice, or preventing its use altogether.

The basis of my remedy, it will be readily perceived, is *fecula*, and with this any medicine may be combined, to suit the wishes of the practitioner. I generally use the formula before described, modified according to circumstances, by increasing or diminishing the strength of the most active ingredient. We all know that, in the healing of sores, wounds, eruptive diseases, &c., nature ordinarily provides a scab, under which a reparation of healthy structure is completed, and the cure effected. Tear the scab off, and the cure is procrastinated. My remedy has a twofold effect: it acts by induction, copying after one of nature's laws in substituting a scab for that which she produces; and it operates simultaneously in fulfilling the rest of the above indications. It is now nearly five years since I commenced the use of it, and I have had no reason to be dissatisfied with its effects in a single instance.

Various powders have been used, heretofore, by divers surgeons, and some at the present day; but as far as I can learn, none unite the twofold object of mine; and few, as I have found, are even capable of producing a serviceable scab. The effects of this remedy I have witnessed as follows:—It affords almost immediate relief to the painful and burning surface, absorbs the discharge, and very soon forms an entire crust over the surface. The tender granulations are protected from the air, and their pruriency arrested by only such resistance as they are able to overcome. The after-secretion readily escapes by the sides of the crust, or through its interstices, without disturbing it or its covering, &c. This artificial covering can be removed at pleasure by any emollient application, and the powder renewed. As the ulcer heals, the crust drops off, as is the case with the natural scab, and opens to view a sound and healthy cuticle, which appears *pari passu* with the desquamation of the powder. This remedy also has an effect, by capillary attraction, of absorbing rapidly the serum from the indurated circumference of an old indolent ulcer; and another decided advantage, of preventing the too great interference of the surgeon and nurses—the efforts of nature being left free and uncontrolled, and the cure not retarded by the operation of adverse means. Without doubt, several ancient remedies obtained credit from this *nimia diligentia medici*, as Sir Kenelm Digby's sympathetic powder, the Royal touch, &c. Sir E. Home says that some ulcers heal better when dressed rarely; and Thompson says that ulcers kept from scabbing will become stationary; and dressing and cleanliness do not answer in all old and indolent ulcers. This, however, must be evident, as John Hunter states, that he has seen in a sore, a white substance, similar, in every respect, to coagulable lymph, and the next day found this vascular.

Another consideration, and which ought not to be omitted, is the easy detection of those who would nourish old sores to secure a home in some public charity; the use of the remedy prevents such imposition. The combining of escharotics with powders was a favorite method of Sir E. Home, which he commended by the illustration of the action of the former on a wart. I have found a similar plan preferable by experience, and there is but little doubt that the indiscriminate use of such severe applications as the former, often produce fungosities, or convert a simple into an irritable ulcer.

In advocating the utility of dry applications, I will call attention to the fact that in almost every species of ulcer and cutaneous disease, some of the ablest surgeons and physicians have condemned the application of greasy or moist remedies. McIntosh opposes the use of unctious applications in impetigo, porrigo and lepra. Abernethy sanctioned the use of powder in erysipelas. Home, Gibson and others, disapproved of poultices and ointments in simple and irritable ulcer; and Thompson says, that practitioners generally agree in reprobating the use of watery remedies in malignant ulcer or hospital gangrene. Although several species of ulcer and cutaneous affection are symptomatic of some constitutional defect—and as a general rule, it would be best to recommend such alterations, and general treatment, as are supposed to modify the system and correct the habits of the patient; yet many of these affections are simply local, and can be treated appropriately by topical agents.

Thompson says, that simple ulcer may assume the appearance of specific; and what is usually denominated by writers, "the fungous ulcer, or hypersarcosis," is only the indolent ulcer rendered irritable. Wiseman states, that it is difficult to distinguish a simple ulcer; that they are all complicated Bateman says, impetiginous disorders are not communicated by inoculation; and that in some of the worst forms of lepra the constitution is unaffected. Earle considered erysipelas essentially an affection of the skin. Plumbe thought the same of impetigo. With such an array of opinion before us for our future guidance, I cannot conceive why surgeons are so timid and fastidious with regard to local active remedies. For my own part, I doubt whether the constitution has so much to answer for, as some represent. John Hunter says, that there is a principle in the human body by which parts are prone to free themselves from disease, and this is evidenced by tumors making their way to the surface; by the bursting of fistula lachrymalis externally, instead of taking the nearer channel of the nose. Thompson says, "every texture and organ of the body possesses physical and vital qualities peculiar to itself;" and if so, we can easily understand why cutaneous textures should become affected without implicating the body. The dangerous consequences of healing up sores by local effects, are considered by Liston to be of unusual occurrence, and capable of being averted by the timely use of issues, &c.* The squeam-

* That sometimes ill consequences arise after the healing of an ulcer cannot be denied; but will it be asserted that the two phenomena ALWAYS, or even GENERALLY, stand in the relation of cause and effect? I must state my opinion in the negative. Some of the worst ulcers the surgeon has to deal with, occur in persons addicted to intemperance, in one way or other. As long as the sore is open and causes him pain and annoyance, he submits in some degree to the orders of his medical attendant; but once cured, the doctor is voted a bore—the bottle and the high seasoned dish are again in request; plethora is induced, and local determination of blood supervenes. Had the directions of common sense, as well as of the doctors, been adhered to, the mischief would probably have been avoided. We are frequently enough censured causelessly; let us not, by encouraging error, lay ourselves open to a still greater amount of vituperation. Let us

ishness of some surgeons—whose opinions are more worthy of the Stahlian pathology of plethora and cacochymy, (with the fantastic application of the former,) than of the present age—reminds me of Slawkenburgius' tale in Tristram Shandy, of the stranger's nose. A portion of the dispute of the faculty I here transcribe, as not being mal-apropos to the subject. "Nature accommodates herself to these emergencies," cried the opponents; "else what do you say to the case of a whole stomach, a whole pair of lungs, and but half a man, when both his legs have been unfortunately shot off!"

"He died of a plethora," said they, "or spit blood, and in a fortnight or three weeks go off in a consumption."

"It happens otherwise," replied the opponents.

"It ought not," said they.

The diseases in which I would anticipate benefit by the process recommended, are—indolent scrofulous sores; excavations in consequence of sloughing; ulcers over large arteries; sinuses; and pustular passages that have been laid open; sores left by burns or chilblains; almost every variety of ulcer, &c. The powder would be found very useful in obtaining a sound foundation in sores with weak action and highly flabby granulations. With regard to cutaneous diseases, I have no doubt that benefit would be derived in porrigo, impetigo and squamous diseases, as it has been already tested and found successful in leprous affections, &c. In many of these cases, opening the pustules, vessels, &c., and removing the false and exfoliating scabs that retard the cure by emollients; then following this up by rubbing in the powder; will be found not only agreeable to the patient, but beneficial in its operation. The powder will absorb the acrid discharge, and prevent it from involving the surrounding skin, which is a point of considerable importance. Besides the above, the application should act beneficially where mercurial preparations may be useful, as rupia, acarus scabiei, &c., certain ulcers—particularly that species described by Sir. E. Home, as occurring on the instep and foot of indolent, luxurious servants of the opulent, an ulcer with thick edges, resembling elephantiasis. The powder diluted, or its basis, is not contra-indicated in erysipelatous surfaces, judging from analogy, from the success of carded cotton as a remedy in the hands of M. Reynaud and Dr. F. M. Robertson. To conclude, my remedy, I think, would be inappropriate in malignant pustule and other diseases needless to recount, as where matter is burrowing and forming sinuses; or, in any case where the process of destruction is greater than that of reparation. And here I close my remarks, omitting much which the limits of this paper will not permit, and which can be better acquired from standard authorities than from my labors. Of course, the same preliminaries would be necessary in the use of my powder as would be requisite for any other remedy, viz.: the laying open of small abscesses, cutting away diseased cellular tissue, poulticing, fomenting, &c., all of which will suggest themselves to the practitioner.

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point out to the patient that not the healing of his sore will endanger him, but his own imprudence or opinionativeness. In such cases are ill effects likely to supervene upon the healing of ulcers, but rarely in others.

Art. VII.—*Case of difficult Labour—Division of the neck of the Uterus—Recovery.* By Benj. R. Hogan, M. D., of Cambridge, Ala., late of U. S. A.

On Saturday night, August 18, 1844, I was called to visit Ann, aged thirty-four, negress and slave. Being unadvised of the character of her case, upon my arrival I found that she was in labor with her first child. I learned from her that she has had *prolapsus uteri* since she was eleven years old, and menstruated at thirteen. The *prolapsus* has been complete, forming *procidentia*, whenever she stood up, until since her pregnancy.

Upon inquiry of the negro midwife who attended her, I learned that she was taken with labor on Friday week previously, and that the waters were discharged on the same evening. When I arrived, I found her upon her feet and hands, suffering the most violent throes, of long duration and rapid succession; she said she could neither lie, sit, nor stand, and was constantly changing her position. Upon an examination *per vaginam*, I felt between the labia, a large, round tumour, completely filling the vagina, which I thought might be a polypus. Upon passing the finger backwards and upwards, with the view of searching for the neck if it were so, I found a small opening, barely admitting the end of my finger. This opening proved to be the *os uteri*, and within I felt the puffy, hairy scalp of the child.

I immediately administered a large dose of acetate of morphine, and sent for my partner, Dr. Benj. E. Cobb, and Dr. Faut, of Prairie Bluff, and my instruments.

The pains were so severe, that I momentarily expected laceration, either at the *fundus*, or *neck* of the *uterus*. Under the influence of the morphine she became tranquil, and able to lie down.

At six o'clock, next morning, the gentlemen both arrived, and assented to the proposition to divide the *neck* of the *uterus*, as the only means of delivery.

The *os uteri* was about the size of a dime, half an inch thick, inelastic, and unyielding. With the catheter I drew off nearly a pint of limpid urine. With the index finger of the left hand, introduced into the mouth of the *uterus*, and passed upwards in the direction of the *symphysis pubis*, whilst the labia were separated and drawn backwards, I introduced a sharp pointed bistoury directly under the arch of the pubis, and transfixed the neck of the uterus about two inches from the mouth, until I felt the point of the instrument upon my finger. I then withdrew the sharp pointed, and inserted the probe pointed bistoury, and cut outwards, as in the operation for *fistula in ano*. The thickest portion divided was nearly an inch and a half of a dense, fibrous structure. The opening being insufficient to admit the forceps, I then took the probe pointed bistoury wrapped the cutting edge with a tape, to within half an inch of the point, and protecting the *perineum* with my finger, I cut posterially, and divided the neck of the uterus nearly an inch. *No hæmorrhage followed the cuts, nor did they give any pain.* These cuts enabled me to introduce a forceps, and deliver a large, well developed child, in a very offensive condition. Nearly a quart of semi-putrid, offensive water followed the child.

The woman was put to bed, with the usual admonitions as to diet and regimen, and recovered in less than three weeks.

I have to-day, (August 14, 1845, twelve months since her delivery,) seen her for the first time. She is now in better health than before her pregnancy—menstruates regularly, and the uterus is constantly protruded, if not supported by a bandage. The *cicatrices* are observable dividing the *os tincæ* by a considerable fissure. The lips are prominent and soft, and the mucous membrane has nearly lost its character. It is dry, and has to the touch, a soft, smooth, elastic feeling. The *pigmentum nigrum* is deposited on the side of the lips of the *os tincæ* (divided by the fissure) as large as a pea. In all other respects the *os tincæ* has now a normal and healthy appearance. There was rather more fullness and plumpness than usual, in other cases of procidentia, which have come under my observation. She informed me, that this swelling of the lips usually took place a week before menstruation, (about which time I saw her,) and continued until the secretion ceased. Her catamenia flows about three days, and, for a week after it ceases, there is some hæmorrhage from the cicatrice of one of the incisions that was made.

This case is interesting in many points of view. It shows that impregnation may take place in a subject afflicted with *procidentia uteri*. In this case I am assured by the subject, that prolapsus occurred in the virgin state; that it has existed, constantly, when not supported, for 23 years. It shows the degeneration which takes place about the neck of the uterus, in such cases, and the change that takes place in the secreting and mucous membrane. And in a practical point of view, it teaches how little danger there is to be apprehended from a division of the neck of the uterus, under the like circumstances.

Professor Bedford's case in a note to Chailly's Midwifery, republished in the New Orleans Medical Journal, for July, 1844, had not reached me at the time of the delivery of this woman. There is some analogy between the cases, and the manner in which the division of the neck of the uterus was performed. Yet the points of difference are various, and I am not aware that a section of the *os uteri* and neck has before been required in this country.

From the history of this woman, I believe she has frequently been impregnated, but generally miscarried about the sixth week.

Art. VIII.—*A Case of "Elephantiasis Scroti," with successful Operation on the same.* By J. M. W. Picton, M. D., of New-Orleans.

The subject of the accompanying sketches, aged about 38 years, and belonging to Mr. J. F. Piseros, of the Parish of St. Charles, was brought to my office on the 6th of June, 1837, by the Hon Alcee Labranche, under the supposition that he was afflicted with hernia. Upon examination, I discovered an enormous mass depending from the pubic region, which I declared to be "elephantiasis scroti," or scrotal hyper-

Fig. II.

Fig. I.

NELSON,
Two months after the operation performed by Dr. Picton.

NELSON,
Previous to the operation performed by Dr. Picton, on October 3d, 1837.

trophy. Mr. Labranche observed that he knew the individual well, and that he was attacked with this malady about ten years before; and that Dr. Price, who then practised upon the coast, in the Parish of St. Charles, had frequently visited him, and pronounced the case an incipient elephantiasis. Since June, 1836, the tumour had considerably augmented in volume and weight. On the 7th of June, 1837, two days subsequent to my examination, I placed him in the "Orleans Infirmary," with the view of obtaining all the facts concerning this extraordinary case, as well as to submit it to the careful examination of members of the faculty of the city. Accordingly, in a few days, I requested the attendance of a large number of medical gentlemen, who confirmed my opinion as to its nature, &c. During that consultation, the mass was weighed, and amounted to *fifty-three* pounds. The opinions of the medical gentlemen present, with reference to any particular plan of treatment, were various; but they generally supposed that a cure was hopeless, and, with the exception of two or three, condemned the proposition of resorting to surgery.

Although I was encouraged, by the constitution of the man, to attempt an operation, yet, with the unsatisfactory result of two or three voices, only, in favor of it, among sixteen or eighteen of my medical brethren, I determined to postpone any definite course until I should have the benefit of their frequent examinations.

In the mean time, I desired to avail myself of certain changes which had occurred in the man, as will be understood by an inspection of the plate, in which, on the left of the tumour, there will be observed a cicatrix, where ulceration took place in 1836. The patient remarked that, at that time, about one pint of serous or albuminous fluid was discharged; that the ulcer continued open nearly four weeks; that the tumour diminished in size during their discharge; and that his periodical pains had subsided. I proposed, therefore, to treat the disease by applying a number of issues at the base of the tumour, and establishing artificial ulceration; conceiving that the facts already before me would justify the delay necessarily incurred for the proper action of this mode of counter-irritation, and more particularly, in a tumour of such consequence. I proposed, also, to administer, at the same time, such agents as might effect a change in the nutrition of the mass, and thereby render the medical effect of the issues as satisfactory as possible.

The day ensuing the general examination, four issues were applied with the potass. pur., and in two or three days they were ready for the usual stimulating applications. Ten days after, four more were made; and the patient put upon the use of iodine, mass. hyd., and a preparation of sarsaparilla. Farinaceous diet, together with frequent bathing, was enjoined. This treatment was pursued, with occasional intermissions, for four or five weeks, with considerable advantage. The issues discharged freely, and the tumour diminished in weight, although not in volume; for a species of erysipelatous inflammation, with tumefaction, appeared around each ulceration, which rendered the parts painful.

Finding that this mode could not do much to prevent an increase of the tumour, I permitted the issues to cicatrize, but continued the internal agents. These were persevered in, with the concurrence of Dr. W. Stone, then surgeon of the Charity Hospital, and Dr. McFarlane, of the

Orleans Infirmary. Dr. Stone had observed the case since its admission into the Infirmary; we met often, and after the most mature reflection, believed that the only possible chance was the use of the knife.

This decision was formed during the month of August, but in consequence of the existence of numerous cases of yellow fever, and the consequent occupation of the physicians, together with the heat of the season, I did not appoint any time to perform the operation.

Every thing, however, being in a favorable condition near the close of September, I informed my medical friends that I should operate on the 3d of October.

In compliance with my invitation, the medical gentlemen whose names are below,* assembled at the Orleans Infirmary, at eleven o'clock, A. M. The preparatory arrangements were completed at twenty minutes after eleven.

I gave a succinct account of the nature and duration of the disease, and observed that three considerations would be involved in the performance of the operation: *First*; the practicability of success, with the preservation of the life of the individual. *Secondly*: an extirpation of some portion, or the whole of the genital organs, if diseased. *Thirdly*; the formation of an artificial scrotum, and the best mode of effecting the union of parts in the event of the healthy condition of those organs.

With reference to the first consideration, it was evident that the venous and lymphatic circulation would be found mainly to contribute to the volume and, perhaps, nutrition of the tumour; and that a division of such vessels, would withdraw from its own particular and abnormal circulation, rather than from the system at large. It is true, that previous to the operation, I had discovered two large arteries, one on each side of the neck of the tumour, yet I believed they would be sufficiently under control. Consequently, there could be no great apprehension from an excessive loss of fluids, with proper assistance.

As the second involved considerations wholly dependent upon the first, nevertheless, they would determine the *utility* of the operation, although its *practicability* had been ascertained.

The preservation of the penis and testicles was regarded by very few of the medical gentlemen, who had examined this case, from time to time, as at all attainable. Some proposed entire abscission, after the mode of Dr. Jacobs, of the West Indies; others, the use of the ligature; another supposed that a removal of a portion of the mass, repeated at appropriate intervals of time, would succeed better.

From the length of time the disease had existed, the complete envelopment of the genital organs, and the traction of the same consentaneously with its growth, it was very reasonable to conclude that their structure and functions had materially changed; that the compression necessarily exercised by such a compact tissue, would nearly destroy the body of the penis, leaving only the urethra, and obliterate the spermatic cords, and, thereby, destroy the testicles.

Since the genital organs were entirely concealed by the encroachments of the tumour, it was impossible to ascertain their positive condition, and

* Drs Davidson, Labatut, Landreaux, Stone, Hunt, Puissan, assistants in the operation. Drs. McFarlane, Thompson, McNeil, Kennedy, Gaillardet, Thomas, Carey, Harris, Meux, Gray, Wharton, Heermans, Harral, Lambert, St. Martin.

in seeking an approximation, even, to their normal state, would be very unsatisfactory, by a reliance alone upon the declarations of the patient; when nature had interposed an almost insuperable obstacle to the exercise of two of the most essential qualities of the surgeon—delicacy of touch and acuteness of vision. The only means I possessed, therefore, was an exploration of the glans, through the protruded prepuce, which is seen near the centre of the mass, about four inches from the orifice. This mode was often resorted to, by the introduction of the finger, and I determined that the penis, at least, was essentially and integrally sound, or so far so, as to warrant the trial to preserve it. My opinion was based on the following grounds: the uniform elasticity and sensibility of the tissue forming the glans, titillation inducing an effort to erection, and an augmentation of its volume, thereby indicating that its erectile power had been retained.

The patient stated that he was subject to pain, and almost always to uneasiness, in the posterior and middle portions of the tumour, and near each groin, in the direction of the inguinal canals. Upon exercising pressure over this track, he experienced much of that peculiar sensibility which belongs to the testicles and spermatic cords. He was also sensible of partial erections, which occasionally produced the sensation of violent distension.

I therefore believed, that the external genital organs had retained, in a great degree, their appropriate functions. The great traction exerted upon the cords contributed much to his pains, and he, therefore, very naturally resorted to a broad suspensory bandage, which was passed beneath the tumour, and crossed over the shoulders behind the head; and upon a recurrence of more than usual pain, he elevated the mass, and obtained relief.

These circumstances justified me in attempting the preservation of those organs; or, at least, to bring them under my inspection during the operation.

With regard to the third proposition, it was highly important to select such a portion of the tegumentary tissue, in forming the artificial scrotum, as would least incur the hazard of a return of the disease, and to make due allowance for the retraction of that tissue, in order to accommodate properly the testicles and elongated cords. That portion was found less diseased from the perineal space down the posterior surface of the mass, about six inches, and covering a superfices of perhaps twenty-five or thirty square inches. The tumour presented here, a less lobulated appearance; the skin was thin and soft, and exhibited the lustre peculiar to the black. I, therefore, selected this portion, and determined to use interrupted sutures.

Operation.

The patient was placed upon a low bedstead, guarded by a hard mattrass, with the nates resting upon its edge. Both of the legs were elevated, and the feet supported on stools; the thighs being separated sufficiently to admit an assistant on my left, to sustain the tumour. An assistant was placed by the side of each of the lower posts of the bed, to secure the knees and feet in their position; and with two other assistants for the trunk and upper extremities, I now proceeded to the operation.

I made an incision five or six inches long, from the upper part of the prepuce, and downward upon the dorsum of the penis, which at once exposed the glans. This incision was then prolonged about three inches. From the extremity of the last, I made a curvilinear sweep over the left half of the tumour which terminated in the *raphé*, about six inches from the centre of the perineum. Owing to the immense hæmorrhage at this stage, and the density of the parts, it was necessary to proceed with great caution. As it was impossible to think of the delay of resorting to ligatures, I used several pairs of Amusat's forceps, and, where the divided vessel was not too large, and could be brought within the grasp of the instrument, I succeeded in securing them. Another difficulty occurred, in an abundant discharge of serum, which prevented the vessels from being brought into immediate view, notwithstanding free sponging.

When the larger vessels had been secured, I repeated the incisions through the divided space, only to encounter, however, more formidable and repeated loss of blood, until at the depth of 6½ or 7 inches, I exposed the sheath of the spermatic cord. After the shortest possible delay in securing new vessels, a free incision was made in the supposed direction of the testicle, and it was soon liberated, and found to be perfectly sound, together with its appendages.

At this period of the operation, two of my assistants having become too much fatigued to sustain the tumour any longer, I was compelled to place it upon my left thigh and return to the *trajet* of the curvilinear cut, and complete it.

The patient, at this stage, fell into a state of great faintness; and whilst he was judiciously plied with stimulants, I determined to avail myself of his condition, and complete the remaining important part of the operation, to wit, the curvilinear cut of the right half of the tumour, which would enable me to remove it without difficulty.

In order that this should correspond as nearly as practicable with the first, and to take advantage of the cessation of the hæmorrhage, I was compelled to retain myself and the tumour in the same relative position, or lose many valuable moments in changing to the right side of the patient, and transferring my charge with the conviction that I must be obliged to resume it, and dissect around the penis. I therefore requested the assistant on that side, Dr. T. Hunt, to make the incision, commencing from the upper extremity of the primitive one, and conduct it to the point, already indicated, on the posterior surface. This was accurately and promptly done. I then passed at once to the spermatic cord and testicle of that side, and discovered them in the same relative position, and equally as healthy as the other.

Nothing now remained but to disengage the penis, and remove any portion of the diseased mass left in the triangular space reserved for the scrotum. This was soon accomplished, and the tumour removed, as well as such parts of it as were necessarily left above the second and third great incisions, in the circumference of its neck.

The posterior flap or space was now brought up, in such a manner that its point should rest upon the corpus spongiosum at the incurvation, and opposite to the *point* on the dorsum of the penis, near the os pubis, formed by the second and third cuts. It was then secured by sutures on each side; the testicles, &c., cleansed, and placed by the side of each other. The penis which was large, and four and a half or five inches long, was enveloped in lint, and kept elevated, the dressing being completed by appropriate bandages.

At the expiration of two months, the patient was discharged, cured; the parts presenting the appearance as lithographed in plate No. 2. I have seen him frequently since that period, and he has been examined by several medical gentlemen of the city. Not the slightest vestige of the disease remains.

The tumour was placed in the anatomical cabinet of the University of Pennsylvania, under the charge of Professor W. E. Horner, where it remains, in a fine state of preservation.

ART. IX.—*Remarks on Yellow Fever.* By JOHN HARRISON, M. D., Professor of Physiology and Pathology in the Medical College of Louisiana. [*Concluded from page* 148.]

PROGNOSIS.

Some of the symptoms I have already spoken of as being unfavorable; but it may be worth while to sum up their value in a general way.

A case of yellow fever promises to terminate well or ill, in proportion to the development of the fever, and according to the absence or presence of nervous symptoms. When the rigor is slight; when the ensuing fever is well developed; when the pulse is open, full, and strong, beating from 108 to 120; the eyes a little injected; the tongue slightly furred; when the pains in the head, back, and limbs are severe; the case is likely to do well, provided it be properly treated and nursed. On the other hand, where there is a strong disposition to sleep; where there is not much complaint heard; where there is either sullenness, listlessness, and extreme languor, or agitation of the mind from fear of death, or any other cause; where the pulse is weak and quick, or easily changed in its beats by muscular motion, such as sitting up in bed; or, where the skin is pale, sallow, or cool to the touch, the case is one of extreme danger. In the above remarks, I refer to symptoms occurring in the beginning of an attack; during the progress of the disease, other symptoms make their appearance; these will be spoken of presently. All marked changes in the usual expression of the countenance, either at the commencement, or occurring as the disease progresses, are decidedly unfavorable symptoms. Contraction of the brows, risus sardonicus, twitchings about the mouth, picking of the bed-clothes, are of this character. It is another unfavorable sign to find the patient listlessly lying on the side of the bed with his head drooping over. Petechiæ are also unfavorable symptoms, particularly if they make their appearance as early as the third day. Great and unaccountable depression of spirits, either in the beginning, or in the course of the attack, is an exceedingly bad symptom. Delirium, coming on late in the attack, is a fatal symptom; so, also, is suppression of urine, which must not be confounded with retention. Sighing and moaning are bad symptoms. Irritability of the stomach, coming on after the febrile excitement has subsided, is another bad symptom; in the beginning, however, I do not consider it of much consequence, if the other symptoms be favorable. Neither do I so consider jactitation, when it occurs early in the attack, and is caused by pain; jactitation without pain is another thing, and a very bad symptom. When the tongue continues to look clean and healthy for a day or two; or when it becomes clammy, after the fever has subsided, the prognosis is unfavorable. Passive hæmorrhages, and black vomit have been already spoken of. With regard to the sweats so common during the febrile stage of the attack, they are thought by many to be favorable. I do not think them of much consequence, one way or the other; certainly, I have lost patients, in spite of every attention and precaution, who perspired finely during the first days.

I believe I have now spoken of the most important of the symptoms, with the exception of the strange and exquisite tenderness, which comes on late in the disease, at the epigastrium, and, indeed, over all the body. It is the most fatal symptom I know of.

Treatment.

Of all the diseases which afflict the human race, there is none that requires more unremitting care and attention on the part of both physician and nurse, than yellow fever. Accidents, or acts of imprudence, which, in other diseases, are mere trifles, are of tremendous importance in this. The mere getting out of bed, has cost many a man his life. Exposure to cold currents of air, or negligence to take the requisite precautions against a change in the weather, has been equally fatal. A man in this disease, however safe the physician may think him, is hovering between life and death—a trifle may decide his fate. Hence the great necessity, the all-important need of good and experienced nurses.

There is, from the very commencement of the attack, a great and rapidly increasing prostration of strength, inconceivable to those who have never experienced the disease. The mind cannot act; the senses, at first exceedingly acute, become during the progress of the disease, indifferent to impressions; the muscular power is almost annihilated; the patient is indifferent to fate, or morbidly anxious about trifles. The extraordinary disturbance in the nervous system, its extreme liability to undergo change from the slightest impressions, enforce upon the attendants and physician, the greatest prudence and solicitude. Experience has taught *them* this, but, unfortunately, all injunctions are frequently lost upon the sufferers. *They* cannot be made to understand how the mere getting out of bed, or even the sitting up in it, can be of so much importance as they are asserted to be. Their feelings deceive them; they make in some unguarded moment the trial, and conviction comes too late.

When a person is taken sick with this disease, no time is to be lost—not a minute. The physician and nurse should be with him as early as possible. His room should be in some quiet place, and, if possible, in the second story, on account of the dampness of the ground-floors. The windows should be closed, for many suffer much from intolerance of light. The room should be well aired, care being taken to protect the patient from currents. All persons, except those attending on the sick, should be rigidly excluded; conversation on the part of the patient, or others, prohibited. These last injunctions are all-important, for it is not an uncommon thing for the sick to be annoyed with unseasonable visitors; who, to gratify an idle curiosity, rush in where they can do no possible good, and may inflict measureless harm. The patient should not, from the very hour of his attack, be permitted to rise from his bed, for any purpose whatever. No matter how supported, or with what precautions and care he be taken out, it is always dangerous, and often leads to a fatal result. His evacuations should be received in a bed-pan, and removed immediately from the room. There ought, if practicable, to be two or more nurses, so that the patient should never be left alone for a single instant. His bed-clothes and person should be kept as clean as possible; but all changes of linen, etc., without absolute necessity, deferred until the patient is out of danger.

As to the medical treatment, the like precautions are necessary. The physician should never forget, for an instant, the peculiar character of the disease—its treacherous nature—the rapidity with which alarming symptoms come on. He must bear in mind that the patient is hourly losing strength; that his nervous system is becoming more and more deranged. He must remember that there is no safety for his patient until the disease has run its course, and convalescence established beyond all doubt. Those who see the disease for the first time, are exceedingly apt to make a serious mistake: the fever subsides on the third or fourth day—the pulse and skin are good—the patient complains of no pain, and the physician supposes him out of danger. The truth is, the danger is then most imminent—the most critical period of the disease has arrived, and the patient is required to be watched more assiduously than ever. It is at this stage that a purgative, or any other medicine improperly administered, may decide his fate.

As to the details of the treatment, they must be left to the judgment of the physician. Any specific treatment is just as absurd in yellow fever, as in any other disease. The physician is not called in to treat an abstraction, but a sick man. The treatment must be varied according to the peculiarities of the cases. Remedies, beneficial in one case, may be most injurious in another; and success in practice will depend, in a great degree, upon the sagacity and acquirements of the physician.

Certain modes of practice, however, have prevailed here, as elsewhere. All have had their advocates, who point to results as evidences of their value. If we were to rely upon the statements of partizans, it would be difficult, indeed, to form an opinion of their respective merits; but it must be remembered, that patients in yellow fever die, and that others get well, under all sorts of treatment. I was once called to an Irishman, who had been sick five days, and who had done nothing but drink whiskey the whole time. He was suffering with great irritability of the stomach, but recovered. It is not, then, from such statements that we can form any correct opinion concerning the *methodus medendi* in this disease. We must fall back upon the broad principles of pathology and therapeutics.

Of the methods in vogue, we may point out three that have had the largest number of advocates. They may be denominated the Calomel, the Depleting, and the Quinine practice. I shall proceed to make some comments upon each of them.

Calomel. I should, perhaps speak of this practice in the past tense; at least I know of no physician in New-Orleans who pursues it, or mentions it with respect. Absurd as it is, however, it has had as strenuous supporters as any other delusion in medicine. It has not been more than twelve years, when he was a bold man who undertook to affirm, that a case of yellow fever might get well without the aid of calomel. The practice seems originally to have been adopted in sheer desperation, and continued from the same cause.

The treatment consists in bringing the patient, as soon as possible, under the influence of the drug. For this purpose, ten, or twelve grains are administered every two, or three hours. Should time pass on, however, and the patient show no symptoms of salivation, the dose is either increased, or the intervals between its administration lessened. Should we ask for some pathological reason for such practice, we are told, that the calomel acts by emulging the liver; that experience has pointed out the necessity of the treatment; and that, every case is saved in which ptyalism occurs. Let us examine these arguments.

As for the liver, the symptoms of the disease throughout its whole course, as well as *post mortem* examinations, show that it is by no means particularly affected. The passage of bilious stools, during the first days of Yellow Fever, is as common an occurrence as we meet with, though not a grain of any mercurial has been taken. We find, also, bile in the gall-bladder after death; so that the whole argument about the liver is just upset by these facts.

It may be contended, however, that the yellow hue of the skin, &c., is caused by the accumulation of the principles of bile in the blood; and that, therefore, the liver should be excited to increased action in order to eliminate these principles from the system. I do not believe that the yellow hue of the skin is owing to bile; we frequently see the skin, at first of a bright pink, gradually assume the yellow tinge, as if it depended upon some change in the coloring matter of the blood. But, for the sake of argument, let us grant that the bile is the cause of it; does the administration of calomel prevent, or even retard its appearance in the slightest degree? Not at all. The yellow skin—the passive hæmorrhages, &c., are just as bad, to say nothing more, in cases treated with mercurials, as in those in which not a grain has been given.

But, whatever effect calomel may have upon the liver, it is very plain that administered as it usually is, it must first act upon an organ just as important to the welfare of the system, and which, *post mortem* examinations show, is by far the most frequently affected. I mean the stomach. The connections of this organ with the rest of the system are so numerous, that some have even called it the centre of the sympathies; its extreme irritability is one of the most marked traits of the disease; its serious derangement is what the physician most particularly dreads; and can any one believe, that we shall shun this danger by administering a mineral drug, such as calomel, every hour or two?

As for experience proving the necessity of administering calomel, I shall dismiss the subject with the remark, that experience has proved just the contrary.

But all cases recover, in which mercury produces ptyalism! Admitting this to be true, which is by no means the case, it is but reasoning *post hoc ergo propter hoc.* The disease runs its course—the fever subsides—the patient recovers *in spite* of the remedy, and the poison introduced into the system then takes effect. Instead of a rapid convalescence and a speedy restoration to health, as is usually the case in yellow fever, he is the martyr of a most noisome and insufferable disease, for weeks or even months; and is fortunate, indeed, if he gets off so well.

In 1833, there occurred a phenomenon which was as common as any other in the disease. It was suppuration and ulceration of the parotid glands. It is now rarely met with, and the reason is, less calomel is given.

The parotids were not the only glands that suffered. In the autopsies of that year, it was as common a thing to find the mesenteric glands swollen and enlarged, as to meet with any other lesion.

By the foregoing remarks, I do not wish to be understood, as inculcating the total abandonment of calomel in yellow fever. Given as a cathartic, in the commencement of an attack, I have seen it act admirably. It causes but little nausea, and will bring away feculent matter, when castor oil, or saline purgatives, fail to do so.

Depletion.—All the characters of this disease would seem to inculcate, in the strongest possible manner, the greatest reserve and discretion in the use of the lancet. Of this instrument, all powerful for good or for evil, according to the mind that directs it, it has been long ago remarked, that, perhaps, its victims numbered more than those of the sword. Sure, I am, it has fully done its work in yellow fever.

On the first days of the fever, before the patient is much prostrated; when the pulse is full and strong; when the pains are severe; when the patient is of robust constitution, venesection unquestionably does good. Whenever, during the febrile period, it is to be feared that congestion is forming in any organ—the brain, lungs, or stomach, the lancet should be employed, but employed with prudence. The physician should never forget—that this is a disease arising from poison—that prostration is rapidly approaching, and that by improperly using the lancet, he is hastening its advent and adding to its intensity. When the patient is of a nervous temperament, or feeble constitution—when any ataxic symptoms supervene, such as nervous delirium, &c., the lancet should not be thought of. Some have used this instrument, as if they thought it possible to bleed the disease out of the body. A greater error was never committed. Large quantities of blood are taken from the patient, who has been made to sit up, and syncope supervenes. In the course of an hour or so, observe his pulse. Has his fever abated? are his sufferings less severe? Not a whit. His skin is as hot, his pulse is as bounding as ever, but has lost its force. Standing some distance from the patient, we can see the carotids violently throbbing. We feel the pulse, and it has a peculiar thrill. Again and again is the lancet employed; and more and more grave, all the symptoms become. The patient begins to wander in his thoughts, and speaks incoherently. "The brain is becoming inflamed," says the physician, "and he must lose more blood;" again the lancet fulfils its office, and a change, sudden and appalling, takes place. The patient becomes cold and pale—a clammy sweat breaks out over the body—the pulse sinks—black vomit is thrown up in large quantities, and death soon follows. This is no fancy sketch—it is what may be witnessed every epidemic year.

The nervous system is particularly liable to derangement in yellow fever; the most fatal of all the symptoms are those which indicate affections of the brain. Now, there is nothing that has a more powerful control over the nervous system than the lancet, and hence extreme caution is necessary in its use.

But for what purpose is so much blood taken from the system? What indication is to be fulfilled? Is it to relieve organs suffering from inflammation? *Post mortem* examinations ought to settle the question. In the worst cases, those of a congestive character, there are scarcely any lesions to be found after death; and as for the congestions usually found in the stomach and intestinal canal, it is far more probable that they are of the same nature with the petechiæ, and other congestions, which we see form on the surface of the body, than that they are the results of inflammatory action.

We are told by Andral, that in idiopathic fevers there is a dimunition of fibrine in the blood. In Yellow Fever the blood is remarkably slow in coagulating, and when passive hæmorrhages occur it will hardly coagulate at all. Assuredly we cannot expect to increase the proportions of fibrine in such a disease, by bloodletting.

With regard to the use of other means of depletion, such as leeches and cups, the same prudence and caution are required. As they take blood, however, in a slower and more gradual manner than the lancet, there is not the same risk of affecting the nervous system. They are, therefore, much safer. Cups are frequently very serviceable in relieving the intense pains in the loins and head, which the patients suffer during the febrile period.

Sulph. Quinine.—This remedy had often been employed as a tonic during the latter days of an attack, but in 1839 a new mode of administering it was adopted at the Charity Hospital, and in the private practice of many physicians. This mode, so far as I know, was first put in practice by Dr. J. M. Mackie of this city, who adopted it at the suggestion of Dr. Thomas Hunt, also of this city. Dr. Hunt was led to believe in its efficacy from a review of the writings of Maillot, published in the July number, 1839, of the British and Foreign Medical Review. From that work I make a few extracts:

"The first of these publications consists of a memoir read to the Royal Academy of Medicine, founded upon observations made or collected by the author, in the garrison of Bona, relative to the destructive epidemics of the years from 1832 to 1835. In those years, the garrison, consisting of between three and four thousand men, 22,530 were admitted into the hospital, and 2,513 died, or 1 in 8; or, according to the more particular statement, there were

"Admitted in	1832,	4033,	of whom	449 died;	or 1 in 7.
"	1833,	6704,	"	1526 "	1 in $3\frac{1}{2}$.
"	1834, 1835,	11593,	"	538 "	1 in 20.

M. Maillot's attention was first and principally directed to determining the analogies existing between the fevers of Bona and those which he had previously had an opportunity of observing at Algiers and in Corsica, with a view to deciding on the propriety of applying to them the treatment which he had found adapted to the latter. The establishment of the characteristic of intermittence, as common to both, seems to have been the result; the fevers of Bona, like those of Corsica and of Algiers, arising in the neighbourhood of marshes, the greater proximity of which to the troops at Bona, gave a severer character to the fever; demanding, as it proved, a prompt and more energetic treatment. M. Maillot maintains the relation of the continued forms to the intermittent; and the tendency of the intermittent, if unchecked, to pass into the continued; and of the continued, if bleeding was employed, to pass into the intermittent or remittent. These circumstances, he says, convinced him that he had not to deal with true continued fevers, the gastro-enteritis or gastro-cephalitis of France. He concluded that the probability was that the affections before him were those spoken of by Torti, part of the character of which is "de intermittente sensim, acutam et malignam migrat;" and resolved on giving the quinine boldly in all the continued cases, without waiting either for remissions or intermissions, which were "only instantaneous when they were obtained." The results, which are very striking, are seen in the diminished mortality exhibited in the above statement."

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"In the Bona fever, M. Maillot says it was possible, as late as the first days of June, to cause a kind of remission of the symptoms. This remission, however, appears to have been no more than the morning remission of symptoms, so common in the continued fevers of our own climate; and even this was only induced after bloodletting; by which sometimes the fever was entirely subdued, and sometimes converted into a distinct intermittent; it often seems, on the other hand, to have run on to malignant and typhoid forms. And, at the end of June, the continued fevers were quite distinctly separated from the intermittent. Yet the continued fevers, it would seem, begin occassionally with a few intermittent paroxysms; after which they pursue their course even without remissions, however slight. The point of practice which M. Maillot is most anxious to enforce is, that, notwithstanding this appearance of continuity, the treatment demanded was the administration of bark in full doses. The same circumstances, and the necessity for this practice, were pointed out by M. Coutanceau in the epidemic pernicious fevers of Bordeaux, in 1805; and his opinions are quoted by M. Maillot, who declares them to be equally applicable to the fevers of Bona of 1832 and 1833. With these convictions, M. Maillot gave large doses of sulphate of quinine in all cases of continued fever, with the exception of some in which there was ileo-colitis; in which, although he thinks he was wrong in making them an exception, he deferred its administration. In all the cases thus treated, in which he enumerates cases of gastro-cephalitis, of acute gastro-enteritis, of follicular ileo-colitis (diarrhœa), of hæmorrhagic ileo-colitis (dysentery), &c., the disease, except in a few instances, was relieved in a few days. In almost all these cases, the patients began to take some light food on the third or fourth day. Of ninety-eight cases of gastro- cephalitis included among them, occurring in the month of July, not one became typhoid; and only five died, of whom two sunk the day after admission into the hospital. In the other cases, the solution of the disease was speedy, and the convalescnce rapid.

"There is something surprising in this account, and we have allowed the reader to share in our astonishment, although, at page 26 of his memoir, when M. Maillot comes to relate particular cases, we find another article of treatment generally preceding the use of the sulphate of quinine, and which, although it is no other than pretty free bleeding, general and local, had not been before alluded to as of the smallest importance.

"'A soldier of the 59th, aged twenty-five years, was admitted into the hospital on the 8th of August, on the second day of an acute and excessively intense gastro-cephalitis. I immediately prescribed bleeding from the arm to fifteen ounces, the application of forty leeches to the epigastrium, and twenty leeches in the course of the jugulars; low diet; lemonade.

"'On the 8th, at the morning visit, the reaction was not entirely subdued; but the condition of the pulse, that of the skin, and all the other symptoms, denoted a remission indicative of approaching remittence or intermittence; and I consider it a continued gastro-cephalitis, passing into intermittent or remittent fever. Low diet; lemonade; twenty-four grains of sulphate of quinine to be taken in a potion at one dose, and immediately.

"'Complete apyrexia established itself during the day. The apyrexia continued on the morning of the 10th: nevertheless, I prescribed another potion of twenty-four grains of sulphate of quinine, fearing that the fever might be tertian, and return the next morning. But the fever did not return; and convalescence went on rapidly. On the 18th, the patient was nearly on full diet. (p. 27.)'

"Such was very nearly the treatment of 295 cases of gastro-cephalitis; except that the sulphate of quinine, in subsequent cases, was given immediately after the venesection; and, in certain circumstances, before any sanguine evacuation, as many of the men had been carried off by paroxysms of pernicious fever, some hours after the opening of a vein. Of the 295 cases thus treated, only twelve died, or 1 in 24. These results were certainly satisfactory: but M. Maillot observes, that such treatment would not be suitable to cases occurring in the north of France, in which dangerous typhoid affections, and (in case of recovery) tedious convalescence, would be the consequences. There can be little doubt that such would be the serious results of similar practice in the continued fevers of England; yet we believe there is much evidence of the most respectable kind among previous writers on the fevers of the Mediterranean, and of Italy, and of Africa, in support of the practice observed by M. Maillot.

"Among the cases of true intermittent fever, 1582 were quotidian, 730 tertian, and 26 quartan. Of these 2338 cases of intermittent fever, the accession took place between midnight and noon in 1652, and between noon and midnight in 686. The greatest number of accessions took place between nine in the morning and noon. 658 of the cases were simple, and 1680 complicated. In 1078 instances the intestinal canal was affected; alone in 343 cases: with the brain in 686 cases; with the lungs in 31 cases; with the brain and lungs in 13 cases. In 25 cases the spleen alone was diseased; and in one case the peritoneum alone. The brain was affected alone in 466 cases; the spinal cord in one; the lungs alone in 103 cases, and the pleura alone in five. In one case, a tertian, there was angina with the formation of a false membrane, and no other lesion. The intensity of all the complications was in direct ratio to the elevation of the temperature; and they were always unfavorably affected by the wind of the desert."

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"In this country, pathologists have not been very ready to admit this theory of intermittent inflammatory conditions of the intestinal canal and brain. M. Maillot presents the doctrine in the least objectionable form. The secondary *congestions*, he says, are ordinarily very feeble in the first accessions, and disperse in the interval between one accession and the next. Complete apyrexia therefore ensues, without functional disorder of the digestive or respiratory passages. But when the accessions are many times repeated, and, above all, when they assume a quotidian type, each leaves some anatomical traces of congestion in the viscera affected. The capillaries soon become unable to disembarrass themselves of the blood which each accession determines to them; the tissues soon become unable to resist a state of congestion so frequently renewed, and the irritation "fixes itself anatomically," and betrays itself by symptoms more or less continued. Hence arises a prolongation of the reaction; that is to say, of the febrile symptoms, thirst, redness of the tongue, headach, heat of the skin, and all the symptoms of a gastro-enteritis, a gastro-cephalitis, a pneumonia, &c., according to the organs which are over-irritated (*surirrités*).

"To these remarks, M. Maillot adds the very important practical observation, that simple irritations, and those not of great intensity, and which yield in the intervals of an intermittent, give rise to symptoms in this class of fevers as marked and violent as those of acute gastro-cephalitis. This circumstance, he observes, if unknown or unattended to, might lead the practitioner to see inflammations where none exist, and to be afraid of administering the quinine, on which alone the hope of preventing the returning accession of congestion must rest. In illustration of his practice, M. Maillot inserts a case in which, after bleeding during the paroxysm to fifteen ounces, the patient presenting the symptoms of acute gastro-cephalitis, twenty-four grains of sulphate of quinine were given at once, thirty leeches were applied to the epigastrium, and there was not another paroxysm. Of 250 cases thus treated, he lost only eleven, or 1 in 22; the fatal cases were all quotidian. In the pernicious forms of fever, with coma, he gave forty grains of the sulphate of quinine at a dose; and in one such case, 148 grains were given in less than twenty hours, and the patient, from being in a state of coma, almost resembling death, became speedily and completely convalescent. In cases of the *algide* form, or with extreme coldness, ether was administered with the sulphate of quinine.

"The most convincing proofs of the correctness of the above practice, and perhaps of the theory also, is that M. Maillot appears to have reduced the mortality in the fearful epidemic he had to contend with from one in 3½ to 1 in 20; for these results cannot be ascribed to any alteration in the character of the disorder; but became sensible when he began to use the sulphate of quinine more freely than he had ventured to do at first, and to bleed less copiously. Subsequent engorgements of the abdominal viscera, dropsy, diarrhœa, so often considered to arise from the use of bark, were scarcely seen in any case; and M. Maillot considers them as the results, not of the medicine, but of repeated paroxysms of the disease."

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"Of the different kinds of bark, M. Maillot prefers the red (*oblongifolia*), not only to the gray (*lancifolia*), which contains only cinchona and not quina, but also to the yellow (*cordifolia*), from which, on account of the quantity of quina it contains, the disulphate of the London pharmacopœia is prepared. But he prefers the sulphate of quinine (disulphate of quina) to all preparations of bark; the doses being more easily regulated, the stomach tolerating it better, and its action being surer and speedier; qualities which practitioners are not all equally inclined, we think, to accord it the possession of. When the stomach rejected it, the sulphate was given in a lavement; if purging or colic ensued, the endermic method was had recourse to; and sometimes the patients were put into a bath saturated with cinchona. In salicine and ilicine, M. Maillot places, as might be expected, little confidence. He seems not to have given any trials to opium; but he quotes the opinions of Lind and of M. Bailly, to show that its power is only sedative, and not febrifuge; and that, if it seems to shorten the duration of the attacks, it does not prevent their recurrence. We have little doubt that there are cases in which opium mitigates the sufferings incidental to intermittents; and in some cases, at least in our own climate, a large dose, given in the cold stage, will put an end to it, and even to the disease altogether. M. Maillot agrees with M. Bailly in condemning the combination of antimony with opium, as useless. With less reason he utterly condemns arsenical preparations. He also reprobates the notion of giving an emetic or even a purgative before commencing the administration of the sulphate of quinine; being of opinion that the attention of the practitioner, being directed to the subduing of abdominal or other irritations by leeches, would be uselessly given to removing any internal sources of irritation, which he looks upon as imaginary, and but the relics of exploded doctrines. In this respect, theory certainly interferes unfavorably with M. Maillot's practice; and that there are even cases of intermittent in which, after the application of leeches to the epigastrium, and the administration of a purgative, the disorder will disappear before a grain of bark is given, every one who has seen much of ague must have found. The general propriety of M. Maillot's rule however, not to delay giving the sulphate of quinine as soon as a complete apyrexia is established, cannot be controverted; and the old doctrine of coction and crisis led, without doubt, to hurtful delay, during which the constitution suffered greatly from repeated paroxysms. He is, as we have seen, decidedly an advocate for giving the sulphate in doses of twenty or more grains, which he administers in four ounces of water; and the time he prefers is three or four hours before the expected attack. His experience in Africa determined him at length not to repeat the medicine more than once or twice after the suppression of a paroxysm; and he has pursued the same plan with success since his return to France. The prolonged use of the medicine is objectionable; and he agrees with M. Hepple in believing that it does not even prevent the return of the fever, after a given time, so certainly as having recourse to it anew, at the expected periods of return; supposing such periods to be ascertained, as stated by M. Hepple, namely, the eleventh and twenty-first day in quotidians and tertians, and between the twentieth, and thirtieth in quartans.

"The unfortunate tendency to a relapse makes it desirable, M. Maillot observes, that the patients should consider themselves as convalescent for two or three months, however well they may appear to be. When relapses take plaee, bleeding, except by leeches, is generally less required than at first; but the sulphate of quinine should be given in increased doses. In some cases a complete change of residenee is indispensable to reeovery; soldiers who were sent home to Franee after obstinate and renewed attacks often recovered health during the voyage."

The new mode of administering the sulph. quinine may easily be inferred from the foregoing extraets. It consisted in waiting for no abatement of the fever, but in promptly giving the sulph. quinine as soon as possible after the attaek. The dose varied from 20 to 80 grains, given in a little cold water. If the stomaeh was very irritable, it was given by injeetion. When the practice was first introduced a cathartic was generally first given, and the administration of the quinine deferred until the bowels were moved. This practice was afterwards abandoned, on account of the loss of valuable time. In strong and robust constitutions, the laneet was employed for the purpose of produeing a temporary remission, during which the quinine was given. This praetiee was deeidedly beneficial. If the first dose failed in eight or ten hours to produce an apyrexia, a second was given. The earlier it was given the better, but *it should not be given after the second day.* This is a most important point in the treatment; I have given it on the third and fourth day, but always with injury to the patient. It inereases the irritability of the stomaeh, and causes the tongue to become dry. In addition to the sulph. quinine, other remedies were used aceording to the circumstances of the case: such as would suggest themselves to any physician.

Before venturing to give the sulphate of quinine in sueh large doses and upon such a raging fever, Drs. Hunt and Maekie performed a number of experiments to test its physiological action. The experiments were performed in 1839, at the Charity Hospital, upon convaleseents from different diseases. From notes furnished me by Dr. Hunt, I transcribe the following cases:

CASE 1st.—At 20 minutes of 10 A. M., pulse 72, took 15 grains of sulph. quinine. At 11 A. M.—Pulse 60; has slept; complains of heat at epigastrium; feels a little dizzy; pupils slightly dilated. At half past 12 P. M., no other change.

CASE 2d. At 10 A. M., pulse 56, took 11 grains. At 11 A. M., pulse 48; has slept; has slight ringing in the ears; sweating freely a short time after taking the quinine. No restlessness.

CASE 3d.—At 15 minutes of 10 A. M., pulse 64; took 9 grs.; at 11 A. M., pulse 52; has slept; pupils slightly dilated; no other change.

CASE 4th.—At 15 minutes of 10 A. M., pulse 100, took 20 grains. At 11 A. M., pulse 96; has slept; pupils slightly dilated; no other change. At 15 minutes past 11 A. M., took 15 grains. At half past 12 P. M., pulse 92.

All these cases were watched. The effects, as narrated, gradually wore off, without any other change.

It may be mentioned here, that when the sulph. quinine was given in large doses, a part of it was found, in the experiments made for that purpose unaltered in the urine.

So much for the method of administering the sulphate of quinine; it now remains to speak of its effects. The fever in most cases was cut short as if by enchantment. I shall never forget the surprize I felt, the first time I witnessed its effects. Three patients in the wards of Dr. Mackie were put under its influence. The conditions of the three were pretty much the same—as also were the results; the description of one case will therefore suffice for the whole. The patient a robust young man of about twenty-eight years, had been taken with yellow fever at 1 P. M. He was prescribed for the same day, at 6 P. M. Ten cups were ordered to the epigastrium, 30 grs. of sulph quinine to be taken by the mouth, immediately after the cupping, and 40 grains by injection. He had been cupped before I saw him, but had not yet taken the quinine. His condition was as follows; pulse 120, full and strong; great heat of skin; great pains in the head, back, and lower extremities; tongue a little furred; eyes heavy and a little injected; great restlessness on account of the pains. I saw him next morning, between 6 and 7 o'clock. He was perfectly free from pain; the pulse was at 84; skin cool; in short, every vestige of disease had disappeared. From prudential motives, he was kept in the hospital four or five days, and then discharged. There was never any return of the disease.

The other cases terminated in a similar manner, and the practice was soon adopted by a number of physicians, myself among the number. The results were, in general, highly satisfactory.

I shall say but few words, with regard to the manner in which quinine acts, to produce such effects. No doctrine of stimulants, counter-stimulants, or sedatives, can reach the explanation. Nothing, that we know of, can be substituted for quinine: its effects are, therefore, specific, and are owing to its chemical character. I believe that it acts, like the poison of which it seems to be the antidote, directly upon the nervous substance—breaking up the whole condition upon which the morbid actions depend, but in a manner, perfectly inexplicable in the present state of science. If there be any way of cutting short an idiopathic fever, it is assuredly to be effected by quinine. But, there are certain facts which lead me to believe, that the disease under consideration runs its course, even after all febrile symptoms have succumbed to the power of quinine, and indeed, after all morbid symptoms, whatever, have disappeared. In 1841, I lost a patient, and in 1842, another; one on the 6th, the other on the 7th day, in whom the fever had been cut short, as usual, by the administration of quinine. They lay in bed for two or three days, without a single symptom of disease, and, indeed, were kept in bed more from motives of prudence, on account of a change in the weather, than for any other reason. Yet they both died with black vomit.

I have remarked in another place, that the pathology of yellow fever was, in part, to be inferred from some points in its treatment. I alluded to the effects of quinine in the first stages of the disease. Surely, these effects give no support to the supposition that the disease arises from local inflammation.

From the above observations, concerning the effects of quinine, it will not, I hope, be understood, that I advocate its administration in all cases whatever. As I have before remarked, there can be no specific treatment for yellow fever, or any other disease. When in the commencement there is great congestion of blood in the brain, or any other important organ; or, where the fever supervenes upon chronic inflammatory diseases, I would most certainly resort to other means. Nor would I be understood as speaking of quinine as an infallible remedy. The practitioner to whom the disease is a new one, will soon discover that in certain cases, particularly those of the congestive and ataxic types, that quinine is as inefficacious as any other remedy. In cases, in which the fever is well and fully developed, it will, unquestionably, cut that fever short, and thus prevent the formation of those local congestions which are produced by the febrile action. In this consists its value, and assuredly, it is a great one.

It remains to say a few words concerning other remedies sometimes employed in the course of treatment. We shall first speak of baths.

Cold Bath.—I have but little experience in the use of cold baths, as I have been averse to employing them from pathological principles. There is a great tendency to sudden changes in this disease, and the congestive state is one of the most fatal forms in which it can present itself. When the fever is high and fully developed, the danger is far less. Even so unfavorable, in my opinion, are any symptoms of congestion, that I always look upon it as a very bad sign, when the patient, after the second hour, complains of being chilly and hot at the same time. Now, cold baths, in many cases, tend to prolong this congestion; or to produce a chill when the fever was about being developed; and in other cases, in which its administration is followed by quick reaction, the fever would have been sufficiently high without it. At any rate, it should only be used in the first days of the disease, for the obvious reasons, that it fatigues the patient to be often taken out of the bed; and in latter stage, it is inadmissible, on account of the prostration of the patient.

Warm Bath—Warm baths in congestive cases may be serviceable in promoting reaction; but when the fever runs high, and particularly if it be accompanied by ataxic symptoms, it does no good, but harm—the skin soon becomes hot and dry—the eyes and face flushed—and the pulse more bounding than before. In the latter days of the attack, its employment is improper for obvious reasons.

Cold Affusions—I have used cold affusions in some violent congestive cases, with the hopes of producing, by the sudden shock, a reaction, and full development of the fever; but, uniformly, without success. The patient when put to bed and enveloped in blankets, had but slight reaction, and that of an ataxic character.

Sponging and Foot Baths—By far the best mode of applying water, is by sponging the surface of the body, and by the frequent administration of hot mustard foot baths. The first should be applied to the head, throat, chest, and upper extremities—the temperature to be regulated by the condition of the patient. If his fever be burning hot, and he complains of heat, he will bear even ice-water. Should he complain that the sponging produces chilliness, the water should be tepid or quite warm—a little vinegar mixed with the water, promotes its evaporation. Ice applied to the head also gives great relief to the patient, but should it produce chilliness, it is subject to the same objection as sponging with water.

The sponging keeps the skin moist—relieves to a great degree the sufferings of the patient, and moderates the burning heat of the surface. The foot baths tend to equalize the circulation, and to relieve the insufferable pains in the legs. The skin should never be permitted to become dry, during the febrile action—and the foot baths should be frequently repeated.

Emetics—In persons attacked immediately after meals, I have employed an emetic of pulv. ipecac., in order to relieve the stomach promptly of its contents ; but have used them in no other way. In this way, I have seen it do no harm. Dr. Cartwright, of Natchez, has employed tart. antim., in doses from three to ten grains, given every one, two, or three hours, dissolved in a little water, or, what he considers better, in the form of pills. It is only to be given in the first stage of the disease, and in cases of congestive or ataxic character. "Tartar emetic," says Dr. C., "used in this state, restored sensibility to the torpid organs, produced secretion, and destroyed the ataxic character of the disease, by establishing a general and equable excitement ; or, in other words, converted an irregular and intractable condition of the system, into an open, plain, and manageable case of fever." I have no experience in regard to this treatment, but think it well worthy the attention of the profession. The great objection to using tart. antim. is, it tends to increase the irritability of the stomach, so prominent a feature in this disease. But the supposition that inflammation exists in the stomach from the commencement, is a mere bugbear; and if, by the administration of any remedy, we can break up those fatal forms of congestion and ataxia we sometimes meet with, and produce an open, well developed fever, we should do so, even at the expense of increasing the irritability of the stomach. When the fever is fully formed, we can manage it—otherwise, not.

Purgatives.—The bowels should be evacuated as early as possible, and thoroughly. They, moreover, should never be permitted to remain unmoved over 24 hours ; in other words, their own proper secretions should be removed. The best purgatives are of the milder kind ; blue pill, or a little calomel, followed in a few hours by a dose of castor oil, and some mild saline laxative. Drastic cathartics do injury by irritating the intestinal mucous membrane. Costiveness is not a very common symptom in this disease, and, after the first evacuations, clysters are generally sufficient to keep the bowels open. The repetition of purgatives, after the bowels are well emptied, do no good, but often a great deal of harm. They seem to dispose the intestinal canal to sanguine engorgements, and consequently, to passive hæmorrhages.

Narcotics.—Opium and the salts of morphia are sometimes administered to check the incessant vomiting which supervenes in the beginning of the last stage ; sometimes, also, they have been given to relieve the hiccough, and sometimes to quiet the patient in nervous delirium. In neither case have I ever seen good effects from their use. Without effecting the object aimed at, they appear to check at once the already too greatly diminished secretions.

Blisters, &c. Applied to the epigastrium, blisters are sometimes of great service in relieving the gastric irritability in the commencement of the last stage. Their administration, however, requires caution, for if they are too large, or administered in improper cases, they do mischief by affecting the already exhausted nervous system.

Stimulants—Such as brandy, ale, port wine, carb. ammon., camphor, &c., are often resorted to, towards the close of the attack, but generally in cases altogether hopeless. In many instances, however, I have thought that they did a great deal of good—the patients recovering under their use. The extreme prostration, to which the patients are generally reduced after the febrile stage is gone by, demands some artificial support to be given to the system. The early administration of a little English ale, or sangaree of port wine, I have often thought has turned the scale of life in favor of the patient. But it must be remembered that yellow fever is not typhus fever, and that the like benefit from stimulants cannot be expected.

The remaining classes of the Materia Medica, such as tonics, diaphoretics, diuretics, &c., require no particular notice.

I have now concluded the task I undertook. In a future number I may offer some speculations, concerning the etiology of this disease.

ART. X.—*Obstetrical Cases in recent Practice.* By A. ALPUENTE, M. D. of New Orleans.

CASE 1. *Difficult Labor—Convulsions—enlargement of the os tincæ by incision—success.* On the 4th of April last, about 7 P. M., I was called to visit a female slave in Philippa street. She was stout, and had enjoyed good health. In the night preceding my visit, she was taken with vomiting, and, towards 5 o'clock, with convulsions; on my arrival I found her in the following state. Her pregnancy had arrived at the last stage; the convulsions with which she suffered, partook of the character of epilepsy; they were due from all appearances, to congestion of the brain, the result of a first labor. On examination, the neck of the uterus was found entirely obliterated, and the os uteri itself barely pervious. Venesection was now resorted to, an anti-spasmodic potion, and a full bath.

At 10 P. M., I visited her again; She was in the same state; there was no dilatation whatever; the contractions of the uterus were strong in the extreme. Two blisters were now applied to the calves of the legs, and an enema of castor oil and assafœtida, administered. After another examination, I requested the aid of another physician, and named Dr. Daret.

At half past 12, I met Dr. Daret; the same symptoms persisted. We now examined the woman with the speculum; the *os tincæ* was reduced to a size barely sufficient to admit a small probe; the neck of the uterus was entirely obliterated; every trace of it had gone, excepting this small aperture which seemed to indicate its position.

After mature deliberation, we concluded that an operation alone could extricate the woman from her great danger, and that immediate delivery was required to put an end to the puerperal convulsions; we demanded, however, the advice of a third physician, and Dr. Guesnard was named. With the exception of the blisters, the same prescription was repeated.

At half past 2 o'clock we met Dr. Guesnard; at that moment our patient seemed to be in a more favorable situation; the puerperal convulsions had ceased a few minutes before our arrival. The speculum was re-applied, but in consequence of the movements of the patient, it was difficult to detect the aperture which marked the seat of the os tincæ; on careful inspection, Dr. Guesnard found the aperture immediately behind the arch of the ossa pubis, and sufficiently dilated to admit the introduction of the finger, by which he was enabled to ascertain that the presentation was that of the vertex. In consequence of these favorable changes, Dr. Guesnard was of opinion that the labour would terminate naturally; every thing in fact, seemed to announce a favorable result. The two consulting physicians then retired, without fixing any hour for future consultation, deeming it unnecessary.

At 5 o'clock I again saw the patient; the puerperal convulsions were very strong, and the *os uteri* was no more dilated than when we left her. Dr. Daret was called again.

At 8 o'clock I again met Dr. Daret; we sent immediately for Dr. Guesnard; the immediate termination of the delivery was now deemed of absolute necessity, since twelve hours had elapsed, and the convulsions still continued.

At 11 o'clock, Dr. Guesnard not having yet been found, and as the convulsions were much stronger, we called on Dr. Tricou. Before the arrival of Dr. Tricou, the convulsions ceased again, and the woman relapsed into about the same state that she was in when Dr. Guesnard first saw her. Dr. Tricou now thought it advisable to postpone all interference for a few hours longer. The extract of belladonna was, however, applied to the *os uteri*, an anodyne enema administered, and venesection again resorted to.

The convulsions soon returned, and at half past 12 o'clock the operation was decided on. The patient was now placed on her back, at the edge of the bed, in the usual position for such operations, and being properly assisted by the attendants, I now introduced through the highly contracted *os tincæ*, the index finger, conducting upon it a probe pointed bistoury, with which I made three incisions, one of about six lines posteriorly, which was directly under the arch of the ossa pubis, and two lateral ones of about four lines each in length; I then introduced my hand slowly into the uterus, and applied the forceps, as we had agreed upon before the operation. When the head was passing through the os externum, the perineum was put severely on the stretch, which induced us to make a few slight incisions on the posterior part of the *labia pudendi* in order to prevent rupture. The contractions of the uterus was strong, and the placenta was thrown out immediately after the delivery of the child. Unfortunately the child was born dead; we instantly resorted, nevertheless, to all the means which might have contributed to the restoration of life had it been only asphyxiated.

All the nervous symptoms ceased as soon as the delivery was over, but the debility which now ensued was extreme, the pulse was small, and between eighty and ninety. The loss of blood was not greater than in any ordinary accouchement. At half past three o'clock I retired, leaving our patient in as satisfactory a condition as could be expected. The next morning, at our visit, we found her in the following state: the skin was dry; the abdomen distended, and very sensitive; general prostration of strength; the pulse was small and frequent; there was very little discharge of blood; there was also retention of urine, which was relieved by the catheter. Poultices were now applied to the abdomen, sinapisms to the lower extremities, and a tonic mixture was ordered. The blisters were also dressed, a hip-bath provided, and a cathartic enema prescribed. By three o'clock, a great improvement had taken place; the pulse had become nearly natural, her strength had improved, and she returned rational answers to all our questions. On the 6th, the abdomen had resumed its natural dimensions, the pulse was about the same, her intelligence had revived, although there was still some tendency to collapse. The catheter was now again resorted to, and a hip-bath, poultices, a laxative enema, emollient injections, cooling drinks, and chicken broth prescribed. On the 7th, the satisfactory condition of the patient continued the whole day, but on the 8th, there was some slight tension and tenderness of the abdomen, but the urine had been freely evacuated; her pulse was also frequent. Eighteen grains of calomel were now prescribed, to be followed by the enema; and the hip-baths, the poultices and the injections were ordered to be repeated. About twelve o'clock this day, two clots of blood were discharged, after which a striking improvement took place. At night, however, the abdomen was again painful, and the pulse frequent and full; the *decubitus* was also unfavorable; there was suppression of the lochial discharge. Sinapisms to the thighs and feet, and a tonic mixture, were now ordered. On the 10th, the pulse was from seventy to eighty, and rather small, the pain had nearly entirely left the abdomen, there was some heaviness of the head, but the evacuations were natural, the countenance was good, and her intelligence had returned; the cooling drinks, the enemata, and injections were again prescribed, together with cold lotions to the head, and a hot foot-bath. After the 11th, the improvement of this patient was progressive; slight purgatives were administered from time to time, and at the present moment, she is in the enjoyment of excellent health.

CASE 2.—*Premature Delivery of Triplets.* On the 19th of September, at 11 A. M. I received a call to visit a poor woman, who had been delivered, during the previous night, of three girls. She had not completed the full time of her pregnancy; labour had come on between the seventh and eighth month; during her confinement she had received no assistance whatever, either from midwife or physician.

I immediately repaired to the corner of Esplanade and Claiborne sts., and there I found a woman about 40 years of age, habituated to hard labour. She is the mother of four children, and had, previously to her last confinement, two miscarriages, shortly after which she became pregnant again. On the 18th, she felt unusually fatigued after her daily occupations; she also felt some pains in the back and about the uterus, but did not think assistance of absolute necessity. In the night, however, the pains increased, and at half past 12 she was delivered of a girl, which presented the feet; after the lapse of half an hour, she was delivered of *another girl*, the vertex of which presented; and about a quarter of an hour afterwards, the placenta and membranes came away, enveloping *a third female infant.* All these children were delivered alive; the last, however, survived but a few minutes, the second died at 9 o'clock the next morning, and the first died on the 20th, at 12 o'clock.

The size of the children was as follows:

Length, from the top of the head to extremities of the toes, extended, 16 inches.

Occipito-frontal diameter of the head, 4 inches, 2 lines.

Occipito-mental diameter of the head, 4 inches, 6 lines.

Bi-parietal diameter of the head, 3 inches.

On my first visit, the woman was rather weak; her pulse was small and frequent, the perspiration was profuse, (she was delivered in a feather bed,) the hæmorrhage was moderate, the uterus still tolerably large, and occupying the left hypochondriac region. On examination, the neck of the uterus was soft, and the *os uteri* small—so diminished in diameter, that I could not succeed in passing my fingers, in order to ascertain within, the cause, if any, of its contractions, which I attributed to the extreme distension that it had previously undergone. I prescribed previously to my departure, a tonic mixture, infus. tiliæ, and chicken broth for nourishment. The next day she was better, and continues to do well.

This is the second case of triplets which I have seen in this city. About two years ago, I was called to visit a woman who had miscarried at the sixth month. In this case there were two male infants enveloped in the same membrane; they were both born dead. The remaining infant was a female, which was contained in a separate sack; it lived but a few minutes. My services were required on account of the attending hæmorrhage, but the recovery of the patient was speedy.

Oct. 18, 1846.

Part Second.

REVIEWS AND NOTICES OF NEW WORKS.

I.—*Mental Maladies; A Treatise on Insanity*. By E. Esquirol, Physician-in-chief of the Maison Royale des Alienes de Charenton, Member of the Royal Academy of Medicine, &c. &c., Paris. Translated from the French, with additions, by E. K. Hunt, M. D of Hartford, Conn. Philadelphia: Lea & Blanchard. 1845. pp. 496.

The publishers will please accept our thanks for this valuable and most interesting volume. We have perused it carefully, and with much pleasure, and would commend it to Southern physicians with our highest encomiums upon its merits. Mental maladies, always so humiliating and deplorable, have engaged the attention of the ablest physicians from the earliest periods; but it is only quite recently that they have been studied with any degree of satisfaction or success. In ancient times the most erroneous ideas prevailed in regard to their nature; they were viewed as the direct visitations of an offended Deity, and as bidding defiance to medical art; hence insanity was called "*sacred disease*." It is true, that sometimes a lucky hit was made by some bold experimenter; and occasionally a case was cured by the influence of some potent drug, doubtless often given with equal uncertainty and indifference whether it would *kill* or *cure*. Like all blind empyricism, the result shed no light upon other cases, and no advancement was made towards a knowledge of the true nature of the disease. If the unfortunate victim happened to escape the dangers of the experiment without being cured, the dungeon or manacles were then his melancholy doom.

The *intimate nature* of mental maladies in their varied and protean forms, still remains a mystery to the pathologist and psychologist; but the advancement of modern science, the exercise of a godlike benevolence and indefatigable observation, have thrown around them a light that has led to the most glorious results. We have indeed learned in many instances, to

"Minister to a mind diseased,"

albeit we cannot yet unravel every link in the mystic chain of cause and effect. Instead of superstitiously viewing all mental aberrations as the *penal visitations* of God upon miserable mortals—thus impugning His boundless mercy and beneficence—we view them as the disordered functions of the brain, the seat of the mind; we examine carefully the physical signs and symptoms that indicate the nature of the lesion; we apply rational remedies to correct the disordered functions of the whole system; and above all we bring to bear upon the perverted judgment, perception, and sensibility of the poor lunatic, a set of moral influences and gentle restraints, by which he is prevented from injuring himself and others; by kindness, patience, and perseverance, he is persuaded and convinced of the error of his ways; and finally reason is restored to its throne.

If, as has been supposed, insanity has greatly increased in modern times, the means of mitigating its horrors have been proportionally enlarged; and the numerous asylums that have been erected in Europe and America for the accommodation of its unhappy victims, together with the ingenious and admirable systems of treatment adopted, may justly be considered amongst the greatest curiosities of the age.

The work under consideration, as the author tells us, "is the result of forty years study and observation;" and in situations commanding the most extensive advantages. The following are the heads under which the various mental affections are discussed, viz. Insanity, Hallucinations, Illusions of the Insane, Fury, Mental Alienation of the recently confined, Critical terminations of Insanity, Lypemania or Melancholy, Demonomania, Suicide, Monomania, Mania, Dementia, and Idiocy.

The author seems to regret that he has not been able to arrange his work systematically, so that a *general idea* should pervade all its parts; "nevertheless, (he says) we shall find a methodical connection in the distribution of the materials of which it is composed."

"The first chapter, entitled Insanity, is a summary of the sentiments prevailing on this subject; the remaining ones are commentaries, and a more full exposition of these views."

Our author defines insanity, or mental alienation, to be,

"A cerebral affection, ordinarily chronic, and without pain, characterised by disorders of sensibility, understanding, intelligence, and will. I say ordinarily, because insanity is sometimes of brief duration, and because at its commencement, and sometimes during its course, febrile symptoms are manifested."

"The causes of mental alienation are as numerous as its forms are varied. They are general or special, physical or moral, primitive or secondary, predisposing or exciting. Not only do climates, seasons, age, sex, temperament, professions and mode of life, have an influence upon the frequency, character, duration, crises, and treatment of insanity, but this malady is still modified by laws, civilization, morals, and the political condition of the people."

"Climates.—Warm climates are not those which produce the greatest amount of insanity, but rather temperate climates, subject to great atmospheric vicissitudes, and especially those whose temperature is alternately cold and humid, damp and cold. We see less of insanity in the Indies, in America, Turkey and Greece; more of it in the temperate climates of the North."

Our author thinks we have exaggerated the influence of climate in the production of insanity; nevertheless, he admits that the foggy atmosphere of England is a most powerful and immediate cause of the great amount of insanity in that country. He says that in marshy districts, dementia and imbecility are more frequent.

"THE MOON.—Does the moon exercise any influence upon the insane? The Germans and Italians believe it does. The English, and almost all moderns, give to the insane the name of lunatics. Certain isolated facts and phenomena, observed in many nervous diseases, would seem to justify this opinion. I have been unable to verify this influence, though I have been at some pains to assure myself of it. It is true that the insane are more agitated at the full of the moon, as they are, also, at early dawn. But is it not the light of the moon that excites them, as that of day in the morning? Does not this brightness produce in their habitations an effect of light, which frightens one, rejoices another, and agitates all? I am convinced of this last effect, from causing the windows of certain insane persons, who had been committed to my charge as lunatics, to be carefully closed. * * * * * * * * At the Hospital Salpêtrière, where practical truths have become in some sort known, among the inmates of the house they have no longer any suspicion of lunar influence. The same is true of the Bicêtre, and certain private houses of the capital. Nevertheless, an opinion which has existed for ages, which is spread abroad throughout all lands, and which is consecrated by finding a place in the vocabulary of every tongue, demands the most careful attention of observers."

It seems that Leuret, Mitivié, and other authors, have, by a series of careful observations, disproved the existence of lunar influence.

Our limits preclude any thing like a comprehensive review of M. Esquirol's work; we shall therefore confine ourselves to the notice of two interesting cerebral affections, viz. Puerperal Mania and Epilepsy, which are most commonly met with in our region, and fall to the management of the general practitioner.

1st.—*Mental Alienation of those recently confined, and of nursing women.* Our author observes that

"The number of women who become insane after confinement, and during or after lactation, is much more considerable than is commonly supposed. In fact, at the Salpêtrière, almost one-twelfth of the women received have become insane under these circumstances. There are years in which the proportion is one-tenth; thus, of 1119 women admitted into the division of the insane, during the years 1811, 1812, 1813, and 1814, ninety-two became insane after confinement, during, or immediately after, lactation; of this ninety-two women, sixty belonged to the years 1812, and 1813, during which there were six hundred admissions. And if, of the whole number of insane women received during these four years, we deduct at least one-third who have passed the age of fifty years, beyond which period women are not exposed to the influences of confinement and lactation, we shall be led to conclude that mental alienation as a result of confinements, during and after lactation, is more frequent than I at first intimated. This is true—particularly among the wealthy. The number in this class, according to the result in my private practice, is almost one-seventh. Astruc, also, had observed that deposits and engorgements of milk are more frequent among the higher than the lower classes of society. But it is certain, also, that mental alienation after weaning is rare among the rich, whilst it is frequent among the poor, who either from necessity or voluntarily, wean their children."

In regard to the time when this affection makes its appearance, M. Esquirol says:—

"Of our ninety-two women, sixteen became insane from the first to the fourth day after confinement, twenty-one from the fifth to the fifteenth day after confinement, and seventeen between the fifteenth and sixtieth day, the extreme limit of the lochial discharge. Nineteen women lost their reason between the second month, or the following one and the twelfth, during lactation. Nineteen women were seized with mental alienation immediately after a forced or voluntary weaning. We may therefore conclude, 1st, that alienation of mind is not more frequent among the recently confined than among nursing women; 2d, that the danger of losing the reason diminishes in proportion as the female is removed from the epoch of confinement; 3d, that nursing women especially poor ones, are much more liable to become insane after weaning than during the period of lactation."

The age at which this affection is most prevalent, is from 25 to 30 years.

"The causes which especially predispose the recently confined and nurses to this malady are, hereditary predisposition, an extreme susceptibility, attacks of insanity anterior to pregnancy, and attacks consequent upon preceding confinements, or during lactation. * * * * * The exciting causes which provoke insanity are errors in regimen, and the moral affections. Coolness; the impression of cold, in whatever way it may be made, is of all errors, most to be dreaded."

Our author mentions as a singular fact,

"that women have been known to become insane after giving birth to a male child, while they were exempt from this accident after confinement with a daughter. We have seen women whose delirium manifested itself only after every second confinement; also, those who fell into the same condition on the third or fifth month of each period of lactation, without any assignable exciting cause."

Of ninety-two cases, forty-six became insane after some moral affection. He adds:

"At Rome, a crown was suspended over the entrance of the house of those recently confined, to give notice that it was a sacred asylum."

Our author asks the two following questions:—

"1. Is the suppression or diminution of the milk the cause or effect of mental alienation?

"We may reply, that insanity manifests itself most frequently among women who do not nurse. Of our ninety-two insane women, twenty-nine were single, and sixty-three married. Now, single women rarely nurse. The greatest number of facts prove that the milk diminishes, is suppressed, or loses its distinctive qualities before the explosion of delirium; but there are observations also, which furnish undeniable proof that the diminution or suppression of milk takes place only after the explosion of insanity.

"2. Does the milk act as a foriegn body in those accidents which follow confinement or lactation?

"The ancients believed that the milk was conveyed to the brain, just as they thought it was sent to other organs; and attributed to metastasis all the accidents which followed its dimunition or suppression. * * * * * The moderns believe, with reason, that after confinement, and during lactation, there exists a lacteal diathesis, which modifies all the secretions of the female, and impresses upon them its own character, &c."

In regard to the result of puerperal mania, our author says:

"Mental alienation following confinement is generally cured, if the predisposition is not too energetic. More than half are restored. Among our ninety-two women, fifty-five were cured. * * * * The mental alienation of this class is terminated by the establishment of the lochia, by the secretion and evacuation of milk, by an abundant leucorrhæa, by mucous, and sometimes sanguinolent alvine dejections, by the return of the menses that have been suppressed ever since the beginning of pregnancy, and by subcutaneous suppurations; very rarely by pregnancy."

In regard to the duration of the attacks, it is quite variable. Our author states that

"Of fifty-five cases, thirty-eight, or rather two-thirds took place within the first six months after the attack."

He says that relapses are frequent, and that we must carefully avoid the circumstances that produced the first attack.

In regard to the mortality, our author observes that he was not a little surprised to find that out of ninety-two cases, there were but six deaths. He asks

"How happens it, that the abdominal affections which occur after confinement are so often mortal, whilst the cerebral maladies which take place at the same period rarely prove fatal? * * * * * * * * * The opening of the bodies of those who have been confined or nursed, and have died after having been insane a longer or shorter period of time, offers, strictly speaking, nothing in particular; nothing which enables us to recognise the material cause of this form of alienation, or to discover its seat."

In respect to treatment, our author says it has undergone many changes.

"Deceived by the gravity of the symptoms, practitioners have, almost always, carried to excess the means which they have employed."

After referring to various plans that have been followed by different physicians, he remarks:

"Bleedings ought to be employed cautiously during the first stage. When the sanguine temperament predominates, and there are signs of plethora, or congestion of the brain, leeches to the vulva or thighs, are useful. The cups, venesections, and sinapisms, applied, now to the legs or thighs, and now to the neck, in connection with a ptisan slightly sudorific or purgative, following the tendency of nature, will be preferred to the means called heroic."

He says that tepid or hot baths have a wonderful effect in seconding the other curative means, but that "*cool or cold baths are dangerous.*" He says, in fine, that puerperal maniacs

"are to be submitted to the same general principles of treatment, with other insane persons; that isolation, the aids of hygiene, and moral means, ought not to be neglected; and these alone have sufficed to effect a cure, although more rarely than in other forms of mental alienation."

Here follow graphic reports of fifteen cases, in which are pourtrayed all the peculiar symptoms of this form of mania.

We have given pretty fully the author's views upon this curious and distressing affection, because it is at all times liable to fall under the care of the general practitioner, who has to manage it in the best way he can, and often under great disadvantages. Fortunately, the disease is comparatively rare in this region.

2d.—*Epilepsy.* Our limits compel us to be much more brief in regard to this affection. Our author says that

"*the pathognomonic character of Epilepsy consists in convulsions, the entire suspension of sensibility, and loss of consciousness.*"

He thinks that the first cause of the evil acts primarily *elsewhere* than upon the *brain*. He says,

"Neither the alterations of the cranium, nor the lesions of the intra-cranial organs, teach us what is the seat, or what are the organic lesions of which this malady is the expression."

He thinks that the spinal marrow has been overlooked in the examinations of pathologists, and says that ten autopsies, (made by himself, assisted by M. Amussat,) "*presented in nine instances, lesion of the medulla spinalis, or its membranes.*" Nevertheless, he says,

"we confess frankly, that pathological anatomy, has, up to this period, shed little light upon the immediate seat of epilepsy."

M. Esquirol gives the most discouraging account of the effects of remedies, upon this most "*rebellious malady.*" In the space of ten years, he had under his charge at the Salpêtrière, three hundred and thirty-nine epileptics, upon whom he tried every remedy that had any reputation; he even went so far as to procure and use *secret remedies*. He says:

"Several of our epileptics submitted themselves to my treatment for several years; but, shall I confess it? *I did not obtain a single cure.* In my private practice, I was scarcely more happy."

He thinks the disease is "*rarely curable.*" The attacks are often suspended under the trial of a new physician, or the use of a new remedy, but they generally recur. He thinks many of the *supposed cures* were nothing but *hysteria*. Various remedies and suggestions are mentioned which have been found to mitigate the attacks; in short, each case presents some peculiarity, indicating to the discerning physician, what is best to be done for its alleviation. In regard to the isolation of epileptics, our author says:

"They ought not to live together in the same wards with the insane, as is practised in most hospitals where they receive both these classes of patients. *The sight of an epileptic seizure is enough to render a well person epileptic.*"

In illustration of the last mentioned remark, we saw a striking instance at the New Orleans Charity Hospital, in 1844. A blind girl, about fifteen years of age, with her companion, a robust girl of about twelve years, were brought from the Poydras Female Orphan Asylum. The former had been laboring, for many months, under frequent *fits*, exactly resembling epilepsy, proceeding from a suppression of the catamenia; the latter, who had never menstruated, having been appointed her nurse and constant attendant, soon contracted the disease; insomuch that whenever the oldest girl was seized with convulsions, which occurred frequently during the day, the younger also became immediately affected and with equal violence. The girls were separated, appropriate remedies were successfully used to restore the catamenia in the elder, and upon recent inquiry we learn that both are now in good health. M. Esquirol might pronounce this hysteria, instead of epilepsy, but what shall we think of the younger case? Can there be hysteria before menstruation? or can hysteria sympathetically produce epilepsy? We should be much pleased to have the opinion of M. Esquirol or Dr. Hunt upon this case.

We must here terminate what we have gleaned from M. Esquirol in regard to epilepsy. The malady is one of the most obstinate and deplorable that afflicts our race; and our researches into its nature, and our efforts to counteract its dreadful influences, both upon mind and body, have hitherto proved most unsatisfactory. But, to use the language of our noble and indefatigable author, "let us not be discouraged; Nature will not always prove rebellious against the efforts of her investigators."

M. Esquirol's chapter *on the Critical Terminations of Insanity*, is full of interest. He is a thorough believer in the doctrine of *crises*, and proposes to demonstrate, that it is in all respects applicable to mental diseases.

"Mental alienation, which the ancients regarded as an inspiration or punishment of the Gods, which was afterwards regarded as a demoniacal possession, and which, in other times passed for the work of magic; mental alienation, in all its forms, and varieties innumerable, differs in no respect from other diseases. Like them, it has its premonitory symptoms, which enable us to foresee it, symptoms which characterize it, a course which is proper to it, periods of increase and decline, a duration peculiar to it, and at length the united efforts of the whole system, tending to terminate it, either by a cure, or by death. Like all maladies, insanity is sporadic or epidemic, hereditary or accidental, idiopathic or symptomatic. It is simple or complicated; continued, remittent or intermittant, acute or chronic. Why should not insanity terminate by *crises?*"

Although this is his belief, he admits that the *crises* of insanity are not yet well understood. He says he has generally observed that in the course of the first month after an attack, *a very marked remission* takes place, after which the delirium returns with greater violence. More cures take place during the first month, than any other period.

Mental Alienation terminates by *resolution*—by predominent activity of the absorbent system; when the patient becomes very fat; by prostration and emaciation; by fever, hæmorrhages, suppurations, eruptions, &c.

In regard to the influence of *coition*, our author remarks:

"We have often seen girls and young widows cured by marriage. Alexander Benoit states, that a maniac escaping from her house, entered a lodging-room, where she was subjected to the brutality of fifteen individuals. Her menses, which had been suppressed, were re-established, and the patient was cured. I understand from a physician who has charge of the insane at Stockholm, that a young maniac, having escaped from his cell, entered the habitation of a young woman who was insane; and that, having given themselves up during the night to the wildest venereal transports, the former was found on the following morning, dead, and the latter, cured."

M. Esquirol never had occasion to observe similar crises in mania, but had sometimes known marriage to cure hysterical melancholy. This chapter contains several very interesting cases. Throughout the work our author dwells with emphasis upon the awful consequences of masturbation, an unfortunate habit generally contracted in early youth, and too much overlooked in parental instruction. How many noble minds have been enervated and destroyed by its malign influences, from total ignorance of its direful results!

We must here close our remarks; but we cannot do so, without expressing our approbation of the valuable additions to the work, made by the American editor, Dr. Hunt. E. D. F.

II.—*Researches into the Physical History of Mankind.* By James Cowles Pritchard, M. D., F. R. S., M. R. I. A., &c. &c. &c. Third Edition. London: Vol. 1, 1836: vol. 2, 1837: vol. 3, 1841; vol. 4, 1844.

In almost all ages, a few men, enjoying tranquility and leisure, have contemplated the phenomena by which they were surrounded, with intense curiosity, and an irresistable desire to discover their causes. Of all these subjects of human investigation, none seems more deserving of our attention than general Ethnography, none better calculated to excite curiosity than that of the origin of our species, and of the circumstances in which the astonishing varieties in color and other characteristics of the races, have originated. The multitudinous relations of the different races, physical, moral and political, would seem to invest the subject with importance, independent of the of the almost instinctive eagerness we feel to tear away the impenetrable veil that shrouds the mystery of our origin and being. It is surprising that, notwithstanding the enticing aspect of these subjects of research, "the natural history of mankind should be the department of knowledge of the most recent acquisition; so recent, indeed, that it may be considered to have been for the first time explored by an author of our own day, the celebrated Blumenbach." It is true, that a few distinguished persons, both in ancient and comparatively modern times, have made many very important observations, and theorized on this subject, but their observations were limited to a comparatively small number of the tribes of the earth, and their theories, visionary and generally founded on the vague traditions of fabulous antiquity, have not been sustained by more extensive researches, and the severe inductions of unbiassed reason. It is only in modern times that these studies began to assume the form of science, under the hands of Buffon, Daubenton, De Lacépède, Pallas, Camper, Blumenbach, Rudolphi, Virey, Desmoulins, Bory de St. Vincent, W. F. Edwards, the Cuviers, the St. Hilaires, Humboldt, Spix and Martius, and others on the continent of Europe; of Hunter, Lawrence, Owen and Pritchard, in England, and of Dr. Stanhope Smith and Dr. Morton in the United States, who each in turn have added something new to the science of their day. When "Blumenbach entered upon the inquiry, however, no work of extensive research had been written on the comparative anatomy of the human races; nor, until he had formed his admirable collection of skulls, did there exist any adequate means of investigating the most important of those diversities of structure which distinguish one tribe of the human family from another." In 1808, Dr. Pritchard delivered his inaugural essay on the varieties of mankind, in which the comparative physiology and pscychology of the different races of men were made expressly the subjects of inquiry. In 1813, the same treatise was enlarged, and published in one volume 8vo, under the title of "Researches into the Physical History of Mankind," and the present work is regarded as the 3d edition of the same work, but, "entirely written anew, every topic comprised being reconsidered, with the advantage of much additional information." In 1817, Lawrence delivered his celebrated lectures on Physiology, Zoology, and the Natural History of Man, which contained much original matter, and combined extensive research with convincing argument; agreeing with Pritchard in maintaining the unity of the species of all the human races, and dissenting from the opinions of Rudolphi, Virey, Desmoulins, Bory St. Vincent, and others, who asserted in the most positive manner, the original diversity of all the races of mankind. The present work of Dr. Pritchard has already reached the 4th volume, and others will be anxiously expected.

The first volume is occupied with the consideration of the important question of general ethnography, on which comparitive anatomy, physiology, and pathology, are brought to bear in a manner that stamps the highest value on the work. It is more especially the part of the work in which these questions are considered, that we wish to notice, as the other parts of the work in which the tribes and inhabitants of particular countries, with their movements, wanderings, &c., are described, cannot with propriety, be fully noticed in a medical journal.

The introduction contains a statement of the subjects of research.

Notwithstanding the interest that inquiries into those varieties in complexion, form and habits which distinguish from each other the several races of men may be calculated to excite, we are apt to allay curiosity and satisfy our minds by constant reference to some hypothesis, whether adequate or insufficient, to explain the phenomena;

"but if a person previously unaware of the existence of such diversities, could suddenly be made a spectator of the various appearances which tribes of men display in different regions of the earth, it cannot be doubted that he would experience emotions of wonder and surprise. If such a person, for example, after surveying some brilliant ceremony or court pageant in one of the splendid cities of Europe, were suddenly carried into a hamlet in Negro-land, at the hour when the sable tribes recreate themselves with dancing and barbarous music; or if he were transported to the saline plains over which the bald and tawny Mongolians roam, differing but little in hue from the yellow soil of their steppes, brightened by the saffron flowers of the iris and tulip; if he were placed near the solitary dens of the Bushmen, where the lean and hungry savage crouches like a beast of prey, watching with fixed eyes the birds which enter his pit-fall, or the insects and reptiles which chance may bring within his grasp; if he were carried into the midst of an Australian forest, where the squalid companions of kangaroos may be seen crawling in procession, in imitation of quadrupeds; would the spectator of such phenomena imagine the different groupes which he had surveyed to be the offspring of one family? and if he were led to adopt that opinion how would he attempt to account for the striking diversities in their aspect and manner of existence?"

"To those who have considered the subject of this inquiry with the greatest attention, and are well aware of all its bearings, the task appears scarcely less difficult to discover a solution of the problem that may satisfy all doubts. It is found to involve a number of subordinate questions in close relation with subjects which have long been themes of controversy among naturalists and philosophers. Among these are investigations belonging to physiology, as well as others of a different kind, comprising researches into the nature of moral and intellectual diversities, and the perhaps still more difficult inquiries which relate to the origin and formation of languages."

It was known to the ancients that all the countries colonized by them were peopled at the time of their discovery, and to these earlier inhabitants the name of "Autochthones," or "Aborigines" was given; and these people, whose physical peculiarities were generally such as to adapt them to the climate and other conditions by which they were surrounded, were considered as the children of the soil. Whenever navigators have discovered hitherto unknown lands, however remote and difficult of access, they have almost invariably found them occupied by tribes of men generally in the lowest state of barbarism, rarely retaining any tradition of their arrival, or of their separation from the rest of mankind. "Other tribes, not so entirely rude, and who appear to have derived from foreigners the first rudiments of civil culture, have preserved the record of an era when they emerged from their primitive barbarism, after having remained from time immemorial in a savage state, ignorant alike of civil arts, and of the existence of civilized men, until some stranger, some Hercules, or Manco-Capac, some child of the ocean, or of the sun and moon, happened to set foot upon their shores." These circumstances have led many moderns to adopt the opinion of the ancients, that each country was originally provided by nature with a peculiar stock of home-born inhabitants. The adoption of this opinion relieves us of the necessity of solving many of the most difficult questions presented by our subject. Physical peculiarities, diversities of the moral and intellectual character, and of languages, originating with the races to which they belong, no longer stand in our way as difficulties requiring explanation. "But this way of getting rid of doubts and difficulties, is more like the cutting a knot than the unloosening it;" and a thorough investigation of the subject will probably lead to a different conclusion.

Unwilling to silence inquiry by appeal to the decisive authority of the Sacred Writings, our author proceeds to the inquiry as if their authority was altogether indifferent to its decision.

The remarkable connexion known to exist between the traditions of the most celebrated nations of antiquity, leads to the inference that they took their origin from a common ancestry, and affinity of languages render their kindred origin extremely probable. Among these we may name the Semitic nations, the Indo-Europeans, and most other Asiatic and European races; and historical testimony may join to these some African nations, the Egyptians, Abyssinians, and Berbers or Lybians. This is as far as history will aid us, and how much is wanting to prove that all mankind derived their origin from the same stock!

"How are we to bring within the pale, the African Negroes with their multitudinous jargons, the Hottentots, the Australians; the Papuas, or woolly-headed tribes of the Oceanic Islands; the Esquimaux, and the Pessarais in the opposite extremities of the New World, and, in the intermediate space, the anciently civilized, but singular Aztecas and Incas? Most of these nations are destitute of any vestiges which point to a common origin, and historical investigations are entirely unavailing."

Unable to obtain satisfactory testimony on the subject of inquiry from history, it remains for us to attempt a solution of the problem by extensive researches into the laws of correlation which govern the organized world, and by examining minutely the condition of individual existence. When, by accurate observations and careful generalization, we shall have arrived at satisfactory conclusions respecting other species of the organized world, analogy in its infallibility will lead us to the truth in reference to man.

"In the way of investigation thus suggested, the enquiry resolves itself into the two following problems:

"1. Whether, through the organized world in general, it has been the order of nature to produce one stock or family in each species, or to call the same species into existence by several distinct origins, and to diffuse it generally and independently of propagation, from any central point; in other words, whether all organized beings of each particular species, can be referred respectively to a common parentage?

"2. Whether all the races of men are of one species;—whether, in other words, the physical diversities which distinguish several tribes, are such as may have arisen from the variation of one primitive type, or must be considered as permanent and therefore specific characters?"

The only means by which we can investigate the history of particular species, and throw light on the inquiry respecting their origin, is to collect facts relating to the distribution of genera and species over the earth. If we find species so dispersed as to be able, generally, to trace the dispersion of each respectively to some point which may be considered as the seat in which it took its origin, there will be good reason for believing that all the inhabitants of the species descended from a common parentage. But if, on the contrary, individuals belonging to the same species are found inhabiting locations separated by vast distances, or insuperable natural barriers, which the powers bestowed by nature on the species cannot have enabled them to pass; "and if such examples are not rare exceptions to a general law," but are of common occurrence, it will be rendered probable that individuals of the same kind had a distinct and separate origin.

"There are only three conjectures as to the original habitations and dispersion of organized beings, which can be considered as in any degree probable. They are:"

1st. That all species of organized beings originated in one common centre, or in one limited tract, whence they have been subsequently spread into the countries where they are now found.

2d. That the same species may have sprung from many different origins, or have been produced in every place where the physical conditions were congenial to their nature.

3d. That each species originated from some particular birth place or centre, and from a single stock; but that the primary habitations of different species were in different regions of the earth.

The first of these hypotheses is contradicted by the uniform tenor of facts," and rendered altogether improbable by the laws which govern the distribution of living beings—regions remotely separated having productions in a great measure at least, peculiar and distinct from those of other countries; and also, that the present inhabitants of the various regions have not a sufficient power of modifying their natures, habits and constitutions, to allow us to suppose that they could ever have existed together in any one region of limited extent.

The second hypothesis is refuted by the fact that nature has not called organic beings into existence in every situation, where the conditions are favorable to their life and growth; and every one is familiar with the fact that animals and plants frequently flourish in countries where they never existed until introduced by man, and in many cases, when once introduced, they have often succeeded better than in their native soil, and have frequently supplanted indigenous tribes.

All the facts yet known unite in confirming the third hypothesis, and lead to the opinion, that every geographical region has been the Eden of some particular species or tribes of organized beings, which have spread in all directions, in proportion to the facilities offered for their diffusion, the greater or less power of locomotion with which they have been gifted, and their capability of bearing changes of climate, and the operation of the physical agencies that may have enabled them to wander.*

Consequently, the extent to which any species will be diffused, depends upon the facilities afforded by its nature and organization, and upon the geographical relations of the region in which it commenced its existence. Organized beings, both of the animal and vegetable kingdoms, of the smallest size and most simple structure, are so universally diffused as to give rise to the opinion that they are produced spontaneously wherever the circumstances favorable to their existence are found: while those whose organization is more complex and the bulk greater, have a much more limited range. The geographical situation of regions modify very greatly the distribution of the species inhabiting them, and the more remote two regions are from each other, and the more impassable the barriers, whether physical, or those of climate, which separate them, the more distinct will be the tribes of creatures inhabiting them. Examining with these views the position of the continents and islands of our globe, we shall perceive some interesting and striking relations. As a general rule, the productions of two regions differ in proportion to the distance by which they are separated, and the difficulties of access. Plants have a greater power of diffusion than animals generally, but the same general law applies to them. As far as zoological researches have yet gone, it may be asserted that no individual species of animals are common to distant regions. It is obvious that the only latitudes where the great continents approach sufficiently near each other to allow of a possible transit of species from one to the other, are those of the arctic regions. In the extreme north the coasts of Asia, Europe and America, form one continuous tract; the narrow strait which separates the old and new continents is frozen over in the winter; the distance is still further broken by intervening islands, and a passage becomes practicable from

* The same system of reasoning, as well as the order of the series of fossil organic remains discovered by geology, leads to the opinions that, 1st., the creation of species is still progressing under the operation of causes and influences not understood; 2d, that only the simplest forms of organic existence can be produced by the operation of observed influences and circumstances; and, 3d, that species of complex structure can only be produced by the progressive development of less complex forms.

one side to the other for such animals as are able to endure the intense cold of the arctic circle. This tract is accordingly one of the great provinces of the animal kingdom, and the same races of men, and other animals, as well as of plants, are found to be common to the regions bordering the shores of all the portions of the Arctic Sea, in Europe, Asia and America. As we proceed south on the continents, the wide ocean sets an insurmountable barrier to the passage of land animals, and renders it difficult for any of the productions of one to spread to the other; and after we leave the hyperborean regions of the continents, the similarity of their productions diminishes, and in the temperate zone, no animals, except a very few birds, and some marine animals, are common to the two continents, and the resemblance of the productions of the earth is limited to the identity of a few plants, the majority of which belong to the lower orders. The number goes on decreasing still farther as we go on south, and when we arrive south of the equator, we find that South America, South Africa, and Southern Asia, possess hardly any species, even of plants, in common; and in New Holland, so remote from other countries, living nature displays a peculiar type, and new forms, which seem to deviate from the laws of co-existence, generally regarded as universal conditions of life.

Climate and seas, however, are not the only barriers to the dispersion of species; high mountain chains, broad and swift rivers, and extensive deserts are observed to form the boundaries of the range of many species. The same general laws likewise govern the distribution of the inhabitants of the ocean, for though every sea has its whales, its porpoises, its seals, its sharks, &c., which to the common observer are much the same every where, the discriminating eye of the naturalist every where discovers differences of species; and that not only the different oceans have many species peculiar to them, but that, in many cases, species are confined to portions of these oceans, comprising but a few degrees of latitude; and Peron and Le Sueur, in speaking of the great collection of animals made by them in their voyage to the South Seas, remark, that "among all this immense aassemblage of antarctic animals, it will be found that there is not one which exists in the seas of the northern hemisphere." *

Having ascertained the course which nature seems to have followed in the production and distribution of species, it becomes important to investigate the question of the unity or diversity of the human species. If it should be found that all the races of men may with propriety be referred to a single species, the universal analogy of the organized world will lead us inevitably to the conclusion that all these races of men, like the varieties of any other species, have descended from a common stock.

"Now the term *species*, is properly used to express a collection of individuals which so resemble each other, that all the diversities between them may be referred to the ascertained operation of physical agencies. In animals of the same species, conformity of structure and appearance, is not the only character that indicates the identity, and we find, likewise, that the principal functions of life in the species, are subjected to nearly uniform laws; and these laws, when known, become valuable to us as specific characters. On the other hand, animals which most closely resemble in form and appearance, but are yet specifi-

* Peron et Le Sueur, Hist. de tous les animaux qui composent la famille des Meduses. Ann. du Museum. Tom. 14.

cally distinct, differ widely in respect to the same particulars; thus, among dogs, though differing so widely in some external characters, the functions of life are uniform in every particular; while in the wolf, though so nearly allied, the same functions take place in a manner that deviates most widely from this uniformity. The period of utero-gestation, (to take a plain example,) in the female of all varieties of dogs, is sixty-two or sixty-three days, while in the she-wolf, it is about ninety days, a period one-third longer than the other. No similar deviation is known to take place within the limits of any one species."

Among the varieties of mankind we find none of those abrupt deviations which are exhibited by cognate species; they all coincide strictly, in respect to the laws which govern the economy, the nature and periods of the phenomena, in the diseases to which they are subject, and the laws which govern their relation to each other, and to different species.

It has long been a prevalent opinion that a criterion may be drawn as to the identity or diversity of species, by the fertility or sterility of the offspring of males and females of the two groups of animals of which there is a question. There can be no doubt that, in general, different species of animals, especially in a state of nature, or wild and unrestrained, do not breed together; but among domesticated animals such crosses are by no means rare, and in many cases the hybrids, as those of the dog with the wolf, and of the domestic goose and brent (*anser bernicla, Bonap.*) are prolific. It would appear, too, that the comparative rareness of hybrids among cognate species in a wild state is attributable, not exclusively, to the want of power to procreate with each other, but rather in a measure to a natural repugnance which individuals of different species, in a natural state, seem to feel for each other. Domestication, by familiarizing individuals of distinct species, with each other, and by excluding them in a measure from free intercourse with their own species, seems to prepare the way for this unnatural union. Still, with the numerous facts which we possess of this kind, it is highly probable that such cross breeds would not continue prolific for many generations, and would not be perpetuated without frequent recrossing with one of the original stocks.

Attempts have been made in this country to apply this principle as a criterion to establish the diversity of the white and black races, as species; but hitherto, facts would rather favor the opposite views: for extensive inquiry among our large mixed population, has elicited nothing that would tend to the confirmation of such an hypothesis. It is true, that this class of population have prevalent types of temperament and constitution which seem to predispose them to certain classes of affections, such as scrofulous and some other forms of chronic disease. Many of the woman, too, are liable to certain affections of the womb, which may be often referred to the prostitution to which such numbers of this unfortunate class are devoted; and partly to the circumstance that many of them, who are by no means overt prostitutes, are constantly in the habit of procuring abortion to conceal their peccadilloes. To these influences, and sexual excitement to which most of them are prone, may be principally attributed the frequency of chronic irritations of the womb, which really seems to be only reason why they are not, as a class, as prolific as whites of the same rank and occupation. When they lead virtuous lives, and marry at a proper age, they seem to have as large families and often to attain to as great an age as the white race.

Animals of different species, even when belonging to the same genera, and however nearly allied, are endowed with peculiar psychical qualities, which are often more strikingly characteristic than even their physical peculiarities. Even among the lower tribes of creation, these differences are most remarkable; each species of any genus obeying principles of action, and displaying modes of energy peculiarly its own, which are common to all the individuals composing it, and distinct from those which govern the actions of other species, however nearly allied. Now a survey of the phenomena illustrative of the psychical character of the most dissimilar races of men, will prove that they all have similar affections and sympathies, are subjected to precisely analogous laws of feeling and action, and partake, in short, of a common psychical nature. The differences, in psychical qualities and endowments, between the varieties of mankind, are in degree, not in kind, and do not prove difference of species, since there are differences equally great between individuals and families of the same nation. The possession of the power to articulate speech, as a general attribute, may be regarded as a remarkable characteristic which strongly links the different races of men together, and separates them widely from all other species of animals.

The remaining diversities which characterize the different races of men, consist of modifications of the form, consistence or color of different parts of the body. We will take a hasty review of these, endeavour to estimate the value of each, and to ascertain whether they are uniform and peculiar for each race, and to what extent analogous diversities are discoverable among individuals within the limits of acknowledged species.

"On extending our view over the organized world, we perceive no other quality so generally characterizing the works of nature, as an infinite and inexhaustible variety. Human art aims at precise uniformity in its productions, while nature seems, if we may be allowed the expression, to be everywhere satisfied with resemblance. Her purposes are attained by displaying that sort of general analogy which is still compatible with individual variety. It is most probable that no two individuals were ever produced in any species with complete and perfect sameness of form and structure."

Among the peculiarities which characterize the different races of men, the diversities of color and texture of the external parts are those which are most apt to attract attention. We will begin by examining the varieties which subsist among the races of men in regard to the phenomena of colour.

"The term *complexion* is generally applied to the skin, but this is well known to be related to the various hues of the hair, as well as the pigment of the eye; and the different colors of the hair, the skin, and the eyes correspond in a perceptible manner. Men who have light colored, or red hair, have generally a fair skin with a sanguine tint, and blue eyes. Black haired persons frequently have light colored skins, but are not generally so fair as the light haired; when exposed to the heat and light they acquire a brown or yellowish hue, and when they are fair, it can only be the result of careful protection from the sun. In some cases the hair and skin are almost milk white; and in these cases the eyes are weak, and the choroid red; such persons are called albinos."

The colour of the eye is well known to depend upon the pigment covering the choroid and iris, and when the pigment is absent, as in the albino, the eye has a red color, arising from the blood circulating in the vessels of these parts.

The variety of hue which the skin displays in the different races of men, depends on substances contained in the *rete mucosum*, or mucous tissue, which is interposed between the true skin, or chorion, and the cuticle. In the negro, this coloring matter is most abundant, and in the albino, it is almost or even quite wanting. The presence or absence of this coloring pigment depends on modifications in the state of the skin, which may take place after birth. There have been many instances, in which persons who were born of white parents, and who were white at birth, have become black in patches, or in some cases, even black all over; and that without being attacked with any thing that could be considered as a cutaneous disease.

"The coloring matter is also liable to disappear, by absorption, from skins to which it is natural. Instances are not unfrequently observed, in which negroes gradually lose their black color, and become as white as Europeans.

"The coloring matter of hair is manifestly of a common nature with that of the skin. It is frequently absorbed; and being removed by vascular agency, leaves the hair colorless, or white. This is well known to take place in old age; and grief, anxiety and fear have frequently been observed to produce the same effect in a very short space of time."

The various complexions of mankind may with propriety be divided into three classes, viz.

"1st. The black haired, or melanocomous, or melanous variety, characterized by black, or very dark hair."

This includes, by far, the greater portion of mankind. It is the complexion chiefly prevalent, except in some of the northern portions of Europe and Asia, and in some limited tracts in other regions. The hair in these races is of various texture and growth, from the long lank hair of the American races, to the fine crisp hair of some African negroes.

The hue of the skin varies in the blackhaired races, from the deep black to the moderately fair complexion. We have the coppercolored nations of Asia and Africa; the olivecolored races of Asia, and we find every shade and gradation, from the black of the Senegal negroes, or the deep olive and almost black of the Malabar Hindoos, and some other nations of India, to the light olive of the northern Hindoos. From that we still trace every variety of shade among the Persians and other Asiatics, to the complexion of the swarthy Spaniards, and the dark-haired inhabitants of Europe. Animals with dark colored integuments present corresponding phenomena.

"2d. The xanthous variety, distinguished by yellow, red or light brown hair, eyes of a blue or other light colour, and generally, fair skin, with more or less of a red tint."

There are many intermediate gradations between this and the other two varieties. This variety abounds in moderately cold regions of Asia and Europe. Among the xanthous nations are the Germans of Tacitus, the Celts, the Finns, Mordouins, and Vatiaks. Individuals, or particular tribes, with xanthous complexions, often spring up in melanous nations. Thus, some Jews, many of the Mantschu Tartars, some Kalmucks and

Burats, some South Sea Islanders, and some Americean Indians, have these characters. This variety sometimes occurs among the darker races; among the ancient Egyptians, we are told, it sometimes occurred, and among many of the black nations of Africa, it frequently occurs; and is so common in some of the tribes inhabiting near the mountains of North Africa, as to lead some to believe they were the descendants of the Vandal troops of Genseric, conquered by Belisarius. Among the Romans it was so rare, that when a child was born with grey eyes, it was considered as something disgusting, and even monstrous. Among animals we find the analogue of the xanthous variety, in the yellowhaired variety of rabbits, dogs, horned cattle, the sandy hog, the sorrel horse, the yellow sheep of Mysore, and many other eases of domestic and wild animals.

"3d. The leucous variety, distinguished by white or creamcolored hair, of a soft texture; the skin light and fair,; the iris red, often having a tremulous motion, and the eye intolerant to light."

Examples of the leucous variety occur occasionally in all countries, and the parents frequently have some other eolor. Albinos are common among the Indians of Darien, among the inhabitants of the South Sea Islands, and the white negroes of Africa have been spoken of by almost all travellers to that country. These latter are often born of black parents. Albinos are not very rare among the people of Europe. Among other species of animals this phenomenon is still more common; and besides white domestic cattle of various kinds, we have white rabbits, white mice, white deer, white elephants and camels, and a great many white individuals among other wild animals of all countries. The albinos of the human race, and of all other species, seem to have delicate constitutions, and are not so hardy as others of the same species.

Having cursorily examined the varieties of eolor, and compared the phenomena of diversity presented by the different races of men, with analagous ones in other species; we will now proceed to the consideration of peculiarities of form, texture and structure of the different parts of the body in the different races of mankind.

In comparing the principal varieties of form and structure which characterize the inhabitants of different regions, we shall find that some of them which have been regarded as characteristic of the negro race, and which are certainly approximations to the form of some quadrumana, are nevertheless met with in the individuals of all the races; thus we find that the slender legs, ape-like feet and toes, the presence of sesamòid bones, the projecting muzzle, the narrow and elongated pelvis, the peculiar position of the glutæi and other muscles, though more common in the negro races than among other people, are occasionally met with among all races, and analagous deviations, much more striking, are very frequent among domesticated animals. We shall find, too, the fatty hump or steatopyga of the Bosjesman and Hottentot women is not exclusively peculiar to those tribes, but is sometimes met with among other nations; and analogous structures are observed in some families of monkies, and in the sheep of Barbary, Egypt, Ethiopia, Arabia, Persia, and India. Other remarkable peculiarities of conformation, which have been considered as characteristic of the same races of men, have likewise been found among other nations. Thus the development of the nymphæ in the Hottentot race, though naturally very great, is undoubtedly exaggerated by artificial means; and, moreover, we find extraordinary development of those organs not unfrequent in the European races, and almost universal among the women of Ethiopia and Nubia; so much so, indeed, that when the Pope forbade circumcision as a relic of Judaism, among the Christian tribes of Abyssinia, the people rebelled against the authority, and the College of the Propaganda sent a surgeon to examine, who reported in favor of the necessity of the operation on the women, and the Pope again authorized the renewal of the ancient custom. *

We shall find that the crisped hair is not common to all the black or negro races of Africa, some of which have it long, flowing, and glossy; neither is it peculiar to the negro tribes, since other races inhabiting distant regions have the same kind of hair; and even in Europe, individuals are sometimes born of parents who had never seen, or, perhaps, heard of a negro, having the hair similar to that of the African negroes. Indeed, we find in the same tribes, strikingly marked differences in the character of the hair; and in different nations, every imaginable gradation may be observed, between the crisp hair of many negro tribes, to the smooth and curled hair of Europeans, and the coarse, lank hair of the American nations. Variations equally remarkable are met with among other species, in the nature of their covering, which may be seen by comparing the different varieties of dogs, the common goat, with that Angora, Anatolia, and Cashmere; and the common cock, with the one having frizzled feathers.

Of all parts of the human body, the head is that of which the peculiarities are most striking and characteristic. The characters of the countenance, and shape of the face depend mainly on the configuration of the bones of the head. In order to form a correct idea of the varieties in the shape of the head, which are peculiar to individuals or races, it is necessary to examine every part, and to compare all the different aspects which the skull presents: the basis of the cranium, the vertical figure, the profile, and the front view, must all be inspected. By proceeding in this way, we find that all the varieties in the form of the human skull may be referred to three principal forms, which are the following:

1. "The symmetrical or oval form, which is that of the European and Western Asiatic nations. In this, the head is of a rounder shape than in other varieties, and the forehead is more expanded, while the maxillary bones, the zygmatic arches, and the other bones are neatly curved, and so formed as to give the face an oval shape.

2. "The prognathous or narrow and elongated skull, of which the most strongly marked specimen is perhaps the cranium of the negro of the Gold Coast. In these skulls, the principal characters are referable to the idea of lateral compression. The cheek bones project forward, but not outward,"

The prominence of the muzzle, owing to this circumstance, and to the obliquity of the incisive teeth, is a feature that gives many negroes their peculiarly monkey-like aspect. Besides the negro races, the elongated skull is common to the Papous, the Alfourous, and Tasmanians of the Oceanic islands.

"3. The pyramidal, or broad and square-faced skull."

In this form the most striking character is the lateral or outward projec-

* Lawrence's Lectures.

tion of the zygoma. The front aspect presents, in consequence, a broad lozenge form, the diagonal points of which are the apex of the forehead, the point of the chin, and the outer angles of the cheek bones. The face is flat; the nasal bones, and glabella or space between the eyebrows, are nearly on a plane with the cheek bones. "The Mongols afford a fair example of this variety of skull, and the Esquimaux an exaggerated one." This form of skull prevails in the Americas and in northern Asia, but examples are found elsewhere, and the skull of the Hottentot races presents this form, strongly marked, in South Africa.

On comparing the principal varieties of mankind, in reference to the form of the skull and other physical peculiarities, we find that they may be included in seven classes of nations. They are the following:

1 The Iranian*, or Indo-Atlantic nations. This division which is synonymous with the Caucasian, Japetic†, or Indo-European race of former writers, is characterized in the following manner. Head symmetrically oval, the forehead handsomely arched, nose narrow, and slightly aquiline; mouth small, the lips, particularly the lower one, slightly turned out. Complexion cannot be considered as among the characteristics of this type, since it includes individuals of all shades, from the fair Europeans to the jet black of many Lybian tribes. To this division belong all the inhabitants of Europe, excepting the Laplanders; and those of Asia south of a line drawn from the eastern extremity of the Euxine, through the Caspian sea, and from thence along the Himalaya mountains to the northern extremity of the Bay of Bengal; also, those of northern Africa as far south as the northern border of Sahara, and the southern boundaries of Abyssinia. On the characteristic traits of this type, our ideas of personal beauty have been founded, and those ideal conceptions of sublime beauty which have been embodied in the Apollo, in the Medusa of Sesocles, and many other specimens of ancient sculpture. This race includes all those people, ancient or modern, who have been distinguished by preëminent intellectual endowments and cultivation, and who have attained to the highest excellence in philosophy, the arts, and in all that can ennoble and dignify human nature. The following are some of the nations or people included in this type, viz.: all European races and their American descendants; the Teutonic, Sclavonic, and Gothic nations; the Celts, Greeks, Romans, Syrians, Medo-Persians, Chaldeans, Jews, Egyptians, Georgians, Circassians, Mingrelians, Armenians, Usbeck Tartars, Turks, Arabians, Affghans, Hindoos, Gypsies, Abyssinians, Moors, and Berbers, and the extinct Guanches of the Canary islands.

2. "The Turanian‡ nations," synonymous with the Mongolian variety of other writers, are characterized by the pyramidal form of the skull, flat and broad face.

"Nose flattened and squatted towards the forehead; cheek bones prominent; the lips large and fleshy, and the chin short. The color of this race is generally some variety of brownish or yellow-olive, which is the prevalent hue; but the

* So called from the fact that the part of Asia termed Iran, by the ancients, has been, from time immemorial, the abode of this race.

† From Iapetus of the Hellenic, corresponding with the Japhet of the Hebrew mythology.

‡ From Turan of the ancients, coinciding with that part of Asia, lying north of Mount Imaus.

color of the women and children is much lighter, and even fair. The hair is long and lank, frequently spare, beard not abundant. The physical senses are remarkably acute."

Mediocre in intellect, they have nevertheless attained to a peculiar civilization in China and Japan; nomadic, wandering in hordes over the steppes of Northern Asia; but courageous, enterprisiug and audacious, they have, when united under capable leaders, been irresistable. Under Attila, Zingis, and Tamerlane, their irruptions involved great empires in desolation and ruin, and were marked by unrelenting slaughter, and a total disregard of the rights of individuals or nations. They well deserved the titles assumed by them of "the hammer of the universe," and "the scourge of God." Among the nations of this race may be mentioned the Huns and Scythians, the different Mongol tribes, the Kalmuks, Mantschoos, a branch of which conquered and still possesses China; the Burats, all the Samoidan, Tungoose, and Kamtschadale tribes, the Finnish races, as the Laplanders and Icelanders, the Esquimaux, the inhabitants of Japan, Thibet and Bootan, Cochin China, Ava, Pegu, Cambodia and Laos; and as we proceed south among the Malays of the islands, the characters of this race disappear and give place to those of other races.

3. The American nations: these people have a general resemblance to the Turanian nations, in having the pyramidal form of skull, prominent cheek bones, long, lank and coarse hair; differ from them in having the features more prominent and better shaped, the face not so flat, and generally the forehead more retreating. Many nations, as the Flatheads, Natchez, Caribs, many of those of Peru and Mexico, some tribes of Oregon, California, and Russian America, modify the shape of the skull by artificial pressure; sometimes in the bi-parietal diameter, and sometimes in the antero-posterior diameter. The color in this race includes various shades of tawny and ferruginous brown. These people are instinctively brave; some nations have exhibited an indomitable love of freedom, and the Araucanians maintained their independence against the Spaniards by a struggle which lasted a century. In intellect, we find many proofs that they are not doomed to the mental inferiority, on which many have insisted. In North America, in many places, they pursue agriculture, they practice many of the arts learnt from our race, and constitute communities which are attaining to a good degree of civilization; while in balmier climes, their philosophic rulers and Incas erected splendid temples to their gods, and ornamented their edifices with zodiacs. The Inca Garcillaso de la Vega has recorded the glories of his ancestors, the emperors of Peru; while we have a meagre record of Mexican splendor, and Uxmal, Copan, and Palenque, which were desolated, and the record of whose civilization was blotted from the page of history, by the fanaticism and barbarity of the Spaniard, have left us memorials of their magnificence only in their ruins.

This race includes all the aborigines of America, except those inhabiting the shores of the Arctic Sea.

4. The Hottentot race. This race, including the various tribes of the Quaiquæ or Hottentots, and the Saabs or Bosjesmans, is characterized by having the skull pyramidal, as in the Turanian nations, and the trapezoidal shape of the face exaggerated, the cheek bones being greatly

expanded, and the lower part of the face very narrow; eyes very oblique, nose very low between the eyes, wide and depressed at the end; the complexion is of a tawny buff color, or yellowish; the hair even more woolly than that of negroes, but does not cover the whole surface of the head, but grows in small tufts at certain distances apart, and is twisted into small lumps about the size of peas. The steatopyga and some other physical peculiarities, are more common in this race than others. All of the dialects of their language are peculiar; their utterance remarkable for numerous, rapid, harsh, shrill sounds, emitted from the chest, and seems to consist of snapping, hissing and grunting sounds, all more or or less nasal. The *tout ensemble* of this race has been generally regarded as disgustiug in the extreme, but we are also informed by some, that some of the women, even of the Bushnen race, have pleasing and even handsome faces. Many travellers have remarked the resemblance of these people to the Turanian nations, and particularly to the Chinese. The Hottentots are mostly a pastoral people, living in moveable huts or tents, and roam with their flocks and herds in search of pasturage and water. But the Saabs of Bosjesman are a degraded branch, and human nature is no where seen in a more miserable condition, nor physical degradation more extreme. They have no fixed habitations, but sleep in holes burrowed in the soil, or under branches of trees. Their food consists of reptiles, grass-hoppers, and ants, and the flesh of the rhinoceros and ostrich constitute their rarest luxury.

5. The Negro race. This race is characterized by peculiarities which are far from being permanent. The hair is generally woolly; the cranium of the prognathous shape, with the forehead more or less depressed, face narrow, the lower part projecting forward, and the nose flat and broad. It has been remarked that in most of the points of deviation in physical structure, which distinguish this race from ours, they approximate to the quadrumanous class of animals. It is with the negro races as with the rest of mankind, the most savage and degraded tribes are the ugliest. Thus we find that the exaggerated negro form is only found in the hot, flat and unsalubrious regions bordering on the coasts of the sea, and near the great rivers of intertropical Africa, and wherever this condition exists they have always been indolent, sensual, stupid and ferocious savages, devoted to the degraded worship of the fetish. On the other hand the higher regions with temperate and salubrious climates, are generally inhabited by people whose appearance is very different from that which is considered as characteristic of the negro. These diversities are found to fill up all the gradations between the strongly marked negro peculiarities, and ferocious barbarism of the Iboes, and the flowing ringlets of many tribes, the light copper color of the Bechuanas and others, and the high, well-developed forehead, well shaped features, and great personal beauty and civilization of the Ioloffs, Mandingoes, Ashantees, and some of the Kaffer nations. Indeed, it seems almost impossible to fix upon any exact line of demarcation between some of the branches of this race, and the Egyptian, Abyssinian, Berber and Arab branches of our own race. Thus, the Barabra of the Nile, though the pure unmixed descendants of the Koldaga Nuba or Kordofan Negroes, have acquired characters nearly resembling those of the Egyptian and Arab tribes; while many of the Lybian or Berber tribes have the color and some other peculiarities of Negroes.

6. The Papuas. The tribes included under this name are characterized by prognathous crania; prominent cheek bones; chin small and well shaped; features rather regular, except the nose, which is somewhat flattened; the skin black with a tinge of yellow; eye-brows long and thick; their beard thin, hair black, very thick and somewhat woolly, and they wear it frizzled out into an enormous periwig, sometimes three feet in diameter, or let it fall on their shoulders in long, twisted masses. This race in its pure state inhabits the northern part of New Guinea, and the islands of New Britain, New Ireland, and the groups of the Louisiade, Bouka, Santa Cruz, and Solomon's Islands, and more or less mixed on the coasts of the islands of Waigou, Sallawaty, Gummen, and Battenta; and the Tasmanians of Van Diemen's Land are also of this hybrid race. The inhabitants of the greater part of Madagascar seem to be of the same race, and are nearly allied to the races of Eastern Africa. M. Lesson thinks they are not the aborigines of the Oceanic islands, but have replaced others by immigration. They live in miserable, scattered tribes, in a continual state of hostility with each other. Their intellect is of a very low order.

7. The Alfarous.—"Under the name of Alfarous, Haraforas, or Alfoers, a race is known which is characterized by prognathous skulls, repulsive physiognomy, flat nose, high cheek bones, large eyes, prominent teeth, hair black, lank, shining, rough and worn dishevelled, and their beards very hard and thick. The skin is of a smoky black." Tribes of this race seem to have constituted the indigenous population of the islands of the great Archipelago of the Southern Ocean. On many islands they have been entirely supplanted by the Papous, and other races, and on the large ones generally, driven from the sea coast, they inhabit only the inaccessible deserts and mountains in the interior. Thus, the interior parts of most of the Moluccas, the Philippines, Mindanao, Madagascar, New Guinea, and the vast regions of Terra Australis, are inhabited by people of this race. They are ferocious, cruel, and gloomy, without laws, without chiefs, at perpetual war, and are solely occupied in guarding against attacks, and avoiding the snares laid for them. Their intellect is reduced as near to that of brutes as is consistent with human existence; they are deprived of conscience, and are incapable of forming any ideas of good and evil. They derive no subsistence from the earth but wild roots; and when the ocean does not cast dead fish upon their shores, they subsist on reptiles, spiders, grubs, and particularly on a large caterpillar that abounds, or some species of eucalyptus. When we consider the revolting picture of humanity presented by these people, when we look upon the brutal treatment of their women, the shocking practices of their warfare, their horrid cannibalism, and the beastly nature of their habits and character, we are excited in turns, by pity, indignation, and horror.

Having now finished a hasty survey of the peculiarities of the principal races we will take a general view of the facts and arguments presented. By an examination of the laws of distribution and co-existence among animals, we were led to the conclusion that if the different races of mankind could be included within the limits of a single species, analogy would lead to the belief that they all originated from one source. We then examined the different races of men to see if the characters by which they are distinguished have the permanency and constancy which characterize specific distinctions. We found that they had not the degree of constancy which is necessary to constitute them specific distinctions, and that in passing from race to race, and from variety to variety, there is no correlation between these variations of character, and that each part may vary without any relation to variations of other parts. We have found, too, that these diversities are precisely analogous to those which constitute the peculiarities of individuals and varieties within the limits of other well established species of the animal kingdom. We are led by this chain of reasoning to the obvious conclusion that all the races of men are of one species, and derived their origin, as in other species, from a common stock. W. M. C.

[*To be continued.*]

III.—*Lectures on the Theory and Practice of Surgery.* By the late Abraham Colles, M. D., for thirty-four years Professor of Surgery in the Royal College of Surgeons in Ireland. Edited by Simon Mc Coy, Esq., F. R. C. S. A. Philadelphia: Ed. Barrington and Geo. D. Haswell. 1845. pp. 420. (Republished in Bell's Select Medical Library.

If proud and haughty England still robs Ireland of her political privileges, and strives to suppress her clamours for "repeal," it must be conceded that the people of the latter country compete with her powerful master for the empire of letters and the peaceful sway of science. This is especially true in reference to the medical profession, which includes some of the first surgeons and physicians of the age, both for extensive learnning and great practical skill. If London still boasts of her Coopers, Abernethys, her Listons, and others scarcely less renowned; Dublin may console herself in her Cramptons, her Jacobs, her Stokes, her Graves, and last, though not least, her Colles. For sound, practical good sense, and that kind of learning which displays more of the useful than ornamental, and enables us to benefit the age, rather than transmit our names to posterity, the Irish faculty are renowned throughout the medical world. Whoever reads attentively these surgical lectures by Mr. Colles, will, we feel confident, bear testimony to the truth of the foregoing statement. As a practical surgeon, Mr. Colles was perhaps unsurpassed; and as a lecturer, his holding the chair of surgery for thirty-four years in one of the first colleges of Europe, is the best evidence of his great talents and influential name. The work before us consists of forty-nine lectures, on all the various subjects connected with the theory and practice of surgery. They are characterized by great vigor of thought, with a singular simplicity of expression, which will fix the attention of the most careless reader. He acknowledges no teacher but experience, and no master but nature. From habit acquired by years of training, Mr. Colles is a close observer of the phenomena of disease, which he describes with so much tact and brevity as to constitute these lectures a real *thesaurum*, a treasure of knowledge. In these lectures, the surgeon, be he old or young, will find laid down those sound precepts and judicious directions for his guidance in cases of doubt and difficulty, for which he may search in vain in other and more voluminous works on the same subject. As little theory as is compatible with the nature and treatment of surgical disease, is introduced in these lectures; they deal with facts confirmed by experience and observation; erroneous views, both in reference to the pathology and therapeutics of surgical practice, are combated by an appeal to *clinical* arguments, or dismissed with a quaint species of sarcasm as withering as it is just.

To give our readers some idea of our author's matter and manner, we shall notice his remarks upon *injuries of the abdomen.*

"Wounds of the abdomen may effect the parietes only, and not penetrate into its cavity; again they may enter the cavity, and only injure its walls; others both penetrate and wound some of the contained viscera. Contusion in some parts of the abdomen may cause instant death in a way not very easy to understand. A man gets a blow, say over the region of the stomach; he drops down, and in a few seconds he is lifeless; and when you come to inquire the nature and extent of the injuries the parts have received, you do not detect a single lesion that could account for that man's death. But contusion without any external mark, may rupture some of the hollow viscera contained in the abdomen, or even some of the solid ones which contain a great deal of blood in their structure, as the liver, spleen, or sometimes the kidney."

Dr. Sunderland of this city has just reported to us a case of rupture of the *vena portarum*, followed by instantaneous death. Two men, just after a hearty meal, engaged in wrestling; the one raised the other and poised him on his right hip, thus flexing the trunk to the left; and whilst held in this position, the deceased gave a sudden cry of suffering, he was laid quietly down, and soon expired. A *post mortem* examination revealed *a rupture of the vena portarum*, just were it penetrates the substance of the liver, with the effusion of a gallon of blood into the cavity of the abdomen.

Mr. Colles condemns in strong terms the practice of some surgeons who formally recommend us to place the patient, in the positon in which he was at the time he received the wound, and then examine with a probe or bougie, the depth, direction and course of the wound. Such a practice is, in the first place, impracticable on account of the facility with which the relative position of the abdominal muscles may be changed; and in the second, dangerous, because we may inflict serious injury upon the internal viscera, and reopen wounds and renew hæmorrhages, which may produce the death of the patient.

On this subject Mr. Colles continues:

"A penetrating wound of the abdomen may be the *indirect* cause of a fatal inflammation of the peritoneum, as in one case I saw; it was a wound of the epigastric artery. In this case there appeared more blood coming from the wound than I thought should come from such a one; the exact cause, however, was not discovered in time, and the patient died of slow inflammation of the peritoneum, of the diffused kind. The quantity of blood effused in this case was really very small, barely enough to separate the peritoneum for a little way; yet its presence produced the inflammation. If the wound be a punctured one, how are we to know if the bowels are injured or not. We are told that *fæces* will appear at the wound; and certainly if they do, it is a proof, and the only real proof, that the intestines are wounded. You are not to trust to the appearance of blood in the *stools*, for the quantity of blood from a wounded bowel must be very small, and the fæculent matter in that portion of the intestine wounded, may not be expelled for two or three days; but with the great uncertainty that must exist as to the state of the viscera in these wounds, it fortunately is of no consequence whatever in a practical point of view; for whether the intestines be wounded or not, the treatment must be the same. If a portion of the intestine be wounded, adhesions may form around the wound, connecting it to some neighboring part, and extravasation be effectually and permanently prevented, and the patient do very well; but if, in your anxiety to know the whole of the mischief, you go on searching with probes, &c. you disturb the adhesive inflammation that nature is perhaps setting up to prevent extravasation, and you will certainly destroy the patient.

No matter how small a quantity of fœces is extravasated into the peritoneal cavity, diffused inflammation must follow. Do not, therefore, meddle with the wound, not even to handle it; there is no excuse at all for touching it; it can only serve to gratify an idle curosity without doing any good, and will put the patient's life into great hazard. If an intestine is wounded, the fœces will sometimes be discharged by the wound, and sometimes not; we cannot tell why this is so, but such is the case. When the fœces do appear at the lips of the wound, is the case more dangerous? No: it is less so, because there is then free exit to the matter and less danger of extravasation. All you have to do is to keep the bowels soluble, and after the first shock of the injury is over, to bleed the patient."

In reference to the signs of internal hæmorrhage, after wounds of the abdomen, our author remarks:

"Suppose a man shot in a duel, the ball enters the abdomen and wounds a large artery—he falls and faints—presently he recovers out of this—but his pulse is remarkably variable; sometimes it is strong and bounding; again it becomes weak and thready; the hæmorrhage is here considerable. The bounding pulse is present, which always indicates that the bleeding is going on; the patient then gets weak, and it stops—very soon he rallies again, and again it goes on, and at length he dies. A man gets a contusion in the abdomen from fall, and a pretty large tumour forms; how are you to know whether this is caused by blood or fœces. A collection of fœces, in such a case, never forms a large *tumour;* it must, therefore, be blood. This patient may recover: but all you can do for him is to bleed, bleed, bleed.

"When the abdomen is wounded and a portion of the intestine escapes, too large to be returned, we are not to puncture the intestine with a view to discharge the gas which it contains, to return it into the cavity; for if we employ a small needle, the mucous membrane instantly blocks up the little orifice thus made, and prevents the escape of air; whereas, if we use a large needle, we are to excite inflammation by leaving an outlet for solid matters to make their exit. The best plan is, therefore, to enlarge the opening; and if the point be one of choice—make the cut in the direction of the muscular fibres of the abdominal parietes—cut upwards, as in the operations for strangulated inguinal hernia. We may depart from this rule, when we have reason to believe that no important artery will be endangered, by enlarging the wound in a transverse or oblique direction. When the protruded intestine is in a state of high inflammation, we must not hesitate to reduce it, nevertheless; because, it is not only excluded from the action of external agents—powerful causes of inflammation, but it will be in the best possible condition to resume the normal state, as the natural temperature of the abdominal cavity, and the smooth and rubricated surface of the peritoneum will have a better effect than the blandest cataplasms pharmacy can devise. But if the intestine is actually in a state of gangrene, we must be careful how we interfere, lest we break up the delicate adhesions, now formed or about to be formed, between the gut and the internal borders of the wound; and extravasation of fœces, invariably productive of speedy death, be consequence. Leave the issue to nature, who always in such cases displays more curative power than the best directed efforts of art."

In regard to the best method of proceeding in wounds attended either with a longitudinal or transverse cut of the bowels, Mr. Colles says,

"All you have to do is, to take a small needle, and a single thread, and sew the *entire wound* from end to end, with the continued or glover's suture; you will not have occasion to put card, or candle, or any thing else into the gut, to sew on, or keep it open. When you have sewed the wound all around, cut the ends of the ligature close, and return the bowels.

"No danger is to be apprehended from returning the thread into the cavity of the abdomen, as was formerly believed, for the thread gradually cuts its way through the gut, whilst coagulable lymph is thrown out with a rapidity sufficient to close the wound made by the progress of the thread, and thus form an effectual barrier to the escape of fœces or fluids."

In reflecting on this subject, we have been lead to believe that some of the fibres of the large nervous cords, such as the ischiatic, might be used as ligatures in such cases. When a large nervous cord is partially divided, it may be split into almost any number of delicate threads, uniformly of the same thickness throughout, and so strong as to require considerable force to rupture them. Would not such ligatures be less irritating, and equally as effectual in deligation of the intestine, or, indeed, any other? It is not foreign substance, it is sufficiently indestructible, as every anatomist knows, and may be taken up, in due time, by the absorbents, and thus disposed of, without detriment to the constitution. These are, of course, crude suggestions, but we hope some experimental philosopher in medicine will reflect on the subject, and if deemed plausible, put the matter to the test.

It is impossible to give even a synopsis of these lectures. The subjects brought to view are so various, the matter so valuable, and clothed in words so simple, yet so expressive, that we find ourselves trespassing upon our limits, without doing justice, either to the memory of the author or to the readers of the Journal. The book will, however, be rapidly circulated in this country; and those who wish to study surgery, stripped of its technical verbiage, must read these lectures as they fell from the lips of this distinguished Irish professor. A. H.

Art. IV.—*Observations on Intermittent, Remittent, and Congestive Fever.* By Professor Barbour, of Kemper College, St. Louis, Mo. (Published by the Class.)

This little pamphlet contains the experience (which seems to be considerable,) of Dr. Barbour, in the treatment of an interesting class of diseases. His observations, in relation to the pathology of intermittent and remittent fevers, are just, as far as they go, without being original. The same may be said of his method of treatment. He recommends us to give quinine, in ten and twenty grain doses, combined with an equal proportion of the compound powder of ipecacuanha. We prefer the quinine alone, or combined with fractional doses of the sulphate of morphia, as less likely to disturb the stomach, and, therefore, not interfering with the absorption of the quinine. In the treatment of congestive fever, he is warm in his praises of cold affusions over the surface of the body. He says, "the remedy which I esteem above all others, in the treatment of congestive fever, is the affusion of cold water." He places the patient, stripped, on a broad plank supported by chairs, and pours cold spring water from a pitcher over the chest and abdomen, immersing the hands and feet, at the same time, in hot water. This done, the patient is thoroughly wiped with dry clothes, put to bed, and well covered. "This," says Dr. Barbour, "is soon followed by reaction, and the patient is rescued from death." We prefer the application of pounded ice to the epigastric centre, and to the head, when the seats of intense morbid heat, as more convenient, and less likely to determine dangerous prostration. It must be remembered that in genuine congestive fever, we have hyperæmia of one or more organs, with anemia of others—too much blood in some parts, with a corresponding deficiency in others. From the first, we may abstract blood by cups, leeches, &c., in some cases, and afterwards apply cold lotions, pounded ice, and the like, over the part, to reduce the morbid heat, and moderate the re-accumulation of blood in the same organs; whereas, in the latter case, powerful revulsives, counter-irritants, dry heat with frictions, must be diligently applied, with a view to invite the circulating fluid and the nervous influence to the anemo-adynamic organs. Hence, we have often ordered pounded ice to the head and epigastrium, and at the same time, covered the extremities with sinapisms and blisters, with signal effect. Dr. Barbour says nothing in his pamphlet, of introducing quinine into the system, either by the "*rectum*," or the "*endermic*" method; and as this paper was read before a class of young men, about to enter upon the practice of medicine, we regard this omission as unfortunate, since his silence, on this point, will be construed into a want of confidence in the methods, and, therefore, condemnatory of the practice. Besides, the excessive irritability of the stomach, often announced by constant vomiting, we believe that the powers of the gastric mucous membrane, as an absorbing surface, are suspended or abolished, in a great measure in these cases, consequently, the quinine will either be rejected, or when retained, it fails to produce its specific effects, in consequence of its non-absorption. Here, lost time cannot be redeemed. We must look to other surfaces less irritable, and endowed with immense absorbing powers. This will be found in the rectum—the *second stomach* of the digestive tube. Give, therefore, from half to a *drachm* of the sulphate of quinine suspended in a small quantity of tepid-beef tea or chicken broth, without loss of time, and repeat the dose, in the same manner, at longer or short intervals, until the pulse rises, the heat becomes equalized, and your patient is rescued from his perilous condition. If this method is impracticable, (which is rarely the case,) and the stomach rejects the medicine, apply a large blister over the epigastrium; and this will not only allay the morbid excitement of the stomach, but at the same time, open a channel for the speedy introduction of the quinine into the system. A. H.

V.—*An Inaugural Dissertation on Hereditary Transmission; submitted to the Faculty of the Medical College of South Carolina, for the degree of M. D., by* Thomas N. Love, *and recommended for publication by the Medical Committee.* Charleston, S. C., 1844.

This is an exceedingly interesting pamphlet, and in point of ability, we doubt not, far superior to the generality of theses submitted to our medical faculties for the degree of M. D. It displays an extent of research and reflection, that does the author much credit, and, if followed up by application and energy, authorises us to predict for him an honorable and successful career in the practice of his profession. He has settled himself in one of the most interesting sections of Mississippi, (Columbus,) and we trust will lend us his aid in the elucidation of Southern diseases, and the development of our medical resources.

Dr. Love will please accept our thanks for his pamphlet. We have read it with much pleasure, but are compelled, from want of space, to forego an analysis of its contents.

6.—*Report on the Medical Department of the University of Pennsylvania, for the year* 1845, *to the Alumni of the School.* By the MEDICAL FACULTY. Philadelphia, 1845.

This is the beginning of a series of annual reports which the Faculty of this venerable institution propose making to their alumni, in regard to their *alma mater*. It opens thus:

"The Medical Faculty of the University of Pennsylvania offer the following statement in relation to the present condition of the school under their charge, in compliance with a wish frequently expressed on the part of their former pupils, and in accordance with their sense of what is due to the institution, and its friends. A medical school which has been in existence eighty years—which exhibits a list of graduates amounting to nearly four thousand four hundred, and has contributed to the education of eleven or twelve thousand physicians, presents a relation to the profession and to the community which is not without interest, and justifies, if it does not call for, an occasional exposition of its concerns."

"The present circular contains catalogues of the matriculants and graduates of the last session, with an account of the present officers of the school, and a brief recapitulation of its condition, in relation to the number of pupils who have attended upon its instructions and received its honors since 1834;" from which we are pleased to learn that "the Institution has lost nothing of its former prosperity;" notwithstanding the numerous sister institutions which have sprung up in all parts of the country. The faculty look upon these "in the light not of rivals, but of co-laborers in a field of usefulness too extensive for occupancy by one or by even a few schools; they view their prosperity with satisfaction, and claim, in their turn, a generous sympathy in their efforts to do good."

We are pleased to see amongst the regulations that

"graduates of five years standing, of respectable medical schools on the *adeundem* list, are permitted to attend the course of lectures upon a general ticket of admission, free of expense, except the cost of the matriculating ticket."

A similar rule, we believe, now obtains with all the medical colleges in the United States; and it is a good one. Every inducement and facility should be offered to practitioners, who, if not poor when they commence, soon become so, to revisit the medical schools and improve themselves in the elements and principles of the profession.

Old Pennsylvania offers in every respect a bright example to her younger sister institutions, and we wish her long life and prosperity.

E. D. F.

VII.—*Proposed new work, by* JOHN W. MONETTE, M. D., *to be entitled* "THE VALLEY OF THE MISSISSIPPI," in two parts:—comprising PART FIRST, or *The History of the Discovery and Settlement of the Valley of the Mississippi*, by the three great European powers, France, Spain and Great Britain; and the subsequent occupation, settlement, and extension of civil government by the United States, up to the year 1845. In two volumes octavo, and comprising not less than five hundred and fifty pages each.

'Westward the Star of Empire takes its way.'

"The whole tenor of this History is to trace the gradual and steady advance of the European colonies and settlements, by the different routes and avenues into the central parts of North America; and the subsequent extension of the Anglo-American population, and republican government throughout the great Valley of the Mississippi; to perpetuate the memories, and to record the early privations, sufferings, fortitude and prowess of the western pioneers, in the occupancy of this great and fertile region; to illustrate the benign influence of free government and liberal principles, aided by the magic influence of steam power, upon the moral energy and enterprise of mankind. We thus trace the progressive growth of the various political transmutations, in the germs of a vast republic, in their advance from detached and feeble colonies, to a great and enlightened nation; and the speedy conversion of a savage wilderness into a highly civilized and commercial country.

"*Part Second*—The Physical Geography of the Valley of the Mississippi, upon a new plan of arrangement; comprising a general description of the Mississippi river, its floods and physical changes in the delta; its great tributaries, and the great river regions; the general face of the country, geological formations, mineral resources, and natnral productions; its aboriginal inhabitants, and animals; climate, soil, and agricultural productions. In two volumes octavo, comprising not less than five hundred pages each.

"Unable to find any similar effort to serve as a model or criterion, the author has been compelled to devise a new and original plan of arrangement, both in the 'History,' and the 'Physical Geopraphy;' yet it adapts itself to the subjects under consideration as a natural arrangement, and differs very materially from any work which has preceded. The matter of this work has been prepared and arranged with much care and patient research; and, notwithstanding some imperfections may exist, it is confidently hoped that it will supply a valuable *desideratum* to private libraries in the United States and in Europe.

"The whole matter relative to the physical changes and alluvial formations in the delta, the result of personal observation in the last ten years, is entirely new, and is not to be found in any books heretofore published, The greater portion of the matter upon the climate, seasons, meteorology, and natural productions, is also new. The geographical statistics generally, have been arranged with great care to precision and correctness."

[Here follows a full outline of the "ARRANGEMENT OF THE WORK," for which we cannot make room.]

"*Terms and Conditions.*—The first part, or the 'History,' in two volumes, being nearly finished, can be completed this fall, and put to press as soon as a sufficient number of subscribers is obtained to justify the publication. The second part, or 'Physical Geography,' in two volumes, is also nearly finished, and can be completed for the press by the close of next spring; and both portions can be published in the course of next year, if the list of subscribers will justify the undertaking.

"The work will be printed with a fair, new type, on good paper, and in handsome style, similar to Bancroft's History of the United States. It will be embellished with a number of engravings and appropriate plates, illustrative of western history and geography, besides maps and wood cuts.

"The binding will be neat embossed cloth covers, similar to Bancroft's History; or in leather binding at a small extra cost on each volume.

"The work will be delivered to subscribers, at $2,50 per volume, payable on delivery—or, $2,25 per volume, delivered at any bookstore in Pittsburg, Cincinnati, Louisville, St. Louis, Memphis, Vicksburg, Natchez, or New-Orleans.

"The 'History' and the 'Physical Geopraphy' are separate works, and may be taken separately.

"Subscribers' names, and lists of the same, forwarded to the author at Natchez, will receive due attention. To booksellers and agents ordering any number of copies, or becoming responsible for any number of subscribers, a liberal discount will be made.

"*Natchez, September*, 1845."

[We invite the particular attention of our readers to the announcement of this important work. We know Dr. Monette to be a gentleman of extensive reading and untiring industry, and we have reason to hope that his labour will supply a *desideratum* the absence of which has been extensively felt in regard to the early history and extensive resources of the Mississippi Valley. We wish him all success in the laudable enterprize, and hope he will receive an ample reward for his labour.

Part Third.

EXCERPTA.

1.—*Observations in Midwifery.* By W. Tyler Smith, M. B., London, Lecturer on Midwifery and the Diseases of Women, at the Charlotte-street School of Medicine.

On the Treatment of Puerperal Convulsions.—Remarks on some of the more important remedies of centric spinal action.—In treating of the pathology of puerperal convulsions, I have endeavoured to show that this disease must always depend on one of two causes,—either on direct irritation of the spinal marrow, or on some irritation of excitor spinal nerves. If there be any truth in this view, it is evident that remedies also should be divided into those which allay irritation of the spinal centre, and those which remove irritation from the incident excitor nerves, or diminish their excitability. A large and important class of diseases are referable to the spinal system, and every branch of this new department of pathology calls for some therapeutic division of this kind. Medicines must be studied with reference to their effects on the different divisions of the nervous system. Unless the spinal marrow be dissevered, therapeutically, as well as physiologically, from the other nervous centres, the anomaly presents itself, of remedies which act as stimulants to the spinal marrow, but as sedatives to the brain, and vice versa. Indeed, on looking to the three great divisions of neurology—the brain, the spinal marrow, and the ganglionic system—remarkable instances at once present themselves of therapeutic agents which affect them severally in the most opposite modes. Thus the ergot of rye increases the contractions of the uterus, an organ chiefly under the control of the spinal marrow, but it depresses the action of the heart, which is under the control of ganglionic nerves; strychnia affects the purely spinal actions to an intense degree, leaving the functions of the brain perfectly intact; while conium, on the other hand, affects, in poisonous doses, both spinal marrow and the brain, producing at once delirium and convulsions.

The spinal system being that which is chiefly involved in puerperal convulsions, all remedies resorted to in this disease must be studied with especial reference to spinal physiology and pathology.

Remedies affecting the spinal system very naturally divide themselves into those which act on the central organ, the spinal marrow, and those which affect the extremities of incident spinal nerves. Those to be considered in the present communication are all of the former class.

Bloodletting.—The action of bloodletting on the spinal marrow is greatly modified by the condition of the circulation. In fulness of the vascular system it is the most powerful sedative of spinal action we possess. Hence, venesection is the grand remedy in the simpler form of puerperal convulsion, where the disease chiefly depends on stimulation of the spinal marrow by excess of blood, or on the mechanical pressure exerted by the blood on that organ, together with the counter-pressure of the distended brain on the medulla oblongata. In such cases, bleeding should be performed with a view to its sedative action on the spinal marrow, and to avert the mechanical effects of vascular pressure from this organ. Alone, it will frequently be sufficient to subdue the disease, particularly when the fits come on before the beginning of labour, or after delivery. But another most important intention of bloodletting should never be lost sight of—namely, that of preserving the brain from injury during the convulsion. Besides the *primary* cerebral congestion, which may have been the cause of the attack by its counter-pressure on the medulla, the convulsive action itself, with the glottis closed exerting great muscular pressure on the whole vascular system, and causing, as it does, the greatest turgidity of the vessels of the head, is a dangerous source of fatal cerebral congestion, or of serous or sanguineous effusion. As in the case of epileptics, women in puerperal convulsions frequently die of apoplexy, produced by the immense pressure exerted on the cerebral column of blood during the fits. It is in great measure from the effects of bloodletting in warding off accident from the brain that bleeding is so universal in this disease. The due recognition of the distinct operation of bloodletting on the cerebral and spinal systems is of the utmost consequence. In plethoric states of the circulation, it is, in this disease, *curative* in its action on the spinal marrow, *preventive* in its action on the brain.

In the absence of definite ideas regarding the effects of bloodletting in this malady, it has been often pushed to excess, or practised where it should have been altogether avoided. In the numerous cases where, besides vascular excitement of the spinal marrow, some irritation of spinal excitor nerves exists as a conjoined cause of convulsion, repeated bleedings will often fail to subdue the disease, unless the *eccentric* irritation be at the same time removed. When irritation of the uterus, the rectum, or the stomach, is in part excitor of the convulsion, bleeding alone cannot be relied on. It may at first diminish the impressibility of the *central* organ, rendering it less susceptible of the incident irritation, but if persisted in to a large extent without the removal of the *eccentric* irritation, it becomes in the end positively injurious, by increasing instead of diminishing the excitability of the spinal marrow.

In vascular plethora, depletion is undoubtedly a sedative to the spinal system, but when the circulation is reduced considerably below par, loss of blood becomes an actual stimulant to this organ. Hence it is that the reports of those who have most pertinaciously followed bloodletting, exhibit the loss of a greater number of patients than those who have been more cautious in this respect. The propriety and extent of venesection must be estimated, then, not by the violence of the disease, but by the state of the circulation in the interval of the fits, and with especial reference to the different effects of vascular plethora and vacuity upon the spinal centre. I should avoid these manifest repetitions had I not thoroughly convinced myself that patients rightly bled in the first instance are frequently subjected to successive depletion until loss of blood itself becomes the cause of the final seizures. Nothing is, I believe, more certain to remove this deplorable source of mischief than the distinct perception of the effects of venesection on the spinal marrow, the true organ of puerperal convulsion.

Similar remarks would apply with almost equal force to the other parts of the common antiphlogistic regimen. Nearly allied to the *modus operandi* of bleeding are the effects of nauseating doses of emetic tartar, which have been found so serviceable in the treatment of puerperal convulsions by Dr. Collins. It is extremely probable that this remedy acts on the spinal system through the medium of its effects on the circulation.

In the convulsions occurring in delicate anæmic women, bleeding is generally inadmissible, becoming, in fact, an exciting cause of the disease under such circumstances. Still, in cases approaching to this state, cautious bleeding may be sometimes necessary to *preserve* the brain from injury, but here venesection requires to be followed promptly by stimulants; such cases are, however, rare in comparison with those in which fulness of the circulation exists at the onset of the disease.

Dilatation of the glottis.—During the attack of convulsion the glottis is partially or entirely closed. The greatest authority on this point, Dr. Marshall Hall, questions if true convulsion could ever occur without this state of the glottis, and the cerebral and spinal congestion it occasions. We know that sometimes the epileptic attack is warded off by the dash of cold water on the face or chest, so as to excite a sudden inspiration and the dilatation of the glottis. It is on the same principle, that of exciting a strong inspiratory act, that we stimulate the nostrils or sprinkle the face with cold water in syncope. Excitation of the incident nerves of inspiration in the same way has been known to prevent the puerperal convulsion.

Harvey gives an instance in which stimulation of the trifacial nerves in the nostrils recovered a woman who became comatose, during labour. Denman also relates an interesting case, in which a convulsion was excited during every labour-pain, but in which he kept off the attacks, until delivery was completed, simply by throwing cold water on the face with a bunch of feathers at each accession of pain. It was found that this mode of proceeding, from which he augured so favourably from its effects in this and other cases, did not prove equally efficacious on all occasions. He observes, that this is "a safe remedy," which cannot be said of all measures resorted to in this disease. It certainly must be productive of benefit in cases where the glottis is not so firmly locked as to render its dilatation by this means impossible. Even if it does nothing to prevent the accession of the fit, every time we can dilate the glottis, and cause a full inspiration, we take off a considerable amount of vascular pressure from the nervous centres, and lessen the proportion of venous blood in the system.

The application of cold.—Cold, applied to the head in the form of napkins, lightly wrung out of cold or iced water, ice itself, or a full stream of cold water poured from a height, has became an approved remedy in puerperal convulsions. It therefore becomes an interesting question—How does cold thus used act on the nervous system? It may act as a sedative on the cerebral portion of the spinal marrow, or it may lessen the distended state of the cerebral circulation, and thus relieve the counter-pressure of the brain on the intra-cranial portions of the spinal system. Probably it acts in both of these modes. When used in the form of the continuous douche, as recommended by Dr. Copland, it would, in addition, tend to excite acts of inspiration, and thus dilate the glottis. The sedative action of cold on the nervous centres would seem to be shown satisfactorily by the reputed good effects of cold applied to the whole length of the spinal column in tetanus.

The application of cold to the spine as well as to the head may hereafter be found beneficial in puerperal convulsions. Whenever cold in any form is resorted to, its use, except for the purpose of exciting the respiration, must be continuous, as the intermittent application of cold, locally or generally, would excite instead of allay the spinal system. The benefit derivable from cold must arise from its local action on the nervous centres, because in tetanus, the purest form of increased morbid spinal action, cold applied to the spine is serviceable, whereas, when applied to the whole surface of the body, it is extremely dangerous, and even fatal.

Administration of opium.—It as an object of very great therapeutic importance to ascertain the true effects of opium on the spinal system. One author maintains that opium diminishes the contractions of the uterus in after-pains, another, that it increases their energy. Some recommend it in uterine hæmorrhage, as an efficient means of exciting uterine contractions, while some blame its administration on the plea that it produces uterine inertia and hæmorrhage. Some, again, maintain that it retards, and others, that it accelerates, the progress of labour. With respect to the propriety of its use in convulsions, there is a great discrepancy of opinion. Though we may not yet have sufficient data to form a perfect and decisive judgment, I believe we can at the present time make a very considerable advance in the right direction.

When the amphibia are in a state of narcotization from opium, the whole excito-motory system is exalted to an intense degree. The slightest irritation of the surface of the body produces universal convulsions. If this fact were applicable to man, it would be an argument to show that it is a powerful spinal stimulant, as it certainly is in the amphibia. In the state of narcotization by opium in man, there is no positive evidence that the incident spinal nerves are more excitor than at other times; still, in poisoning by opium, convulsions do not unfrequently occur as one of its toxicological effects. On the contrary, in poisoning by belladonna, true convulsive action is very rare, and it has been found by Dr. Hutchinson, of Nottingham, a successful cultivator of spinal pathology and therapeutics, that belladonna exerts a sedative influence on the spinal marrow in tetanus. That opium does not, in man, allay excitement of the spinal marrow, is shown, by its failure in the treatment of tetanus and hydrophobia, the purest and most intense forms of morbid spinal action. The patient may be poisoned by opium without any reduction of the spasm.

Mr. Bonney, in a paper on the effect of opiates, ingeniously suggests that they prove indirectly stimulant to the reflex actions, because the arrest of the cerebral functions they occasion increases the muscular irritability. I think there are reasons for supposing that, besides this effect, which is very probable, opium is a direct excitant of the spinal system. It aggravates convulsions, when there is already a state of insensibility from other causes, and when, therefore, this explanation could not be received. It is the general opinion of practical men, that opiates are injurious in the convulsions of children, in epilepsy, and in puerperal convulsions; and it is certainly of little or no value, probably, indeed, prejudicial, in tetanus, hydrophobia, and other severe diseases of the excito-motory system.

Some striking distinctions may be made respecting the administration of opium under different circumstances, particularly in puerperal convulsions. If a dose of opium be given in this disease in a full state of the circulation, before bleeding, there is an aggravation of the disorder; while if it be given in puerperal convulsions in an anæmic subject, or after excessive depletion, it is of great service. If, in a case of convulsions, opium be given at the commencement, it is dangerous in its effects; but the same medicine is frequently valuable in the advanced stage of the same case when the vascular system has been powerfully depleted. Thus it would appear evident that in convulsions with a full state of the circulation, opium is a *stimulant* to the spinal marrow, while in convulsions with anæmia, it is destinctly *sedative*. It is certainly an important point in practice, that the effects of opium in puerperal convulsions depend on the state of the circulation; that in plethoric or inflammatory conditions it is always dangerous, while in anæmia and debility it may always be used beneficially.

Emotion.—The regulation of emotion is of considerable importance in preventing the accession of convulsions when they are threatened, and in averting the return of the attacks, in the intervals where *conciousness* is retained. Mental excitement of every kind should be soothed, and avoided as much as possible. The sight of the infant, of alarmed friends or relatives, unpleasant intelligence, noises in the sick chamber, or still more trifling matters, have caused or renewed convulsions. Perfect quiet and repose within the sick room, and the absence of all signs of excitement on the part of the attendants, are of the utmost consequence: the calm or timid look of the professional man may either excite or prevent a fit. The *psychical* effects of emotion upon the spinal marrow—an otherwise purely *physical* organ, in health and disease—is one of the most striking and indubitable facts furnished by the pathology and physiology of the spinal system.—*London Lancet, Sept.*, 1845.

2.—*Case of Gunshot wound, attended with Secondary Hæmorrhage, in which both carotid arteries were tied at an interval of four and a half days.* By John Ellis, M. D., of Grand Rapids, Michigan.—*From the New York Journal of Medicine.*

Grand Rapids, Michigan, June 18th, 1845.

Mr. Editor:—Theories and speculations in our profession are fast giving way to facts. Diseases which were once considered incurable are now cured, and operations which were once regarded as impracticable, are now performed with confidence and safety. Some of these improvements have been derived from accident, and some by experiment. Whenever an important case has been treated by uncommon remedies with more than ordinary success, or where a surgical operation has been performed, which has hitherto been regarded as impracticable, successfully, or even without success, it becomes the duty of the practitioner to report the same to the profession, that it may stand as a beacon, or that it may add one to the mass, from which we are to deduce practical conclusions. According to the light which I now possess, I am led to suppose that the following case is one which may be of service to the profession; therefore, I feel it my duty to report the same, and shall cheerfully leave for you to decide whether it is worthy of notice or not. I think I have seen cases reported where both carotids have been ligatured, within as short a time, successfully, but am not certain. We can find but two instances recorded in our libraries; in one case both were ligatured at the same time, and the patient died; in the other, fourteen days apart, and the patient recovered.

Peltish Hill, aged twenty-one, while setting a trap in the woods, October 21, 1844, twelve miles from this place, was mistaken for a bear by a companion, and received the contents of his rifle: the ball struck him near the centre (and immediately above the spine) of the scapula of the left side, passing out, after making a flesh wound of about two inches and a half, towards his neck, and after about the same space it entered his neck over the centre and posterior edge of the sterno-cleido-mastoid muscle, passing up through the centre of his tongue, and out of it to the right of the mesian line, struck the lateral incisor, cuspidatus, and bicuspidatus of the right side, knocked them out, and the alveolar process, external to them; passed then through the upper lip, leaving a ragged opening through it. I saw him a few hours after it was done, and found that he had lost but little blood. I brought the edges of the wound in his lip together with adhesive plaster and two or three sutures, and dressed the other wounds with cold applications. He was suffering but little pain, but an entire inability to swallow even liquids, which appeared to be owing to the injury and swelling of his tongue. We prepared a litter, and had him brought to this place. Fearing secondary hæmorrhage, I directed that he should be carefully watched night and day by two intelligent assistants, and directed them to compress the *carotids* and the orifice of the wound in case of hæmorrhage. Very little inflammation followed, which was doubtless owing to his being deprived of food and drink for about three days, on account of his inability to swallow. At the end of three days, I introduced a flexible catheter into his œsophagus and injected some water and nourishment; the next day he was able to swallow with difficulty some liquids, and soon recovered the power. The seventh day, during the night, I was sent for to see my patient; hæmorrhage from the wound in the tongue had take place, but was readily subdued by compressing the carotid of the left side, and the orifices of the wound; upon removing the pressure, no bleeding returned. The following night I was sent for, and was accompanied by Dr. Platt. My patient had lost considerable blood, and it had been with difficulty restrained until we arrived, by pressure, which caused him considerable pain. We placed him upon a table, and with the assistance of Dr. Platt and a student, I ligatured the left carotid artery, below the omo-hyoideus muscle; an operation attended with a good deal of difficulty, owing to the swollen state of the parts, the necessity of keeping up pressure, the bad position of the parts owing to the necessity of keeping the mouth in a certain position to prevent his being strangled by the blood, and the necessity of operating by candle light. When we arrived at the common sheath, we found the *descendens noni* in its usual place; we drew it to one side, and upon opening the sheath came in contact with a large nerve, even larger than what I have been accustomed to see in the par vagum, in the dissecting room; we supposed it to be this nerve, and drew it to one side, as it was directly in front of the artery; as I passed the needle, I separated the artery and vein a little, but saw no appearance of the nerve in its usual place. No unpleasant symptoms followed the tightening of the ligature save a slight coldness of that side of his face and an occasional throbbing pain beneath the sternum, and in the direction of the ligatured vessel. Our patient appeared to be doing well until the eleventh day from the accident, when he had a return of hæmorrhage, which was readily subdued by pressing upon the right carotid and the two orifices of the wound. There was a slight pulsation in the left temporal artery, the first I had felt since the application of the ligature. There was a return of the hæmorrhage during the night and several times the next forenoon. He could not endure pressure upon the right carotid for any length of time, and we had to depend upon pressure upon the two orifices of the wound, which caused a good deal of pain, especially in the direction of the ninth pair of nerves. He was becoming very restless under the pressure, and was very anxious to have us do something to relieve him. In consultation with Drs. Platt and Shepherd, of this place, we came to the conclusion that the only chance to save the patient, was to ligature either the lingual, or the carotid of the right side, and as we could not satisfy ourselves whether the hæmorrhage was the lingual of the right side, or from the unligatured end of the left carotid, together with the fact of there being a good deal of tumefaction under the angle of the jaw, so much as to prevent our feeling the corner of the os hyoides, we came to the conclusion to ligature the carotid. Accordingly, with the assistance of Drs. Platt and Shepherd, I applied a ligature to the right carotid, four and one-half days from the time we ligatured the left. The operation was attended with no difficulty; the internal jugular vein overlapped the artery to some extent; the descendens noni and par vagum were found in their place. We passed two ligatures beneath the artery, and then tied one of them over a cork applied to the vessel. For convenience, we had him in the sitting posture during the operation; when we tightened the ligature, no disagreeable effects followed; no fainting; no bad feeling about the head; and all the perceptible change was a slight paleness, and a cessation of pulsation in both temporal arteries, and of the hæmorrhage. In the course of the next hour, his pulse increased in frequency from 95 to 140, but soon came down to 110. No difficulty of breathing. The first ligature was cut over the cork and removed, the other tied, and the wound dressed with sutures and adhesive plaster. For the first twenty-four hours our patient remained comfortable, but at the end of that time a hacking cough and difficulty of breathing; pain in his chest and heaviness; pulse 120, rather full, for his reduced state. Took about twelve ounces of blood from his arm, and a few ounces by cupping, with but little if any *present* relief; gave a little of the tincture of belladonna for his cough. I saw my patient four or five hours after; he complained of feeling worse, more difficulty of breathing, and pain; pulse about the same. I put a drop of the *tincture of aconite* into a glass of water, and gave him a teaspoonful. Saw him about four hour after; he expressed himself as feeling better; breathing easier; pulse 110, and less full. I directed him a dose of the aconite whenever he felt a return of the difficulty of breathing, and the belladonna for his cough, and directed him to abstain from all fluids from the first of his difficult breathing. Under the above treatment, the difficulty of breathing subsided; pulse came down in a few days to 80; neither of the wounds healed by first intention, but soon commenced discharging a healthy looking pus.

The ligature from the left carotid came away on the 17th day, and that from the right on the 14th from its application. The wound on the left side continued to discharge for several weeks, when the portion of the artery between the ligature and wound sloughed, and came away in three pieces at different times. The last portion about one inch in length, I have now in my possession. The young man now enjoys comfortable health, and is attending to business. No perceptible pulsation can be felt in either temporal artery.

There are several reasons which make the above case very interesting. It shows the comparative safety with which both carotids can be ligatured, so far as the brain is concerned, and the danger of pulmonary congestion. It shows with what rapidity the anastamosing branches of the opposite vessel supply blood enough to give rise to pulsation in the temporal artery, and of course the danger of hæmorrhage from the unligatured end of the artery, where it is not possible to ligature both ends of the wounded vessel.

3.—*Gonorrhœa and Syphilis with reference to forensic Medicine and Therapeutics.* By Dr. Cormack. (From the London Lancet, September, 1845.)—Dr. Cormack's object is not to give minutely and formally the diagnostic signs and treatment of those numerous affections which may be *suspected* to be of venereal origin. He speaks only of some of those classes of cases, regarding the origin or appropriate management of which we are apt to be craved earnestly for prompt judgment. The subject is considered under the following heads :—

1. Gonorrhœa.

1. Purulent discharges from the genitals of female children.
2. Ditto, ditto, from the genitals of female adults.
3. Ditto, ditto, from the urethra of young boys.
4. Ditto, ditto, from the urethra of men.

2. Syphilis.

1. Condylomatous excrescences.
2. Syphilitic skin diseases.
3. Congenital syphilis.
4. Prophylaxis of syphilis.

The first part of the memoir concludes with the following resumé :—

"1. That a variety of causes, constitutional and local, may (either singly, or in conjunction with one another) give rise to discharges which cannot be distinguished from those occasioned by impure coitus, to which the name of gonorrhœa is commonly applied.

"2. That these affections ought to be treated as inflammatory catarrhal affections of the genito-urinary mucous membrane ; but that when they depend upon the scrofulous taint, the diet, regimen, and medicinal substances, known to prove constitutionally beneficial in such circumstances, should be chiefly relied on, along with mild local astringents."

Dr. Cormack treats at considerable length, of condyloma, which he regards as a distinct and primary venereal disease. He concludes an interesting argument on this subject, with the following query :—

"Is it not likely, then, that condylomatous disease originates in those who neglect to remove by ablution the fluids ejected during the venereal excitement of masturbation or sexual congress ?"

Syphilitic Skin Diseases.—Their differential diagnosis from other cutaneous affections is succinctly and clearly set forth. Their treatment is thus very briefly treated of:—

"The diet must be carefully regulated, and the state of the bowels, skin, and kidneys, made matter of special attention. All this must be done by the simplest means which can be devised; for the system is always in a more or less irritable state, and exceedingly abhorrent of most drugs, and of all, except in very moderate quantity. There are few uncomplicated cases which cannot be cured by the warm bath every second, third, or fourth night, and the internal use of corrosive sublimate, or arsenic, or both, at alternate periods. Mischief in place of benefit will result in giving them in large doses. In bad cases it is always of advantage to begin the treatment by taking from three to six ounces of blood from the arm. In obstinate cases this may be repeated several times with great benefit, at intervals of eight or ten days. Hydriodate of potash is sometimes more useful than either of the remedies mentioned. It was first employed in secondary syphilis by Brera, in 1821, and his example has since been followed by very many, among whom deserve special notice, Ricord, Biett, Baumés, Wallace, Sperini, (of Turin,) Schutzemberger, (of Strasburg,) and Guértine, (of Antwerp.) In the hands of these physicians it seems to have cured every form of secondary venereal disease. The doses and method in which it is recommended to give corrosive sublimate and hydriodate of potash have already been mentioned at p. 33. Of the arsenious acid, I commonly prescribe one-twentieth or one-twenty-fourth of a grain, to be taken four times a day or oftener, in a large draught of water, or other simple vehicle.

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The questions in connection with *Congenital Syphilis* which come before us as practitioners and medical jurists are:

"*a.* At what age does it appear?

"*b.* In what form does it appear?

"*c.* Can it be communicated by an infant to its nurse?

"*d.* What treatment ought to be adopted before birth?

"*e.* What treatment ought to be adopted after birth?

"*a. At what age does it appear?*—Though the syphilitic taint of either parent is a common cause of early abortion, yet the lesions found on dissection are insufficient, in the present state of intra-uterine pathology, to give clear testimony *per se*, as to the real origin of the mischief. Another source of great difficulty is this—that syphilitic infants, in the vast majority of cases, are born in an apparently healthy state, and appear to be doing well, till all at once, the dreadful taint declares itself by indubitable tokens.

"Cliet, recording his experience of infantile diseases in the wards devoted to foundlings at Lyons, to which from 16,000 to 17,000 are annually admitted, remarks, 'that it is one of the diseases which most commonly afflict abandoned children, and that it is all the more appalling and dangerous, that it is extremely rare for it to manifest itself at birth by evident signs.'

"Christofori, physician to the Foundling Hospital of Bologna, states as the result of his observations, that in whatever way syphilis be communicated to infants, it most commonly manifests itself between the ages of one and three months; in very few individuals has he seen it appear later; in almost none under a month; and in no instance was a newly-born infant admitted with the disease unquestionably developed.

"The testimony of good observers, possessed of so large and excellent a field as that of the physicians just quoted, corrects the error into which many have fallen, of supposing that infants tainted with syphilis are usually born with the unequivocal marks of it upon them.

"It is important to remember, that a variety of appearances met with in infants, are often without any rule, pronounced by practitioners to be venereal, such as purulent discharges and cutaneous diseases. Those who may wish to balance for themselves the value of the opinions of authors on this subject, ought to bear this in mind. Interesting though this subject be, limited space prevents me from entering upon the further examination of it in this place.

"*b. In what form does it appear?*—Syphilitic infants generally suffer from sore throat and hoarseness. Pustules are the most common manifestations of congenital syphilis. Their most frequent situation is in and around the mouth, upon the genital organs, buttocks, and upper part of the thighs. They soon ulcerate, coalesce, form fissures, and yield an offensive discharge. They may be mistaken for congenital small pox, of which a good number of cases have been recorded. There is an instance of this kind recorded by Jenner, and in Guy's Hospital Museum I have seen, well preserved in alcohol, an infant covered with small pox. Similar cases have been recorded by Sydenham, Mauriceau, and many others.

"The same characters which point out an eruption in the adult to be syphilitic, will do this also in the youngest infant.

"*c. Can it be communicated by an infant to its nurse?*

Dr. Cormack answers this question in the affirmative, and cites cases.

"*d. What treatment ought to be adopted before birth?*—Dr. Tuhrmann's case shows, in a most remarkable manner, the advantage which may be derived from administering mercury to the mother during her gestation. When the fœtus is deeply tainted with syphilis, abortion is less likely to be caused by salivation than by refraining from it. In support of this statement, many authors of the highest practical authority might be quoted: Vanoni, for example, recommends mercurials; and Dr. William Campbell cites cases from his practice in which a similar treatment proved of signal benefit.

"*e. What treatment ought to be adopted after birth?*—No remedy can supersede mercury. It may be given in various ways with nearly equal advantage. I do not understand the grounds upon which some prefer inunction to all other methods. When I have wished to bring infants of a month or younger under the influence of mercury, I have of late directed one grain and a half of calomel, and twelve of prepared chalk, to be mixed together, and then divided into twelve powders, one of which is to be administered every third hour. If the gums are hot and painful at the end of this time, the medicine is suspended for twelve or twenty-four hours, to be resumed at similar intervals till the disease is cured. In addition to the internal use of some preparation of mercury, we must not neglect the frequent employment of the warm bath, and the application of soothing lotions, liniments, or cerates, to the sores, if such exist.

"*Prophylaxis of gonorrhœa, syphilis, and condyloma.*—The means adopted to guard against venereal contamination *in coitu* are various, and some of them are so well known as not to require notice. Those which are most to be trusted to are lotions for removing or decomposing the poison before sufficient time has elapsed to admit of its being absorbed. Thorough ablution with water may be sufficient, but chlorinated washes are justly mostly relied on.

"Ricord has correctly observed, that absorption of the poison does not take place in the male till after ejaculation, on account of the turgid state of the organ up to that time. In the male, a simple and tolerably efficacious preventive measure is said to consist in making water immediately; by doing so, any matter which has lodged in the meatus, at the root of the glans, or on the inside of the prepuce, will be dislodged.

"From the great extent of the exposed surface in females, and the difficulty of applying lotions effectively to the upper part of the vagina, they are not likely to prove so successful as in males.

"It is hardly possible to bring an injection into contact with the upper part of the vagina and neck of the uterus, if the patient, as is commonly the case, be seated upright in a bidet, or in some other nearly vertical position. If the fluid reach these remote regions at all, it can only remain in partial contact with them for an instant; but the chances are that it will not reach them, as is shown by some experiments which Ricord made to elucidate this point. With the assistance of the speculum he placed pieces of dry charpie on the neck of the womb, withdrew the speculum, and threw up a colored injection, the patient being in a vertical position. In each trial the charpie was withdrawn untouched by the fluid. The proper position, then, for a woman to be in when a vaginal injection is being used, is lying, with the pelvis raised, so that the upper part of the vagina be its most dependent point.

"It is said that a nostrum, called the prophylactic soap of Dr. Pfeiffer, has been made the subject of successful experiment in the Venereal Hospital of St. Petersburg. Five hundred parts of this compound were found to contain six parts of corrosive sublimate, four of tannin, and forty-five of chloride of lime; besides the saponaceous substance, there were discovered, in addition to those mentioned above, a variety of other ingredients, all of which, however, seemed to be unimportant.

"If it be not beneath the dignity or beyond the province of the physician to cure venereal disorders, it is not improper for him to search out and promulgate means for their prevention and extinction. It must be remembered, that they are diseases which not only afflict those who contract them, but which descend as blights and cankers from generation to generation, being the prolific sources of suffering and death at all periods of existence, but especially during infancy and early life."

4.—*Important Cases of Poisoning by Arsenic.* (From the *British American Journal of Medical and Physical Sciences.*)—We have been favored by a friend with four numbers of the *Carlisle Journal*, containing the minute details of a very important investigation by W. Carrick, Esq., coroner, on view of the body of Mr. John Graham, yeoman, of Grimsdale, whose death was supposed to have been the result of the administration of poison. Deceased was a man well known in that part of the county in which he resided, as an intelligent, industrious, amiable, and highly respectable farmer. Suspicion pointed to the son as the author of the unnatural crime, who was not only Mr. Graham's heir, but also a farmer standing high in his profession for his intelligence and success in many departments of agriculture. During the progress of the investigation, which was adjourned to several sittings, and which appears to have been conducted in a manner highly creditable to the coroner, rumors got abroad that suspicious circumstances attended the decease of the son's wife, an event which occurred in the month of November of last year. Shortly after her death, suspicions as to foul play extensively prevailed, but they were speedily lulled, nor were they revived until the death of his father, under precisely similar symptoms, enhanced, too, after the development of similar symptoms, though not terminating fatally, in six other individuals, all of whom partook of the same *cake*, at different intervals, and which appears to have contained the poison. The evidence criminating the son in the death of the father,, appears to have been by no means conclusive; it amounted to no more than his having been *near a place in which stood a pot of yeast*, and which evidently had been employed in the manufacture of the cake. But this circumstance, coupled with the singularity of his demeanor during his father's illness, and after the fatal issue, and the undoubted fact of his having been the guilty agent in his own wife's destruction, points to his active agency in this case also, with a probability, amounting almost to a certainty. The verdict of the jury, in the case of John Graham, the father, put the mildest construction possible on the occurrence, recording it as their opinion—"that the deceased died from the effect of poison wilfully administered to him; and they record their verdict of wilful murder against some person or persons unknown."

"The case of the wife is particularly interesting in a medico-legal point of view, presenting another instance of the ready detection of arsenic in the human body, after months of internment, and the decidedly preservative powers against rapid decomposition of the animal tissues which that substance possesses. The evidence criminating the husband in this case was most conclusive, and the jury unanimously returned their verdict in three counts:

1. "That Margaret Graham died from taking arsenic.
2. "That such arsenic was administered by design.
3. "That the person who administered it was John Graham."

In the chemical examinations requisite, and undertaken in both instances, Reinsch's test has been brought prominently forward, and its value amply demonstrated. We subjoin the medico-legal reports of both cases, as possessing great interest, and being very creditable to the parties concerned in this department of the investigation.

Thursday, May 22.—*Post Mortem Examination of John Graham.*

Externally.—Considerable discoloration of the depending parts of the body, and signs of commencing putrefaction.

Internally.—Brain healthy.

Chest, Lungs.—Right one shrunk, and much smaller than the left, apparently from a previous attack of pleurisy. A great number of old adhesions of considerable length.

Left lung healthy, though a few slight adhesions existed there also.

Heart.—Healthy in every respect, and containing a small quantity of blood.

Œsophagus.—Considerable inflammation of the mucous membrane of the left side of the pharynx, or upper part of the gullet, which was of a red color, and became much brighter on exposure to the air. There were also several dark spots caused by blood effused below the mucous membrane, as was seen on removing that membrane. The same appearance continued the whole length of the gullet, though in a much slighter degree.

Stomach.—Its inner surface near the cardiac orifice (or where the gullet joins it) was of a uniform redness, and presented the appearance of a severe inflammation having existed before death. On exposure to the air, the redness became much brighter, and more distinctly marked. The redness was of a triangular form, with its base next the termination of the gullet; it extended along the lesser curvature of the stomach, gradually tapering to a point close to the pylorus, or other extremity of the stomach. On removing the mucous membrane, which covered the inflamed part, morbid redness was visible along with several patches of effused blood. The discoloration along the larger curvature of the stomach was slight, though here several small spots of effused blood were also seen.

Duodenum, or first portion of the small intestine joining the stomach.—Marks inflammation were also here present, with numorous spots of effused blood, about the size of split peas, in the course of the blood vessels, as seen on removing the mucous membrane. The marks of inflammation were seen both in that membrane and in the tissue below on its removal. Rectum slightly vascular, but not more so than might have been expected at his age.

The other abdominal viscera did not present any appearance requiring comment.

CHEMICAL ANALYSIS.

1. An ounce weight of the cake was cut into pieces, and boiled in six ounces of distilled water, with two drachms of muriatic acid. Three small bundles of the finest copper wire were sucessively introduced, and boiled in this liquid. On their removal, the bright colour of the copper was found to have been completely converted into an iron grey.

2. A comparative experiment was then made with the same quantity of muriatic acid, distilled water, and copper wire. After boiling for the same length of time, the copper was removed unchanged.

3. The iron grey wire having been previously carefully washed and wiped dry, was then introduced into a test tube, and heated to a low red heat. A white ring sublimed, and the copper wire lost its iron grey colour. On examining the white ring through the microscope, an abundance of crystals with equilateral triangular facets or surfaces were distinctly seen. About a drachm of distilled water was then introduced into the tube, and boiled till the white ring was dissolved, the copper wire having been previously removed. When cold, the three following tests were applied:—

1. On the addition of the ammoniaco-nitrate of silver in solution, there was a well marked yellow curdy precipitate, which soon became brown.

2. On adding a solution of the ammoniaco-sulphate of copper, a very slight green colour resulted, not very distinct at first, but which, on standing, deposited a well marked green precipitate.

3. On passing a few bubbles of sulphuretted hydrogen gas through the remainder in the tube, a bright yellow was immediately the result.

The liquid in which the cake had been boiled was then filtered, and a stream of sulphuretted hydrogen gas was passed through it. The excess was driven off by boiling, and the liquid filtered. A very copious orange-coloured precipitate which had settled in the bottom of the vessel was carefully collected and dried. A portion of it was then heated in a test tube, with twice its bulk of black flux, which had been previously carefully dried. A beautiful metalic ring was sublimed, brilliant, shining, and with a distinct, dull, granular surface internally.

The Flour.—This was submitted to the same process that the cake had undergone. There was no deposit upon the copper wire, which came out untarnished.

Butter.—The same steps were gone through as with the cake and flour. The copper wire came out untarnished.

Contents of the Stomach.—These were filtered, two drachms of muriatic acid added, and copper wire boiled in the liquid. No change was produced in the copper wire.

Mucus scraped from the Stomach.—This was dried on filtering paper, and put into a test tube with wood charcoal, and heated. No result could be obtained on account of the empyreumatised moisture which obscured the tube.

The Stomach.—This was cut into pieces, and boiled in a gill of distilled water, with half an ounce of muriatic acid. The liquid was then strained, and boiled with half an ounce of acetic acid to deposit the animal matter. After straining, this was again boiled with animal charcoal, (purified and recently incinerated,) for the purpose of clearing the liquid. Copper wire boiled in this became partially coated with grey. On treating this wire in a test tube, a white crystaline ring was sublimed, in which numerous octohedral crystals and triangular facets were distinctly seen.

The Liver.—About one-third, cut into small pieces, was boiled in eight ounces of distilled water, with two ounces and three quarters of muriatic acid. Copper wire was boiled in this. It was partially coated with a grey colour. After washing and drying, it was heated in a test tube. There was a faint white ring sublimed, but no characteristic crystals could be seen under the microscope.

Sixteen ounces weight of the liver was put into an evaporating basin of Berlin porcelain, with two ounces aud a half weight of pure sulphuric acid, previously tested. This was put in a sand bath, where it was allowed to remain till it became carbonized. To the ash six drachms of pure muriatic acid, and the same quantity of pure nitric acid, were added. The whole was then placed in the sand, both evaporated to dryness and incinerated. The ash was powdered and boiled in six ounces of distilled water. One drachm and a half of muriatic acid was then added, and copper wire boiled in it. On its removal, it was of an iron grey colour, with here and there the copper tinge. On heating the wire to a low red heat in a test tube, a white ring was slowly sublimed. On examining this in the microscope, the octohedral crystals and triangular facets were distinctly seen.

The Blood.—To four oz. of this, half an ounce of muriatic acid was added. Copper wire boiled in this became of grey colour, and on heating it to a low red heat in a small test tube, a white crystaline ring was observed under the microscope. This showed the octohedral crystals, and triangular facets, with remarkable distinctness.

REPORT.

From the well marked appearances of acute inflammation in the pharynx, stomach and duodenum, and from the circumstance of arsenic having been detected in the substance of the stomach, the liver, and the blood, we are of opinion that the death of the deceased was caused by taking arsenic.

Thomas Elliot, Surgeon.
Richard James, M.D.

Friday, June 6, 1844—*Post Mortem Examination of* Mrs. Margaret Graham, of Kirk Andrews, *who died on November 27th*, 1844.

Grave deep—soil dry—coffin made of oak, and quite perfect. The nails not rusted, and the inscription on the plate very little erased. The words were Margaret Graham, aged 45 years, 1844."

The winding-sheet was marked with the initials M. H., with the figure *i* below, and was damped with a yellowish fluid. It was also completely covered with mould, which presented the appearance of fine cotton wool spread over it. On removing this substance from the face, the latter appeared of a yellowish brown colour, and the skin almost of the consistance of shoe-sole leather, except over the gristly part of the nose, where it was soft. The upper lip was shrunk, and the teeth projected beyond those of the lower jaw. The eyelids were soft, moist, and much depressed; the eyeballs collapsed, but their different textures quite distinct. The hair was of a grey colour, long and very easily pulled out. The integuments of the chest were of a dullish green colour in front, and the cuticle peeled off with the slightest rubbing. The integuments of the belly presented the same appearance,—they were soft and elastic.

INTERNAL EXAMINATION.

The Head.—On removing the upper of the skull, a large quantity of air was found distending the outer membrane covering the brain. The different membranes presented nothing particular in appearance. The brain itself was very soft, but its component parts could be easily distinguished by their difference in colour. There was no appearance of any disease discovered.

Chest—Lungs.—Their surface was of a pinkish grey colour, and healthy mottled appearance. Air was effused here and there between the lungs and pleura, or membranes covering them. The lungs crepitated under pressure, floated in water, and after being well squeezed, could not be made to sink. They were healthy internally, and free from adhesions externally. About a pint of bloody serum was removed from the two cavities of the chest.

Heart.—There was no fluid in the pericardium, the interior of which was of a redder colour than natural. The interiors of the two cavities of the right side of the heart were of a deeper red colour than natural, and contained a small quantity of fluid and clotted blood. The remaining two cavities of the heart were empty, and presented a healthy appearance.

Alimentary Canal.—The soft palate in a great measure deficient.

The Gullet.—Its internal surface was much redder than natural, with a number of small transparent gritty particles adhering to it.

About two inches from the stomach, we found a transparent crystal, about the size of raisin stone, firmly adhering.

The Stomach..—The end where the gullet joins was of a dull, dark, red colour externally, where in contiguity with the spleen. The other end of the stomach presented the same red appearance. There was slight venous congestion along the small curvature.

Internally.—The contents of the stomach, (not more than three or four tablespoonfuls of fluid,) were collected. The internal surface presented great redness, corresponding in situation to that seen externally; numerous gritty particles, some yellowish, others transparent, were seen in the mucus, and firmly adhering to the lining membrane of the stomach. On the posterior surface, a small yellow spot was seen, about the size of a split pea at a short distance from where the small bowel arises.

The small and large intestines were found nearly empty. The gritty particles, such as had been noticed in the gullet and the stomach, were seen in great numbers along their whole length in the mucus, and adhering to the lining membrane. At some places the bowels were much reddened, with here and there patches of a greenish yellow colour.

The remaining viscera presented no appearance requiring special notice.

It is worthy of remark that the whole of the body, with the exception of the brain, was in a wonderful state of preservation.

CHEMICAL ANALYSIS.

The Contents of the Stomach.—On careful analysis, no traces of any poison were obtained.

Mucus of the Stomach.—A portion of this was carefully scraped off, washed in distilled water, and allowed to settle. A crystaline sediment was deposited, carefully dried, and heated with black flux, a characteristic metallic ring sublimed, bright and shining externally, dull and granular internally.

The Stomach—Was cut in small pieces, and boiled with an ounce of water, and half an ounce of muriatic acid. Several bundles of copper wire were successively introduced, and on their removal presented an iron grey appearance. On heating one of these bundles in a test tube, a white ring very slowly sublimed, which appeared distinctly crystalline to the naked eye. Under the microscope it showed octohedrons, with equilateral triangular facets, with remarkable distinctness.

The Intestines.—These were cut in pieces, and carefully washed in distilled water. This water, on being allowed to stand, deposited a crystalline sediment, which was carefully removed, washed, and dried. This was then heated in a test tube with black flux, newly dried at a red heat; a broad characteristic metallic ring was then sublimed. The part of the tube containing this ring was cut off and enclosed in another tube. On the application of heat, the metallic ring sublimed in the form of a white crystalline ring. A pocket magnifying glass distinctly showed the octohedral crystals, and triangular facets. A few drops of distilled water were boiled in the tube containing these crystals, and divided into three parts. The first of them gave a yellow curdy precipitate, with a solution of the ammoniaco-nitrate of silver. The second gave a green precipitate, with a solution of the ammoniaco-sulphate of copper. The third was exposed to a stream of sulphuretted hydrogen gas, and gave a beautiful yellow precipitate.

REPORT:

From the circumstance of our having detected arsenic in the gullet, stomach, and bowels, in considerable quantity; from the remarkable state of preservation in which we found the body, and from the appearances observed on dissection, we are of opinion that the deceased had taken arsenic in sufficient quantity to cause death.

THOMAS ELLIOT, Surgeon.
RICHARD JAMES, M. D.
GEORGE SINNISWOOD, M. D.
JOSEPH CARTMELL, M. D.

Part Fourth.

MEDICAL INTELLIGENCE.

(FOREIGN.)

1.—*Egyptian Opthalmia.*—The distinguished Surgeon Clot-Bey makes some interesting remarks on the cause of this malady. He regards it as an endemic in that country, "where it assumes characters unknown in other countries. Common throughout Egypt, it is more so in the northern parts than near the equator, more frequent in town, than in the country, and more so in cultivated regions than in the deserts; it spares no class or temperament, and one attack produces no exemption. Animals, as well as men, are subject to this disease; dogs, cats, horses, asses, cows, camels, and all quadrupeds are frequently attacked by it; and though it is neither so common nor so intense in them as in man, it is not uncommon to see animals with spots on the eyes, and even with an eye lost.

Opthalmia prevails at all seasons, but it is more frequent in the hot season.

Much has been written respecting the causes of Egyptian opthalmia. Some have attributed it to the intensity of the light, to the reflection of it from a sandy soil, or from the white-washed houses; others to fine dust, carried by the wind and lodged on the membranes of the eye; others to the presence, in the atmosphere, of irritating saline particles, as of soda, nitre, chloride of sodium, &c., others have regarded it as due to the action of the sirocco (*khamsin*). But these explanations are insufficient. If the disease was produced by the action of the sun's rays; why is it so rare in the localities where these rays are the most intense, in upper Egypt, and Nubia, for example? If the disease was produced by dust or sand, why is it unknown in the desert? If by saline particles in the air, why are not those most frequently attacked by it, who work in nitrous soils, or among rocks abounding in saltpetre. This opthalmia, then, is produced by some other, than the alleged causes. These are causes probably metoorological, of climate, or other which have hitherto evaded our investigations."—*Clot-Bey, Apercu general sur l'Egypte.*

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2. *Case of Continued Priapism.* By John W. Tripe, Esq., Surgeon.—R. W——, aged twenty-six, a seaman, of stout make and florid complexion, stated, that he arrived from Calcutta about the 12th of April, 1844, and had lodged with a female ever since. On Friday night, the 26th of the same month, he experienced an unusually fierce desire, with intense erection of the penis, which latter lasted throughout the night, with but little mitigation, being attended with pain of the left side of the organ, and an inclination towards the same side. The symptoms continuing, although congress was frequently

resorted to, induced him to send for medical aid about 2 P. M. of the day following, when I found him in the condition above described.

On examination, the corpus spongiosum was found moderately turgid, especially the gland, which was situated near to the anterior superior spinous process of the ilium, and almost touching the abdominal parietes ; the corpora cavernosa were fully distended and firm, scarcely yielding to pressure, and without any perceptible difference, either in color or firmness, at the part from which the inclination towards the left side commenced. When its restoration towards the median line was attempted, the pain was much increased. Beyond these, he did not experience any uneasy sensation, nor was his health in any way deranged, the skin being rather moist, the tongue clean and moist, the pulse 75, full and soft, and the bowels open.

Let a cold lotion be applied, and let him take a quarter of a grain of tartar emetic, with one grain of powdered opium, every fourth hour ; and three grains of calomel, with six of compound extract of colocynth, immediately.

April 28.—Slightly improved, the pain and inclination towards the left side being lessened, and also the distention of the corpus spongiosum. Repeat the medicines. Nine, P. M.—The pain is slightly increased. Let six leeches be applied to the part.

29.—Decidedly better. The pain is much alleviated, and the corpora cavernosa rather flaccid. The organ has assumed the median line, and forms nearly a right angle with the abdomen. Repeat all the medicines.

30.—Much worse. The condition of the parts resembles that first described, with the exception of the inclination to the left. On close questioning, he acknowledges having had frequent communication since the 26th, and with the usual results, so that I advised his removal to the London Hospital. The report during the time he remained in the hospital was furnished by the senior dresser :—

R. W——, admitted for priapism, under the care of Mr. Luke, on the 30th of April, 1844. The corpora cavernosa are very much distended, but not the corpus spongiosum, or glans ; the left crus of the penis is very firm.

Let him be bled to sixteen ounces, and twenty leeches be applied to the perineum. Apply a lotion of spirit in lime water, and give one scruple of calomel and rhubarb.

The pain was relieved by these means, but the tension of the parts was not at all diminished.

May 1st.—About the same.

Let him have some house medicine, and saline antimonial mixture three times a day.

2d.—Not at all relieved, the disease being as intense as ever.

Let him have two grains of calomel every third hour.

5th.—The mercury has produced salivation, therefore it is discontinued ; the organ is not so tense ; the angle which it forms with the abdomen approaching nearer to a right angle.

10th.—Left the hospital of his own accord ; in fact, much against the wishes of Mr. Luke. The penis is more flaccid, and forms a right, instead of an acute angle with the pubis.

After leaving the hospital, he returned to his former abode, having free intercourse with the same female, until he left England for Sydney. During this time he had erections, and complete communication ; the former proceeding to the usual extent, and afterwards gradually subsiding to the state in which the organ was previously to the venereal orgasm.

On leaving England, (May 18th,) his condition was as follows :—Corpus spongiosum flaccid, corpora cavernosa moderately tense, and forming only an angle of about 45° with the pubis.

By letter, dated September, the condition last described remained without alteration for more than three months after he left England, but after that became rapidly palliated, and is now quite removed. On his return home he was perfectly cured, and without any ill results from his accident.

The remarks required by a case of this kind are but few. The lesion appears to have been caused by effusion of blood into the cells of the corpora cavernosa, which remained in a semi-fluid condition. The time (four months) during which it continued, is very remarkable, whilst the perfect recovery of the patient eventually, seems to show that any instrumental mode of cure should not be attempted, unless undoubted signs of gangrene appear. In a case treated by Mr. Calloway, and published in the *Medical Repository*, for April, 1824, venesection, leeching, the warm bath, tobacco enemas, tartar emetic, and nitre, were exhibited, and camphorated mercurial ointment was rubbed into the part, but without any benefit; wherefore, on the sixteenth day after the erection occurred, the left crus of the penis was punctured with a lancet, and a large quantity of dark grumous blood let out. By pressing the part, both corpora cavernosa were emptied through the aperture, which was followed for a few days by the escape both of pus and blood. The patient recovered, but never regained the power of erection, and therefore remained impotent for the remainder of his life, forming a marked contrast to the success of the foregoing case. In Mr. C's case, it occurred during, and was not diminished by repeated connexion. *London Lancet.*

Commercial-road, *March* 19, 1845.

3—*Phthisis Pulmonalis in marshy localities.*—Much has of late been said, particularly by the French and Italian physicians, of the influence of marshy localities in controlling and curing phthisis pulmonalis. An array of facts, in favor of the curability of phthisis by a residence in swampy regions, has been recently put forth by Dr. Boudin, who displays much zeal and great learning on this subject. The English and American physicians, always cautious in advocating new medical doctrines, have not as yet given in their adhesion to this system of therapeutics, partly because a sufficient number of observations has not yet been collected to establish these new views; and partly for want of such statistics as would be required to confirm a fact of immense importance to all mankind.

The observations which we shall here introduce, are interesting, not so much because they seem to strengthen the views of those who advocate the curability of phthisis by sojourning in low marshy districts, but chiefly as illustrative of the fact that large cavities in the lungs, attended with hectic and its concomitants, may *cicatrize* and ultimately heal without much aid from internal medication.

M. de Crozant has published the four following cases of phthisis pulmonalis cured by a residence in the marshes of Nièven. The two first were communicated to him by Doctor Lizen. We extract them from the paper published by Dr. Boudin in the 65th number of the *Annales D'hygiene Publique*, for 1845.

Observation I.—On the 13th July, 1829, I was requested to see J. C. Meurier, aged 27 years, a robust man, for some time a resident of Sully-Latour, in a mill, situated on the left bank of the Noain. Nothing was known of his ancestors. This man informed me that he had never been seriously ill; but had been subject to a dry cough, which produced some apprehension, but had never forced him to abandon his labors. He had been bed-ridden for two days, and complained of great difficulty of breathing, without much expectoration. At the anterior and superior part of the left lung, there was flatness on percussion. By auscultation, the respiratory murmur appeared to me feeble at this point. As he had fever, and as the pulse was strong and developed, I bled him and prescribed repose, a low diet and mild diluents. I saw this man again on the 29th of August, a month and a half afterwards; he then had diarrhœa, was emaciated, and troubled with night sweats. The fever had sensibly diminished: little appetite: troublesome cough: abundant expectoration; the sputa were *purulent*, of a bad taste to patient, and sometimes mixed with blood.

By auscultation, I discovered "*du souffle*" and a very evident pectoriloquy over the spot. I was in no doubt as to the nature of the case, and I pronounced an unfavorable prognosis. I prescribed Martin's pills, gum and moss with syrup of tolu. From the 29th of August to the 3d of October, I saw the patient five times, and beyond my expectations, he was much better, although the signs furnished by auscultation were nearly the same; he regained his appetite, expectorated much less, but had constant cough; yet his digestion was good. The reporter lost sight of his patient until the 24 June, 1830, at which time he was astonished to receive a visit from his quondam patient, who assured the doctor that he was perfectly well, and had resumed his usual labors. He now auscultated the man's chest, and recognized the existence of a cavity by the persistence of the "*bruit de souffle;*" yet he had ceased to cough. This man was not seen again until three years afterwards, when the Doctor again examined his chest, and was delighted to find the cavity quite obliterated.

Observation II.—On the 13th April, 1839, the same physician was requested to visit C. P. Buffière, in the commune of Sully-Latour, to examine his daughter aged 19 years, of delicate frame and feeble constitution, sick for some time. For one year, she had not been regular—she coughed and expectorated much, and when seen by the physician, she had sweats—diarrhœa—considerable cough, and an abundant expectoration of purulent and fœted sputa—constant fever with loss of appetite. On practising auscultation, pectoriloquy was distinctly recognized, and all the signs of a large cavity occupying the superior and anterior part of the left lung, were revealed. The parents were informed that their daughter was phthisical, and a suitable regimen with some balsamic pills were prescribed. On the 27th of the same month, the patient was without change. She was lost sight of and was thought to be dead. Fifteen months after this time, Dr. Lizen met his patient in the forests, watching a flock of sheep. She had perfectly recovered her health.

Observation III.—In the month of August, 1839, Dr. de Crozant was desired to examine a female domestic, living in a retired village in the commune of Sully. He found his patient pale and emaciated, and complaining of a fever which had confined her since early spring. The fever was quotidian, and had hitherto resisted all the preparations of quinine. She informed Dr. de C. that her sputa was abundant, and that during the winter she was harrassed with a dry cough; at this time the expectoration was abundant. The sputa was sometimes mixed with blood. She complained of oppression, palpitation, abundant sweats at night, loss of appetite, and occasional diarrhœa. All the physiological signs led Dr. de Crozant to pronounce this case phthisical, and further to declare to her friends that if the case continued to grow worse with equal rapidity, death would soon take place. She passed the winter without change, and in the spring of 1840, she had fever, coughs, sweats, and the usual attendants. Emaciation and great atrophy of the tissues, with debility, continued up to August, when Dr. de C. saw her for the second time. He was now enabled to discover the existence of an enormous cavity in a state of suppuration, in the summit of the right lung.

There were also gargouillement, soufle, and other physical signs of tubercular excavations. Throughout the chest there were mucous and sub-crepitant râles here and there; the summit of the right lung was the seat of the same râles, but nothing else. The diarrhœa became harrassing—constant fever—and irregular chills increased the complication. For two months, Dr. de C. saw the patient almost daily, in the country; but adopted no regular course of treatment, convinced of the inutility of medicine in this case. At this time, the patient left the village and went into the country and took lodgings in a country house, seated near the borders of a stream, and surrounded by swampy grounds and unhealthy pools—long famous for the prevalence of marsh fever. In May, 1841, she was in the last stage of marasmus and consumption. In the left lung, the souffle persisted with the same force. No gargouillement—the râles had disappeared throughout the chest, except under the right clavicle. The sputa were always purulent, but hard and round. At this period, she was affected with frequent syncope, during which she was expected to breath her last. She rallied from this state, and was attacked with fever, followed by an erythematous eruption, which occupied nearly the entire surface.

She passed the winter tolerably well; and in the spring of 1842 she recovered considerable strength—in the summer, she was greatly improved, so much so, that Dr. de C. was rather disposed to question the correctness of his diagnosis than believe a cure had been effected under such unfavorable circumstances. He now auscülted her chest, and found on the left side the *bruit de souffle*, as formerly stated; without any accompanying râle, even under the right clavicle; more fever, sweats, good appetite, rarely diarrhœa; no bloody sputa for six months. In 1843, Dr. de C. again saw his patient, when she enjoyed perfect health; neither cough nor expectoration; able to engage in laborious duties. For a year past she has been perfectly well, with the exception of an occasional attack of intermittent fever in the month of March, easily cut short, however, by quinine. She continued to enjoy excellent health, whilst residing in the same place; all the signs, both physical and physiological, having passed away.

Observation IV—was a young man, aged 18 years, residing at Moulin-Neuf, upon the Noain, of phthisical parents, light complexion,and weakly constitution. He had spit much blood—had night sweats—sometimes obstinate diarrhœa—cough—and thick sputa—and fever in the afternoon. He had been confined to bed for three weeks, and when seen by Dr. de C., he was in an extreme state of emaciation. His strength was gone—his voice feeble and changed—loss of appetite, &c. Bad cough—purulent expectoration, floating in a large quantity of bronchial mucus. On the left, some crackling under the clavicle—a little flatness behind—a souffle without any râle; the respiration was loud in the rest of the lung. On the right side, in front, under the clavicle, distinct cavernous *souffle* with *gargouillement*, and a very flat sound; the rest of the lung seemed sound. Dr. de C. visited this patient four or five times during the two months he remained at Sully-Latour. The only medicine prescribed was an emetic. He left the patient to die, as he conceived; and the following year, 1842, Dr. C. was informed that his patient was on his feet, without cough, without expectoration, and enabled to labour.

In 1843, he saw this young man, and was gratified to find him vigorous, active, and bearing on his exterior the evidences of internal strength. He auscultated his chest, and discovered only in the anterior and superior part of the right side, the respiratory murmur a little intense, with normal sounds throughout the rest of the thoracic cavity. A. H.

4—*Caustic Injections in the treatment of chronic irritation of the bladder.* By Dr. Debeney. (*Journ. des Connaiss & Med. et Chirurg, for April,* 1845.)

Analogy first led M. Debeney to try caustic injections for chronic irritation, or catarrh of the bladder. This mode of reasoning has led to some of the most useful discoveries in our therapeutics, and is, moreover, the most certain method of attaining to correct knowledge on many obscure points in the science of medicine. The argument here used, says M. D., is extremely simple; he first asks if the vitality of the mucous membranes is identical? and is not the membrane which lines the bladder a mucous tissue? Both these questions being answered in the affirmative, with the exception of certain parts destined to special functions, as the alimentary canal, M. D. then proceeds to detail four or five interesting observations, in which the best effects were obtained by throwing caustic injections into the bladder. Many of the cases cited, in addition to obstinate catarrh of the bladder, were also complicated with strictures, gonorrhœa, and seminal losses.

The injection used is composed of *four grammes* of crystalized nitrate of silver dissolved in *thirty grammes* of water. His mode of using the injection we shall give in his own language. "Having forced into the urethra, by means of a glass penis syringe, as much of the caustic fluid as possible, I close the meatus by pressing the extremity of the gland between the thumb and index finger of the left hand; then by a gradual pressure toward the root of the penis with the two first fingers of the right hand, I push the liquid so far that not a drop escapes when the pressure is suspended. It is evident that the injection must pass into the bladder, else where could it go. The pain was considerable, but quite supportable; the usual phenomena of cauterization well manifest, and at night there was slight accession of fever. Suffice it to say, that in a few days, the irritability of the urethra and bladder were allayed; less pain in passing urine, also retained much longer without inconvenience. Two or three injections were generally adequate to effect a permanent cure." Our author seems to repose unlimited confidence in the remedy above proposed, and we see no reason why it should not receive a fair trial. It must be confessed that, heretofore, chronic cystitis, irritable bladder, and all forms of cystic disease have not been generally amenable, even to the most judicious medication; let us, then, try a mode of practice, which comes highly recommended both by analogy and experience.

.5—*Of the Urine and its relations to practical medicine.*—(*Bulletin General De Therap.*)—For some years the laborious researches of a number of physisicians, and particularly of M. M. Solén, Donné and Becquerel, have directed the attention of practitioners to the nature and the different qualities of the urine as excreted in various pathological circumstances. The works of certain toxicologists, by demonstrating that poisonous substances, when introduced into the *prima seceernda* by means of absorption, are for the most part eliminated from the animal organism by the secretion of the kidneys, have, on the other hand, given a new interest to the examination of this fluid, when viewed in its relations to therapeutics. It was on considerations of this kind, that Mr. Berzelius was induced to put forth his late valuable work on the subject under review.

In the first part of his work, the author, after examining the structure of the renal apparatus, and tracing the chemical history of the urine, makes known the passage of certain bodies through this liquid. Thus, after the free application of mercurial ointment, (to the surface,) the salts of mercury are detected in the urine. This can be verified by desiccating the sediments which it deposits in standing, then submit it to calcination, and we have globules of mercury.

The nitrate of potash, the yellow cyanide of potassium, and a great variety of other salts, thus pass into this liquid; among the latter, the salts of iron should be comprehended. After the exhibition of a large quantity of iron, we sometimes find the urine presents a light bluish or greenish tint, produced, according to Berzelius, by the combination of the iron with the ferro-cyanic acid, which would itself result from the decomposition of the different animal matters in the living organism.

After the injestion of tartaric and oxalic acids, the urine deposits, "*par le refroidissement*," the tartrate and oxalate of lime, which may be increased by adding to the urine, the chloride of calcium.

Malic, citric and tartaric acids, communicate to the urine acid properties. The active principles in the infusion of the nut gall passes also into the urine, for a persalt of iron in solution produces a black precipitate.

When succinic acid is swallowed, we find it in the urine; but the same is not the case with benzoic acid, which, according to the observations of Woehler, Boye and Leaming, is tranformed into hippuric acid.

After the administration of iodine, the urine holds in solution the iodides of potassium and ammonia.

We likewise find the alkaline carbonates, the borates, the silicates and the chlorates in this fluid: it is the same in regard to the yellow cyanuret of potassium; but the red cyanuret is transformed into the yellow cyanuret: the sulphuret of potassium is but partially decomposed; the remainder is oxydized during its sojourn in the torrent of the circulation and is eliminated in the form of sulphate of potassa.

The vegetable salts, with potassa and soda for their bases, are transformed into the carbonates; in fine, they render the urine alkaline, and impart to it the property of determining effervescence by the addition of an acid. We observe a physiological phenomenon precisely similar after the free use of certain fruits, such as apples, &c., which contain, as chemistry demonstrates, the citrate of potassa, or the malate of this base; this peculiarity justifies the employment of these fruits against calculus, composed of uric acid, and explains, at the same time, the excellent results which are obtained from this kind of medication in similar cases. Many coloring and aromatic substances likewise pass into the urine, without undergoing any alteration. Those substances which experience more or less change in passing through the kidneys, are alcohol, ether, camphor, the pyrogenic animal oils, musk, the coloring principles of cochineal, turnsol, the salts of bismuth and of lead, and, also, contrary to the facts observed by B. Jones, the mineral acids which, according to the author, never communicate acidity to the urine. M. Berzelius concludes his work by a chemical examination of the urine in a pathological state, laying down propositions as well in regard to the best mode of treatment to be pursued in some of these states, as well the course to be pursued in an analytical examination of the urine.

The new researches of the learned Swedish chemist, in making better known than formerly, the action of those medical substances which the organism eliminates without alteration by means of renal secretion, and those which are modified in the living economy, so as no longer to be recognised in the *urine*, adds more precise notions to the mass of knowledge already acquired upon the mode in which pharmaceutical agents act in the very substance of our organs, and will contribute doubtless, to enlighten us as to the best medicines to be employed in the various diseases of the kidneys.

6—*Difference of health in Town and Country.*—We take the following interesting extract from a notice of the Report of the "Health of Towns Commission," by Mr. J. R. Martin, a member of the committee—in the July number of the Medico-Chirurgical Review.

The difference in the amount of mortality in the agricultural, compared with that in the manufacturing districts and towns in England is most striking. The ratio ranges from one death in 53 to one death in 29 of the inhabitants annually, a deplorable example and proof of the difference in the physical condition and comfort of the respective classes of our laboring population. It is reckoned moreover that, out of every 1000 births, 221 only die under five years of age, in our agricultural districts; while not fewer than 385 die annually, under the same period of life, in all of our closely-built towns. This is a sad reflection; and yet no one can doubt for a moment but that the amount of mortality among our artisans might be most sensibly diminished by the introduction of appropriate sanatory regulations. When we remember that in Birmingham there are at least 33,000, in Manchester 83,000, and in Liverpool no fewer than 100,000, human beings compressed within the compass of a single square mile, and a vast proportion too of these inhabitants living in a squallor, wretchedness, and vice, how can we be suprised at the high rate of mortality in such places, more especially in the last named town, our great western sea-port? Read what Mr. Martin says on the subject:

"Resuming the comparison again, we find that, in a thousand deaths in the country districts, 202 persons attain the age of 70 years; while in Liverpool, for instance, but 90 persons out of a 1000 attain the same age; and while the average age at death in agricultural Rutlandshire is 38 years, it is stated to be but 21 years in Liverpool. Taking the same population, it has been shown by the Register-General that in four years a greater number died in town districts than in country districts by 99,752. Again, out of 1,000,000 of persons living, there occurs annually in the country, and where the population to the square mile is but 199 persons, 19,300 deaths; but in towns and where the population to the square mile is 5108 persons, there occur 27,073 deaths. We find also that fever—the great disease of adolescence and manhood—the disease that most afflicts men and women at the most useful and valuable period of life—the great destroyer of mankind in every climate—is bred and propagated in an especial manner in large towns; that towns present exactly in proportion as they are closely built and inhabited, the largest proportion of sickness and death from fever, not only as compared with the population, but with the total number of deaths from all causes. The fevers of the crowded quarters of London and of all the great towns is annually assuming a more formidable character, with an increase of its contagious virulence and power of propagating itself; its type everywhere indicating increased depression in the powers of life, as shown by the progressive lowering in the tone of the nervous and vascular systems."

Besides the continually devastating effects of fever among the lower classes of the population in large towns, we find that almost every disease, without exception, is more fatal in the manufacturing than in the agricultural districts of our land. So much is this the case, that the increase of deaths among children is four-fold by epidemics, and nearly ten-fold by convulsions, in towns as compared with the country. Among adults too, the prevalent epidemic diseases are more than thrice as fatal in Liverpool and Manchester than in the country; while the deaths by diseases of the lungs are nearly double, those by diseases of the nervous system are as 5½ to 1, and by diseases of the digestive organs as 2½ to 1. All observation, continues our intelligent author, goes to demonstrate that the liability to consumption increases in an enormous ratio with the increase of crowding and its accompaniment, defective ventilation." * * * *

The following remarks, on "the various losses occasioned to the public finances by *preventable diseases*," are full of force and truth:

"It has been calculated that the total number of orphan children, on account of whose destitution relief was given from the poor-rates in the year ending Lady-day, 1840, was 112,000. Of the parents of this number, we are confident that accurate investigation would demonstrate, full one-half died of preventable disease. The loss to the industrious classes consequent on sickness alone, has been variously estimated. One of the lowest calculations, rates the number of days of sickness in the year, experienced by a man, his wife, and two children above 12 years of age, at 29 days, or about one-thirteenth of the entire year. Estimating the weekly earnings of such a family at 40*s*., we have here a great loss by labor alone, without medicine and other contingent expenses. 'But this is vastly below the mark, although quite enough to prove how truly economical it would be in every way, to expend the same money upon airy, salubrious lodgings, conducive at once to the health, morals, and respectability. In fact, there can be no doubt that the enormous sums spent every year in hospitals, infirmaries, and union workhouses, are incomparably greater than the expenditure necessary for preventing diseases and pauperism.' This I believe to be true and easy of proof: indeed, we have only to turn to the singularly valuable 'Report' of Mr. Chadwick, to perceive as clearly as need be, how vast are the charges on account of sickness and mortality which are of easy prevention—how enormous the charges on the reduced duration of life—on the reduction by sickness of the periods of working ability or production—on the machineries for the suppression of much of the vice and crime, which comes within the province of the police—as well as for the relief of much of the destitution which comes within the province of the administrator's relief. According to the rate ascertained in eight Unions, Mr. Chadwick concludes that, in all the Unions, about 27,000 cases of premature widowhood occur, and more than 100,000 cases of orphanage—all which may be ascribed to removable causes. Mr. Hawksley estimates the loss in Nottingham alone, 'by the pressure of removeable causes of sickness and mortality at 300,000*l.* per annum.' It is quite unnecessary to pursue this subject further. Innumerable details in proof are now before the public, and further description would but weaken the effect."

Mr. Martin, after shewing how much might be done by a system of more stringent parliamentary enactments and police regulations respecting the removal of all refuse and filth, the thorough ventilation of lanes and dwellings, the abundant supply of water for the purpose of washing as well as of food, the improvement of the sewerage, the removal of burial places to the environs, the establishment of public baths, and of parks and open play-grounds, &c., concludes his Report with some admirable observations on the reciprocal influences of morals on public health, and of destitution and defective diet on the moral character.

AMERICAN MEDICAL INTELLIGENCE.

1—The following communication, from Prof. Mitchell, of Transylvania University, came to hand just in time for publication in the present number. We insert it with much pleasure, and beg the author to except our thanks for his kind attention.

Typhoid Fever, an Objectionable Term.

Messrs. Editors: I have been long impressed with a deep conviction, that the term *Typhoid* fever is not only an objectionable term, pathologically regarded, but that it has done, and is now doing a vast amount of mischief. This conviction is based, not at all, on any sort of theoretical predilection, but on actual observation of disease, and the mortality resulting, as I honestly believe, from false views, and the *do nothing* treatment thereon predicated. There is not a feature in the cases now called, by almost all practitioners, *Typhoid fever*, that I have not seen, separately and united, ever since I began my professional career as Lazaretto physician of the Port of Philadelphia, in 1813. At that time and every year since, just such cases have fallen under my observation, over and over again, and they were always true remittents, often associated with decided bilous symptoms, and speedily assuming a tendency to take on a low form, which was even then designated, as the *Typhoid state of fever*. And if there be any thing in my medical career on which I have felt disposed to congratulate myself, more especially, it is the early and the continued conviction, that in nearly all the fevers I ever saw, even at the Philadelphia Lazaretto during a three years residence, as well as in private practice, the prominent feature presented to my mind, was *periodicity*, the tendency to remit or intermit. I have seen, as others have, cases terminating fatally, where there was but one paroxysm. To such I have no reference, at this time; but to fevers that run on, just according to the right or wrong views of the practice, from two weeks or less, to four weeks or more.

I feel impelled to speak out most emphatically, on this subject, because my conviction is increased with every case I see, of what is called typhoid fever; and from the mortality that every where meets the eye and ear, in newspaper notices and daily conversation. I object to the name, because,

1st. It is calculated to beget unnecessary alarm in the patient, as well as the friends.

2d. Because it is identified, by a fatal association, with a *do nothing practice*, which will generally be fatal, unless nature comes to the rescue.

3d. Because false views thus leading to false practice, lay the foundation, very often, of incurable tubercular disease, even though the patient survive the febrile attack, after some five or six weeks confinement.

4th. Because the fever is, in every case that I have ever seen, (and I hazard nothing when I say that I saw hundreds of cases in Pensylvania, before I left the East), sufficiently impressed with the tokens of remission, to justify the use of the sulphate of quinine, largely, by the mouth, or blistered surfaces, or per anum; or all, if need be, combined.

The physicians in our country are losing sight of the remitting character of fever, just as they have done in Ireland. A reference to the late Dublin Medical Transactions will show, that the learned physicians there are getting awake on this point, and frankly confessing that they have suffered themselves to overlook this essential feature. Hence you find that Byrne and others, are treating what some there persist in calling typhoid fever, or continued fever, most successfully with the sulphate of quinine. And it is well known, that some of the most successful practitioners in France at this day, treat the so called Typhoid fever of that country, with the same article, with decidedly happy and speedy results.

To such an extent has the mania operated in our country, that it is scarcely needful to ask a Doctor, what is the matter with his patient, since, as a thing of course, it must be *typhoid fever*. What else should it be? It is an old saying, "that a man may as well be out of the world, as out of the fashion," and our profession, more than any other, bows to this maxim. Some of the newspapers proclaimed the case of the late Professor Richardson, to have been *typhoid fever*, when in fact it was as certainly a remittent, as any case I ever saw. This opinion was not formed by a hasty visit. I spent two whole days, or nearly so, with him, during the last of which, the febrile heat was scarcely perceptible for six or eight hours, the skin pleasant, and every thing as it should be for the free use of the sulphate of quinine, per anum, or otherwise. Unfortunately, the patient refused to follow the advice of his medical friends, and the case passed beyond the reach of professional aid.

To give you a fair specimen of these cases, as they occur, allow me to cite the essential particulars of one to which I was lately called in consultation, by a respectable country practitioner, who thinks with me, that, what is called typhoid fever, prevailed abundantly more than 20 years ago, under another cognomen. This disease has been fatal in the surrounding country, to such an extent, as to cause general alarm. If you ask a non-medical man, what is the matter, the reply is, "he has got *this fever* that is going about." And they will express their wonder, what can be the cause of this new fever, that is so fatal. The case I refer to, was of this sort. The patient, a very respectable farmer, had been sick ten days, when I first saw him. He had been on a fishing excursion and was exposed in such a manner, as would be likely to induce an ordinary remittent. He had been taking small doses of calomel and ipecacuanha; the whole however, not amounting to anything like an active treatment, though on the whole, judicious. I remained long enough and heard enough, to be assured that here was a *bona fide* remittent. For several hours, in the early part of the day, little or no sign of the fever was present. His pulse was 90, his eyes suffused, bowels easily acted on, and stools such as might be expected under the use of calomel. There was a constant listlessness, indisposition to move a fibre, and inclination to sleep all the while. The patient could not turn in bed, and lay constantly on his back, excepting when moved by his attendants. There was some disturbance of the stomach, amounting to mere irritability, and this opened the way for a blister, which was applied with good effect. I advised that the denuded surface be dressed with a strong cerate of sulphate of quinine, and the same salt to be given by the mouth, as freely as practicable, in the forepart of the day. Sponging the surface and full bathing in a tub, had been servicable, and was advised to be repeated when febrile heat increased.

Three days afterwards, I was summoned to see him again. There had been two bloody discharges from the bowels, one of them containing a gill of blood, and this excited alarm. Two blisters had been applied to the calves of the legs, and with excellent effect. I found the patient with a pulse somewhat reduced, being about 86, the eyes improved, the countenance more animated, a disposition to converse about his sickness, and every feature of the case more like healthful animation, with the exception of his tongue, which was dry and brown. I satisfied myself that the remission was even more obvious than it had been, and therefore urged a dressing of cerate to the blisters, containing 30 grains of the salt of quinine to be applied early next morning, and as large an amount to be thrown up the rectum, in two small injections, made of flaxseed tea and vinegar; an ounce of the latter with two of the former.

This treatment had the desired effect. The patient began to improve rapidly, and continued to do so, until complete recovery.

I regard this case as a pretty fair specimen of the typhoid fever of the West, and am fully borne out in the belief, that its character is partially remittent. Before I close this letter, allow me to say, the symptoms designated *Quininism*, are much abated by the endermic or injection mode of exhibiting the sulphate. This I have repeatedly observed. Yours, &c.,

THOS. D. MITCHELL.

Lexington, Ky., Oct. 1845.

If by the term *Quininism*, here used, the author means to designate the disagreeable *deafness*, *ringing in the ears*, *and roaring in the head*, with which some persons are nearly always affected by the *sulphate of quinine*, we would respectfully add to the valuable suggestion of Prof. Mitchell, of giving it by the skin or rectum, with the view to avoiding them, the superior advantage of the *ferrocyanate of quinine*. This form of the medicine seems to exercise all the febrifuge and antiperiodic virtues, without producing the disagreeable effects above mentioned. It is much prescribed in this city, in cases where the head or stomach cannot bear the *sulphate*. We very recently had an opportunity of witnessing its good effects in a case of malignant intermittent, associated with painful dysenteric symptoms. The gentleman formerly resided in the *malarious* Western District of Tennessee, where he had occasion to learn that whilst the sulphate of quinine displayed, upon his system, its usual beneficial influence, in breaking up the paroxysms of fever, it at the same time, produced temporarily the most distressing effect upon his brain. He apprised us of this, but we deemed the remedy indispensable, and prescribed a scruple of it, in five grain doses, on *the well day;* to be repeated the following morning. When we called on this day, we found that he was omitting the quinine on account of its distressing effects upon his brain the previous evening; he had been made almost *perfectly deaf.* As we could not persuade him to continue it, we warned him of the consequences, and advised him to keep himself warm. But the paroxysm came, and he got through it with such difficulty as to leave but little doubt that another would be attended with the most imminent danger. With the concurrence of Dr. Stone, who was now called in consultation, we determined to give the ferrocyanate, in five grain doses, every two or three hours during the intermission. The patient was now sufficiently alarmed to be perfectly submissive; he asked no questions, nor made any remonstrances. We gave him about half a drachm of the ferrocyanate in this manner, with the most satisfactory result. He had no return of paroxysm, and could scarcely be made to believe that he had been taking any form of quinine, so free had his head been from its usual effects. F.

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2. *Salacine—the Surgeon General's Circular.*—It redounds to the general reputation and to the high medical character of our country, that the highest medical officer in the Government is distinguished for his zeal in the profession for which he was educated. In the following circular, issued by him, an ardent desire is manifested for determining an important question; and to accomplish this object, there is a minuteness of detail required, in the returns to be made at Washington, which must yield the most satisfactory results. We shall be happy to publish these results, whenever attainable. The following is a circular, signed by Thomas Lawson, Surgeon General.

"Sir,—The Medical Purveyor at New York has been directed to issue to those Military Posts at which miasmatic diseases are of frequent occurrence, a supply of *salicine* (the active principle of the bark of the common *willow*) a medicine which has been recommended by high authorities for its *febrifuge* and *anti-periodic* virtues.

"Inasmuch as the supply of the *sulphate of quinine* is at best precarious, and as, moreover, it may be diminished, at any time, by an interruption of our commercial relations with foreign nations, it becomes the duty of officers of the Government who are entrusted with the health of those engaged in the public service, to use their best endeavors to provide a substitute for a remedy so highly valued, and so universally employed.

"I have therefore deemed it advisable to submit the salicine to trial on a large scale, with a view of ascertaining to what extent it may be relied on as a substitute for the sulphate of quinine, in a case of emergency, and accordingly I have to request that you will institute a fair and impartial trial of its remedial powers, in your practice, in all cases of miasmatic disease in which the administration of quinine may not be indispensably requisite—and in such other cases as you may think proper.

"You will forward to this office a special report of your observations on the subject, on or before the expiration of the current year, noticing particularly the following points, viz:

"1. the doses in which you have employed salacine, with their effects.

"2. The disease, and conditions of the system in which it has been administered—and with what effect.

"3. Whether you found it more, or less, liable to irritate the stomach than sulphate of quinine.

"4. Any bad consequences, you may have observed to follow its employment, attributable to the medicine.

"5. Any combinations you may have found to affect its activity; and what preparation of the system you have found necessary before its exhibition.

"6. Your opinion of its *modus operandi.*

"7. Its value as a remedy, as compared with sulphate of quinine, and other medicines of similar properties.

"8. Brief and concise notes of cases in which it has been employed in your practice—as numerous as practicable.

"It is proper to add, that as the profession at large will, doubtless, be interested in the results of these observations, they will probably be given to the public in such form as will be most creditable to the observers."

3.—*Heat in July*, 1845. The following is taken from the Boston Medical and Surgical Journal for September. We annex a comparative statement of the height of the thermometer at New-Orleans on the days mentioned.

The late hot weather was pretty severely felt throughout the Northern States. It is said to be the hottest season experienced since 1825, but, according to some statements, not as hot as it was then. In that year, from July 11 to July 16, the thermometer ranged from 100 to 108½. Within the same dates this season, it has ranged from 91 to 102½. The papers from different parts of the country, in giving the state of thermometer, have recorded many deaths which were occasioned by extreme heat. The number of deaths in some of the cities has largely increased. Below is given the state of the thermometer in various places on the days specified:

July 9th.—Charleston, S. C., 94. New Orleans 88½.

12th.—Rochester, N. Y., 87. New York, 84. Burlington Vt. 100. Pittsburg, Pa., 102½. Hudson, N. Y. 90. Salem, Mass., 103. New Orleans, 84.

13th—Boston, 98. Philadelphia, 101. New York, 88. Albany, 98. Brooklyn, 95. Baltimore, 95. Greenfield, Mass, 100. New Orleans, 87½.

14th.—Philadelphia, 102. New Orleans, 90.

16th.—Rochester, N. Y., 102. New Orleans 90.

N. B. By reference to Mr. Lillie's remarks in our last number, page 275, it will be seen that the thermometer never rose so high at any time during the summer in New Orleans, as it did in the Northern cities.

4.—*Death of Professor Richardson.*—Departed this life on the 14th of September, in the 62d year of his age, William H. Richardson, M. D., Professor of Obstetrics and Diseases of Women and Children, in the Medical Department of Transylvania University, after an illness of little more than two weeks. Some time prior to the last attack, he had suffered from gastric and intestinal derangement, which recurred with increased violence and persistence, inducing a sympathetic fever, that terminated his mortal career.

Professor R. was a native of Virginia, but had resided in Kentucky for about half a century, filling various posts of respectability, the most prominent of which was the chair of the medical school, now vacated by his decease. Identified with the first organization of the department, he never ceased to perform his duties with fidelity and punctuality. For many years was a most laborious practitioner, and therefore, extensively known. His office was filled with pupils who are now scattered abroad in the West, some of them occupying a high rank in the profession. The large concourse of relations and friends that followed his remains to their final resting place, gave evidence of the strong hold he had acquired on the regards of the community.—*Western Lancet.*

TO THE MEDICAL PUBLIC.

Lexington, Ky., Sept. 17th, 1845.

The Chair of Obstetrics and the Diseases of Women and Children, in the Medical Department of Transylvania University, is at present vacant; and with a view to fill it in the best possible manner, applications for the appointment, are invited from the medical profession. Communications on the subject must be sent to the Dean of the Medical Faculty, prior to the 30th day of January next, when the selection will be made. It is proper to state, that the successful candidate will be required to make Lexington the place of his permanent residence, and that the name of no other applicant will be made public.

M. C. JOHNSON,

Chairman of the Board of Trustees, Transylvania University.

The Vacant Chair of Obstetrics.—The above notice refers to a *permanent* appointment. The duties of the Chair, for the coming session, will be performed by the Professor of Materia Medica and Therapeutics, (Dr. Mitchell.)

The conductors of the various medical journals in the United States, will confer a favor, by noticing the above.—*Ibid.*

ALABAMA MEDICAL SOCIETY.

At a meeting of the Alabama Medical Society, in May last, it was resolved to offer a silver cup as a premium for the best essay on the pathology and treatment of congestive fever. The essay and the name of the writer, to be deposited with the secretary of the Society, on or before the first Monday in December next.

VINEGAR IN CASES OF NARCOTIC POISONING.

Dr. Clapp finds vinegar an excellent adjuvant to emetics, in cases where narcotics have been taken into the stomach in doses to overcome the excitability of that organ. He has succeeded in bringing on vomiting by administering this acid when the emetic was about to fail. He mentioned to us the following instances. A man, in a fit of mental despondency, swallowed an ounce of laudanum on an empty stomach. In about an hour he was visited by Dr. Clapp, and was found insensible, with stertorous, convulsive breathing. Sulphate of zinc was administered to the extent of a hundred grains, and his fauces were tickled with a feather, but vomiting was not induced. The Doctor gave him a pint of vinegar; emesis soon took place, with the relief of all the alarming symptoms.

Two children swallowed a number of seeds of the stramonium at different times. In the case of the first, the ordinary means of exciting emesis were tried ineffectually, and the child died. In the second, vinegar was given, free emesis was the result, and the patient recovered.

These facts are valuable, and knowledge of them may save the lives of many individuals. We know how often children are sacrificed by the indiscreet use of opiates, and how frequent cases of poisoning by opium, the Jamestown weed, &c., are becoming in this country. If vinegar gives activity to emetics in such cases it is an important auxiliary. Let it be tried.—*Western Jour. Med. and Surg.*

American Physicians in Europe.—We are pleased to learn that three of our American medical professors have visited Europe during the past summer, and have been treated by the faculty there with marked attention and kindness. They are Dr. Meigs, Professor of Midwifery in Jefferson Medical College, Dr. Hamilton, Professor of Surgery in Geneva Medical College, and Dr. Lawson, Professor of General and Pathological Anatomy in Transylvania University.

It seems that Dr. Meigs attended a sitting of the French Academy of Sciences on the 2d of June, from the proceedings of which, published in the Gazette Medicale de Paris, we make the following extract:

" *Cyanosis of New Born Infants.*—Dr. Meigs, Professor of Midwifery in the College of Philadelphia (Jefferson Medical College), read a note on this affection, in which he says the child dies from the presence of dark non-oxygenated blood in the brain, where its presence is hurtful, not because it acts as a poison, but simply because it is incapable of exciting the motions of innervation in that organ. It is useless to insist on the universally known fact that the persistence of the foramen ovale is the cause of cyanosis.

" As the occlusion of the foramen ovale is prevented by the torrent of blood flowing from the inferior vena cava, raising and keeping raised the inter-auricular valve, which is thin and floating, it occurred to Dr. Meigs to place the cyanosed child on the right side, with the head and trunk somewhat raised, so that the inter-auricular septum should be maintained horizontal, and the blood contained in the left auricle should press with its whole weight on the closed valve. Dr. Meigs has frequently seen the blue colour disappear at the very instant the infant was placed in this position, proving that oxygenated blood only entered the arteries.

" Dr. Meigs stated that he had thus saved the lives of fifty or sixty children in one hundred; whereas, as is well known, all the means hitherto tried have failed."

We take this from the Bulletin of Medical Science for September. At the close of the number, the editor remarks:

" In a preceding page will be found a notice of a paper on *Cyanosis*, read before the French Academy of Medicine by our esteemed *confrère*, Dr. Charles D.

Meigs, during his short sojourn in Paris. The composition and reading of a communication in French before this learned body, was less a task and a venture for Dr. Meigs than for most American or English savans, owing to his familiar acquaintance with the language in its idiomatic form of expression.

"Hardly was time allowed us to transmit to our printers the account of the sitting of the Academy at which Dr. Meigs was present, when we had the pleasure of shaking hands with our friend, on his return among us; he having, in the meantime, visited Belgium, Germany, and Switzerland, prior to his again visiting England on his way homeward.

"Dr. Meigs speaks in the warmest terms of the manifestations of comity and kindness displayed to him by the professional gentlemen, both in England and on the Continent. How far these were given, as a tribute to his personal and professional worth and position, and to his being, for the nonce, a representative of American physicians abroad, we leave to our readers to judge; not wishing, on our part, to incur the accusation of flattery by giving utterance to our own convictions and feelings."

NEW ORLEANS, NOVEMBER 1, 1845.

HEALTH OF THE CITY.

Summer with its enervating influences is gone; and we welcome with cordiality the bracing winds of Autumn. Long, dull and tedious have been the days since the *business season* in our city was brought to a close, and our large transient population, together with a great number of our restless fellow-citizens, betook themselves to other climes; some to return to their distant and long deserted homes, some to attend to their affairs at other places: and others roaming in pursuit of health, pleasure and recreation. Autumn brings them back with renewed energies, and, we are happy to say, with renewed attachment to our charming city. They left us to the dangers of *the dreadful pestilence*, though not without imploring for us the protection of heaven; on their return, they find that we, too, have been blessed, and that we are still here to welcome them with a heartfelt greeting. We have been favoured with a degree of health probably not surpassed by that of any place visited by our wandering citizens; and we hope and trust the time is not far distant, when the large numbers who reluctantly leave their homes every summer, impelled by a sense of duty and safety, may feel fully authorized to remain here the year round. It is a serious thing for families to break up housekeeping annually, and absent themselves from home for several months. Indeed, it may be viewed as a calamity which those in moderate circumstances can but illy bear, and which can hardly be provided for by the most unremitting exertion and toil during the business season.

It would be useless to recount all the inconveniences, losses, trouble and expense necessarily attendant upon this course; they are sufficiently familiar to every body. It is also generally known that our creole and acclimated citizens enjoy as good health as any in the world, and that with the exception of yellow fever, no city is more exempt from disease than New Orleans. How forcibly, then, are we reminded of the importance of studying carefully the cause and origin of this disease. Other cities which were in times past frequently scourged by yellow fever, appear to have escaped it for a long period, by proper attention to sanatory regulations. What has ever been done in New Orleans ***expressly with***

with this view? Surely, very little. It is true that our city is more cleanly than it was but a few years ago. The pavements have been extended, and the swamp in the immediate vicinity has been drained to a considerable extent. But all these improvements have been prompted by motives of immediate interest—for commercial advantage—and to enhance the value of property. As we remarked in a late number, the health of the city has doubtless been *incidentally* promoted; but how much remains to be done—how plainly it is indicated to the most superficial observer, and how easily it may be effected, and with what immense benefit to the health of the city—are questions of the gravest import to our municipal authorities. We are aware of the opinions of some narrow-minded and selfish individuals, that our city is already *too healthy*, and that nothing but frequent and severe epidemics can keep off the million who are eager to come here, and who, they say, would divide and fritter away the business of the place until it would be worth nothing to any one. These are not the opinions of the liberal and philanthropic members of the medical profession, nor, we trust of our enlightened councilmen. Then let the effort to improve the health of the city be made; let all judicious precautions and regulations be adopted and executed, and let us see the result. Let our city grow until it rival London, Paris, and Pekin in size. Business will keep pace with its increasing population, and a lively competition will serve not only to sharpen our intellects, but to extend the benefits, and lighten the labours of life.

We by no means wish to encourage our countrymen to quit the free air and invigorating occupations of rural life for the purpose of confining themselves within the narrow limits of a city residence, with its necessarily contaminating influences upon morals; but as we have devoted ourselves to the study of disease and the preservation of health, we feel bound to point out whatever we think calculated to promote the health and prosperity of our city.

In regard to the "difference of health in town and country," and "the various losses occasioned to the public finances by preventable diseases," we refer the reader to an interesting extract from the "Health of Towns Commission," (England,) at page 389, of this number.

As to the health of the city at the present time, we have only to say that it must be perfectly satisfactory to the heartiest well wisher. The only prevalent disorder worthy of notice is scarlet fever, which still exists to a moderate extent in the upper, lower, and back parts of the city, leaving an extensive intermediate space almost entirely exempt from the disease.

Throughout the whole summer we have been assailed by predictions of a violent epidemic. All things seemed to favor the probability of such a result. The river was falling during the months of July, August, and September, at its lowest stage, the mud banks were left bare to emit their offensive effluvia; the heat of these months has been almost unpresedented, the range of the thermometer was considerable, and we had several sudden and extreme changes of temperature in the latter month. The amount and distribution of rain has been about an average. With every change of weather and influence, new changes have been rung in the tone of these dismal predictions; when the great heats of summer began, we were to have pestilence ; when the rains began, we were sure of it, it had hitherto been too dry for yellow fever ; when the river fell, this was the *sine qua non ;* and finally, it was the autumnal change that was to work the wonder. With all there auguries, and all these influences, we should certainly thank Providence for the almost entire exemption from disease, which the city has enjoyed.

Great alarm and excitement was produced about the 11th of September by an article published by the President of the Committee of Public Hygeine, announcing the existence of yellow fever, with gloomy forebodings of a severe epidemic. This measure was certainly premature, as facts have shown, and with diligent enquiries we have not been able to hear of a decided and unquestionable case of the disease in this city.

HEALTH OF THE COUNTRY.

We have to continue a most favorable report the general health of the southern country. The only exception we must make, is in regard to the western district of Tennessee. We regret having to go to press without receiving special letters on the subject from any of our correspondents in that region ; but by the newspapers, and from persons who have recently come from there, we learn that within a short period past there has been an extraordinary amount of sickness. It consisted almost entirely of intermittent fever, and was attended with but slight mortality. A friend from Tennessee informs us that the sickness broke out suddenly and simultaneously over a large extent of country, immediately after the first cool spell of weather in September. It had continued unusually healthy up to that time. The frost that has occurred since will doubtless soon put an end to the sickness.

With this exception, on, the whole of the Southern country, so far as we have been able to learn, has been *uniformly more healthy than was ever known before.* And be it remembered, this has occurred during one of the hottest summers ever experienced. Rains, too, have been frequent and abundant. Some limited sections, as about Montgomery, Ala., and Woodville, Miss., have suffered from drought ; but this has not been general, nor can the summer be said to have been a dry one. Let it also be remembered with regard to the immediate vicinity of the Mississippi river, that this healthy year has succeeded one of the greatest overflows ever known. We allude to that of 1844. These facts are worth recording for future reference. The seeds of malarious diseases may require a much longer period of incubation, than we are aware of. Observations have heretofore been confined to the connection between these diseases and the sensible state of the weather at the time of their appearance, and they have proved altogether fallacious. Let us now endeavour to trace back their causes more remotely. This can only be effected by careful observation, and a faithful record of facts.

We give extracts from letters received from our correspondents since our last number. They will please accept our thanks for their kind attention.

" U. S. Naval Hospital, Pensacola, *October 17th,* 1845.

" Gentlemen: On my return from a visit to the Northern States, I had the honor to receive your letter, in which you were pleased to name me your correspondent, for the medical news of this particular section. So far as it may be in my power, I shall take pleasure in complying with your wishes, and in this introductory number, I invoke your indulgence in favor of the following brief notes, for many of which, I acknowledge my indebtedness to Dr. John Thornly, the Assistant Surgeon of the Hospital.

"The summer may be said to have commenced in April, from which time to the close of September, the mercury in the thermometer ranged uniformly above 80° F. during the day, and but few degrees below that point during the night. The temperature was rendered tolerable (in the shade) by the sea-breeze which generally blew from the southward and westward until midnight, or thereabouts, alternating with the land-breeze, which prevailed from the time mentioned till morning.

"This remark applies more particularly to this locality, as the inhabitants of Pensacola, situated eight miles higher up the bay, all agree in pronouncing the past summer one of almost unprecedented heat as well as drought.

"The highest degree of temperature noted, was 100° F*, on the 22d of July, and but 10 1-2 inches of rain fell from January to September. The mercury in the barometrical tube was never above 31 or below 30 inches, and thunder storms were rare.

"Diseases have been few and generally light; the most prevalent has been diarrhœa, which has yielded readily to anodynes and farinacious diet. Four cases of apoplexy, three of which proved fatal in 45 hours, have occurred during the summer, within and near the Navy-yard, and the intelligent physician who saw them is of opinion that a previously disordered and irritated state of the bowels, in connexion with exposure to solar heat, contributed largely towards forming the predisposition to that appalling malady.

"On the whole, this section of the country has enjoyed an extraordinary exemption from disease, and the naval forces operating in the Gulf have, thus far, been equally favored. The whole number of men in the squadron which seeks this port for refreshment and supplies, is about 2,000, and the number of patients from it found actually under treatment in this hospital has not, at any one time, exceeded thirty-nine.

"The diseases most frequently met with in cases sent to the hospital were chronic, of which pericarditis and rheumatism figured largely.

"Only one death has occurred in the institution during the season, and that from injury of the spine producing inflammation, which travelled up to the brain. Autopsy revealed lumbar and hepatic abscess.

"In September, rain fell to the amount of 9 1-16 inches, and since the 1st of the present month, we have had 11 1-2 inches; consequently the swamps and water courses are full. A cold north wind succeeded the copious rains, and slight frost was observed on the morning of the 13th instant, a few miles in the interior.

"No complaints, except those of a mild character, exist any where within the sphere of my acquaintance, and all apprehension about autumnal diseases has ceased.

*The exposure of the thermometer was not altogether fair, it was hung upon a brick column under the gallery of the basement and there was reflected heat upon it in the afternoon.

"When I become again settled in the routine of my duties, I purpose giving you such statistics and comments as, I trust, may be found acceptable."

Huntsville, Ala., August 21, 1845.—Our correspondent writes as follows:—"Huntsville, a village containing near three thousand inhabitants, is situated in a valley, originally rich and fertile, known by the name of the Tennessee Valley. The country is densely populated. Most of the good land has been cleared and brought into cultivation some number of years since. The water is pure, and for the most part impregnated with lime. Through the whole extent of this valley, the Tennessee river flows, a distance of something over one hundred miles. A considerable portion of the land, when first settled, was rich and very productive, but owing to improper culture, it has become, in many places, greatly exhausted. Since the country has been open, there is very little local cause of disease, except in the vicinity of creeks and rivers. Intermittent and remittent bilious fevers, are the prevailing summer and fall descases.

"From the month of January last up to the present date, we have been blessed with more uninterrupted good health than for many previous years. Except a few scattering cases of typhoid pneumonia and typhoid fever, (termed here slow fever), during the winter and spring, we have had no disease of a serious character. The months of June, July, and August, have been generally dry, hot, and oppressive, and most physicians anticipated, (at the least), the usual quantum of disease. A few cases of intermittent or remittent fever, are now met with in almost every neighborhood, but so far as I have been informed, no epidemic, of any character, has visited the country. Recently we have had in our town and vicinity, a few cases of cholera morbus, and a tendency to such diseases.

"Some sixty miles southwest of this place, in the same valley, during the months of June and July, a malignant erysipelas, assailing the throat, head, neck, &c., (vulgarly called the *black tongue*), made its appearance in several families, and was attended with great mortality. One gentleman, as I am informed, lost upwards of twenty of his family. The disease did not extend itself much beyond the immediate neighborhood (embracing a few miles in extent) in which it first appeared. It is generally considered contagious."

Nashville, Tenn., Sept. 9, 1845.—Our correspondent writes to us:—"During the month of June there were 27 interments in the Nashville cemetery, of which the sexton reports 2 of diarrhœa, and 1 only of fever; rheumatism, dropsy, consumption, making up the balance. In July, there were 18 interments, 2 of diarrhœa, 3 of fever, with the usual complement of other diseases. During the month of August, (of which there has not been a report made), there has been in Nashville a tendency to febrile affections, much greater than the exhibit of June and July would seem to indicate. The character of the diseases has been such as prevailed here every summer and fall, intermittent and remittent, with this peculiarity, perhaps, that there has been more involvement of the nervous system, than during corresponding seasons generally. Nashville may be justly considered more exempt from febrile diseases than the surrounding country. In some neighborhoods, near us, there have been and are yet a great many severe cases of remittent congestive fever. I have been able to learn nothing peculiar in their type. There is but little infantile diarrhœa in this country. In fact, compared with southern Mississippi, I would say the country is free from it."

Columbus, Miss., Aug. 15, 1845.—Our correspondent from this place writes—"Our town is situated on elevated rolling ground east of Tombigby river. Our streets are in better order than they have ever been; the low grounds, in the immediate vicinity, are well drained; our population is active, industrious, and temperate' and numbers about 3,000. We have had, thus far, a remarkably healthy year; no prevailing epidemic. Bowel affections during the spring were common among children, and among the adults many cases of pneumonia. During the summer, still less disease—mostly intermittents and billious remittents of a mild character. In the prairies west of our town, 10 or 15 miles, they have had more sickness than they have ever had, and considerable fatality. The most common forms of cases were typhus fever, and typhoid pneumonia; these cases were confined to a few large plantations of negroes, perhaps owing to their badly constructed houses. On the river, above and below town, there has been a number of cases of 'chills and fever,' but not much fatality. We have had but few cases of congestive fever.

"I find in a daily memorandum I have kept of the weather, &c., that *January* was a remarkably warm and pleasant month; 7 days of unpleasant weather; the early part of the month was very warm, and some sickness.

"February.—This was also a remarkable month; almost equal to the month of May. Peach trees in bloom the 10th; considerable sickness; rained only on 3 days.

"March.—'This has been the most delightful March I remember ever to have witnessed,' is a note made at the close of the month. The middle of the month is marked with sickness. Rained on 8 days.

"April.—Was a delightful, calm month; first part healthy; the latter part, bowel affections common; some cases of pneumonia. Rained but 4 days.

"May.—The first part of the month, bowel affections among the children; several cases of pneumonia, and hard to control; rain 10 days; warm; thermometer, (F.) never over 90° in the shade; much complaint in the middle of the month of '*flies dying;*' considered as an omen of much sickness. This month very healthy.

"June.—Warm, dry, and remarkably healthy. Mild cases of intermittents; four or five very slight rains; thermometer (F.) never over 96°.

"July.—first part healthy. On the 7th and 8th the mercury stood 98°. About the 10th several hard rains, followed by some complaining; two or three cases of pneumonia. Latter part of the month, warm, dry, and healthy, a few cases of bilious remittents.

"August.—The first days very cool; mornings very cool, indeed. About the 9th and 10th, some hard rains. In the country, at this time, (16th), there is more sicknes than usual. Bilious remittents, and a few cases of typhus fever."

Memphis, Tenn., August 15, 1845.—Our correspondent writes:—"The health of Memphis this summer, thus far, has been unprecedented. Having fallen to my lot to act as Secretary of the Board of Health, and to make weekly reports, my attention has been thus more particularly directed to the general state of health pertaining to the city and surrounding country, and the remarkably healthy condition of the city as well as of the whole district has been a source of astonishment, both to the physicians and people generally. Our oldest physicians here state that they have never known so healthy a summer in this region of country, and that the city was never more exempt from disease than it is at the present time. We usually have a good deal of fever here, of a bilious remittent character, at this season of the year, and, sometimes, cases of a very severe and fatal form, but this summer there have been very few cases, and these very mild and manageable, none of them proving fatal, except one or two among the lowest grades of the population.

"The great healthiness of the season we are disposed to attribute to the unusual, and we may say, unparalleled dryness of the spring and summer; dry springs and summers being attended, as a general rule, with a greater degree of healthfulness—and wet seasons, *vice versa.* We may also remark that, notwithstanding, the intolerable heat with which we have been afflicted, we have had no cases of *coup de soleil*, a disease which, from the reports, has been so fatal and common in cities both north and south of us."

Parish of St. Mary, October 14, 1845.—We have received as follows:—"This section of country has never been healthier, at this season of the year, than at present. During the month of September, we had considerable sickness of mild character, principally intermittent, and easily cured. The measles and hooping-cough are still prevailing in some neighborhoods, but generally mild, and seldom requiring medical attendance. Every fever has assumed the intermittent form. I have even seen the measles accompanied with chills and fever; and have used quinine to arrest the paroxysms without any detriment to the eruptive fever.

"I have seen fewer cases of congestive fever this season than during any previous season I have spent at the South.

"In the early part of July, a number of cases of congestive fever occurred in the prairies in the vicinity of New Iberia, and of a fatal character. The cases all occurred in one extensive family. Fourteen deaths occurred in this one family. I have not learned the number of persons attacked.

"I have lately seen some of the physicians from adjoining Parishes, and they all represent that this season has been the healthiest for a number of years. There has been but little sickness in Opelousas.

"From the excessive quantity of rain, during the summer months, most physicians anticipated an unusual amount of sickness, but, thus far, their anticipations have been disappointed."

Woodville, Miss., October 11, 1845.—Our correspondent at this place writes as follows:—"My report this time is the same, pretty much, as the preceding. Our town and neighborhood have been blessed with better health this year than has been known for a great number of years, and we may now no longer fear the appearance of the usual autumnal visitation, as the season is far advanced, and at present very cold, threatening frost to-night. There have been some cases of intermittent fever, and some remittents of the double tertian type; but no deaths that I have heard of, from them. Some have died of phthisis and intestinal disorders, but only five in this vicinity; and one man killed by a negro.

"Since the 25th August, there have been rains on twelve days, and many fluctuations of temperature. Fires were required on the 20th Sept. Winds during that month from NE, E, SE, and during the past 10 days N, NE, NW."

Montgomery, Ala., October 11, 1844.—Our correspondent writes:—"No alteration in regard to health has taken place in our city since my last. What I then said, I might with propriety repeat."

Galveston, October 2, 1845.—The following is from our correspondent at this place:—"It affords me pleasure to state that from the period of my last report up to the 10th September, the health of our city continued uninterrupted; since that time, however, quite a number of cases of fever have occurred, mild in character, and yielding readily to the most gentle medicines.

It is now the 2d of October, and a cold and bracing norther is blowing, which will doubtless banish from the minds of our citizens all fears of an epidemic; and it may with truth be said that Galveston has never been freer from disease than during the entire summer of 1845.

Vicksburg, Miss.—Our esteemed correspondent at this place was in town a few days since, and informed us that Vicksburg continued in the enjoyment of almost interrupted health.

Mobile and Natchez.—Our daily intercourse with these cities enables us to say that the state of health in them is about the same as that of New Orleans.

HOSPITAL REPORTS.

NEW ORLEANS CHARITY HOSPITAL.

Monthly Report for August and September, 1845.

MAIN BUILDING.

August—Admitted: Males, 434; Females, 79; Total, 513.
Discharged: Males, 436; Females, 80; Total, 516.
Died: Males, 37; Females, 7; Total, 44.
Remaining on 1st. September, 298.

INSANE DEAPARTMENT.

Admitted: Males, 10; Females, 8; Total, 18.
Discharged; Males, 5; Females, 1; Total, 6.
Died: Males, 4; Females, 0; Total, 4.
Remaining 1st September, 58.

MAIN BUILDING.

September—Admitted: Males, 591; Females, 91; Total, 682.
Discharged: Males, 493; Females, 80; Total, 573.
Died: Males, 36; Females, 7; Total, 43.
Remaining on 1st October, 332.

INSANE DEPARTMENT.

Admitted: Males, 17; Females, 4; Total, 21.
Discharged, Males, 16; Females, 2; Total, 18.
Died: Males, 2; Females, 0; Total, 2.
Remaining 1st October, 60.

It will be seen by the monthly statements that the admissions into the this Hospital have been numerous, notwithstanding the extraordinary healthiness of the season. This proceeds from the free access allowed to all persons, no matter how trivial their complaints, or whether they be proper objects of charity or not. The mortality will certainly compare favorably with that of any similar institution of equal size.

SURGICAL WARDS.

We regret to say, that owing to the indisposition of the visiting surgeons, Drs. Hester and Mercier, we have not been furnished with any reports from these wards for the present number. Dr. Hester is absent from the city at the present time, for the purpose of recruiting his health. One capital operation (amputation of the arm) and a number of minor ones, such as for *hydrocele, stricture of the urethra, fistula in ano, cataract, strabismus, &c.*, have been performed since our last number, some of which will probably be reported at a future time. There is in Dr. Hester's wards at this time an exceedingly interesting case of *traumatic tetanus*, which has been under treatment for two or three weeks, and seems like recovering. It will probably be reported.

MEDICAL WARDS.

These wards have abounded in a variety of interesting cases, and we have a number of reports on hand, but as we find our space is already nearly filled, we have concluded to postpone them for the present, with the view to make room for Dr. Fenner's Report on Salicine.

Service of Dr. Fenner, Wards No.10 and 12.

Report of 20 cases of Intermittent Fever treated chiefly by Salicine; and a comparative statement of 20 cases of the same disease treated chiefly by the sulphate of Quinine.

Before seeing the "*Circular of the Surgeon General of U. S. Army on the subject of Salicine*," (to be found on a preceding page), I had determined to embrace the fine opportunities presented by this large Hospital, into which are annually admitted nearly 2,000 cases of Intermittent Fever, with the view to test its virtues, and to see how far it might be relied on as a substitute for quinine. The following cases, in which I endeavored to give the remedy *a fair trial*, will not, I hope, prove uninteresting to the profession, as they cover, in an informal manner, the whole grounds of inquiry expressed in the Surgeon General's *Circular*. As to the subsequent *comparative statement*, it may be taken for what it is worth. I would only caution the reader not to be led by it to do injustice to the actual merits of Salicine. I am inclined to think it possesses some virtues; but have no idea it can ever be relied on as a *substitute for quinine*.

Case I.—J. M., an English sailor-boy, aged 16, of robust constitution, engaged recently on a tow-boat plying between this city and the Balize. Was seized with an ague on the 20th May, at 7 A. M., followed by a burning fever, which sweated off towards night. He took no medicine, and continued to have a regular recurrence of the paroxysm every day, until the 26th, on the evening of which he entered the Hospital. The House Surgeon prescribed *ol. ricini, and lemonade.*

May 27th—At my morning visit, 9 A. M., I saw him first. He had had his accustomed chill, and was now laboring under burning fever, and severe pains in the head, back, and limbs. His bowels had been freely purged, and he was beginning to sweat already. *Ordered salicine grs.* v *in solution, every* 2 *hours during the evening—the same every hour from* 4 *o'clock in the morning—hot infusion of sage—and hot bricks to feet.*

28th—9 A. M. Found the patient in same state as yesterday morning. There being no salicine in the house, prescription had been omitted. *Ordered the same.*

29th—9 A. M. Found patient under a chill, though it had come on an hour later than usual. *Ordered salicine* ℈ i, *to be taken at once, late this evening—the same early in the morning—hot drinks and hot bricks to feet as before.*

30th—Found him in the chill again; costive; tongue foul. *Ordered calomel and pulv. rhei, a. a. grs.* x. *to be given on the decline of the fever. Repeat the salicine* ℈ ii.

31st.—Found patient as yet clear of chill; but he had it an hour or two afterwards. *Ordered salicine* ℈ ii, *to be repeated.*

June 1st—Found him in chill again, notwithstanding all precautions. As the patient was now a good deal exhausted, and the salicine had been fairly tried, I determined to resort to quinine. *Ordered sulph. quinine* ℈ i, *in* 8 *pills. Take one every* 2 *hours this evening, until* 4 *are taken—early in the morning take* 2 *pills, and repeat in an hour—hot drinks, &c., as before.*

2d—Patient missed the paroxysm. *Quinine repeated.*

3d—Missed paroxysm again. *Repeat quinine.*

5th—*Discharged, cured.*

Remarks—The quinine seemed to act most promptly here, after the salicine had been fairly tried, and *failed.* The previous treatment, however, may have prepared the patient for a ready cure. Amount of salicine given ʒ ii; quinine ʒ i.

Case II.—D. J., Irish drayman, aged 36; was taken with a chill, about noon, May 25th. Had another slight one on the morning of the 26th; and a more severe one at 9 P. M. of the same day, followed by hot fever. May 27th, had another, and came to the Hospital. I saw him in the evening; then cool and sweating. *Ordered salicine grs.* x, *at* 6 *P. M.—again at* 9 *P. M. Early in the morning to take grs.* x, *every* 2 *hours—hot drinks, &c.*

May 28th—Found patient in a profuse sweat—no chill—rested well. *Repeat the salicine as before.*

29th.—Rested well—missed chill. *Repeat salicine.*

30th—Convalescent. *No medicine.*

31st—Discharged, cured.

Remarks—The salicine acted like a charm in this case. Amount given, ʒ ii.

Case III—J. R., steamboat-hand, aged 27, just from Red River. Has had chill and fever regularly every other day since the 18th of May. Has taken nothing but some Lee's pills; had chill on the 27th May, and came to Hospital.

May 28th—Morning; is now cool and sweating; has pain across the epigastrium, back and limbs; usually has violent head-ache during the fever. *Ordered salicine grs.* v, *every* 2 *hours to-day—and every hour to-morrow morning—hot drinks, &c.*

29th—Patient sweating copiously; says he had a high fever during the night, without any perceptible chill. He has taken ℈ii salicine; has slight head-ache. *Ordered to take* 2 *more doses of salicine to-day, and to continue it to-morrow morning.*

30th—Patient sweating; says he had severe head-ache last night; is costive. *Ordered calomel and rhub., each grs.* x—*to continue the salicine after its operation*

31st—Had fever last night; feels better this morning; cool and sweating; slight head-ache; costive. *To take a dose of ol. ricini, and continue the salicine.*

June 1st—Feels better than usual; very slight fever last night; bowels open. *Continue salicine.*

2d—Feels very well. *Continue salicine.*

4th—Discharged, cured.

Remarks—This was an obstinate case. Amount of salicine given, about ʒ iiiss.

Case IV—M. G., Irish drayman, aged 29; attacked with chill on the 22d May, at 11 A. M.; had regular recurrence at the same hour every other day, until the 28th, when he came to the Hospital. Has taken no medicine. I saw him first at 5 P. M.; he is now sweating freely; has slight head-ache; tongue foul; one stool daily. *Ordered ol. ricini* ℥ ii *at once—to commence at day-light the following morning, and take salicine grs.* v, *every* 2 *hours—hot drink, &c.*

29th—Slept well; bowels freely purged; now sweating freely; slight head-ache. *Continue salicine.*

30th—Found patient with fever rising; chill came on earlier than usual. *Ordered purg. enema; repeat salicine.*

31st—Feels very well; cool; sweating; hungry. *To continue salicine.*

June 1st. Feels very well; rather costive. *To continue salicine—ol. ricini at eve.*

2d. Missed chill yesterday, but had slight fever and head-ache about 1 o'clock P. M. Feels well now. *Continue salicine, grs.* v *every* 2 *hours.*

3d. Rested well, but does not feel very comfortable; threatened with a chill. *To take grs.* v *salicine every hour—hot tea—hot bricks to feet.*

4th. Has missed chill 3 days; convalescent.

5th. Discharged, cured.

Amount of salicine given, ʒ iiss.

Case V.—J. S., German laborer; has had chill and fever regularly every day for 12 days past; taken no medicine; entered Hospital, evening, May 30th.

May 31st. Found him cool and sweating; expects chill to day at one o'clock. *Ordered salicine grs.* v. *every hour—hot drinks, and hot bricks to feet.*

June 1st. Patient had chill as usual, on yesterday. This morning is cool and comfortable; taking grs. 5 salicine every hour. *Continue.*

2d. Had chill again yesterday; now cool; has taken grs. xv already this morning. *Ordered grs.* x *every hour until chill time; hot drinks, &c.*

3d. Patient had chill and fever yesterday, as usual. The salicine has had a fair trial, and we will now see the effects of quinine. It is now 9 A. M., patient cool and sweating; has already taken grs. xv salicine this morning. *Ordered grs.* v *quinine in solution, every hour until chill time—to-morrow morning grs.* v *every hour.*

4th. Patient had chill again yesterday; feels very well this morning. *To continue the quinine, grs.* v *every hour—sinapisms to the extremities —infusion of sage.*

5th. Feels better than usual; missed chill yesterday. *To continue the quinine—porter and full diet.*

6th. Missed chill. Discharged.

Remarks—Here we again witness the superior efficacy of quinine. About ʒ iiss of salicine was given before quinine was commenced. After this, there was but one chill. The amount of quinine used was about ℈ iiss.

Case VI—G. R., native of Scotland, aged 18, laborer, and fisherman on the lake, where he was seized with intermittent fever about the last of May. Had a chill and fever on two days in succession; then came to land, and took a dose of calomel and jalap, and some castor-oil. These changed the fever to a *tertian*, and it continued so until June the 5th, when he come to the Hospital.

June 6th. Saw him first; he is clear of fever this morning; has no pain; bowels open. *Ordered grs.* v *salicine every 2 hours.*

7th. Had chill as usual, yesterday; clear of fever this morning. Viewing the case as an obstinate one, resolved to combine piperine with the salicine. *Ordered salicine* ℈ ii, *piperine, grs.* x., *in* 12 *pills; take one every* 2 *hours, until* 2 *P. M.; afterwards, every hour.* (*His chill time was about* 7 *P. M.*)

8th. Feels better; no chill yesterday, being his well day; rested well last night; sweats freely; taken 11 pills since yesterday morning, 4 of them this morning. *Continue as yesterday.*

9th. Neglected prescription yesterday; took but one more pill; had chill, and spent bad night; is costive and has head-ache. *Ordered ol. ricini* ℥ iss; *continue pills sal. and pip. afterwards.*

10th. Feels very well. *Continue pills every hour.*

11th. Had slight chill and fever at 9 o'clock last night; sweated freely; has head-ache this morning. *Ordered blue mass grs.* x; *continue pills salicine and piperine.*

12th. Feels very well. *Continne sal. and pip.*

13th. No chill; convalescent.

14th. Discharged, cured.

Remarks. In this case 2 scruples of salicine alone had no effect.—He afterwards took about ʒ ii salicine, and ʒ ss piperine, combined, when the disease yielded.

Case VII. J. W. L. Irish laborer, aged 35; has had intermittent fever for 2 weeks; chill every day about 4 P. M.; has taken a dose of salts; and on the 8th June says he took at one time 25 cents worth of quinine, bought of an apothecary; entered Hospital the same day, and had a lighter chill than usual.

9th. 9 A. M. is cool and sweating; thirsty; costive; nausea. *Ordered salicine grs* 50, *piperine grs* x. *into* 12 *pills. Take one every hour; sinapised poultice to epigast.; apply sinapisms to extremities at* 2 *P. M.*

10th. Feels better; missed chill yesterday evening; is sweating profusely; quite weak. *To continue pills, and have brandy and water.*

11th. No chill yesterday; slept well; skin rather cool and clammy; no pain; thirsty; tongue clean. *Continue treatment.*

12th. Same as yesterday morning; had fever in the night; has taken 11 pills; *Ordered purg. enema, porter, and full diet.*

13th. No chill; feels well. *Continue porter and full diet.*

14. Had slight chill and considerable fever last night; apyrexia this morning. *Ordered grs* v *Salicine alone, every* 2 *hours, porter,* &c.

15th. No chill; feels pretty well. *Continue salicine.*

16. Convalescent.

Remarks. Amount of salicine given in this case, about ʒ ii ss; piperine about ʒ ss.

Case VIII. R. B. Irish mechanic, aged 45; entered Hospital, June 9th; has had a chill every morning at 8 o'clock, for three weeks; taken no medicine; has nausea during the chill; bowels open; has now just gotten out of a chill, and is sweating freely. *Ordered blue mass grs* v. *repeat at night, salicine grs* 50 *piperine grs* x. *into* 12 *pills. Take one every two hours from* 12 *to* 9 *this evening—one every hour in the morning.*

10th. Rested badly; disturbed by a hard dry cough; is now about to have chill. *Continue pills—have syrup morphiæ at bed time—apply sinapisms to extremities to-morrow morning.*

11th. Says he had very slight chill and fever yesterday; feels much better; no chill as yet; cough still troublesome; bowels too loose. *Ordered chalk mixture for bowels—continue sal. and pip.*

12th. Missed chill yesterday; cough very hard and dry; diarrhœa better; pulse quick. *Ordered brown antimonial mixture for cough; continue chalk mixture p. r. n.*

13th. No chill; the intermittent is cured.

Remarks. This man remained under treatment for cough and diarrhœa a week or two longer; but had no return of the intermittent, and was discharged, cured. He took salicine about ʒ ii, and piperine about ʒ ss.

Case IX. N. K. Irish laborer, aged 26; subject to intermittent fever for 3 months past; has had a chill regularly every day for the last week; entered Hospital June 9th; took nothing.

10th. Morning visit; patient is clear of fever, but has slight headache. *Ordered salicine grs* x, *every hour—hot infusion of sage—hot bricks to feet.*

11th. Had chill yesterday; apyrexia this morning. *Ordered to repeat the salicine as on yesterday—sinapisms to extremities before chill time—hot drinks, &c.*

12th. Had slight chill again yesterday in spite of every thing; this morning, cool and sweating. *Repeat remedies.*

13th. Chill again yesterday; apyrexia this morning, but complains of pain in the head and ears; sweating; tongue clean; bowels open. Salicine alone has had a fair trial, and *failed. Ordered salicine grs* 50, *piperine grs.* x. *into* 12 *pills—take one every hour until chill time.*

14th. Slight chill again; *continue pills, &c.*

15th. Chill again, patient quite debilitated. Salicine, both alone and combined with piperine, has had a fair trial; let us now see the effect of quinine. *Ordered grs* v *quinine every hour till three doses are taken; hot drinks, &c.*

16th. Slight chill and fever again, but 2 hours later than usual. The 15 grs of quinine made his head roar and ache considerably; feels pretty well this morning; slight headache; cool and sweating. *Ordered grs.* iiss *quinine every hour.*

17th. Feels very well; missed chill yesterday; took xv grs. quinine *Repeat quinine.*

18th. No chill; convalescent.

19th. Discharged, cured.

Remarks. This was a very obstinate case, in which the superior efficacy of quinine was strikingly shown after salicine and piperine had both failed. Amount salicine given alone, ʒ ii; sal. and pip. cómbined grs. 50, and x.—Quinine ℈ ii.

Case X. A. G. Irish labourer, aged 38, having had a chill for 3 days in succession, entered Hospital in the evening, June 13th; has taken nothing.

14th. Morning—find him cool, sweating, costive, has headache; expects chill at 3 P. M. *Ordered salicine grs* x. *every hour—4 doses. Purg. enema, hot drinks, &c.*

15th. Had severe chill yesterday evening, with violent headache, as usual. Apyrexia this morning. *The salacine to be repeated—ol. ricini at night.*

16th. Chill and fever again yesterday evening at 5 o'clock, 2 hours later than usual; bowels freely purged; now feels very well; cool and sweating. *Continue salicine* ℈ ii. *during the day.*

17th. Slight chill and fever again, but still later; headache; copious sweat; did not take the full amount of salicine; bowels too loose. *Ordered tinct opii, gutt.* xx. *Repeat salicine.*

18th. No chill last evening, though he suffered from headache. Feels better this morning; observed that his face, chest, and extremities, are thickly covered with small petechiæ; patient is certain they are not *musquito bites. Continue salicine, combined with a little Dover's powder and calomel.*

19th. Patient no better; had chill and fever again last night. Bowels very loose; skin clammy; very much debilitated; tongue covered with a moist dirty fur; pulse 80, and soft; petechiæ fading. Afraid to trust salicine any farther, on account of the critical condition of the patient. *Ordered, sulph. quinine, pulv. g. camph.* a. a. *grs.* x. *tinct. opii, gutt.* 40; *mucilage Acaciæ* ℥ ii. *Take a table spoonful every* 2 *or* 3 *hours.*

20th. Patient much better this morning; had no chill last night, but a return of the headache towards day. Bowels checked; skin warmer; not sweating so much. *Ordered the solution of Quinine alone,* ℥ iv.

21st. Patient convalescent; rested well, and has an appetite.

Remarks.—Here was a most obstinate intermittent, attended within violent headache, diarrhœa, excessive perspiration, and petechiæ. The salicine had a fair trial, and I am disposed to think the patient would have been lost, if more efficient remedies had not been resorted to. Amount of salicine given, upwards of ʒ ii. Amount of quinine about ℈ ii; of camphor, ℈ i. There was no chill after he commenced taking quinine.

Case XI. P. T. Irish labourer, aged 31; subject to attacks of intermittent fever for two months past; arrested several times by quinine; entered Hospital, after the morning visit. June 16th. Fever then going off. House Surgeon gave him a dose ol. ricini.

17th. Found him sweating freely; slight headache; thirst; bowels free. *Ordered salicine* ʒ ss, *in 3 doses—one every hour.*

18th. Chill again yesterday evening, though later, and lighter. Now sweating freely, and feels better; complains of pain in the eyes.

Repeat salicine.

19th. Chill again yesterday evening, and suffered very much with head; apyrexia this morning; nausea; has vomited twice. Concluded to *repeat salicine once more.*

20th. Missed chill; rested well; feels much better.

Repeat salicine.

21st. Rested well; has an appetite. *Convalescent.*

Amount of salicine used in this case, ʒ ii.

Case XII. J. A, Irish labourer, aged 30; was attacked with intermittent fever about the 10th June, whilst ditching on a plantation below the city; had a chill every day till June 17th, when he entered the Hospital. He had taken no medicine except a cathartic. He was at once put upon salicine; ℈ ii a day. In three days the paroxysms were completely broken up, without any other medicine; and on the 22d June, he was discharged, cured.

Amount of salecine given, ʒ ii.

Case XIII. D. H. Irish labourer, aged 25; was attacked with intermittent fever on the 15th June; had a paroxysm every day, took no medicine; entered Hospital.

25th. Found him cool and sweating; no pain; expects chill at 12, M.; bowels open. Ordered salicine ʒ ss, in 3 doses; one every hour; hot infusion of sage, &c.

26th. Had slight chill and fever yesterday, about 4 hours later than usual. This morning is cool and comfortable; tongue furred; no stool. *Repeat salicine; ol. ricini in the evening.*

27th. Was seized with vomiting yesterday evening at the chill hour; had fever afterwards; bowels freely moved without medicine. Feels pretty well this morning.

Repeat salicine.

28th. Feels much better; no chill or fever yesterday; has thirst; bowels loose. *Ordered iced gum water, and light nourishment.*

29th. Rested well; missed chill; *Convalescent.*

Remarks. The salicine seemed to do very well in the last two cases. *Amount used in last case,* ʒ iss.

Case XIV. J. McN. aged 25; entered Hospital on the 30th June; had a double tertian for 10 days past; taken nothing.

July 1st. Saw him first; clear of fever; no pain; costive; expects chill towards midnight. *Ordered ol. ricini immediately; salicine* ℈ i *at* 2 *P. M.—repeat at* 8 *P. M.*

2nd. Missed chill and fever last night; feels very well this morning. *Ordered salicine* ʒ *ss, at* 11 *A. M.*

3d. Missed paroxysm; feels well; asks for his discharge. *Am*[illegible] *salicine given* ℈ iiiss,

Case XV. J. B. Dutchman, aged 29; entered Hospital July 1st. had had chill and fever every day for the last three; taken nothing; has distressing pains in the limbs and joints; costive. *Ordered comp. pulv. jalap,* ʒ ii.

2nd. Chill yesterday evening; now clear of fever. *Ordered Salicine* ℈ ii, *to be taken during the day.*

3d. Missed chill yesterday; feels much better. *Ordered Salicine* ʒ i *to be taken at* 3 *doses.*

4th. Missed chill yesterday; convalescent.

5th. Discharged, cured. Amount of salacine ℈ v.

Case XVI. T. M. Irishman; entered Hospital July 3d.; has had chill and fever every day for five days past; taken two doses of calomel; now slightly ptyalised; costive; expects chill this evening. *Ordered purg. enema, and* ʒ *ss salicine at one dose.*

4th. Missed chill yesterday; feels better; still costive. *Ordered salicine* ℈ ii *ol. ricini.*

5th. No chill; rested well; convalescent.

7th. Discharged cured. *Amount salicine* ℈ iiiss.

Case XVII. J. A. Irishman, aged 30; entered Hospital July 9th. has had chill every day for 4 days past; also a troublesome cough.

10th. Saw him first, clear of fever. *Ordered salicine* ʒ ii, *in two doses; one immediately, the other late in the evening, Brown mixture for cough.*

11th. Missed chill; feels much better; *repeat medicine, of yesterday.*

12th. No chill; paroxysm completely broken.

Remarks. I here gave the salicine in *large doses*, and it did very well, in arresting the intermittent. The patient, however, continued to be t[illegible] some days with a hard dry cough that came on every night, and caused great pain in the head. It seemed to be of a nervous character, attended with but little expectoration. He was a pale anæmic subject. I afterwards gave him quinine, in 10 grain doses; then the *mistura ferri comp.*—porter, and generous diet, under which he entirely recovered. *He took* ʒ iv *salicine.*

Case XVIII.—M. H. Irish laborer, aged 27; has had intermittent, fever two weeks; first every day, then every other day; taken two doses of calomel in the time. Entered Hospital, June 9th. House Surgeon prescribed *ol. ricini.*

June 10th. Cool and clear of fever. *Ordered salicine grs.* v, *every* 2 *hours to-day; and every hour to-morrow morning.*

11th, Slight chill and fever yesterday; feels better than usual this morning; has neglected to take the medicine this morning; expects chill in 2 hours. *Ordered salicine grs* x, *immediately; again in an hour; hot infusion of sage; and hot bricks to feet.*

12th. Missed chill yesterday; feels very well this morning. *Continue Salicine.*

13th. Missed chill again; feels very well; has taken 15 grs salicine this morning. *Take 5 grs more.*

14th. Had slight chill again yesterday; now feels very well. *Repeat salicine.*

15th. Missed chill. *Repeat salicine.*

16th. Discharged, cured.

Amount of salicine taken, ʒ ii.

Case XIX. P. C. Irishman, aged 48; attacked with chill early in the morning, Aug. 4th; entered Hospital in the evening; took nothing.

Aug. 5th. Found him suffering with chill, accompanied by the usual symptoms; bowels open. *Ordered salicine* ʒ i, *to be taken at 6 P. M.—the same dose at 4 A. M. to-morrow morning.*

6th. Found patient with hot fever, but sweating; had chill 2 hours sooner than usual, notwithstanding the large dose of salicine. *Repeat salicine as before,* ʒ ii.

7th. Feels much better; missed chill, but had slight fever. *Repeat salicine* ʒ ii.

8th. Much better; missed paroxysm; costive. *Ordered gentle purgative; salicine* ℈ ii *this evening; the same early to-morrow morning.*

9th. Convalescent. *Porter and full diet.*

12th. Patient was placed on watch last night, and had another chill early this morning; fever now sweating off. *Ordered quinine* ℈ i, *this evening; the same early to-morrow morning.*

13th. Missed chill; convalescent.

15th. Discharged, cured.

Remarks. This man took salicine ʒ vii and ℈ i, *nearly an ounce.*—The intermittent was promptly arrested by it; but upon a relapse, it was *more promptly* arrested by ℈ ii of quinine.

Case XX. J. W. Irishman, aged 35; entered Hospital evening Aug. 4th; had chill every morning for three days; with severe pain in the head and back; taken nothing; house surgeon prescribed dose *ol. ricini.*

5th. Found him warm and sweating; tongue dry; thirsty; but little pain; bowels free; expects chill about 11 o'clock A. M. *Ordered salicine* ʒ iss; *one half immediately; the other early to-morrow morning; cups to nucha and loins, if pain returns.*

6th. Chill again yesterday, followed by severe pains in back and loins; was cupped; feels better; no pain; sweating freely. *Ordered salicine* ʒ i, *immediately, repeat to-morrow morning early.*

7th. Patient *much worse;* had a violent paroxysm yesterday at 12 M. and spent a wretched night; is very pale and weak; respiration somewhat hurried and laborious; restless; pulse frequent, small and weak; tongue dry; skin rather cool and clammy.

With symptoms so unfavorable, afraid to trust salicine any farther.—*Ordered quinine* ʒ ss *at once; sinapised poultice to epigast.; sinapisms to extremities; hot wine whey.*

Evening visit. Patient had missed paroxysm, and was much better, *Ordered quinine* ℈ i, *this evening; and* ʒ ss *at once early in the morning.*

8th. Quite easy; rested well; now disposed to sleep; tongue rather dry; some thirst; sweating. *Ordered quinine* ʒ ss *at once, stimulating friction to extremities.*

Evening. Patient worse; had chill at 11 A. M. and high fever afterwards; is now hot about the head and body; extremities cooler;

tongue brown, and perfectly dry in the centre; says he has no pain; disposed to sleep. *Ordered enemas of iced water, no 2; cold applications to head; quinine* ʒ ss, *by enema. at* 8 *P. M.;* ℈ i *in the morning.*

9th. Feels better; rested pretty well; tongue not so dry; skin moist; no pain. *Ordered quinine* ʒ ss. *at once; frictions and sinapisms to extremities.*

Evening.—Fever again, without any perceptible chill; skin hot; tongue dry. *Repeat enemas of iced water; cold water to head and chest; take* 5 *grs. quinine in the morning.*

10th. Patient no better; considerable fever; skin hot and dry; tongue brown and dry; bowels free and easy. *Ordered cold water to head and body; iced gum water to drink; no medicine.*

11th. Patient much improved; rested well; has slight fever; sweating freely; tongue not so dry; bowels easy. *Ordered infusion of chamomile; lemonade.*

12th. Has slight fever, but is sweating; tongue still rather dry in the centre; has thirst. Was seized in the night with hard dry cough and uneasiness in the chest. The cough has ceased this morning, but his respiration is rather hurried. He has an appetite, and took a few spoonsful of milk and mush this morning. *Ordered cold mucilaginous enemas; mucilage to drink; continue infus. chamomile.*

14th. Patient has continued to mend; now clear of fever, and hungry; convalescent. *Light nourishment.*

15th. Opthalmia attacked the right eye; on the second day an ulcer appeared on the cornea, and progressed rapidly in spite of local depletion, counter-irritation, and the nitrate of silver. He was sent to the eye ward, under the care of Dr. Mercier, where he entirely recovered.

Remarks. This was one of the most obstinate cases to be met with, and I think, would have undoubtedly terminated fatally under the use of salicine alone. It was trusted as far as prudence would justitify. He first took of Salicine ʒ iiiss; then of quinine ʒ iii; ℈ i. and grs v. An enormous quantity surely; yet I doubt whether the patient would have been saved by any less.

Thus have I given careful observations, of the effects of salicine in twenty cases of intermittent fever, taken promiscuously, and all occurring within a short period. I had commenced its use in two other cases, and would willingly have reported a much larger number, but desisted on account of *the expense* of the remedy. Perhaps no place in the world presents greater advantages for observations upon this disease than the N. O. Charity Hospital. It is very tedious either to prepare or to examine reports of this kind; but if faithfully executed, they offer the best medium of information, both in regard to the effects of remedies, and the nature and progress of disease. It is hoped that these twenty cases will prove in some degree instructive. Let us sum up their results, and see what conclusions they will authorize. Although these statistics may not be mathematically exact, they are sufficiently so for our purpose.

Total amount of medicine given in the 20 cases. salicine ℥ vii, ʒ vi, ℈ ii; piperine, ʒ i ℈ ii; quinine ʒ iv.

Largest amount given in any case; salicine, ʒ vii; piperine, ʒ iss; quinine, ʒ iii, ℈ i.

Smallest amount given in any case; salicine, ʒ i, grs x; piperine, grs. xii; quinine, ℈ ii.

Average amount of salicine to each patient ʒ iii, grs viii.
It was first tried alone, in all the 20 cases.
It succeeded when given alone, in 11 cases.
It failed when given alone, in 9 cases.
Of these, it succeeded when combined with piperine, in 4 cases,
Quinine had to be resorted to in 6 cases.

Average time of sickness previous to treatment, $7\frac{1}{2}$ days.
Do " " under treatment $6\frac{1}{2}$ days.
Greatest number of paroxysms, after salicine was commenced, 6 days,
Smallest " " " " " 0 "
Average " " " " " 9 "
There were one or more paroxysms after salicine was commenced, in 14 cases.
There were none, in 6 cases.

I am informed by the Apothecary of the Hospital that the cost of salicine was \$2 00 per oz. Consequently, the value of the amount used in these 20 cases, (say $7\frac{3}{4}$ oz.,) was about \$15 50; and of the average amount given to each patient, (say ʒ iii,) 75 cents. The article appeared to be fresh and genuine.

I prescribed it in the forms of solution, powder and pill; in doses varying from *five grains, to one drachm;* and at intervals of from one hour to twelve.

The general effects of the remedy appeared to be *tonic and diaphoretic.* The appetite and strength were generally improved, and the sweating was profuse. I observed *no unpleasant effect* that I could attribute to the remedy. Where it failed *to do good, it did no harm.*

Comparative statement of 20 *cases of Intermittent Fever treated with the sulphate of quinine.*

After the foregoing observations on the use of salicine were completed, I resolved to note 20 cases of intermittent fever treated mainly with quinine; with the view of ascertaining the relative *efficacy* and *cost* of the two remedies. Twenty recent admissions were taken throughout the wards of the Hospital, and of course under the care of different physicians. Upon inquiry I found that no two of them administer the remedy alike; some of them prescribe it in large doses, and at long intervals; others, the reverse; some give it alone; others, in combination with blue mass, opium, or morphia. As minute notes were taken of these 20 cases, as of the preceding; but for fear of wearying the reader, I will only give the *results*, and the conclusion to which they brought me.

Whole amount of quinine used in the 20 cases, about ℥ iss.
Largest amount given in any case, ʒ i.
Smallest amount given in any one case, grs. xviii.
Average amount of quinine given to each patient, grs. xxxvi.
It was given combined with sulph. morphia, grs. xii, to gr. $\frac{1}{2}$, in 2 cases.
" " with ext. opii, grs. xviii, to gr. i, in 4 cases.
" " with blue mass; grs. vi, to grs. x, in 1 case.
" " alone in 13 cases.
All the cases were promptly cured.
Average time of sickness before admission; 10 days.
" " after 4 "

Greatest number of paroxysms in any case after taking quinine, 2 days.
Smallest " " " " " " 0 "
Average number of paroxysms, 3|5 "
There were one or more paroxysms after taking quinine, in 10 cases.
There was none, in 10 cases.

The cost of quinine was $3 25 pr. oz, consequently the value of the whole amount used in these 20 cases, [say ½ oz.] was $4 87 ½, and of the average amount, (say 36 grains,) about 25 cents.

Candour compels me to state that the cases treated by salicine were generally more severe, than those treated by quinine; it will be recollected that one of them was so malignant as to be with difficulty saved by upwards of ʒ iii of quinine, after having previously taken ʒ iiss of salicine. The salicine cases occurred chiefly in the months of June and July, when intermittents usually assume their worst form; the quinine cases all occurred about the first of October, when the disease is generally mildest. These circumstances are worthy of grave consideration, lest we be enduced to underrate the actual virtues of salicine. However, taking the two sets of cases as they are presented to us in the foregoing comparative statement, and reviewing the effects of the two remedies in their various combinations beforementioned, we are brought to the conclusion that *the average amount of quinine required to cure* 20 *cases of intermittent fever, and costing* 25 *cents, is fully three times as efficacious as the average amount of salicine required in a like number of cases, and costing* 75 *cents.*

The comparison made in this instance cannot however be considered a perfectly fair one; but when the foregoing reports are taken in conjunction with others that will doubtless be made from the medical department of the Army, they may aid in leading us to a proper estimate of the virtues of salicine. How many of the foregoing cases would have had their paroxysms broken up merely by *the change of residence, and attention to regimen, without any medicine whatever,* must remain a matter of conjecture. My own opinion is, there would have been a goodly number. Hence the importance of exercising a sound judgment and careful observation in regard to the action and comparative value of medicines. E. D. F.

Oct. 17, 1845.

MORTALITY OF NEW ORLEANS.

We gave in our last number the number of deaths in August, up to the 15th. The whole number for that month was 251—classified by Dr. Lewis, Secretary of the Board of Health, as follows, viz.:

White Male Adults	White Female Adults	Colored Male Adults	Colored Female Adults	White Male children	White Female children	Colored Male children	Colored Female children
85.	31.	18.	14.	37.	31.	17.	18.

List of DEATHS and DISEASES in the City of New Orleans, during the month of September, 1845, *viz.,*

September.— Fevers, Yellow* 1; Bilious Remitttent, 2; Pernicious Intermittent, 4; Congestive, 5; Typhoid, 12; Adynamia, 2; Puerperal, 1. Fracture of the Cranium, 3; Apoplexy, 3; Meningitis, 6; Hydrocephalus, 1; Cerebral Congestion, 4; Ulceration of the Mouth, 1; Angina Maligna, 1; Bronchitis, 3;

Croup, 1; Pertussis, 1; Asthma, 1; Apoplexy of the Lungs, 1; Pnuemonia, 3; Phthisis Pulmonalis, 27; Hypertrophy of the Heart, 2; Disease of the Heart, not specified, 3; Ramollissement of the Stomach, 1; Duodenitis, 1; Gastro-Enteritis, 8; Enteritis, 3; Ulceration of the Intestines, 2; Diarrhœa, 9; Cholera, 3; Volvulus, 1; Dysentery, 7; Vermes, 2; Cancer Recti, 1; Diseased Liver, 1; Peritonitis, 2; Ascites, 1; Hypertrophy of the Liver, 1; Metritis, 1; Stricture of the Urethra, 2; Chorea, 1; Trismus Nascentium, 16; Tetanus, 5; Convulsions, 8; Epilepsy, 1; Delirium Tremens, 5; Paralysis, 1; Scarlatina, 13; Anasarca. 1; General Dropsy, 2; Anemia, 1; Marasmus, 4; Dentition, 3; Congenital Debility, 2; Intemperance, 4; Luxation, not specified, 1; Drowned, 5; Gangrene, not specified, 1; Old Age, 1; Still Born, 15; Unknown, 16. TOTAL, 238, which Dr. Lewis classifies as follows:

White Male Adults	White Female Adults	Colored Male Adults	Colored Female Adults	White Male children	White Female children	Colored Male children	Colored Female children
87.	24.	12.	16.	41.	30.	12.	17.

* Questionable.

ABSTRACT OF A METEOROLOGICAL JOURNAL FOR 1845.

By D. T. Lillie, at the City of New-Orleans

Lat. 29° 57′ Lon. 90° 7′ west of Greenwich.

1845. weekly.	Thermometer. MAX.	Thermometer. MIN.	Thermometer. RANGE.	Barometer. MAX.	Barometer. MIN.	Barometer. RANGE.	COURSE OF WINDS.	Force of wind in ratio 1 to 10.	RAINY DAYS.	QUANT. OF RAIN. INCHES.	QUANT. OF RAIN. HUNDREDTHS.
1845.											
Aug. 16	89.7	72.0	17.7	30.20	30.12	.08	S.W.	2	3	1	377
23	92.5	76.0	16.5	30.22	29.96	.26	N.	$1\frac{1}{2}$	2	0	188
30	89.0	74.7	14.3	30.16	29.96	.20	S. S.W	$2\frac{1}{4}$	2	1	810
Sep. 6	86.5	71.0	15.5	30.08	30.00	.08	W.S.W	$1\frac{1}{2}$	5	3	021
13	87.5	74.0	13.5	30.16	30.08	.08	N. N.W	2	3	0	429
20	85.5	70.5	15.0	30.15	29.90	.25	N.	2	1	0	930
27	83.0	63.0	20.0	30.14	29.86	.28	N. E.	3	0	0	000
Oct. 4	83.7	64.0	19.7	30.13	29.98	.15	N. E.	$2\frac{1}{2}$	3	1	040
11	77.5	51.5	26.0	30.16	29.93	.23	S .E.	$1\frac{3}{4}$	6	3	715
18	71.0	54.0	17.0	30.37	30.14	.23	N.	$2\frac{1}{2}$	0	0	000

REMARKS.—The Thermometer used for these Observations is not attached to the Barometer, but is a self-registering one, and is placed in a fair exposure. Regular hours of observation, 8 A. M., 2 P. M., and 8 P. M.

The Barometer is located at an elevation of 19 feet above the level of the ocean, and is suspended clear of the wall of the building.

The Rain Guage is graduated to the thousandth part of an inch, and the receiver is elevated 40 feet from the ground.

[illegible]

THE NEW ORLEANS

MEDICAL AND SURGICAL JOURNAL.

JANUARY, 1846.

Part First.

ORIGINAL COMMUNICATIONS.

ART. I.—*Practical Observations on Chronic Diseases of the Uterus; with Cases.* By H. J. HOLMES, M. D., of Hinds county, Mississippi.

The many diseases to which the uterus is subject, especially in the South, have in a very great measure been overlooked, and, until recently, have escaped the observation of most medical men in the country. No matter what the disease may have been, the general terms of schirrus, cancer, and prolapsus uteri, have been indiscriminately applied; and without having a correct knowledge of the pathology of the diseases, a large portion of our treatment has been more or less coupled with empiricism. We are very much indebted to Duparque, Lisfranc, and Ashwell for their invaluable works upon the pathology and treatment of diseases of the uterus. They have presented to the profession a surer and better method of treating these diseases, by the use of the speculum; and, by the great number of cases and cures given in detail, have convinced the faculty, (or the greater portion of them), of the efficacy of this mode of treatment over the old plan, still adopted by many—a plan which, I am convinced, is often useless, and has done more harm than the disease itself. Without the aid of the speculum, it is impossible to form a correct diagnosis; and the remedies must, in a great degree, be inappropriate and uncertain.

Being thoroughly convinced of the many advantages the modern mode of treatment possessed over my former mode, I commenced an infirmary at Spring Ridge, Hinds county, Miss., in the spring of 1842. At first I met with but little success in procuring patients, on the score of prejudice, and opposition of the faculty. It was not many weeks before I had the management of some cases that were considered hopeless. They came to me as the only alternative, with very little confidence of being relieved. An examination being made with the speculum in the usual way, I at once detected the seat and nature of the disease, and informed the patients of the fact. These cases treated by the plan which I will mention at a proper time, being successful, others, that were equally as hopeless, came also; and the great number cured has in a very great measure removed the prejudice against me, and I hope against the practice also. Many of our leading physicians have provided themselves with a suitable instrument, and are ready to investigate these diseases by the only plan which can be successful. Having attended upwards of one hundred and thirty cases at my Infirmary and its vicinity, the opinions which I have formed in regard to the pathology and treatment of diseases of the uterus will vary in some degree from the very distinguished authors from whom I shall quote.

The histories of some of the cases which I have attended are quite interesting, and I hope will prove equally so to the faculty.

The diseases to which I propose calling attention are leucorrhœa in its various forms, engorgements, ulceration—simple and fissured, dysmenorrhœa, prolapsus uteri, and menorrhagia accompanied with ulceration.

In giving a description of leucorrhœa, my remarks will be confined principally to discharges from married females. With all females there is a sufficient amount of mucus thrown off by the follicles, which is intended to lubricate the parts. When there is an abundance of this, it is termed leucorrhœa. In every form of leucorrhœa, and in most of the circumstances in which it occurs, it will be dependant either upon functional, inflammatory, or some organic disease of the uterus. If it be dependant upon congestion, or acute metritis, the discharges will consist of a transparent glairy fluid; if from chronic metritis, it will be uniformly of a puruloid character. These discharges have been, and are still, treated generally, without knowing from whence they proceed, or upon what pathological state of the uterus they may depend. Hence our frequent failures in treating them. An inspection of the os tincæ enables us to distinguish its special causes, and the speculum shows how far it extends towards the uterine orifice, whence this product is seen to flow when the inflammation affects the mucous membrane of the uterine cavity.

Almost all authors upon female diseases have yielded to the opinion that this discharge is dependant upon debility, either constitutional or local; and to this theory alone may be attributed the many failures by tonics and astringent washes. The more recent writers upon the subject have established the fact that leucorrhœa, either vaginal or uterine, is dependant upon inflammation, either of the neck or mucous membrane lining the cavity of the womb. The object of this paper is to add my humble testimony to this fact, which I shall be able to do by well attested cases. I propose to divide the discharges into two classes only.

1st. Those which are dependant upon simple congestion or engorgement of the uterus.

2d. Those which are dependant upon ulceration of the mucous membrane upon the neck or within the cavity of the uterus.

Sanguine engorgement of the uterus is the result of various determining causes, and it consists in the accumulation of a greater quantity of blood in the vascular system or parenchymatous structure of the organ than usual.

The plethora does not always constitute a state of disease; it exists, for example at every menstrual period, and is then a part of a genuine physiological action. It is also observable after parturition, and is essential to gestation. When, however, it occurs at any other periods than those normal epochs which we just mentioned, or when it transcends certain limits in intensity or duration, it becomes a genuine pathological state, and demands attention. Every congestion of the uterus consists of two stages; an active, during which the blood is determined to the organ in greater quantity than usual; and a passive, in which the blood, being already accumulated in the tissues, is retained there.

Engorgements of the uterus, besides their spontaneous development at the menstrual periods, and after delivery, are produced by numerous causes, which may be arrayed into two classes. The first acts by exciting a flow of blood to the parts; such are violent moral emotions, or exercise and general stimulants, to which we may add certain emmenagogues, and the excitants proper to the genital organs, as coition and masturbation; the second comprehends such causes as operate upon the exhalent vessels of the uterus, to prevent that local depletion which in normal cases is the crisis of congestion. It is thus, for example, that cold, lively moral impressions, and the intemperate use of astringents act during menstruation, and subsequent to accouchement. In the first instance, the menses are suppressed; in the latter, the lochia; while in both the persistence of the fluxion coöperates to continue or increase the congestion.

The tendency of the engorgements present at the menstrual periods, and which follow accouchement, is to enlarge the uterus, if they be not relieved by the occurrence of the discharges which are usual at these epochs. It nevertheless happens in the former instance that many repetitions of the menstrual period are requisite before any permanent congestion takes place; the blood, which is urged by the fluxionary movement rapidly passing into the general circulatory system upon the failure of this primary condition of menstruation.

Of this spontaneous resolution of the uterine plethora, we have frequent examples in the phenomena immediately antecedent to puberty. There is at this time a periodical afflux to the uterus, with such a condition of the exhalent or secretory apparatus as prevents the escape of the fluid which is destined, in the economy of nature, to unload the vessels. Yet no disease ensues, until by repeated sanguine determinations, without equivalent discharge, the organ becomes the seat of permanent congestion.

These engorgements require attention, for if not attended with all of the inconveniences of displacements, &c., they are apt to terminate in inflammation, and the most serious organic alterations. The simplest form of leucorrhœa is the result of hyperæmia, or vascular congestion. The discharge in its incipient stage is opaque, glairy or transparent mucus; producing but little inconvenience until the system becoming involved, the attention of the patient is directed to her condition, and for the first time she seeks relief. The appearance of the uterus at this time is that of a deep crimson colour. The speculum, or any other substance, coming in contact with it, presses the blood out of the capillaries, which returns after this pressure is removed.

The symptoms that accompany congestion of this class are less severe than those of metritis. They are swelling, tension, and a sense of weight about the pelvis; lumbar, sacral, and inguinal pains; the pains more or less prolonged, and frequently repeated, during which the uterus seems to contract itself violently as if to express the blood which has engorged it. Peculiar pains called colics or cramps of the uterus, tenesmus, or severe griping, also occur; and these pains are sometimes so extremely violent, that the patient bends forward during their continuance. In the mean time, pressure, as well as the vaginal touch, prove the insensibility of the engorged parts, at least in the interval of the spasms or gripings. The reverse of this is the case with engorgements by inflammation, which is the seat of these violent pains, and of a constant sensibility. Duparque says the immediate results of these engorgements are,

1st. To increase the weight of the uterus, and cause displacements.

2d. To become an obstacle to the return or future establishment of the menstrual periods.

These engorgements readily pass into chronic inflammations, and, if not relieved, into the most profound organic transformations.

I proceed now to detail some of the cases that have come under my care.

Case 1.—Mrs. ———, aged 24, of nervous sanguine temperament, full habit, fair complexion, black hair and eyes, short and thick neck; menstruated at the age of sixteen; at the third or fourth period she got her feet wet, which, for the time, produced but little derangement during that period. An interval then of eleven months occurred before its return; but after its second appearance, continued healthy for five months. She married in December, 1837, and three days afterwards started to this state on horseback. Her period made its appearance while travelling, and continued six days. She experienced no inconvenience from it while riding. The succeeding period came on attended with a sense of weight and tenderness of the abdomen, together with pain in the back and hips. The discharge was more profuse than at any time before, though it did not continue more than the usual length of time, six days. Her situation continued thus for twelve months, when a change of color and consistence was perceived, it being not so free, but coagulated. She remained in this situation up to the winter of 1839; for relief she was accustomed to use warm baths and teas. In the commencement of 1840, a white milky discharge made its appearance, which would decrease in quantity as each period approached. In 1842, the discharge assumed a yellowish and creamy appearance. In the spring of 1845, she had frequent hysterical paroxysms.

She came to my Infirmary the 10th day of June. An examination being made with the speculum in the usual way, the uterus was discovered to be very much engorged, with ulceration of the glandular follicles; the neck of the uterus very much elongated; the whole mucous membrane of the vagina highly injected, with patches of ulceration in various parts of it. The vagina was besides coated with a thick discharge of a puriform character. The introduction of the speculum produced considerable heat, and a burning sensation in the vagina. She had not enjoyed the marriage privileges for many months on account of pain; she rests and sleeps badly. An effort was made to pass a small sized probe, but the os tincæ being so much contracted, I did not succeed in passing it up to the first contraction of the neck. She had frequent palpitations and deep-seated pain under the left mamma; the fifth, sixth, seventh, eighth, and ninth dorsal vertebræ were very sensitive; pressure over the second, third, and fourth lumbar vertebræ gave pain in the uterus.

Treatment.—I bled Mrs. ——— 16 oz., and gave a brisk cathartic. The next day applied six large leeches to the os tincæ. Leeches were applied every other day for one month; in the mean time she took one of the following pills every night;

℞ Ext. comp. colocynth. ʒ i.
Ext. rhei. ℈ i.
Blue mass. ʒ i.
Aloes ʒ i.
Divide into 100 pills.

At the same time the following wash was used three or four times a-day: ½ drachm tinct. iodine in two ounces of water, thrown into the vagina at each time, and retained for a few minutes. At this time her period came on; the discharge was free, healthy and without pain. Blisters were applied to the sensitive dorsal, and to the lumbar vertebræ, and sacrum alternately every sixth day. On the 20th of July, uterus soft and pliable; not sensitive; the speculum passing for the first time without pain; rests and sleeps well; no pain in the chest or heart; commenced using the first and second sized probes—passed them readily to the first contraction without pain—in passing the probes through this contraction gave pain in the back and hips,

Commenced the use of the following wash, half drachm nitric acid, to ℥ xxviii water; syringeful three times a day.

22d July. Passed the third sized probe above the contraction, but produced considerable pain, for ten or fifteen minutes.

24th. Uterus more healthy in appearance; discharge more thin and transparent; ulcers upon the vagina entirely healed; mucous membrane of the vagina free from congestion; and assuming a healthy appearance.

26th. Notwithstanding she has been leeched every other day, excepting the time of her periods, she has gained flesh and strength, and to day complains of pain in the heart with some symptoms of hysteria, caused probably by the fatigue induced by walking too much yesterday.

Bled her ℥ xvi, which relieved the full sensation in her head, and pain in the heart.

28th. Passed the probes to the fundus of the uterus; gave pain in the hips and back; leeches applied; and nitrate of silver used for the first time in Lallemand's porte-caustique. Gave pain in the abdomen, with a sense of throbbing in the uterus.

30th. Applied leeches; and painted the neck of the uterus with tinct. iodine; examined by touch; uterus still smaller, the elongation having passed off; not sensitive when touched. I omitted to say that I had painted the neck of the uterus with the tincture of iodine every time I attended her, and I may add, that it is one of the best resolvents for engorged uteri; of this, however, I will speak at a proper time.

August 1st. Leeched; applied the caustic; used the iodine; the discharge ceasing, and becoming more transparent and tenacious.

2d. Applied the usual number of leeches; uterus entirely healthy in appearance; patient free from any unpleasant feeling about the uterus; discharge is a little tinged with blood.

4th. Leeched; used the caustic for the last time.

She was leeched up to the 20th of August; at which time she was free from all disease, her periods coming on regularly and without pain; and she remarked that she felt like a new being. From this date up to the time of her departure, I leeched her occasionally.

I have recently received a letter from this lady, in which she informs me that her health is entirely restored.

Remarks. It will be perceived that, this lady had simple engorgement for two years and a half at least; at which time it passed to the second stage. Then a white milky discharge made its appearace, which continued for two years. Chronic inflammation having been formed in the mean time, ulceration was the effect of it, and this last, the cause of the yellowish and creamy discharge.

Case 2.—Mrs ———, aged 29; dark hair and eyes; of leucophlegmatic temperament; had enjoyed fine health until some time in the year 1842. One day, while visiting a neighbor, was caught in a severe shower during her period. That evening and night, complained of headache, pain in the back and hips, with suppression of the catamenia. She was bled, purged, warm teas and fomentations given and applied, which afforded temporary relief for the time being. A few days before her next period, she complained of pain and soreness in the mammary glands; pains in the back, and tenderness of the thighs. Warm baths and teas, and a preparation of the compound tincture of guiacum were given freely. These remedies established the discharge partially, but there was great suffering at the catamenial period. In the spring of 1844, she visited me; complained of a sense of weight in the pelvis, much tenderness of the abdomen; cold extremities, head and ears; with more or less palpitations constantly; great determinations to the head, with flushing of the face. Upon examination by the speculum, the uterus and vagina were found highly engorged, and irritable; the follicles very much enlarged, and seen all over each labium of the uterus. No ulceration to be seen; uterus hard to the touch, and neck elongated; the os tincæ and canal sufficiently open to admit a fourth sized probe; discharge thin and transparent, scarcely staining the linen; the whole spine more or less sensitive. This lady was treated upon the same plan as detailed in case No 1. Her period came in the second week after her arrival, with less pain. Extremities cold, with soreness of the mammæ. The discharge continued four days. The leeches and caustic were used every third day, together with the pills and wash, up to the next period. The menstrual secretion at this time was free, healthy, and consistent; the mammary glands were free from soreness; pain very slight in the back and hips. From this time a very decided change began to manifest itself in her complexion; appetite and strength. The uterus and vagina assumed a more healthy colour, free from discharge; and when the third period came on, all unpleasant feelings of weight, soreness, and tenderness, had disappeared, and to her astonishment the catemenia made its appearance without a single premonitory symptom, and without her knowledge. She returned home the twelfth week after her arrival, entirely cured.

Remarks. I saw Mrs ———, the 24th June, 1845. There had been no return of her disease ; complexion very fine and rosy; weighed 125 lbs; said her health was never better.

CASE 3.—Mrs ———, aged 18 ; of sandy hair, hazle eyes, of sanguine temperament ; full plethoric habit ; while returning from a ball in the fall of 1842, became very chilly, and by the time she reached home had a severe chill of near two hours' length. This was succeeded by a slight fever, which continued until next morning ; the symptoms being headache, pain in the back and hips.

The great exercise that she underwent in dancing, and the sudden change of air from a heated room to a chilly atmosphere, gave her the chill, and arrested the catamenial discharge on the fifth day. The usual remedies were prescribed by the physician called in, which afforded relief. The next period came on with pain, restlessness and headache ; the discharge for the first day bring very scanty, but the balance of the period more profuse. She had several periods in like manner.—She had taken large quantities of compound tincture of guiacum during the periods, and in the intervals had taken large quantities of balsam copaivæ, and tincture of cantharides. In the spring of 1843, her health being very delicate, she was advised to visit me. In our conversation she complained of a dragging sensation in the pelvis ; pain in the hips and thighs ; strangury, with great irritation of the os externum.

Appearance of the uterus. The cryptæ or mucous glands very much enlarged ; vagina very red and inflamed ; white discharge ; great pain in introducing the speculum.

There was also great determinations of blood to the head, with a burning sensation in the cerebellum and pericranium. She had worn a pessary for many months, to her great injury.

Treatment.—Bled her ℥ xvi ; applied next day six leeches to the uterus; profuse hæmorrhage after the leeches had dropped off, to arrest which cold water injections, and sponging the abdomen were resorted to. Two of the purgative pills were administered, which produced several operations of a bilious character. Every second day afterwards the same number of leeches were applied ; but little hæmorrhage occurred after the first leeching. Wash (half drachm of the tincture of iodine to two ounces of water) used three times a day. This plan of treatment was continued until her period came on. The period made its appearance with but little pain ; discharge sufficiently free, with fewer unpleasant feelings.—Six weeks elapsed before the second period, during which she passed the time more comfortably than the first. After this passed off, an examination being made by the touch, the uterus was discovered to be less sensitive, and a good deal reduced ; soft, and pliable. After this time the leeches were applied every third and fourth day ; the uterus being painted, every time I attended her, with the tincture of iodine. The uterus being now in a condition for the probes, I commenced the use of them after each leeching. I found it very difficult to pass the smallest probe more than half an inch for several times. Finally, the contraction was broken up, and the largest sized probe would pass readily to the fundus of the uterus. The caustic was applied in the same way by Lallemand's porte-caustique, for three weeks, by the use of which the discharge ceased from the cavity of the uterus altogether. When the third period came on, she was free of any pain or unpleasant feeling whatever; and the discharge was healthy in consistence and quantity. The appearance of the uterus continued to improve; free from sensibility; soft and pliable; no secretion. Discharged, cured. Three months after her return home, she discovered some return of the disease. The periods were regular, but accompanied with some pain at each return. On her return to my Infirmary, she complained of soreness of the uterus upon touch; great pain in the act of coition, (as I learned from her husband. The uterus presented a very highly inflamed surface; mucous membrane of the vagina red throughout; slight discharge of a transparent colour. The same means were resorted to again; to wit, leeching, wash, pills, iodine, and caustic. She remained six weeks, at which time she was again relieved of all disease. Two months after her departure from my Infirmary, conception took place; she passed the full time of gestation, and is now the mother of a fine son in good health, and bids fair to have any number.

Remarks. The irritation of the uterus and vagina was, in the second instance, brought about by an excess in venery. This her husband admitted. For the first time since their marriage, pleasant sensations were produced in the act of coition, which led to too great an indulgence of it. I ought to have remarked that in the treatment of this case, she was blistered alternately at the small of the back, and between the shoulders.

I pass now to the second variety of leucorrhœa, dependant upon hard engorgement, with ulceration, either within the vagina, upon one or both labia, or within the canal and cavity of the uterus. Dr. Buel's description of hard engorgement being so very accurate, and coinciding so much with my own experience and observation, I cannot do better than to quote his description of it. He says:

" This form of disease, which is extensively prevalent, and the cause of so much suffering, has until of late years been overlooked or misunderstood. That it is a very frequent cause of a train of chronic affections, which a great number of females in young and middle life suffer under, cannot be questioned. Nothing is more common than to find ladies who have been a few years married, complaining of pain and weakness in the back, attended with more or less leucorrhœal discharge, inability to stand for any length of time, or to walk any distance without suffering sometimes severely, together with general debility and feebleness. It is, perhaps, impossible to recognize fully and discriminately the nature of the affection, without the use of the speculum. A practised finger might, indeed, recognise it by touch alone, but to become thoroughly acquainted with it, it is necessary that it should be seen. From notions of delicacy, (a false delicacy it must be considered,) many females suffer for years, without disclosing their condition to any practitioner; or if a physician is consulted, the symptoms are often treated without a resort to the only means by which their real nature can be ascertained and understood. Remedies are directed to relieve the debility, or check the discharges, without an attempt to strike at the root, by a removal of the cause. The symptoms which lead us to suspect the existence of engorgement of the cervix, are 1st, pain; this is referred most frequently to the back, about the region of the lumbar vertebræ, and sacrum; pain in the hypogastric and pubic regions. It frequently affects the hips, and extends down the thighs in the track of the crural nerves, which are affected both by sympathy, and by pressure produced by the engorged uterus. A sense of weight in the uterus; they describe this as a sense of dragging, of bearing down as if something were about to escape. They are frequently impressed with the belief that they have a falling of the womb. Not unfrequently, when a physician is consulted, relying upon the opinion of the lady alone, a pessary is introduced to relieve the supposed prolapsus, which only aggravates the symptoms. Strangury, leucorrhœa, and tenesmus, are symptoms which accompany this disease. A generally impaired state of health is a frequent symptom of engorgement of the cervix. Muscular debility, loss of appetite, paleness, lassitude, sense of fatigue at the most trifling exertion, and all that nameless train of symptoms which usually accompany what is termed feeble health in females, is a common result of this affection. A large class of married females, particularly in cities, suffer under this train of symptoms for years without being aware of the true cause; or, if so, from motives of delicacy never disclose their sufferings. The causes of engorgement of the cervix uteri are various. A sedentary mode of life, neglect of exercise, a plethoric condition of the system generally, and all causes which act unfavorably upon the general health, may become causes of engorgement of the cervix. The proximate or predisposing causes are, its peculiar situation in the body, its dependent condition, the abundant supply of blood sent to it; the shocks to which it is exposed in parturition, in coition, &c., sufficiently account for its being frequently the seat of morbid action."

The intention of this paper is partly to prove, that this variety of the discharge is never seen in any of the various affections of the uterns, without granular erosions, or ulceration. I am thus minute, that I may call the attention of the profession to that particular character ot the discharge as indicating enlarged follicles, ulceration of the vagina along the canal, and within the cavity of the uterus. Dr. Buel's report of the diseases ot females in the New York Dispensary, was published in the January number (1844,) of the *American Journal of Medical Sciences.* His cases of simple engorgement gave the same colour and consistence to the discharge as I have mentioned, to wit: in his first case he does not mention that she had ulceration, but that she had abundant leucorrhœa; second case, profuse white discharge; third case, the same; fourth case, under the head of granular engorgement, he mentions the discharge as being of a profuse white or yellow color; eighth case, of ulcerative engorgement; says the discharge was at first yellow, afterwards white. Ninth case, presented a large ulcer on the inner surface of the anterior lip, a yellowish discharge from the cervix, and adjacent parts of the vagina. Tenth case, granular erosions; complains of pain and weight in the hypogastrium; has a constant discharge of yellowish matter.

In examining a very able and interesting account upon the pathology and treatment of leucorrhœa, by William C. Roberts, published in the May and July numbers of the New York Journal of Medicine, it will be seen that of fifty-nine cases reported by him, the discharge in all, with one or two exceptions, was of a yellowish and creamy appearance.

Ulceration or enlargement of the cryptæ or glands was detected by the speculum in every case. All mucous membranes when in a state of congestion, irritation or inflammation, secrete a peculiar discharge according to the extent of the inflammatory engorgement. When there is a great determination of blood to any particular organ, the blood-vessels become engorged. Nature, to relieve itself, adopts this method of throwing off a secretion. From the uterus and vagina, the discharge is generally of a starch-like or opaque mucus; when the inflammation is of a more chronic form, we see the discharge assuming a different color, especially when from the cavity of the uterus. It is more thick, and sometimes gelatinous; the chronic inflammation, having continued so long, and the irritation being so great, that the cryptæ or glands do not escape the ulcerative absorption that is going on in the part; and hence the amount of ulceration that we see in the canal and upon the neck of each labium of the uterus. Leucorrhœa is not then a disease of itself, but dependant upon either a functional, inflammatory, or organic disease of the uterus. It may be, and sometimes is, symptomatic, but rarely. In all the cases I have witnessed, I am thoroughly convinced of one fact, that when the discharge is either of a milky or starch-like appearance, or even transparent, and possessing some degree of tenacity, that it is either dependant upon some irritation, congestion, or engorgement; and when it assumes a greenish or yellowish appearance, we may be equally certain that there is some enlargement of the follicles or cryptæ, or that ulceration exists either within the canal, or embracing the os, and the neck of the uterus.

Physicians should be very particular in their examination and enquiry of the patient as to the nature and colour of the discharge; for if they bear in mind the color and appearance, alone, they may at once determine the fact so far as ulceration is concerned.

Frequently, in my conversations with ladies, when they have presented themselves at my infirmary for treatment, I have been able to inform them of there being an ulcer present by their description of the discharge alone. Physicians have been, and are still, divided as regards the seat of leucorrhœa in its incipient stage. I can only add my testimony of its being seen passing directly from the os tincæ in nineteen-twentieths of the cases I have attended. But two cases have occurred in my practice where I found it otherwise; those were severe cases of vaginitis, produced, no doubt, and kept up by excessive venery. In bad cases of leucorrhœa, with or without ulceration of either labium, the mucus which bathes the neck, and looks thin, yellowish or greenish, and very much like pus, when accumulated in quantity at the bottom of the vagina, may be seen to adhere to that which is still lying in the canal, and extending within the uterine orifice, and with great difficulty removed by the sponge. After it has been removed from the uterus, and mixed with water, it loses this character and appearance, and seems to be only transparent, or semi-opaque, streaked with white striæ or flocculi; sometimes almost as uniformly white as lard, and not misceable with water. I was pretty well convinced in my first examinations, that a great deal of this fluid was pus; but having read Dr. Roberts' opinion in regard to it, I fully concur with him in my doubt whether simple ulcerations and excoriations of the neck secrete anything save a transparent mucus from the exposed follicles on their highly colored surface. Dr. Roberts' says, that he has seen this yellow discharge when there was no ulceration, and supposes it to be always uterine. He further adds that it is only to be seen in the worst cases of this disease, and the stain which it causes on the linen most often indicates that the affection is aggravated, and the probability of the existence of ulceration.

I have no doubt, had the probes been used so as to have dilated the os tincæ, the uterus rendered pliable and soft, in the mean time, by leeches and then the labia of the uterus separated, an ulcer would have been discovered along the canal, in every case, he alludes to.—One or two ladies presented themselves with the yellowish discharge, and at first neither the enlarged cryptæ or follicles, nor an ulcer could be seen; but after some weeks' treatment, the engorgement having been removed, by the use of the probes the labia could be separated, and an ulcer seen along the canal giving rise to this yellowish puriform discharge. I repeat it again, that this may be considered a certain diagnostic sign of some ulceration before an examination is made; and I hope when this matter, which may be considered of little importance, is fully investigated, and the fact fully established, it will be a sufficient test of the activity of uterine phlegmasia, and to the physician a full guarantee of the existence of an ulcer, and the consequent great necessity of an examination being made by the speculum.

When, however, our inquiries elicit from the lady, that the discharge consists simply of a transparent or slightly opaque mucus; or should it stain the linen greyish; or, like starch, give the appearance of a slightly stiffened spot, we may content ourselves that it proceeds from vascular congestion or engorgement; to cure which it is equally as important to use the speculum as when the most grave diseases exist.

If the pathological views which we have advanced (and for the correctness of which I refer to the cases reported by Dr. Roberts, as well as the cases I am about to report,) be true, it will be found that in forty-nine out of fifty cases, the leucorrhœa proceeds from the cavity of the uterus.—Taking this view of the matter, I know of no other mode that will be attended with the same success as the use of the speculum in the treatment of diseases of the uterus, In the conclusion of this part of my subject, permit me to quote Dr. Robert's remarks in regard to it. He says: "But while we have questioned its frequency, and adduced evidence to justify our incredulity, we have not wholly denied the rare, but possible, occurence of cases of either, dependant, (if the reader will) upon simple atony of the part, or an increase, not appreciably morbid, of the secretory irritation upon which the phenomena of either depend. The term "weakness" has long been a cloak for ignorance, and the more enlightened pathology of modern times has established the nature of all chronic fluxes upon a more rational and scientific basis. Few are now known to depend upon a state of inflammation, more or less acute. If there be other causes for the uterine lesions, we have not encountered a case which countenances the idea. The lesions of the vagina and uterus, with which we have shown that leucorrhœa is so invariably connected, are not sufficiently appreciable, and seldom curable without the aid of the speculum, an instrument as indispensable in the treatment of the diseases of these organs, as the stethescope in those of the heart and lungs, and to the non-use of which, the errors of our predecessors are referable.

It is not even now the custom to employ it in the treatment of leucorrhœa for two obvious reasons, the disagreeable nature of the investigation, and the natural repugnance of both physician and patient to its use. But if the physician can but become assured of its value and necessity in these cases, he will, in justice to his patient and himself, recommend and employ it. Its use will then become customary, and surprise at its proposal will soon cease to be felt. Nay, surprise may even be expressed if the usual means of a full investigation be not resorted to, and the sufferer with leucorrhœa will as certainly look to be examined with the speculum as the pthisical patient does to be percussed and auscultated. The objections to its use must yield to the sense of its utility; and when conscientiously and properly urged, there will be found, after all, few sensible and right-minded females, who will object to its employment. When properly used, few will refuse their consent to a repetition. We trust that no other than a conscientious belief, founded both upon our ideas of the nature and cure of the affection, and the opinion of others, impels our advocacy of it in the disease in question; and whenever it shall come to be generally employed, much suffering will be speedily obviated, many errors in diagnosis corrected, many a barren woman will become the joyful mother of children, and many a case of ultimate degeneration into incurable malignity, will be prevented."

It has so far been my pleasure, since the establishment of my Infirmary, to be instrumental in saving many interesting wives, who since recovery have borne children, and are now happy mothers.

Case 1st. *Ulceration and engorgement of the os tincæ, involving the anterior and posterior labia.*—Mrs. ———, aged 38, of leuco-phlegmatic temperament, the mother of one child by her first husband. An interval of eight years occurred before her second marriage. She had been the subject of leucorrhœa for many years. She complained of a heavy dragging sensation, weakness and pains in the loins; some difficulty in walking; great tenderness of the abdomen; frequent disposition to urinate. Examined by the speculum and touch; found cervix enlarged, hard, and sensitive, with a large ulcer seated upon the anterior and posterior labia, and extending along the canal; profuse yellowish discharge adhering to the ulcer.

Treatment. May 12th, applied four leeches to the os tincæ; a good deal of hæmorrhage from the bites of the leeches; painted the entire neck with tincture of iodine; used the following wash: half drachm of nitric acid to 28 oz. water; syringeful used three times a day. Pills taken every night, to keep the bowels well open. The leeches, iodine, wash and pills, were continued up to the 24th; cauterized the ulcer with the nitrate of silver. 26th—Reported a good deal of sloughing from the ulcer, with yellow secretion. 27th—Leeched; applied caustic and iodine. 30th—The ulcer becoming less fissured, and more smooth; the wash throws off a white membrane from the uterus and vagina.

June 3d. Ulcer improving in appearance, reducing in size; secretion becoming transparent and white; leeches, caustic, and iodine painted around the sides of the neck; wash continued; pills, producing bilious operations. 6th—Abdomen less tender; pains in the back and hips much slighter; dragging sensation disappearing; uterus feels lighter and more natural; ulcer improving. Caustic to the ulcer; wash and pills continued; blister applied to the small of the back. 9th—Ulcer much smaller; getting smooth and healthy in appearance, very little secretion;

what there is, comes from the womb; strength improving; complexion much better; caustic and leeches applied. 12th—Ulcer the size of a dime, smooth and healthy; probed the uterus with first and second sized probes. The probe passed readily as far as an inch and a quarter, when it was opposed by the natural contract on between the cavity of the neck and cavity of the uterus. First size passed half an inch within the contraction, producing pain in the abdomen and hips; second size passed with difficulty, increasing the pain. Applied leeches and caustic. 15th—Period came on tolerably healthy; continued four days; 19th—Ulcer still reducing; uterus by touch becoming soft and pliable; the neck of the womb and vagina less congested, and assuming a very healthy colour; passed first and second sized probes with difficulty into the uterus, producing excruciating pain in the abdomen and back; leeched, and applied caustic. Rested badly the night of the 19th, with slight head-ache and chilly sensation; appetite not so good. Warm water to the abdomen, and thrown into the vagina. 20th—Pains subsided, some soreness of the abdomen; discharge from the uterus transparent, slightly tinged with blood. 22d—Ulcer small and healthy; first and second sized probes passed with little pain; third sized probe passed nearly to the cavity of the uterus; some pain in the abdomen and hips; soreness of the groin; less discharge; leeches and caustic applied.—25th—Ulcer quite small; less inflamed; entirely smooth. Passed 2d and 3d sized probes into the cavity of the uterus. In withdrawing probes, they were slightly tinged with blood, and a thick gelatinous fluid; pain continued an hour after probing; leeches and caustic. 28th.—Ulcer confined nearly to the os tincæ; 3d and 4th sized probes passed into the uterus without much pain; leeches and caustic. I might remark here that when the 3d sized probe can be passed, the uterus will yield to almost any size, with half the amount of pain; the uterus being soft and pliable, the two contractions yield without any difficulty. July 1st—Leeched ulcer, which was confined to the os tincæ and canal alone; secretion of a muco-purulent character: passed Lallemand's *porte caustique*, charged with caustic, into the cavity of the womb; a stick of caustic, tied to a small piece of wood, passed one inch and a half along the canal; acute pain in the back, with a sense of throbbing in the uterus, for one or two hours; warm water, thrown per vaginam, afforded relief. 2d and 3d—Slight hæmorrhage, with a purulent discharge. 5th—Same application made, and leeches applied. 8th—Same, with less pain, &c. 11th—Same, with very little effect; says she cannot perceive any secretion. 14th—On separating the labia of the uterus, no ulcer can be seen; no discharge. This lady was leeched up to the 20th, at which time she expressed herself as being relieved of all unpleasant symptoms, and says she felt like a new being. Slept very well; appetite good, and complexion fine. I have seen this lady frequently since, and up to this time she continues well.

Case 2d.—*Ulceration and Engorgement.*—Mrs. ———, aged 22, of sanguine temperament, dark hair and eyes, sallow complexion, skin dry and husky, a good deal emaciated; had been in delicate health since the birth her of her child; has had leucorrhœa for several years, with *ardor urinæ*; complains now of a sense of weight in the pelvis; pains in back and loins; tenderness in the thighs; pains in the right side over the liver; digestion not good; bowels constipated, the tone of the stomach a good deal impaired by the great amount of balsam copaivæ, tincture of cantharides; strangury had been produced several times by large doses of the cantharides; she had worn a pessary for many months, but the inconvenience she suffered from it compelled her to discontinue the use of it. Riding on horseback, or long walks, produced great uneasiness in the pelvic region; she had taken one or or two trips to sea with partial benefit, as regards her general health. This lady came to my Infirmary in the month of January, and remained about three months. The speculum exposed a large fissured ulcer, seated upon both labia, and extending along the canal of the uterus, very angry and inflamed, and bleeding upon the slightest touch; neck of the uterus considerably elongated and enlarged; the speculum and touch produced considerable pain; vagina highly colored; discharge very profuse, and of creamy appearance; menstruation regular and healthy; could pass fourth sized probe into the uterus; the sixth, seventh and eighth dorsal vertebræ, and third and fourth lumbar vertebræ, sensitive upon pressure; the former producing difficulty of breathing, and the pressure upon the latter producing more or less pain in the pelvic viscera.

The same treatment was adopted in this as in the first case of ulceration, viz: leeches, caustic, blisters, &c. It was, however, continued three full months before an entire cure was effected. I have attached a copy of a letter which I received from her husband, in regard to her her health, to wit:—

"Representative Hall, July 23d, 1843.

"Dear Doctor.—The health of my wife as far as can be told from external appearances, vivacity, and increasing strength, &c. is now restored; she has been regularly improving in this respect so rapidly and uninteruptedly ever since she left your care, now about seven or eight weeks, that I confidently think and believe her health to be not only perfect, but permanent. She says, she never feels the least pain, or any of the feelings and symptoms that attended her for three years prior to her submitting to your treatment; she was, as you know, perfectly prostrated in strength, flesh, and spirits, and I feared in constitution too, by the various treatments and quantities of medicine from physicians. For a larger portion of that three years her suffering from various symptoms was continual. Some physicians, and eminent ones, both in Kentucky and this State, treated her disease, some for liver-complaint, some for dyspepsia; some for one thing, and some for another; and for the last year it was growing rapidly worse every week, until my attention was directed to your proposition to relieve female diseases. I think she was under your care, eight weeks.

"I am, very respectfully,

"W."

Case 3d.—*Ulceration and Engorgement.*—Mrs. ———, aged 30, of sanguine-nervous temperament; the mother of one child; had labored under uterine symptoms for seven years, during which time she had been subject to frequent attacks of chronic diarrhœa. These attacks were so severe and protracted at times as to have induced her friends to despair of her ever being relieved. She had been under the management of the most eminent and skilful physicians in the country during the greater part of the time, with but little benefit. She had visited Cuba, the sea-shore, and finally gave up all hope of being restored to her former health. In the latter part of May, 1842, she visited her sister for the purpose of placing herself under my care. She complained of all the symptoms as detailed in the preceding case. Examination by speculum and touch detected a large ulcer seated upon each labium, and extending itself within the canal of the uterus. Neck of the womb very much engorged, and sensitive upon touch, and bleeding from pressure of the blades of the speculum; mucous membrane of the vagina very red and inflamed; yellow discharge adhering to the ulcer, and passing from the os tincæ, very acrid, and in some degree tinged with blood, no doubt from the introduction of the speculum, pain being produced by it. The same plan of treatment was adopted as in the preceding cases, with the same happy effects, as I have since learned by a letter from the husband of the lady.

Remarks —The diarrhœa ceased altogether in a few weeks after the treatment was commenced. She has had only a single return of it, and at that time it was prevailing as an epidemic in the city in which she resided.

September 25th, 1845.—Her health continues good to this day. She has had two children since I discharged her, and is now enciente with the third.

QUERE. Was not the diarrhœa in this case produced and kept up by the inflammatory engorgement of the uterus and vagina; the large intestines being affected by sympathy?

DYSMENORRHŒA.—For fear of being considered a little too prolix, I must make the balance of my remarks as brief as possible, in regard to the other diseases of which I yet have to speak.

The term dysmenorrhœa is applied to those cases in which the act of menstruation is accompanied with pain in the region of the womb.—The disorder varies in intensity in different individuals, being in some very slight, and occurring only occasionally in consequence of cold or some temporary derangement of the health; whereas, in others, it constitutes one of the greatest sources of distress to which the female is ever obnoxious. the menstrual office being never performed without agonizing pain. I have seen persons who entertained the greatest dread of the approach of the catemenial period, from the certainty they had of passing through an ordeal of extreme anguish.

The suffering of the lady at this period is sometimes severe beyond description, it resembles in intensity the pains of labour, or of abortion, properly so called; for to either, the case may be said to have a strong analogy. It usually commences by a slight menstruous discharge, which is pretty suddenly arrested; pain now instantly ensues; which is described by ladies to be of a forcing, bearing down kind, returning at longer or shorter intervals until a membranous substance or coagulum is discharged. This has been supposed to be similar to the deciduous coat of the gravid uterus; the discharge of this decidua being attended with severe pain, rendered greater, probably, by the excessive irritation of the womb which causes all its contractile efforts to be more acutely painful. That this state of things is entirely dependant upon a highly congested or irritable condition of all the tissues that compose the uterus, no one will doubt. The phenomena of dysmenorrhœa go to prove this fact, by the amount of pain produced, the great change in the menstrual secretion, and the size and appearance of the organ, &c. Besides the position above, we also assume the ground, that a great majority of cases are produced in a great measure by some mechanical cause, the os tincæ being so small as not to admit a probe much larger than a bristle. This may take place from the great amount of coagulable lymph deposited in the neck; and involving more or less the sides and fundus of the uterus, producing what we might term hypertrophy; the cause of which I believe to be the effect of chronic engorgement. In one of the cases that came under my care, I found the canal up to the first contraction sufficiently open to admit a common sized probe; but I met with as much difficulty in dilating the balance of the canal as in any other of my cases.

It is contended by many who have written upon this subject, that the seat of the disease is upon the lining membrane of the cavity of the uterus. I cannot, from my experience and observation, believe this to be the case; I have invariably in every case used the probes, passing them to the entire fundus, and in no case has it produced any pain when the instrument came in contact with the fundus or upper part of the cavity; but invariably the pain has been complained of while passing through the contraction along the canal, and the pain mostly complained of, was in the abdomen and back. This contraction of the canal is not only to be found in persons who have never had children, but may be seen in ladies who have borne children, the disease occurring afterwards. The two following cases will show the os uteri, impervious in one, and contracted to its smallest extent in the other.

Case 1.—Mrs. ———, aged 22, of sanguine temperament, light hair, blue eyes, and fair complexion, some two weeks previous to her marriage (six years ago,) received during her period a severe wetting, which checked the menstrual discharge, and was accompanied by the usual symptoms. Bleeding, emmenagogues, and sudorifics, were prescribed for the time, which afforded partial relief. Her next period (after marriage) came on very partially, attended with a good deal of pain, headache, restlessness, &c. For the next four years the dysmenorrhœa continued to increase, notwithstanding she had medical aid all the time. The disease continued to grow worse until confirmed hysteria was produced. These hysterical paroxysms returned with more violence two or three days preceding, and during the continuance of her periods, producing cramp, cold extremities, great difficulty of breathing, with the other symptoms that usually accompany this disease. Finding no relief, or very little, from the physicians who had attended her, she was induced to visit and consult me. After hearing a full description of her case, I remarked to her that she no doubt had dysmenorrhœa with chronic inflammation of the uterus, and advised an examination with the speculum, to see the extent of the disease. The examination was made next morning, in the presence of several ladies who were then under my care. The uterus had a considerable degree of hard engorgement and enlargement with more or less induration of the organ, and very sensitive to the touch. I attempted to pass a small probe into the os tincæ, but found it impervious. I asked her if she had had her menses lately; to which she replied that for the last year her periods had been very irregular, occurring at intervals of fifteen and twenty days, and once or twice there was an interval of six or eight weeks ; and that for the last two months she had had no discharge at all, but during that time she had hæmorrhage from the bowels for several days in succession. I remarked to her, that the hæmorrhage from the bowels was owing to a suppression of her period. A large tumor was discovered through the walls of the abdomen on the right side of the uterus, about the size of an apple ; pressure on it produced pain in the uterus, with more or less nausea. I then commenced a course of treatment by applying five or six leeches to the uterus every second or third day ; mild purgatives, &c. This was repeated for some ten or twelve days, when I commenced the use of the probes, to drill out the former opening, with but little progress. Blisters and stimulating ointments were used over the tumor, or enlarged ovaria, and in front of the uterus. After the inflammation had been partly subdued by the leeching and blisters, I commenced again using the probes every third day, with some force, until I succeeded in opening the proper canal, so as, after some weeks trial, to admit one the size of a crow's quill. The use of the probes produced pain in the back, abdomen, and tumor ; slight fever, headache, &c., which, however, would pass off after a good leeching. When the next period came on, the fluid passed through the os tincæ, and continued for four days, with a decided improvement in the appearance and consistence of the discharge, compared with her former periods. The leeching was still continued, with the use of mild and stimulating washes to the vagina. By this time a decided improvement began to manifest itself in her complexion, strength, and appetite ; she became more cheerful, and less disposed to her frequent paroxysms. A large seton was applied over the tumor, with a view of relieving the soreness that she still complained of; this was worn for several weeks, with great relief. A few days after this period, a profuse leucorrhœa from the uterus made its appearance, which produced considerable irritation of the bladder, and so acrid as to excoriate the vulva. The washes that she had used having but little effect in checking this discharge, the nitric acid wash and the nitrate of silver were used for some days alternately, with but little effect, also, in restraining it. I was induced then to try the tincture of iodine, diluted with water, thrown directly into the uterus with a small male syringe, through a female catheter. The strength of the iodine was increased gradually until the tincture itself was used at full strength (say 40 grains to one ounce of alcohol) ; this had the effect of changing the secretion to a thin transparent fluid, in some degree tenacious. I commenced with four grains of the nitrate of silver to the ounce of water, increasing it gradually to ten grains to the fluid ounce. I will remark here, that these washes into the uterus produced some pain and a sense of heat, with pain in the lower part of the abdomen and back, which would continue from one to two hours. The discharge still continuing, and knowing that the caustic used in powder through Lallemand's *porte caustique*, produced less pain, and was more harmless in its effects, this was applied every third day with the happiest effect. The frequent paroxysms that she had when she came to my Infirmary, were combatted with the usual remedies of friction to the extremities with mustard, brandy ; warm bricks to the feet, and fomentations to the abdomen ; (for during those attacks she had incessant pain in the uterus and the ovaria,) anti-spasmodics of ether, camphor, and assafœtida, &c. She had no paroxysms after the uterus became pliable and soft, and the engorgement relieved. All the symptoms of dysmenorrhœa disappeared, as well as the tumor in the groin; and when she left, February, 1843, she was entirely free from all disease. During the summer of 1844, she visited the Madison Spring, and while there drank freely of the water, which, in the course of ten days, produced violent gastro-enteritis; she was attended by several physicians, and after several weeks, recovered so much as to be able to walk about the house; but being reduced to a mere skeleton, and being unable to indulge in any article of diet, she was induced to visit me again, with a hope that I could relieve her dyspeptic symptoms. She was in some degree apprehensive that her old disease had returned again. On her second visit to my house, I examined the uterus, and found it still healthy, pliable and soft, free from any engorgement, and without any discharge. Her periods occurred every twenty-eight days, without the least pain; no engorgement or tenderness of the right ovarium. I remarked to her, that her great emaciation and dyspeptic symptoms had been produced by the water which she had drunk at the spring; (I do not know the medicinal qualities of the water,) and that she was then suffering from sub-acute inflammation of the stomach and bowels; to relieve which, I gave the pills of blue mass and some Dover's powder, for a few nights only, so as to produce some action on the liver, and then commenced using the iodide of potash.

℞ Iodide of potash ℨ i.
Water ℥ xvi.

A table spoonful of this solution was given three times a day. In a few days a decided improvement began to manifest itself; the redness of the tongue disappeared, and in a short time she could indulge in almost any article of diet, without any unpleasant effect. After the first bottle of solution had been used, I increased the quantity of the iodide ℨ iss, to ℥ xvi water; giving the same quantity of the solution three times a day, and in six weeks her health was restored. I received a letter from her husband in January last, (1844,) stating that she enjoyed interrupted health, and that she weighed 148 lbs.

Remarks.—This lady visited me a few weeks since, complaining of pain and soreness over the liver. I examined the liver, found it very tender upon pressure; very much enlarged and indurated; and extending half way round the abdomen. I examined the uterus; it was still healthy, soft and pliable; her periods came on every twenty-eight days; healthy in appearance; free from coagula, and unattended with pain. I advised her to visit a mineral well of the Rev. Preston Cooper, four miles from my residence, that has made some very astonishing cures of chronic diarrhœa, chronic affection of the liver, &c. She remained three weeks. On her return again to my house, with a view of showing the great change in her appearance, I found upon examination that the liver seemed to be reduced to its proper size. She was free from pain, and I have no doubt, now, that she will continue to enjoy good health. September 20th, 1845.

Case 2.—In the description of this case, I have used the lady's own language. She says:

"I had been afflicted with dysmenorrhœa twelve years. The disease was contracted from being caught out in a heavy rain during the time of the menstrual discharge, and when much heated by exercise. My periods afterwards were preceded some six or eight days by cold feet, headache, pain in the back, tenderness of the breast; and generally, great pain at the time of the period, the discharge was unnatural in appearance, of less quantity than proper, and in the form of coagula; the discharge of these coagula was always attended with the most excruciating pain; this was succeeded by prostration and general debility, from which I scarcely recovered before the approach of the succeeding period. I should also remark, that the period was invariably attended with nausea, and frequently with vomiting. I suffered immensely for those long years, until finally, in 1844, I found my health becoming so seriously and permanently impaired, that I was much alarmed, and placed myself under your treatment; from which I am happy to say my health seems to be perfectly restored. N."

This lady came to my infirmary in January, 1845. On examination by speculum, the uterus was found very much engorged; elongated; unyielding to the touch, and very sensitive upon pressure; the os uteri and canal contracted to the smallest degree; from which a slight transparent discharge was seen passing; the introduction of the speculum gave considerable pain, and a sense of heat.

She was leeched, and the uterus painted with the tincture of iodine, every second and third day, for one month. In the mean time the period came on; it was very scanty; with coagula, pain, &c. I now commenced the use of the probes; and it required several trials before I could pass the smallest end of my least probe as far as the first contraction; every effort of the probes produced intense pain in the uterus and abdomen, with chilly sensations, headache and fever; my exertions to pass the probes were continued upwards of eight weeks after she came, before I could succeed in dilating the canal sufficiently large to admit the introduction of Lallemand's *porte caustique*. This was, however, accomplished three weeks before her third period came on; and the application of the caustic was made at different times, with sufficient intervals between them, to permit the effect of each application to pass off, before it was again applied. By this means the leucorrhœal discharge ceased, the leeches, washes and iodine were continued until the uterus became soft and pliable. When her third period appeared, the discharge was tolerably free, attended with less pain, &c.; so great a change having taken place, she supposed she could return home with the hope that the improvement would continue. While at home she was accustomed to keeping late hours, in receiving company and attending balls and parties; in doing which, she exposed herself to the night air, and upon several occasions got her feet wet. The two succeeding periods came on while at home, with nearly the same amount of pain, as her former periods. By my request she returned again, and remained ten weeks. I found her uterus a good deal inflamed and sensitive; the leeching, probes, wash, pills and iodine, were resorted to again in nearly the same manner as in her previous treatment. In addition to this, blisters were applied to the lumbar region, every tenth or twelfth day; and at this time, from the appearance of the uterus, (it now being soft and pliable,) the nature of the discharge, the absence of any pain, or any of the premonitory symptoms to which she has heretofore been accustomed; the great change in her complexion, the amount of strength and flesh, (being at least fifteen pounds heavier than when she came,) and last, but not least of all, the great change in her general health, induces me to declare without any hesitation, that her case is one of my most perfect cures. I omitted to say that this lady had used the cold bath every morning, while at home, immediately after getting out of bed, to her great injury, no doubt.

Prolapsus Uteri.—This term is generally understood to mean every degree of displacement, from slight depression to protrusion of the organ through the os externum. So varied are the opinions of medical men in regard to the cause, and the pathological state of the uterus, at the time this disease is said to exist, that to detail them, would occupy more time and space than my paper will permit. To present my own opinions, (and they are at variance with the whole profession, with the exception of Lisfranc,) would be arrogating to myself more than I am willing to do. I therefore beg leave to quote his remarks in regard to the cause and pathology of this disease; they entirely coinciding with my own opinions, formed from observation while treating this disease. I feel confident that after this disease has received that investigation which it so justly demands, and the speculum becomes more extensively used, we shall be more enlightened in regard to its etiology and treatment.

Lisfranc says that—"displacements of the uterus are astonishingly frequent. I have demonstrated this fact too frequently to my clinical class at *la Pitié*, for it to be doubted. But it is generally believed that these affections are almost always essential [primary.] I am not of this opinion, and it has caused grave errors in therapeutics to be committed—for on admitting it, the descent or deviation of the womb is alone treated, and the uterine engorgements are neglected until they become incurable. I have advanced elsewhere, that I will prove, whenever it may be required, that the descent, prolapse, anteversion, retroversion, and lateral inclination of the uterus, are excessively rare when the organ is exempt from hypertrophy. For more than fifteen years I have especially directed my attention to this important point of pathology. I have examined thousands of women, and to the present time, I have found some few cases only, in which these morbid affections existed without a sensible increase in the size of the uterus. When the uterus is engorged in its entire circumference, it descends parallel to the pelvic axis. If its increase of volume prevails in front, there is anteversion, and the contrary when such augmentation occurs at the posterior portion of the organ; lastly, when the induration exists upon one side, to that side it inclines.

"It requires but the simplest knowledge of physics, to perceive that a pyriform body, somewhat flattened, suspended in the pelvis by four supports, must, if its anterior portion acquires a considerable thickness, execute a movement which will carry its superior portion towards the symphysis pubis, and vice versa. But I have often heard the following questions asked by men who object to these new ideas: Is this hypertrophy, which is observed in displacements of the uterus, primary or consecutive? If it followed the displacements, it is evident we should very frequently meet with these affections alone, for before the development of the engorgement, the patient would suffer pain, and in examining them, we should find such engorgement absent—but essential displacements being in fact exceedingly rare, every one must admit their production by the hypertrophy. If required, there is still another proof in favor of this opinion. The engorged womb is displaced. I confine my treatment simply to the engorgement. I cure it, and the organ shortly after resumes its natural position in the pelvis. These statements are neither idle nor curious; every one reflecting upon the subject, must see that it is not a matter of indifference whether we exclusively treat any displacement of the uterus, or whether we attack an engorgement of that organ. The engorgement, in fact, must be the primary object of treatment; and if degeneration of structure has not occurred, we are sure to cure the disease."

The following case will illustrate our position. I intended to have given several more, but as I have already extended my paper to a greater length than I intended, I hope it will suffice.

Mrs. ———, aged 24; spare habit, brown hair, blue eyes, fair complexion; was married at the age of sixteen—had had some irregularity of her periods previously. Eight months after marriage, had a miscarriage without any known cause, unless from a short journey in a buggy to Vicksburg. She returned home, five days afterwards, on horseback—distance, eighteen miles. In four weeks her catamenia made their appearance again, very profusely. She continued in this condition for two years, with very profuse leucorrhœa of a white, milky appearance, which reduced her very much in flesh and strength; this was checked by astringent washes. Nine months elapsed before she conceived again; carrying her child to its full time. Her health continued to be very good for three months; at this time she received a wetting, by having a pail of water thrown on her by accident. She became instantly chilly, and in ten hours a very profuse leucorrhœa tinged with blood, made its appearance, accompanied with nausea and debility; pain in the chest and loins. She remained in bad health for thirteen months; during which time she was advised to travel. This improved her general health so much, that conception again took place. Eight months after conception she had a severe attack of fever; and her physicians, deeming her case almost hopeless, produced premature labor. She was able after a time, to nurse her child until its death, at nine months. She had two abortions in the succeeding twelve months; with leucorrhœa; pain in the back and loins; heavy dragging sensations; tenderness and swelling of the abdomen, digestion bad; stomach and bowels very much inflamed. She was advised to keep her bed, and a large tampon was applied; this being very painful, she was compelled to abandon it. She had used Thompson's abdominal supporter for many months; had taken large doses of copaivæ and tincture of cantharides, tonics, &c. The cantharides produced a copious discharge, resembling semen masculinum; frequent emissions of this, with lascivious dreams; coition very painful, producing great determination to the head; a sense of heat in the cerebellum, with blindness. This lady was reduced to a mere skeleton, unable to walk across the room; and when brought in a carriage to my Infirmary, on the sixth of June, was supposed to weigh 85 lbs.

Appearance of the Uterus, by the speculum and touch.—Uterus very much engorged and elongated; vagina red and inflamed; a large fissured ulcer seated upon each labium of the uterus, embracing the os tincæ and canal; very sensitive upon pressure; a large portion of the back part of the uterus very much indurated, interspersed with lumps or knots; this part exquisitely tender, producing pain in each groin and hip, with nausea and faintness; the fundus thrown back, and making considerable mechanical pressure upon the rectum; profuse yellow discharge; bowels constipated. She was drycupped frequently upon the stomach and bowels; took blue mass for ten or twelve days; took of the iodide of potassium, ℥ ss to viii sarsaparilla; of this she took a teaspoonful three times a day. In the meantime the leeches were applied every other day to the uterus; purgative pills were given *pro re nata.* Twenty days after her arrival, four leeches were applied to the neck, and that portion of the vagina immediately in front of the hard tumor upon the back of the uterus, alternately, every second or third day. July 15th.—ulcer very much reduced in size, the lump on the uterus the size of a filbert; a little tender to the touch.

This lady was leeched regularly after this; the uterus painted with iodine, caustic applied to the ulcer every third day, and extended to the cavity of the uterus by Lallemand's instrument, up to the 20th of August, at which time the uterus was reduced to its proper size. It was soft and pliable, in a good position, free from lumps or knots, there was no discharge or leucorrhœa, and the patient free from all of the symptoms heretofore complained of, such as a dragging or heavy sensation of the uterus; walks to her meals, a distance of forty yards; has gained at least twenty pounds in flesh; her periods coming on regularly, without pain or any inconvenience.

Sept. 6.—Rode three miles on horseback without pain or any unpleasant symptom. Says to-day that she is more fleshy than she has been for many years, and remarks that she feels like a new being.

8th.—Examined the uterus; find no discharge; uterus healthy, soft and pliable; *in situ;* no tenderness of it whatever by the touch or speculum; can indulge in any article of diet; stomach and bowels in fine order. Discharged, cured.

October 4th, received a letter from her, which states that she returned home, a distance of one hundred and fifty miles, has no discharge, and that she still continues to gain flesh.

If I had not extended my paper to such a length, I would have given one or two other cases, equally as interesting as the one above. In conclusion, I will add one more remark, in all candor, that nine-tenths of the ladies who placed themselves under my care, have been fully impressed with the idea that they had prolapsus uteri when they came; and two-thirds of that number had worn pessaries of some kind, to their great injury. I found it a very difficult matter to remove that impression, until they had been relieved of all the symptoms that accompany engorgements of various kinds.

I pass now to my last subject, Menorrhagia.

Duparque's description of this variety of soft engorgement is so very accurate, that I must beg leave to refer the reader to it; commencing at page 100, and ending at page 109, inclusive.

The primary indication in this engorgement, and the means of fulfilling it, are the same as in the preceding species; but in addition to the remedies there indicated for the purpose of arresting or changing the local determination which maintains the congestion, we are called upon to adopt others, with especial reference to the hæmorrhage, which by its continuance or violence, constitutes, in many instances, the immediate source of danger. Among the anti-menorrhagic remedies, astringents justly occupy the first rank; but, in order that they may produce any beneficial result, they must be adapted to the pathological state. If the physician, occupied with a mere incidental phenomenon, (hæmorrhage,) overlooks the fundamental disease, and resorts to astringents with a view of restraining the flow, before the local determination which maintains it is arrested, he will infallibly perpetrate mischief. He may secure, it is true, the object of his limited therapeutics—he may restrain the hæmorrhage—but he will inevitably precipitate his patient into greater peril by aggravating the engorgement, or converting it into a state of inflammation, far more dreadful in its consequences, than the primary malady. On this subject Duparque has given us some very instructive lessons, and we might appeal to the experience of every physician at all conversant with uterine diseases, for parallel cases to attest the mischievous results of premature administration of astringents, under the condition here stated. There is, nevertheless, a point in hæmorrhagic engorgements, where this class of medicines will be found to exert the most favorable effect; but this point does not consist with a persistence of the fluxionary movement, or an active state of the capillary circulation of the uterus. It is only after the local determination has been subdued by appropriate treatment, or the uterine tissue has fallen into a state of atony, denoted by the long duration or violence of the sanguine discharge terminating in general debility, that astringents are indicated. Under these circumstances the most auspicious results may be anticipated from their employment. It is in this same relaxed state of the uterine fibre, that ergot has acquired a reputation in the treatment of hæmorrhagic engorgement; nor is it difficult to comprehend its *modus agendi* By its power over the contractility of the uterus, its tissue is condensed, with the effect of throwing back the engorged fluid into the current of the circulation, and of placing the organ in a condition to resist their return. This explanation of the action of ergot is consonant to its general properties, but after all, it may not comprehend the whole secret of its almost miraculous power in this form of uterine hæmorrhage. M. Duparque claims for it an astringent property, and does not hesitate to aver that he has arrested by its use, hæmorrhage from other organs besides the uterus. In illustration of our views, and the great efficacy of treating hæmorrhagic engorgements by the use of the speculum, I refer the reader to the fourteen cases reported by Duparque, and in addition, I here present two of the many cases of menorrhagia I have treated successfully at my Infirmary.

CASE 1.—*Menorrhagia with Ulceration.*—I give here the language of the lady herself, in the description of this case. She says: "in 1836, I was married at the age of eighteen, in fine health, and continued so until 1837, when I had a miscarriage, which injured my health for a short time. The following year I was delivered of a fine boy, at which time I received such injuries from improper treatment by my attendant, (an old woman,) that my general health was entirely destroyed for the last seven years. I have been almost constantly under the treatment of the most eminent physicians, or travelling to different watering places, but without receiving any thing but temporary relief. My case was treated for prolapsus; but since it has been investigated, it proved to be nothing more than a soft engorgement of the uterus, with ulceration. I have also labored under menorrhagia and fluor albus; the periodical discharges lasting from eight to ten days; and the whites continuing until the return of each period; always keeping me in such a state of debility, that I could enjoy nothing. For the last eight months, my disease increased so rapidly, that I felt that without relief I must die. I came to Dr. Holmes; and have every reason to believe that my disease is entirely removed."

This lady was about 28 years of age, dark hair and eyes, fair skin. and tall stature; very much reduced in flesh.

Appearance of the Uterus.—Uterus large, soft and spongy; very red, with an ulcer seated around the os tincæ, the size of a dime, and extending within the canal. I could pass my largest probe to the fundus of the uterus, without pain; a large collection of yellowish discharge was seen upon the ulcer and surface of the vagina.

Case 2.—*Menorrhagia, with Ulceration.*—Mrs. ———, aged 30, of bilious temperament, dark hair and eyes, remarkably tall; the mother of three children; had suffered with painful menstruation for many years. At the birth of the last child, she suffered but little, and was able to get up within the usual time. Her health continued pretty good until within the last two years; about which time she noticed a good deal of leucorrhœa and profuse hæmorrhages at each period, accompanied more or less by pain in the back, and soreness of the abdomen. Her periods grew worse, as regards pain and the amount of hæmorrhage. The last period she had before she came to my Infirmary, was so very profuse, as to produce great exhaustion; each period continued from ten to twelve days, and at intervals of twelve or fifteen days. They observed no regularity whatever. She was a good deal blanched in appearance.

Appearance of the Uterus.—The uterus very much enlarged; soft and flaccid; the os tincæ so much relaxed as to admit my fore-finger; ulcer about the size of a twenty-five cent piece, surrounding the os, and extending within the canal; being very uneven and fissured; large quantity of yellow discharge, thick and tenacious, great tenderness of the abdomen, with strangury; urine thick and high colored; could pass my largest probe to the fundus of the womb.

Remarks.—These ladies were treated three full months, by being leeched every third and fourth day; the uterus was painted with tinct. iodine; caustic applied to the ulcers and extended within the canal to the cavity of the uterus, by Lallemand's instrument. The first few times the leeches were applied, the hæmorrhage from the bites was so profuse as to require cold applications to the abdomen, and also to be thrown within the vagina. Tonics were used daily; purgative pills *pro re nata.* At each period the decoction of ergot was given: ergot ℨ i, to aqua bullientis ℥ viii; a table spoonfull of this given every half hour, according to circumstances. Between the periods they took one of the following pills, three times a day:

℞ Precip. carb. ferri ℥ i.
Ext. gentian. ℥ i.
Pulv. ergot. ℥ iss.
Sulph. ferri ℈ i.
M. Make into 50 pills.

Washes of the tincture of iodine, and of nitric acid, were used alternately every day. This treatment had the happiest effect in saving (I have no doubt) two very interesting ladies, who could not, without relief from some source, have survived the disease very long. They are now in fine health, and I have no doubt of their bearing children again.

In conclusion permit me to add, that the periods of these ladies came on before they left, perfectly healthy, about the right quantity; and while the period was on them, they could take any kind of exercise without increasing the amount of the discharge.

I deem it proper to make some remarks in regard to my remedies. A few suggestions in applying the leeches may be of service. The uterus having been exposed by the speculum, and the secretions removed by a mop or sponge, the next thing that will require the attention of the physician will be to pass, previously dipped in water, two pieces of paper, one of which is to be placed on each side of the speculum, extending down the sides of the vagina; a probe of sufficient size must be passed into the os tincæ, and covered with a small piece of paper also, so as to guard against the entrance of the leeches. I once had this to occur in my practice, and the amount of pain felt and witnessed can be better imagined than described. Suffice it to say, that it was excruciating beyond description, and every moment I expected to see my patient thrown into convulsions. By using this precaution, the leeches can be conducted to the uterus without taking hold of the sides of the vagina.

Leeches.—There is nothing equal to the local abstraction of blood in any one of the various congestions, engorgements, or indurations of the uterus. It is necessary to apply a sufficient number, to obtain at each time a full bleeding. Their good effects in relieving the heavy dragging sensations, and pain in the iliac and hypogastric regions, are most striking. The sensibility and tension of the parts subside after they have been applied a few times. This effect is as readily perceived by the feeling of the patient, as the gradual disappearance of all the congestion or inflammation of the parts, is perceived by the eye. The strength of the patient is less affected by local, than general bleedings. Analogous effects may be obtained by the application of leeches to the labia, inside of the thighs, abdomen, and back; but it requires a greater length of time to produce an effect, than if applied locally to the uterus. The number of leeches should be proportioned to the extent of the inflammation, the strength of the patient, and the amount of engorgement. They take hold without producing any pain or irritation, and in ten or fifteen minutes fall off. After sliding out of the speculum, they should be pressed between the fore finger and thumb of each hand, and placed in a bottle to themselves; by this means we can get them to draw every other day.

Iodine.—Next to the local abstraction of blood from the uterus, ranks the tincture of iodine applied to the neck of the uterus, (60 grains to one ounce of alcohol,) in relieving inflammation, congestion, engorgement, and induration. Its application produces from five to ten minutes, a sense of heat and smarting, and in the course of twenty-four or thirty-six hours, a slight exfoliation, if applied too strong. The only way in which I can account for its good effects in acting as a resolvant, is that it permeates the neck, and, I have no doubt, the body of the womb, quickens the absorption, and assists in removing from the affected tissue the materials for its alteration; or, that it assists in promoting resolution by lessening, or determining to other parts of the system, the fluids from which the diseased mass is elaborated; or, it assists in accomplishing the same end by reducing the innervation of the part to its normal state, and thus enabling it to resume and carry on its proper physiological actions. I have witnessed the good effects of this medicine so often in the different kinds of engorgements, and even in the incipient stages of schirrus, after the uterus had been sufficiently leeched, as to induce me to venture the assertion that to its specific and stimulating effect I owe one half of my success in reducing engorgements, and making the uterus soft and pliable.

Nitrate of Silver in Powder.—It is useless for me to speak more of the effects of the nitrate of silver, than when applied to the cavity of the uterus; its effects upon mucous membranes and indolent ulcers being so well known to the profession. It will be perceived that I have already mentioned the good effects derived from its use in leucorrhœa. I have seen it produce more direct and prompt effect in arresting this discharge than any other of the means I have used. I have often been surprised to see with what promptitude and efficacy nitrate of silver modifies fungous, injected, and engorged tissues, the results of a prolonged inflammation. A short time after its use, the tissues become disgorged, and a change of action is set up; the part acquires a new energy that effectually protects it from relapse, to which the patients are exposed when cured by other means. I venture the assertion that when it has been tried by others, they, as well as myself, will be satisfied with its good effects in uterine leucorrhœa. It should not be used until the greater portion of the inflammation of the uterus has been removed by leeches. I have used it frequently in every case I have treated in the last two years, without its producing any bad effect in a single case.

Probes.—The object of the probe, is to ascertain whether there be any contraction of the canal or not.

They are as indispensable in the treatment of diseases of the uterus, as the leeches themselves, especially in the treatment of dysmenorrhœa and leucorrhœa. By them, we are enabled to overcome any contraction of the canal with a view of using the nitrate of silver locally to the cavity of the uterus, or the diseased part. They should not be used until the uterus becomes, in some degree, free from inflammation, soft and pliable, and should be used then with much caution, gentleness and judgment. In using the probes, the smallest should be used first, and the other sizes as the part yields. They should be passed carefully to the first natural contraction existing in the part, say 1 to 1 $\frac{1}{4}$ inch, and then press the point firmly and steadily against it, at the same time rotating it slightly, taking care that it be directed in a line with the canal of the os tincæ. The pressure is to be continued for five or ten minutes; if too much pain be produced, sufficient time must be allowed the patient to recover; this must be repeated at each time of leeching the lady. The result of this process at each operation will be, that the anterior part of the stricture seems to become relaxed to a greater or less extent, and at last the instrument penetrates the first stricture; it will then pass to a second stricture, between the cavity of the neck and cavity of the uterus. The same obstruction or resistance will have to be overcome in the second, as in the first, when the instrument will pass entirely into the uterus. This may be known by the hard feeling the instrument will convey to the fingers when in contact with the fundus. If gentlemen are not particular at this stage of the probing, the upper contraction will yield so as to permit the probe to come in contact with the upper part of the uterus; the same feeling will be conveyed to the hand without the instrument having passed through it. I have myself been deceived often, and have been astonished at the nitrate of silver not being able to arrest the discharge after two or three applications; it was owing, however, to this circumstance. The larger probes must be used in the same way. The time required in opening these strictures will vary in different cases, some being more easily overcome than others. The distance that the probes will pass will be from five to seven inches, Their use produces more or less pain in the abdomen, back, and hips; chilly sensations; flushes of heat; occasionally headache, which may continue from one to twenty-four hours. All the irritation produced by one probing must be allowed to subside before a second is attempted.

ART. II.—*Brief Sketch of Subjects embraced in the Science of Botany, with its relation to Medicine, and some of the inducements for engaging in its study.* By J. L. RIDDELL, M. D., Professor of Chemistry in the Medical College of Louisiana.

Botany is the science of plants; or to use more words, we may say botany is that science which treats of the internal structure, external forms, natural history, sensible properties, the various uses, and the methodical arrangement, of all those organized beings usually called plants; or of all those living yet insensible objects which have organised cellules, roots, stems, leaves or flowers. There never could have been a time since the commencement of the human era, when man was wholly unacquainted with botany; because he is, and ever must have been, in part dependant upon the vegetable kingdom for the daily supply of his wants. At the present day, perhaps more than three-fourths of the food of mankind is derived directly from the vegetable kingdom; and it is the opinion of some that a still greater proportion of vegetable food was consumed in the early ages of human history. Many vegetable productions are now extensively used as aliment, which were entirely unknown to the ancients. To mention one among a multitude of instances that might be adduced, the common Irish potato, now so much cultivated and esteemed, was concealed in its native wilds, among the mountains of Peru, until after the discovery of America. I have myself no doubt but that there are now, in our country, even in the state of Louisiana, many wild, uncultivated and neglected plants, which by domestication and proper attention in our gardens, might be rendered valuable articles of food. To particularize, I am of opinion that the large bulbs of the Crinum Americanum, and Pancratium Mexicanum, of the swamps in this vicinity, might be abundantly produced in this soil by culture, and might prove of value as articles of food. Something might be done with the purple and crimson leaves of the prolific Callicarpa Americana, a small shrub, a native of this state; and probably the only vine that will ever succeed well in the delta of the Mississipi, is the native muscadine, the soco of the French, Vitis rotundifolia, of botanists. If Italian grapes or others be desired, they can probably be produced by grafting the indigenous soco.

By a *species* in botany, as well as in other departments of natural history, is meant an individual kind, as white clover, red clover, apple, pear, plum. Botanists are now acquainted more or less imperfectly, with near 100,000 kinds which they regard as distinct species; and this estimate includes all that have been discovered and enumerated up to the present time, in all parts of the known world; on land and in water, salt and fresh. The North American continent from the Isthmus of Darien, to the remote frozen regions explored by Franklin, Parry and Ross, probably contains more than 12000 distinct species. I have taken some pains to determine with respect to the number already known to be indigenous to Louisiana; including all yet observed by Darby, Robin, Bartram, Teinturier, Drummond, Dr. Hale, Professor Carpenter, and myself, and as yet we are in the neighborhood of a thousand species. I believe there are a thousand more yet to be discovered.

From the extent of the science of general botany, including as it does an almost numberless multitude of different objects, it is the custom of botanists generally to devote themselves to the study of the plants in the country where they reside. This is certainly very proper. The subject is then comparatively limited and of easy acquirement.

The necessity of systematic arrangement in contemplating or studying such multitudes of different objects, cannot but be obvious to every one.

The mind of man is inadequate to comprehend an apparent chaos of never-ending anomalies and variations, which the vegetable kingdom certainly *seems* to present, unless it fixes upon some common points of resemblance, and views the different kinds as collected into large assemblages of kindred tribes and families. Moreover, not until we qualify ourselves to perceive clearly the order, the beauty, and the harmony—the close affinities and the remote analogies that pervade this great department of nature, can we contemplate this part of creation with those emotions of admiration and delight which the subject is capable of inspiring.

It deserves to be considered that the student, unaided by competent oral instruction, can have no reference to the names of the plants which grow spontaneously about him, except through the medium of botanical classification. Very few persons in this country are competent to instruct in practical botany; and most of our American botanists have had to acquire their knowledge from books and from nature.

Probably a very large majority of the native plants of Louisiana, have heretofore been noticed and named, and more or less accurately described.

Their names with abbreviated descriptions are contained in the universal Floras of Linnæus, Persoon, Sprengel, or De Candolle. There are similar works, containing merely the plants of North America, as Eaton's Manual and Torrey & Gray's New Flora of North America, now in course of publication. But it would certainly be a very discouraging task to read through a volume of 1200 or 2000 pages of technical description in order to find a description which would suit a single plant we chance to meet with, to us unknown. Classification so abridges this labor that we may often refer to the name of an unknown plant in one of these Floras, having the plant in flower before us, with nearly the same facility that we ascertain the meaning of a word by reference to a lexicon.

Plants classified and described according to the Linnæan system, so as to distinguish each one from all others, are managed thus :—

A very limited number of leading characters or circumstances are seized upon, and by the different combinations of these circumstances each individual species is particularized. All those plants, for instance, are embodied into a class, which possess one or more of these well-marked circumstances in common. By pursuing the same method throughout, all known plants may be comprised in a very limited number of general classes. In the same manner, but using other characters equally simple, each class may be subdivided, and by carrying still further the divisions and subdivisions, we at length arrive at the individual species that compose the whole.

Notwithstanding the constant progress made in botany, the system of classification proposed more than a century ago by the celebrated Linnæus stands yet unrivalled, in respect to the facilities it affords new beginners in making an acquaintance with plants. It is so exceedingly simple, that a novice scarcely requires a half hour's explanation to comprehend and master it. I have time to advert to the Linnæan classes only, without descending to the orders and subdivisions.

Linnæus grouped the vegetable world into twenty-four CLASSES: and these classes are distinguished by certain prominent organs always in the flower, called stamens. In the first class of Linnæus the flower has one stamen, in the second class, two, in the third, three, in the fourth, four, in the fifth, five, in the tenth, ten, in the fiteenth, six, four long and two short ones: and so on for the rest, in a manner equally plain and simple. Suppose with Eaton's Manual of North American Botany, and some indigenous unknown plant before us, we wish to find the name of that plant. We first examine the stamens and find it belongs to a certain class; we next, in a manner equally plain and obvious, refer it to a certain order of that class—next, in the same way, we refer it to a certain division in that order, then to a particular genus or family in that division; and lastly, to a certain species in that genus. Thus, we find out the name of the plant, and thus we have it in our power to refer in books to all that is known respecting it, whether relating to its medicinal qualities, economical uses, or natural history. And lastly, we thus have it in our power, to communicate intelligibly to the scientific world abroad, any thing which we ourselves may discover respecting the qualities and uses of the plant.

I am not alone in saying, that no one can become a botanist, without collecting and forming with his own hands, *a hortus siccus* or *herbarium.*

Collect specimens in flower of the different wild herbs you meet with, and dry them by pressing them between many thicknesses of brown paper, or old newspapers. Arrange these specimens in a book, or otherwise, and if you are not skilful enough to affix the right names to accompanying labels, get some competent botanist to name them for you. In this way you come to possess a correct knowledge of plants; and I know of no other way so cheap and so effectual.

The natural method of classification has come greatly into vogue of late years, and some botanists in the height of their zeal in its favor, predict that it will ere long supplant the Linnæan system. But the truth is, they do not interfere with each other. They have different ends to attain. The design of Linnæus is to afford the learner a ready reference to the name of an unknown plant, and, in this respect, it can probably never be equalled by any other plan which it is within the limits of possibility to contrive. It is the sole aim of the natural method, to arrange plants in subordinate groups, according as they are allied to each other by botanical affinities. The philosophy of the natural method can best be comprehended, by adverting to the probable origin of botanical affinity. It is not irrational, in my opinion, to regard the organic creation as progressive, and as liable to modification and change, from influencing circumstances. These modifications of structure or habit may not be so great, in the comparatively short period of human life, or human history, as to strike us forcibly. But after the lapse of many successive series of ages, great changes from the original type may be produced. In this way, even under our own observations, what we call varieties, take origin; as of the apple, maize, &c. Now, after a very great length of time, after variety has had time repeatedly to strike off from variety, if the most dissimilar of the varied progeny be compared together, botanists would probably regard them as specifically distinct, or as distinct kinds; and yet they would be clearly connected by a close botanical affinity, such as we now observe among the acknowledged species of the same genus.

Now, if we carefully scan the vegetable kingdom, we can perceive this kind of relationship existing in all degrees. All may be referred to comparatively a few primitive types of structure, which types themselves are now only represented by a complex and diversely related progeny.

The sole object of the natural method of botany is, by studying the structure and form of the different vegetable organs, and determining their comparative importance—to arrive at a knowledge of the real affinities, or relationships with one another, of different families and tribes of plants. It is the design, therefore, to classify or group them as nature groups them. The most celebrated cultivators of this department of botany are, Jussieu, Du Candolle, Robert Brown and Lindley, and I may add our countryman, Dr. Asa Gray, of Cambridge, Mass.

It is important in relation to medicine, to bear in mind that those plants which are closely allied by botanical affinities, must always possess similar medicinal virtues. For instance, all the malvaceous plants are mucilaginous and demulcent; all the gentian tribe are bitter and tonic; all the grass tribe are nutritious and harmless; all the extensive tribe of Cruciferæ are stimulant, antiscorbutic and nutritious. Similarity of properties often runs through a very extensive group of plants, though the extreme species may be very remote on the scale of relationship. Yet we are sometimes presented with seeming contradictions to this rule, even in kindred species of the same genus. But I think most of these seeming contradictions will disappear, when we remember that we are entitled to expect similarity of medicinal qualities, only in the corresponding structure of corresponding organs or parts. For instance, because the juice of the pulp of the lime is acid, cooling and anti-febrile, we are not, therefore, to expect that the rind of the lemon is to present the same virtues—we must look to the corresponding part, the pulp of the lemon.

Certain virtues highly developed in some plants, may be wanting in kindred species; merely because the organ or structure containing those virtues in the first plant, may be impartially developed, or wanting in the last. The Irish potato, (Solanum tuberosum,) may be said to be mild and nutritious in reference to the tubers of its root, but the same cannot be said of the deadly nightshade, (Solanum nigrum,) because here those tubers do not exist. Therefore, in predicting the qualities of an unknown plant, from its resemblance and affinity to one whose qualities are known, we must never look for similar virtues, unless in corresponding organs and structures.

In conclusion, with respect to the pleasant inducements for the cultivation of this science, it may be remarked, that if we set about collecting, preserving and classifying the plants and flowers growing wild about us, we soon feel a degree of zeal and elevated pleasure, which none can understand but those who have experienced them. After a time, every novelty we find, is like the finding of a rich treasure; and especially if the plant be new: that is, unknown to botanists, or not described in the books; in which case we have the exulting reflection of adding something to the sum of human knowledge, which promises to remain a monument of our industry, so long as human science shall endure. Our minds and bodies are so constituted, moreover, that a study or pursuit like that of botany, contributing to health, is sure to produce contentment and happiness. It leads us to walk forth and admire the works of creation; to hold communion with the giant trees of the forest, the silent tenants of the prairie, and to inhale the fragrance and appreciate the symmetry and beauty of the short-lived flowers of the field. All is change, succession and decay. By these things we are admonished of our own transitory nature. The contemplation of such objects awakens our better feelings, and causes us to forget for a while the jostling, sordid world we live in.

ART. III.—*Cases in Country Practice.*—1. *Fungus Hæmatodes*—2. *Necrosis of the Tibia*—3. *Fracture of the Skull*—4. *Atrophy of the Colon.* By GEORGE BANKS, M. D., of Clinton, Mississippi.

CASE I.—*Fungus Hæmatodes, involving the knee-joint—amputation—recurrence ten months after the operation—death.*—A negro man, aged about 38 years, the property of T. L. Dabney, Esq., of general good health, some time in April, 1843, complained of much uneasiness, amounting to pain, in a small tumour about the head of the tibia. It increased gradually in size, and was fluctuating to the touch. It was opened by Mr. Dabney, bled very freely, and indeed was stopped with difficulty. It increased afterwards with great rapidity, throwing out a large fungus, with most exhausting discharges of bloody serum. In July, about the 18th, I saw him with Dr. Moncure, whose patient he was, and who gave the above history of the case. We found the patient very much reduced, with hectic fever, and accelerated pulse; it was evident that amputation afforded the only hope and that was but faint, since the lymphatics were found diseased, with enlargement of the glands. We removed the limb at the middle of the thigh, and in five weeks, the patient, who was a blacksmith, was apparently well—robust, hearty, and able to do full work. On examination of the amputated limb, nothing unusual was discovered, save the great size of the veins in the substance of the tumour, which formed sinuses, lined apparently by the true lining of the veins. The medullary character was well marked; it had removed in a great degree the bony matter from the heads of the tibia and fibula, and supplied its place. We resorted to alteratives, such as iodine, sarsaparilla, &c., to eradicate the disease completely from his system, if possible. He continued well until May, 1844, when he complained of great pain about the loins, and there was evident tumefaction of the part, together with a discharge of bloody urine. Hectic fever again appeared, with rapid emaciation. The disease had returned, and in a short time he sank. The body was not examined.

The pathology of fungus hæmatodes is so very obscure, that any light having a tendency to explain the nature of so formidable a disease, would be of great importance to the profession. That it is a disease of nutrition, like every other organic disease, is clear; but, it is certainly local at one period, and even when the system is fully contaminated, the removal of the ulcerated tumour seems to suspend for a time, the destructive tendencies of the poison. Can that be explained, consistently with theory?

Case 2.—*Necrosis of the Tibia, of two years duration—Operation, &c.*—Jacob, the property of M. L., aged about 28, was placed under my care in March, 1844, for long standing ulcer on the lower third of the tibia. On examination I found the bone much thickened, and a sinus leading to a *sequestrum* of considerable size. I determined to operate for its removal, and assisted by Dr. Williamson, cut down on the tibia and removed two circles, with a medium sized trephine. The sequestrum was immoveably fixed, and as yet attached to the shaft. We determined to await the action of Nature, as no symptoms authorized, in our opinion, a resort to chisels, mallets, and forcible means for removing the bone under such circumstances. About twelve months after the first operation, I found the *sequestrum* loose, considerably reduced by the absorbents, so that I removed it readily in several fragments. The wound is now nearly closed, and the cure certain.

I think, in many cases, where a free opening is made to the *sequestrum*, a little patience would do away with the necessity for those violent means, which are frequently resorted to in such cases.

Case 3. *Fracture of the Skull—the bone driven into the brain, and* *not removed—Recovery.*—Fagan, an Irishman, of robust general health, somewhat intemperate, about 33 years of age, in a *rencontre* was struck on the head with a short shovel, about the middle part of the left parietal bone, and fell senseless on the pavement, with copious hæmorrhage. He gave no sign of life for some minutes, but he gradually roused up, and when I saw him, about one hour after the reception of the injury, I found him stupid, but readily roused and rational, though evidently under the influence of liquor. On examining the wound, it was found about two inches in length, about the sixteenth part of an inch in width, (the thickness of the shovel,) through the skull, without any depression of the edges, and penetrating the substance of the brain from three quarters of an inch to an inch. It was clear, the missing bone was driven into the brain. I attempted to remove it, and succeeded in removing a few small spicula; but soon found that the attempt to find the depressed portion would probably cause more injury than the bone itself; I, therefore, desisted. Indeed, the symptoms did not justify the steps which would have been necessary for its removal. The external wound was brought together by suture, the cold water-dressings applied, and antiphlogistic remedies assiduously used. He had much paralysis of the right arm and side, from the first. It was found necessary to bleed him freely twice in a few days after the injury. He went on to recovery afterward without any particular backset, and in about fifteen days I left the case. A remarkable feature in this case deserves notice:—the patient lost in a great degree, the recollection of the names of things, and it cost him intense effort, with throbbing in the wound, to recall the names of persons with whom he was familiar. The wound was about the parietal protuberance, (the organ of caution, as located by phrenologists.) He has not, up to this time, entirely recovered from the paralysis, which increases from imprudence, and exposure to the sun.

CASE 4. *Atrophy of the Colon, following Cholera.*—A. R. was aged about 35 years, at the time of his death, in the summer of the present year, 1845. In 1833, he had an attack of the cholera, which, though subdued, left him with a very irritable condition of the colon, as evinced by frequent attacks of dysentery. However, he suffered no particular uneasiness in the intervals of his attacks, save a sensation of burning, and deep seated pain about the left iliac region, which was greatly increased by purgatives of the mildest nature, and then amounted to insupportable oppression, attended by throbbing of the aorta. His bowels, when not purging with dysentery, were very costive.

In 1839, his health gradually gave way, attended by much depression of mind and vertigo, which rendered the upright posture very distressing, and induced his physicians to suspect disease of the brain. A great variety of practice was resorted to, though the true nature of his case was never fully recognised. It was soon found that all purging medicines greatly injured him, and that generally he went for several days without an operation; and when procured by injections or otherwise, it was very small. In 1840, the patient took to his bed, and never left it for any time during the five subsequent years.

When first examined by me in 1842, his symptoms did not differ materially from those already detailed, save in greater torpidity of the bowels, and a freedom from the vertigo, complained of at an earlier period of his disease. He had a disposition to scurvy, which was attributed to the nature of his diet, principally slops, &c. This was a good deal benefitted by a resort to subacid fruits, &c. He died from an attack of fever, unconnected with the organic change in the colon, only so far as the long continuance of his disease rendered him liable, through debility, to succumb to an acute attack.

Examination, eight hours after Death—External Aspect.—The body very much emaciated, (his illness continued about five weeks,) some fulness and hardness about the abdomen; very offensive odor about the mouth and fauces, with a bloody sanies exuding. On opening the abdomen, the stomach was found highly injected, with an exudation of blood on the mucous surface, which last was very much softened, especially about the great curvature. The duodenum was also much inflamed; the effects evidently of the acute attack. The colon was of the natural size from the valve to the commencement of the transverse arch, where it was reduced to a size hardly admitting the little finger, and the walls as thin as blotting paper; being, indeed, composed of peritoneal and mucous membranes—the muscular and cellular being entirely removed in many places; which condition, with some irregularity, was continued to the sigmoid flexure. It was filled with scybala and a glutinous fœces, which must have been in the gut for a long time. The gall bladder was very much distended with bile, though the liver seemed healthy. The rest of the body was not examined.

Cases of atrophy following cholera are noticed by several modern writers, though I am not aware that it has been followed by any other disease. I am satisfied, however, that a patient may survive it for a long time, and it is highly probable, if it could be detected with certainty, that an artificial anus at the caput coli, might restore the patient to health.

Art. IV.—*An Inquiry into the Causes of Disease.* By Wm. P Hort, M. D., of New-Orleans.

It is reasonable to suppose that an inquiry into the origin and causes of disease was cotempoiary with the Science of Medicine itself. If we go back to a period anterior to that of authentic history, we find that Æsculapius was reputed to be the founder of medicine. According to Homer, his sons Machaon and Podalirius were the physicians of the Grecian army at the siege of Troy. The description of the plague in the Grecian camp, produced by the exhalations from the marshy ground near which the army was encamped, establishes the fact that an inquiry had been made into the causes of disease, long before the time of Homer. Æsculapius is supposed to have lived 1280 years before Christ. The siege of Troy took place about the year 1184 B. C.; and Homer, according to the best critics, lived 900 years B. C. Now Homer describes a fact which must have been known at the time of the siege of Troy; we can therefore trace an inquiry into the causes of disease back to the time of the sons of Æsculapius, and it is probable that this inquiry was begun by the father. As Æsculapius is called the founder, so is Hippocrates, who was born about 406 years B. C. styled the Father of Medicine. He diligently investigated the causes of disease, but does not appear to have discovered any thing more than what was previously known. It seems proper to make a distinction between the origin and cause of a disease. The former signifies the first appearance of a disease, or the time when it was first noticed, and the latter refers to the circumstances which produced it. Most of the ancient writers observed new diseases. The time of the introduction of hydrophobia and elephantiasis into Europe is marked by Celsus and Pliny. Dio Cassius mentions a new disease contracted by the Roman army in Arabia Felix. Another disorder, the lichenœ or mentagra, lost to us, was imported from Asia to Rome, according to Pliny, and raged among the Roman nobility. Thucydides and Plutarch ascribe the great plague in Athens during the Peloponesian war, to the multitudes of rustics who were introduced into the city by Pericles, and crowded together in huts within the walls; and Livy imputed the first plague in Rome to the number of inhabitants penned up in its narrow limits. (*Ferriar; Med. Hist.*)

During the middle ages, several strange diseases suddenly appeared, and as suddenly disappeared, which almost depopulated the countries through which they passed. These have been accurately described by medical writers; but what they have written is simply a history of the disease, and not an explanation of its cause.

Glisson, Hunter, Diemerbrock, Sydenham, Willis, Sennertus, Ferriar, Lind, Pringle, Cullen, and a host beside, have endeavoured to investigate the causes of disease. Some have presented us with ingenious theories, and others with wild speculations and obscure notions, better calculated to bewilder than to enlighten the mind. In short almost every eminent physician, and distinguished professor of medicine has had his own peculiar theory as to the cause and nature of disease, Thus we find solidists at one time, and humoral pathologists at another. Then every thing is referred to the nerves, and we are treated with an essay on sympathies, and morbid sympathetic movements. Nor must we forget the septon and the septic acid of the Dutch School of Medicine, to which at one time, almost all diseases were referred. It would appear, then, that the question of the cause of disease is as unsettled as it was three thousand years ago. But the researches of medical men, like those of the alchymists, have not been altogether useless. They have discovered some important facts, which serve as a foundation for future discoveries. Thus, where there is rich alluvial soil, or low swampy lands in tropical climates, we find disease prevailing, and it is referred to the exhalations emanating from the surface of the earth, and in some instances, from certain plants. Again, in more Northern climates, putrid and malignant fevers of the typhoid type, are clearly traced to an atmosphere vitiated by human beings crowded together in rooms that are badly ventilated, where there is a deficient supply of food, or perhaps vitiated food, with a total disregard of cleanliness. But what those exhalations, and that animal effluvium consist of, is altogether another thing. Chemistry as the science of *inorganic* matter, has thrown no light upon the subject, and probably never will. It remains, then, to be seen what assistance we can derive from organic chemistry in discussing this interesting subject; but before doing this, it may be proper to make some preliminary remarks.

Of chemistry as a *science*, the ancients knew literally nothing. Taking a departure from the time of Aristotle, we find that the Arabians were the first to devote themselves seriously to the study of chemistry. Rhazes, Avicenna, Avenzoor, and others, may be said to have laid the rough foundation. They were followed by Paracelsus and the alchymists. Stahl next appeared, and although erroneous in theory, he nevertheless laid the foundation of a regular science, or rather, improved the foundation that had been begun by the Arabian chemists. The next great advance was made in the discovery of hydrogen by Cavendish, of nitrogen by Rutherford, of oxygen by Priestly, and of carbonic acid gas by Black. As they discovered the four most important elements in the science of animal chemistry, we must date the development of that branch of science from the time of these discoveries, which were made during the last quarter of the 18th century. They were followed by Lavoisier, Guyton de Morveau, and Berthollet, who introduced a new technical nomenclature, which was adopted in 1787. But the most brilliant discoveries have been made in the present century. The galvanic apparatus of Volte enabled Sir Humphrey Davy to demonstrate that the fixed alkalies were compounds of oxygen with metallic bases, Chlorine was discovered by Scheele, but Davy established its elementary nature, as well as that of iodine. It was, however, the discovery of the atomic theory, or doctrine of chemical equivalents, by Dalton, aided by Vauquelin, Gay Lussac, Thenard, Berzelius, and Thompson, which at once elevated chemistry into the rank of a fixed and certain science. Dr. Wollaston constructed the logametric scale of chemical equivalents, which, when proper care is taken, may be considered as accurate as tables of interest, or as the principles of mathematics. Gay Lussac discovered cyanogen; which, as it was the first radical compound discovered, and is more fertile in results than any other discovery yet made in organic chemistry, entitles him to the honorable post of pioneer in this rich and important science.

Up to this period, although organic chemistry had hardly been considered or spoken of as a science, and much less as the true basis of physiology, yet the foundation was laid, first by the use of the microscope, which revealed to Lewenhoeck and Spallanzani the wonders of a new creation; secondly by the discoveries of oxygen, nitrogen, hydrogen, and carbonic acid; and lastly, by the discovery of cyanogen. Since these results were obtained, Redtenbacher, Laurent, Valentin, Gerhardt, Dumas, Boussingault, Raspail, Johnston, Mulder, Roget, Liebig, and many others, have made most diligent researches in this department of science, attended with brilliant and astonishing results. Organic chemistry treats of those substances which result from the ordinary laws of matter, influenced by the mysterious laws of vitality; it brings to light an infinitude of new compounds, and those compound radicals which, in their chemical relations, act precisely as elements. From it we learn that a certain class of substances called nitrogenized, furnish the elements of nutrition; while another class called non-nitrogenized, support respiration. Both are necessary to carry on the operations of nature, and to preserve life, but neither can perform the functions of the other; we also derive correct views of the source of animal heat. In the union of oxygen and carbon which is going on in all parts of the system, a continued slow combustion is kept up in the formation of carbonic acid, Despretz has found that the combustion of 1 oz. of carbon will raise the temperature of 78.15 oz. of from 32 to 212°, and if, as he says, 13.9 oz. of carbon are daily converted into carbonic acid in the body of a healthy adult, evolving 195531 degrees of heat, there is no longer any difficulty in accounting for the production of a regular and necessary supply of animal heat. In organic chemistry, we see most beautifully illustrated the simplicity and the resources of nature. What an immense variety of compound substances resulting from a few elements, has already been discovered, and as yet there appears to be no limit to our researches; an unbounded field is open for scientific investigation, particularly when we reflect on the mysterious connection existing under certain circumstances, between inorganic and organic matter.

But, it is time to turn our attention to the bearing which this branch of chemical science has in relation to the causes of disease.

I shall begin by enumerating as far as may be known to me, or that I can sustain by authority, the animalcules, and organized matter, whether animal or vegetable, which are found in the human body and in animals, and which are not a cause of disease. Animalcules, called spermatozoa, are found in the vas deferens and in the vesiculæ seminales. They were discovered by a student at Leyden, and first described by Lewenhoeck. There are several varieties of these animalcules, the most remarkable of which exist in the vertebrata, investigated and described by Wagner, and in the invertebrata, by Von Siebold. The motions of these spermatozoa, in many instances, continue for several hours after the death of the subject from which they are taken. By some physiologists they are considered as simple organized matter. (*Todd and Bowman, Physical Anatomy.*) Mandell has described the animalcules of the brain; also, those in the blood, which are in some cases very abundant. (*Mandell and Goodfellow.*)

Animalcules have been discovered in the saliva, supposed to form when dead, the tartar around the teeth. (*London Medical and Chirurgical Review.*) Animalcules called cilia, are found on many epithelium membranes; certain surfaces, which are in their natural and healthy state lubricated by fluid, are covered with a multitude of hair like processes of extreme delicacy of structure and minuteness of size. They are called cilia from cilium, an eye-lash. They are generally conical in shape, attached by their bases to the epithelium that covers the surface, in which they play, tapering gradually to a point; they are disposed in rows. During life, and for a certain period after death, these filaments exhibit a remarkable movement of a fanning or lashing kind, so that each cilium bends rapidly in one direction, and returns again to the quiescent state. (*Todd and Bowman.*)

Animalcules of different kinds are formed in the sebaceous glands in different parts of the body. They are found in all persons, especially when the skin is torpid. They multiply in sickness. In healthy persons, one to three may be found in each follicle. (*Todd and Bowman.*)

Animalcules (monads) have been found in all the mucous secretions.

(*Raspail.*) I shall now consider those which may, or may not, be a cause of disease.

Animalcules have been discovered in the sputa of consumptive persons, not known to be either the cause, or result of diseases. (*Bennett.*)

Raspail speaks confidently of the existence of organized matter in the expectoration of grippe, catarrhal affections, laryngitis, and bronchitis. (*p.* 620.)

The last time the yellow fever prevailed in Philadelphia, myriads of animalcules were discovered in the fluid of the stomach. (*Dr. Harlan.*)

Worms of different species have been found in various parts of the body:

1. Acephalocystis endogena, in the liver and abdominal cavity.
2. Acephalocystis multifida, in the brain.
3. Echinococcus hominis, in the liver, spleen, and omentum. (*Bory St. Vincent.*)
4. Cystercus cellulasæ, in muscle, brain, and the eye.
5. Animalcula echinococci, in the liver.
6. Diplosoma crenata, in the urinary bladder.
7. Tenia solium, in the small intestines.
8. Bothriocepalous, in the small intestines.
9. Distoma hepaticum, in the gall bladder.
10. Polystoma pinguicola, in the ovary.
11. Trichina spiralis, in muscle.
12. Filaria medinensis, in cellular tissue.
13. Filaria oculi, in the eye.
14. Filaria bronchialis, in the bronchial gland.
15. Tricocephalus dispar, in the cœcum.
16. Spiroptera hominis, in the urinary bladder.
17. Dactylius aculeatus, in the urinary bladder.
18. Strongylus gigas, in the kidney.
19. Ascaris lumbricoides, in the small intestines.
20. Ascaris vermicularis, in the rectum. (*Owen.*)

Dr. Bird states that minute animalcules of the genus Vibrio, are sometimes developed in urine, so soon after passing it, as to lead to the idea that their germs must have existed in the urine before it passed the bladder. They have been found in abundance in cases of syphilitic cachexia, and in mysentery disease.

Capillary fungiform productions have been observed by Fuchs and others, in several exanthematous eruptions.

Erenburg has observed the chaetophora meteorica growing on the scales of the salmo eperlans.

Owen observed in dissecting a flamingo, a green vegetable mould growing on the lining membrane of tubercular cavities in the lungs, and in the smallest ramifications of the bronchial tubes.

Serrurieur and Rousseau mention having noticed a vegetable mould in a hind. (*Cervis Axis.*)

Lagenbeck observed a high degree of fungous growth, in the body of a man who died of typhus; it extended from the amygdalæ through the œsophagus to the cardia.

Gruby and Delafond state that one, in fifty dogs, is affected with minute filaria in the blood; such dogs, however, enjoy good health; the blood in such animals is usually redder and more serous: regimen, exercise, loss of blood, &c., neither influence the number, form, nor movements of the filaria. When the serum of a dog thus affected, was injected with the blood of an animal not verminous, no filaria were produced; but when the blood was injected, they were produced, but without affecting the health of the subject. These filaria are only found in the blood.

Wilson and Goodsir have observed in the discharges from the stomachs of dyspeptic persons, and in the stomach after death, a microscopic cryptogami plant, to which Goodsir has given the name of carcina ventriculi.

The pollen of flowers contains animalcules. (*Raspail.*)

It is probable that the phosphoric light emitted by fish, and observed in the wake of a vessel at night, is occasioned by animalcules. In dark nights, the fishermen on the coast of Scotland are aware of the approach of large quantities of fish, by a light thrown on the clouds from the sea. When this fact was announced to me, I scraped a few scales from a herring, which were agitated for a minute or two, in a small phial, with water. On examining a single drop of the water with the microscope, numbers of animalcules were seen of an oval shape, rather sharpened at the ends; they were incessantly revolving, presenting alternately a luminous and a dark side. In short, as a general rule, every animal and every plant, is more or less affected with parasites, some peculiar to it, and some not so, and with organized matter, which may or may not be a cause of disease.

But there are diseases of the human body, of animals, and of plants, which are known, and acknowledged to be produced by animalcules, or by vegetable or animal organized matter. Psora or scabies is now universally acknowledged to be occasioned by animalcules which have been distinctly seen and accurately described. Avenzoor appears to have been aware of this fact; he speaks of an animalcule which exists in the skin. It 1657, Scaliger speaks distinctly of the Acarus Scabiei; its form is globular; it can scarcely be seen with the naked eye; it burrows under the skin, producing a burning and itching sensation. Linnæus treats of the same animalcule as the cause of scabies. (*Journal des Progres des Sciences.* T. 4.) And it is fair to conclude that the following diseases described by Willan, originate from a similar cause. 1. Psoriasis guttata; 2. psoriasis diffusa; 3. psorias gyrati; 4. psoriasis palmaris; 5. psoriasis labialis; 6. psoriasis scrotalis; 7. psoriasis infantilis; 9. psoriasis inveterata.

Porrigo is a disease occasioned by a growth of organized matter; by some supposed to be vegetable; but in this, as in many other instances, there may be some doubt as to the correct classification. Wilson gives it the name of acarus folliculorum. Dr. Wallace, of New York, says it has been ascertained to be a vegetable which even sheds its seed.

The chigger or chigre (pulex penetrans) is a very troublesome insect in tropical climates. Its attack may be warded off, if promptly attended to; but if neglected, it will produce a very serious and distressing disease. It generally attacks the feet, producing an intolerable burning and itching sensation; it generates rapidly and spreads in every direction under the skin; its course can be traced by a hard red line. I have seen in Florida, the bones of the foot and particularly of the toes, laid bare by this disease. Cuvier says the ulcer caused by them is cured with difficulty, and sometimes proves mortal.

The vermis medinensis—vena medinensis—or vermiculus capillaris—or Guinea worm, produces also a very troublesome disease. This animal is common in both Indies, in most parts of Africa, occasionally at Genoa, and in other hot countries. It resembles the common worm, but is much larger; is commonly found in the legs, but sometimes in the muscular part of the arms. While it moves under the skin, it creates no trouble; but, at length, suppuration takes place, and the animal protrudes its head. Considerable skill is required to extract it, for if it is drawn so forcibly as to break it, the part left within creates intolerable pain. In the Edinburg Medical Essays, mention is made of one that was three yards and a half in length. (*Hooper's Dictionary.*)

The tape worm, tœnia osculis marginalibus—tœnia osculis superficialibus—cucurbitinæ—is a cause of disease producing very distressing symptoms. They have been seen from ten to one hundred and sixty feet long. (*Chapman.*) The ascarides and the lumbricoides, the round worms, have already been alluded to as not necessarily producing disease. It is probable that they feed on the secretions of the intestines, and that when these secretions are suddenly carried off, they produce disease by acting on the inner membrane of the bowels or stomach. Chapman attributes the following diseases to their irritation. Opthalmia, paralysis, aphonia, croup, hydrocephalous, phthisis, dysentery, pleuritic pains, stupor, lethargy, mania, febris verminosa, epilepsy, &c. There is a round worm from four to six inches long, almost transparent, terminating at both ends in a horny sharp point. I have seen it ejected from the stomach by the operation of an emetic, but do not recollect ever to have seen it passed by the bowels. Cases have occurred where this worm has caused death, by perforating the coats of the stomach.

Both the ascarides and lumbrici have been found in a well near Cork, and it was remarked, by Dr. Barry, if I mistake not, that the inhabitants, in the vicinity, who drank the water, were very much troubled with worm. Worms covered with coagulum were found in the pulmonary artery of a dog, from one inch to ten or eleven inches long; the dog sickened and died in ten weeks from the beginning of the attack. No other cause could be assigned for his death. (*Wright—London Lancet, American Edition, p.* 354.)

Myriads of animalcules in lively motion have been seen in the matter vomited from the stomach during life; also, in sanguineous exudations of the gums and nostrils, and in the blood from the capillaries of the skin: they were considered a cause of disease. (*Goodfellow.*)

Raspail has no doubt that an insect is the cause of cholera, whose location is in the lower part of the intestines. He also believes that plague, yellow fever, cholera and malignant fevers of every kind are caused by parasites; and also all cutaneous diseases. Linnæus considered the exhalations from the Pontine marshes to be organised matter. Professor Harrison regards the cause of yellow fever to be of an organic nature. (*New Orleans Med. and Surg. Jour.*) Donné has discovered in the pus of chancre, by the aid of the microscope, animalcules which are constantly present, and which he regards as the cause of the transmission of the disease. (*Annales de Sciences Naturelles, series* 2, *vol.* 6, *p.* 157.) The same author has observed in the secretions around the glans penis in cases of gonorrhœa, a species of vibrio; another subject inoculated with this pus, produced a pustule from which a fluid escaped, abounding in the same animalculæ. The same fact has been observed by Dujardin. Hydatids may be classed amongst the organized causes that produce disease; they are always connected with serous membranes. They are found in the abdomen and ventricles of the brain, and more frequently in the liver, kidney, and lungs, where they produce diseased action of those viscera. The common color of hydatids is white, yet they are occasionally seen of a light amber color. (*Bailie.*) "On the inside of a hydatid smaller ones are sometimes found, which are commonly not larger than the heads of pins, but sometimes as large as even a gooseberry. Hydatids of the liver are often found connected with each other, but sometimes they have been seen to enclose each other in a series, like pill boxes. The origin and real nature of these hydatids are not fully understood; it is extremely probable, however, that they are a sort of imperfect animalcules. There is no doubt at all that the hydatids in the livers of sheep are animalcules; they have been seen to move when taken out of the liver and put into warm water; and they retain the power of motion for a good many hours after the sheep has been killed. The analogy is great between hydatids in the sheep and those of the human subject." (*Bailie.*) They have been found in the brain of a sheep, and sometimes on the end of the cartilages of the eyelids, or on the conjunctiva. (*M. De St. Ives.*)

Klencke, of Brunswick, has shown that many contagious diseases owe their transmissibility to morbific cells which seem to possess a semi-individual life. He has demonstrated the contagion of melanosis in an experiment performed on a horse. Some of the cellules found floating in a black pulpy mass of a melanotic tumor were inserted beneath the skin, and the result was the production of a growth similar to the original one. He also says that various condylomatous tumours, as well as ozenia, choriza, &c., are transmissible in the same way, and that cellules of a recent choriza are very different from the confervæ of ozenia, and that as the disease declines, the cellules gradually disappear, and are replaced by sporules of confervæ. He further mentions that he has detected the morbific cells of the malignant carbuncle in all the yellow colored discharge that flows from the gangrenous sore; and he has detected those of hydrophobia not only in the salivary glands of dogs affected with rabies, but also in a wound caused by their bite. According to his researches the proximate cause of vaccina and variola is the existence of morbid cells in the circulating fluid, and the severity of these diseases is in general proportional to the number of cells developed. "Ergot, secale cornutum, a fungiform growth, a disease of the secale cereale, occasioned by an insect which penetrates the grain and feeds on its amylaceous parts, and leaves its poison in the parenchyma. This poison is probably the larvæ of the insect. Ergot has a singular effect on the animal economy. The meal or flour, sprinkled on a wound, coagulates the blood, excites a heat, and then a numbness in the part, and soon after in the extremities. Bread made from it, produces intoxication, lassitude, a sense of something creeping in the skin, weakness of the parts, with convulsions occurring periodically. It produces a species of dry mortification, which commences in the toes or fingers, and which gradually extends itself. It is called necrosis cerealis, and has proved fatal in some parts of France." (*Hooper.*) Dr. Wallace of New York observes, speaking of ergot, it is probably that the species of fungus* will vary with the plant, and that the variety of the fungi will produce different effects; thus, the dust from one kind of fungus or other parasite may produce yellow fever, from another cholera, and so on. The same writer says parasites, then, are the chief sources of disease.

Epidemics are frequent among silk worms; they are attacked by an epizootic fungus called muscardine. When a worm dies there arises from its surface a multitude of minute fungi, whose roots had been previously spreading under the skin; it then spreads sporules in every direction, infecting all the worms and other insects about the establishment. (*Audoin.*) A form of the same disease, (spheria entomorhiza,) is very destructive to the wasps in the West Indies, and to those in our own country. (*Carpenter.*)

Fungi have been found in the air cells of the eider duck. (*Deslongchamps.*)

In man all the vegetation yet discovered have been found connected with the matter effused into the textures in scrofulous constitutions. (*Bennet.*) He remarks on the association of parasitic vegetation with tubercles; "the fungi found growing on the tuberculous cavities of the lungs, and others discovered by Schonlin, and described by Gruby, constituting scrofulous eruptions of the skin, grew on a finely granular amorphous mass, which presented no evidence of organization." The fungi found by Rousseau and Serrourier in the paroquet, grew on a species of false membrane. Pigeons are also frequently destroyed by the same kind of parasitic vegetation.

According to the observations of Valentine the parasitic confervæ found growing upon fish are connected with the diseased state of the animal.

Psorosperms have been described by Muller, also the corpuscules, noticed by the same naturalist, as constituting a peculiar disease of the swimming bladder of the gadus callurias.

Laurent has observed coyptogamic vegetations in the eggs of the Limax agrestis, which more or less impede the development of the embryo; and they may therefore be considered a cause of disease.

Bennett gives an excellent account of the mucedo of favus, and has illustrated his observations by some beautiful delineations. The same disease has been found in the mouse.

Sycosis has recently been made the subject of research by Gruby of Vienna. This gentleman has announced the discovery of a cryptogamic plant developed in the root of the hair, in the form of sycosis, to which he assigns the designation mentagra contagiosum. In harmony with the view entertained by Mr. Gruby, he suggests that favus should form a new order with two other diseases of vegetable origin; aptha and sycosis contagiosum. The itch of which I have already spoken, affects horses and dogs as well as man; but in each case, the disease is produced by a different animalcule, although the disease presents the same appearances. (*Raspail.*)

Epidemics amougst the lower animals, and amongst plants, are

* Acinula clavus, and by some considered the cause of the disease in rye.

governed by the same laws as those which affect man; and as far as the subject has been investigated, the cause has been found to be either animal or vegetable.

The smut in wheat and other grains is known to be a species of fungus called puccinia. (*Carpenter.*)

The rot in cotton is occasioned by a minute fungus of the genus spheria, which attacks the bowls. The rust in the same plant is also a spheria, which attacks the root, permeating all the tissues of the root, and lower part of the stem, cutting off nutrition from the upper part, causing the death of the leaves, and finally of the plant. (*Carpenter.*)

The peach is sometimes attacked by a minute fungus of the same kind, which is epidemic and often very destructive. (*Carpenter.*)

The disease of the potato which has created such havoc for some years past in Europe, has been discovered in France to be a species of the fungi. It is probable that the murrain, as it is called, that has destroyed the potatoe crop in Ireland lately, is owing to the same cause. (*Carpenter.*) There have been several remarkable epidemics this year affecting animal as well as vegetable life. In the spring we were informed that the sea from Long Island to the Vineyard Sound was filled with dead fish. Recently it has been stated that the terrapins and fish in the Dismal Swamp, North Carolina, were dying by thousands. In fact every kind of animal matter is subject to the depredations of other animals; some reside on the surface alone, and do not produce disease; some attack the skin; others penetrate the tissues, producing various degrees of irritation. Some are confined to certain districts, as the Guinea worm, chigger, &c., and others abound everywhere. The varieties of organized matter or of animalcules may be as great as the ever varying circumstances favorable to their production.

From the time of Hippocrates it has been observed that the atmosphere which produces malignant fevers, causes the production of myriads of insects. And the myriads seen by the unassisted eye are but as an unit when compared with the myriads on myriads which are revealed by the use of the microscope, and of whose existence man would never have dreamt without the aid of that instrument. Errhenberg has determined that the smaller monads are near one twenty-four thousandth part of an inch in diameter; and he has estimated that there are *five hundred millions* of them in the space of a cubic line, or drop of liquid which he examined. These monads are the smallest of visible animalcules, and they have been spoken of as constituting "the ultimate term of vitality." (*Roget.*) Without the aid of the microscope some insects, millions of times larger than a monad, would have been considered the ultimate term of vitality; and there may be, and no doubt are, organized beings as many millions of times smaller than a monad as *it is*, compared with an insect visible to the eye. The human mind can form no just conception of the ultimate term of vitality any more than it can of the ultimate molecule of matter. In either case there is a limit which we cannot transcend; but though man may find, from the finite nature of his faculties a point beyond which he cannot extend his enquiries and prosecute his researches, yet there is no limit to creative power, and to the wonders of creation. The infusory animalcules, or infusoria were so named by Muller, a Danish naturalist, from the circumstance of their swarming in all infusions of vegetable or animal substances that have been kept for a sufficient time; it is to the microscope alone that we owe our knowledge of their existence, and of the curious phenomena they present. (*Roget.*) Of these animalcules there are innumerable varieties: monas, rotifera, cyclidium, volvox, cercaria, kerona, vibrio, gonium, kolpoda, urcolaria, vorticella, proteus, volvoa globator, volvoa conflector, &c. The mode in which infusory animalcules are produced and multiplied is involved in much obscurity. Many distinguished naturalists adopting the views of Buffon have regarded them as the product of an inherent power belonging to a certain class of material particles, which in circumstances favorable to its operation tends to form those minute organizations, and in this manner they explain how the same organic matter which had composed former living aggregates, on the dissolution of their union reappear under new forms of life, and gives rise to the phenomenon of innumerable animalcules starting into being and commencing a new, but fleeting career of existence. Yet the analogy of every other department of the animal and vegetable kingdom is directly opposed to the supposition that any living being can arise without its having been originally derived from an individual of the same species as itself, and of which it once formed a part. The difficulty which the hypothesis of the spontaneous production of infusory animalcules professes to remove, consists in our inability to trace the pre-existence of the germs in the fluid where these animalcules are found to arise, and to follow the operations of nature in these regions of infinite minuteness.

The discoveries of Errhenberg relative to the organization of the rotifera, go far towards placing these diminutive beings on a level, both in structure and in function, with the larger animals, of whose history and economy we have a more certain knowledge, and in superseding the hypothesis above referred to for the explanation of the observed phenomena. In many of these animalcules he has seen the ova excluded in the form of extremely minute globules, the twelve thousandth part of an inch in diameter. When these had grown to the size of the seventeen hundredth part of an inch, or seven times their original diameter, they were distinctly seen to excite currents, and to swallow food. The same diligent observer detected the young of the rotifera vulgaris, perfectly formed, moving in the interior of the parent animalcule, and excluded in a living state, thus constituting them viviparous animals, as the former were oviparous. Other species, again, imitate the hydra, in being what is termed gemmiparous, that is producing gemmules, (like the budding of a plant), which shoot forth from the side of the parent, and are soon provided with cilia, enabling them when separated, to provide for their own subsistence, although they are of a very diminutive size when thus cast off. (*Roget's Ani. and Veg. Physiology, vol.* 2.)

However strongly, then, some mysterious facts may appear to support Buffon's fanciful theory, it is certainly untenable when we consider the facts and striking analogies that can be arrayed against the doctrine of an inherent power in material particles capable of producing organic matter.

Aristotle speaks of an acarus which is engendered in old cheese. (*Journal des Progres des Sciences.*) Of these acari or mites, Cuvier observes they are oviparous, and excessively prolific. (*Animal King-*

dom, 3.) How then do the animals from which these ova spring, get into the cheese, found as they often are, imbedded in the heart of a solid cheese, which would seem to be impervious to external agents? The organic matter from which they originated, must have been deposited in the milk from which the cheese was made. The same remark will apply to those active little skippers, which are of the color of the cheese, and have the same taste as cheese, and are often found imbedded, when they can have no cônnection with the atmosphere or external agents. "The most mysterious circumstance in the natural history of the Infusoria, is the susceptibility which some of them possess, of remaining an indefinitely long time, in a perfectly dry and seemingly lifeless condition. The rotifer redevivus, or wheel animalcule, which was first observed by Lewenhoeck, can only live in water, and is commonly found in that which has remained stagnant for some time in the gutters of houses. But it may be deprived of this fluid, and reduced to perfect dryness, so that all the functions of life may be completely suspended, yet without the destruction of the vital principle; for this atom of dust, after remaining for years in a dry state, may be revived in a few minutes, by being supplied with water. This alternate suspension and restoration of life may be repeated, without apparent injury to the animalcule, for a great number of times Similar phenomena are presented by the vibrio tritici, an eel-like animalcule, which infests diseased wheat, and which, when dried, appears in the form of a fine powder. On being moistened, it soon resumes its living and active state. (*Ridell.*) The gordius aquaticus, or hair-worm, which inhabits stagnant pools, and which remains in a dry and apparently lifeless state when the pond is evaporated, will, in a like manner, revive in a very short time, on being again immersed in water. The same phenomenon is exhibited by the filaria, a thread like parasitic worm, which infests the cornea of a horse. (*Roget; Physiology.*) In the Edinburg Encyclopedia, it is stated that the wheel animalcules have been thus resuscitated from a state of dormant vitality, as many as seventeen times in succession, and that the presence of sand is necessary in the fluid, or they will not revive. (*Ridell.*) Such facts as the foregoing, are amply sufficient to explain those vital phenomena, which would otherwise be to us both inexplicable and mysterious. In no other department of animated Nature, are we presented with such strange and anomalous modes of reproduction. Many of the globular monads and vorticellæ increase by spontaneous and equal division. The living globule will, at first, appear as if encircled by an equatorial band, which will continue to be drawn more and more tight, until a complete separation occurs, each portion being an independent monad, which in turn is bisected like its parent. In this manner, a mysterious multiplication goes on indefinitely. The monas uva consists of four or five corpuscules in a cluster, by the spontaneous separation of which, the species is propagated. (*Riddell.*) "The volvox globator consists of a spherical membranous sac, filled with liquid, in which float many more diminutive globules like itself. These have precisely the same structure as the enveloping membrane, even to containing within them a series of still minuter spherules. Observers have thus seen the fifth generation in the same individual animalcule. The gonium pectorale has an angular flattened body, containing sixteen corpuscules, which subsequently become distinct animalcules, like those in the volvox." (*Riddell*)

Bennett observed a curious being, provided with a beak, in an infusion of hemp, which, fixing itself to some solid substance, assumes a spherical form, and rotates irregularly until it bursts into four animalcules. It is, indeed, astonishing to observe, how short a time is sufficient to bring these beings to full maturity. An infusion of beet yields a species that increases by detaching obliquely, a small piece of its own substance, which, after a lapse of a single day, is also capable of propagating; and the vorticella ramosa exercises the power of reproduction, within a few hours only, after having been itself ushered into existence. So far as investigation has been carried, this proposition has been fairly established, that animal or vegetable food is essential to every subject of the animal kingdom. It must, therefore, follow, if the proposition be universally true, that the infusoria feed on organic substances; and it is probable that many of them subsist on corpuscles more minute than themselves. Goeze has seen the trichodim cimex, a bristly microscopic creature, of an oval form, seize upon and devour the lesser animalcules with great voraciousness.

I have been spared much trouble and research by quoting freely, in the two last pages, from an admirable treatise on miasm and contagion, written in 1836, by Professor Riddell, and read before the Cincinnati Medical Society.

It has been seen that the science of chemistry, relating to mere matter, has never been able to develope the true causes of disease.

An inquiry has, therefore, been instituted to ascertain what light animal chemistry, or physiology and chemistry combined, aided by the microscope, can throw upon this interesting subject.

A great variety of cases has been brought forward, of diseases produced in man, in animals, and in plants, by organized matter, either animal or vegetable, and the facts are sustained by the highest authority. This, at least, furnishes strong presumptive evidence, that all malignant fevers attributed to miasm, or to animal effluvium; all the exanthematous, and in fact, all diseases whatever, except such disturbances in the system as may arise from external injury, or the internal administration of poison, and, perhaps, in a few cases, from sudden and great changes of temperature, or any excessive indulgence of the appetite or passions, are produced by organized matter, in some form or another.

It is a well established fact that in all parts of the world where local circumstances exist to produce malignant diseases, there myriads of insects abound; and no doubt, for each one of those insects visible to the eye, there are countless series of myriads of animalcules, which neither the eye nor the microscope can discover. As different plants and animals are found in different climates and in different countries, so, no doubt, different kinds of animalcules are generated according to local circumstances. Thus plague is probably produced by animalcules peculiar to the valley of the Nile, and from thence propagated to Smyrna, Constantinople, &c. As the cause of cholera, we would expect to find another kind of animalcules in the Valley of the Ganges, and the variety producing yellow fever, in the yellow fever region; and so on, with regard to every kind of malignant disease of atmospheric origin, caused by what is usually called malaria. No such disease is contagious, but all diseases caused by human effluvium are more or less contagious. The animalcules, then, that produce yellow fever, do not extend their influence beyond the individual, who can never communicate it to another person; but the animalcules that produce the contagious malignant epidemics of the Northern latitudes certainly extend their influence from one person to another, either directly, or indirectly; and the following case goes far to prove that organized matter is the contagion itself. I recollect to have read many years ago, an account of what is called the Black Assize of the city of London. The prisons were very much crowded with state prisoners; the malignant hospital or jail fever broke out, attended with great mortality. On a certain occasion several prisoners were brought out of the pestilential atmosphere, and arrayed before the recorder and several assisting judges; the court room was crowded, and a window was opened to admit air. All to leeward of the current of air that passed over the prisoners, including the recorder, some of the judges, lawyers, jurymen and spectators, contracted the jail fever, of which nearly all died.

Now the prisoners had not at the time, nor had they previously had, the disease in question. They therefore could not communicate to others a disease which they had not themselves, by radiating that human effluvium which is the result of the disease. The disease, then, must necessarily have been produced by the germs of the animalcules that are the cause of the jail fever, which were on the clothing of the prisoners, and carried from them by the current of air to those persons who contracted the disease. And there is nothing absurd or unreasonable in this supposition, when we reflect how indefinitely small the atoms of this organized matter may be, and in what an extraordinary manner its vitality may be maintained.

But the question may be asked, how does organized matter act on the system? Inasmuch as parasites have been found in almost all parts of the body, and which they could only reach by means of the blood vessels, then it is evident that organized matter can make its way into the blood vessels, and become developed in organs; and let this be the answer to the question proposed.

We have now positive and varied proof that very many diseases, attacking not only the human family, but animals and plants, are occasioned by parasites, or organized animal or vegetable matter in some or another form. Reasoning by analogy, it would be a fair inference to suppose that all diseases, with very few exceptions, are occasioned by a similar cause; and especially, when there is no shadow of proof or argument to establish any other doctrine.

Professor Riddell remarks: "Miasmatic poisons, when applied to the animal system, generally require several days before the obvious development of any effect. This time, called the latent period, affords a strong argument in favor of the organized nature of the poison; for ordinary poisons never delay their action so long; whereas, if contagion consists of living corpuscules like the ova of insects, or the germs of plants, they would naturally require time for their development and multiplication."

It has been established by the experiments of Moscati and Boussingault, that organic matter exists in extremely small quantities in the noxious air that hovers over marshes. Moscati, many years ago, suspended in the air, over the rice grounds of Tuscany, a globular glass filled with ice. An abundant deposition of dew took place upon its surface, which, when collected, appeared at first to be pure limpid water. There was soon, however, an appearance of little flakes, possessed of properties peculiar to animalized matters, and finally, at the end of some days, the liquid putrefied completely. (*Riddell.*)

These facts are testimony directly to the point, and they are not yet exhausted, In a memoir read before the French Academy of Sciences, in 1834, M. Boussingault reports some striking experiments tried by him at Cartago, in South America. In the middle of a swampy meadow, in every instance, carbonaceous matter was detected in the dew, by the addition of sulphuric acid. He remarks ; "The results obtained prove very clearly that in marshy places, during the precipitation of dew, there is an *organic matter* deposited with it." (*Riddell.*)

The following experiments made by Professor Riddell, " with a view of detecting the aerial miasm of small pox," are so much in point, and so interesting, that I should do injustice to the subject were I to omit to quote them. The apparatus is thus described : A perfectly clean ounce phial was half filled with distilled water ; a small glass tube, with a capillary orifice, was made to terminate near the bottom, the upper and much larger portion of the tube bending horizontally to receive the silver nozzle of a delicate pair of bellows ; several turns of gauze were passed around the mouth of the phial, embracing the tube, and the whole was securely fixed in an appropriate frame-work. This apparatus was carried to the city* pest house, and under the superintendence of Dr. Herron, it was placed on a table two or three feet from a small pox patient, just in that stage of the disease when the circumambient air was supposed to be most contagious. The bellows were blown by the nurses pretty constantly for twelve hours, thus presenting a great amount of noxious air to the distilled water, The apparatus was left undisturbed until it came into my possession, three days after, when I made the following experiments.

1. One-fourth of a drachm of the water contained in the phial, evaporated very slowly in a watch glass, over an alcohol lamp, left concentric circles of a whitish substance. Upon bringing this residue under the object glass of a good microscope, I discovered that it consisted mostly of long crystals, which shot from each other at right angles. The outer margin of each concentric band was less distinctly crystalline, and evidently contained some other substance.

2. A minute drop of sulphuric acid, carefully distilled and collected on a glass rod, so as not to leave the slightest trace when evaporated from clean glass, was placed upon some of the residue. [Experiment No. 1.] The application of heat rendered the acid black, and upon complete evaporation a dark stain was left, thus showing the presence of organic matter.

3. Upon adding a drop of pure sulphuric acid, to near an eighth of a drachm of the water, and expelling the water by a careful heat, the acid became black. This experiment, as well as the one which follows, was

* Cincinnati.

performed upon a piece of Florence flask, rinsed in clean water, and then heated to redness over an alcohol lamp, in order to remove every trace of organic matter.

A drop of the water hastily evaporated, left a whitish residue, not crystalline to appearance, but consisting of extremely minute grains. Upon the application of a high heat short of redness, it became dark colored, indicating the presence of organic matter by the charcoal liberated. A still higher heat, in contact with air, removed the dark color, and left a mere trace of white adherent powder.

These results compel us to believe, that organic matter was communicated to the distilled water by the air which was transmitted through it. This matter did not exist in the air by virtue of its volatility, else in the first experiment, it would have been dissipated by evaporation. It was most likely in the form of organized corpuscules, sustained in the air by their exceeding small size. I have quoted these experiments in detail, that others may know precisely how to repeat them, and that doubters may have an opportunity of fully satisfying themselves.

There is one more curious experiment, on snow water, by the same author, which I think it worth while to transcribe : "In the winter of 1833, I prepared several bottles of water, from clean, recently fallen snow. They were tightly corked, and kept for three or four months in the shaded corner of a room, where the water was not liable to be frozen. At the expiration of this time, having occasion to use some of the water, I observed that the lower portion of each bottle was traversed by myriads of delicate dark colored filaments, bearing a close resemblance to some of the fresh water algæ. Upon removing the cork, a most unpleasant odor was exhaled, similar to that of animal putrefaction. No one, I presume, will doubt, that the living germs of this curious organization came down from the high regions of the atmosphere, in conjunction with the snow."

This experiment is interesting, because it shows how epidemics, such as the cholera, for instance, can travel over the world, and it accounts for the disease appearing in some countries, and leaving others untouched ; as the living germs descended to the surface of the earth, or were borne up to the higher regions of the atmosphere ; and there is no other possible way of accounting for the erratic and extraordinary course of that pestilence, when it last left the valley of the Ganges and passed over the civilized world.

It has been already observed that the celebrated naturalist Linnæus, considered the miasm of the Pontine marshes to be of an organized nature, and doubtless he was right, for how could the miasm of the Pontine marshes be carried to Rome, eighteen miles, unless we admit the existence of organized matter, in minute germs or corpuscles? Mere material atoms would be dissipated and scattered in every direction by the wind, before a sufficient quantity could arrive at a point eighteen miles off, to produce disease; while a very small number of minute corpuscles, having in them the germ of vitality, and the power of rapid generation, would suffice to engender a pestilence. With such facts and such authorities arrayed in proof of the organized nature of the causes of disease, am I not justified in calling upon the enlightened members of the medical profession, in all countries, to turn their attention to this subject, and to investigate it calmly and impartially? No one can deny that its claims are strong. It is the true field of science, which invites our patient and persevering researches, and where we shall not labor in vain. It is, at least, the only one left, where with the present lights of science, we can have any reason to expect success. In the discussion of this subject, and in its elucidation, physiology and chemistry *must* go hand in hand.

Much confusion arises from the improper use of terms. We read of remote, and predisposing, and exciting, and ultimate causes of disease, and some authors call the first departure from the normal condition, the proximate cause; thus confounding cause and effect. Liebig, in his chapter on the theory of disease, says, "disease occurs when the sum of vital force which tends to neutralize all causes of disturbance (in other words, when the resistance offered by the vital force,) is weaker than the acting cause of disturbance." This may be more clearly expressed in fewer words, to wit: whatever tends to disturb the equilibrium on which the normal state depends, is a cause of disease. This is, no doubt, true, and this disturbance of the equilibrium of forces, is the first link in the chain of abnormal motions and symptoms. We want but two causes to operate in the production of any disease, under any circumstances; and in many instances, one is sufficient. Any thing that has a tendency to weaken the vital power—the force of resistance—whether it be cold, or want of food, or vitiated food, or excessive fatigue, or sensual indulgence of any kind, is a *predisposing* cause of disease; and that malaria which is in the atmophere, be it mere matter, or organized matter, for it amounts to the same thing, is the *exciting* cause of disease. Thus one man by prudence may resist the cause of yellow fever, or cholera, while another by dissipation, so weakens the power of resistance in the system, that he falls an easy prey to the devouring pestilence.

Liebig is right when he combats the opinion that putrefaction is produced by the microscopic animals, and when he observes "the presence of microscopic animalcules often perceived in such enormous numbers in putrefying matter is not particularly to be wondered at, since they find there the conditions of their nutrition and development." [*London Lancet*, *p*. 367.] It is to be regretted that this distinguished physiologist and chemist has not written a chapter on the causes, as he has on the theory of diseases. Whether the cause of pestilence is of an inorganic nature, or whether it is organized matter, is certainly interesting in a scientific point of view, but in a practical point of view it is of comparatively little importance, since the same condition of things that would produce malaria, is equally favorable to the production of animalcules; therefore whatever means may be employed to suppress exhalations from the earth and from putrefying substances on the surface of the earth would equally prevent the generation of animalcules; they cannot be generated in pure air or in pure water.

In concluding this article, it is due to Prof. W. M. Carpenter to acknowledge that the mass of information herein adduced respecting parasitic animals, animalcules and organized growths, was kindly furnished by him, he having for many years past, observed and collected such facts, with reference to their bearing upon the origin and causes of disease; and it is much to be hoped that ere long he will, over his own signature, give the results of his inquiries to the world.

My attention was first called to this subject in 1839 by Professor J. L. Riddell, the first writer I have met with who has consistently advocated the organized nature of miasm and contagion. (*Western Journal of Medical and Physical Sciences; March*, 1834.) At that time I entertained very different views; but after a fair examination of the question, I feel pleasure in sacrificing early prejudices and long-cherished theories for what I believe to be the cause of science and of truth.

ART. V.—*Observations upon the Treatment of Wounded Arteries.* By WARREN STONE, M. D., Professor of Surgery in the Medical College of Louisiana. [Continued from the September number.]

CASE 3d.—Mr. G———, aged about 27 years, of good constitution, was admitted into the Maison de Santé, or Stone & Kennedy's Infirmary, in December 1844, for femoral aneurism. He represented that about a year previous, when in the act of lifting a barrel of molasses, his right foot slipped, and the leg was forcibly abducted. He instantly felt a sudden pain in the upper and inner part of the thigh, and from that time had observed the tumour, which had gradually increased until he was disabled. The ligature was advised, and as the tumor was high up, I thought proper to tie the artery above the profunda, and with this view, tied it about three-fourths of an inch below Poupart's ligament, where the size of the artery was such as to satisfy me that I was above the profunda. Pulsation ceased in the tumour; the wound united as kindly as usual in such cases. The angles united by the first intention, and the rest of the wound filled with healthy granulations, leaving merely a sinus around the ligature.

Some days after the artery was tied, I observed upon examination that pulsation existed in the artery to within an eighth of an inch of the ligature. Subsequently the pulsation could be traced past the ligature, although the tumour was quiet and subsiding rapidly. I was, of course, convinced that I had tied the artery just below the bifurcation, and warned the patient of the additional danger of bleeding, and of the importance of absolute rest. He, however, grew impatient of confinement and on the twelfth day, made unnecessary exertions, and profuse hæmorrhage took place. Mr. McManus, the very able house surgeon, made pressure over the artery, which controlled the bleeding for the time, and sent for Dr. Carpenter (who was near) and me. On my arrival I found every thing prepared for tying the artery. I decided that the case could be treated safely without another operation. The case was simply this: the artery had given away in consequence of the position of the ligature which left no space for a coagulum. But every thing was in a healthy condition; and when the ligature was removed there was merely an orifice, which had only been kept open by the necessary presence of the ligature, and prevented from being filled by granulations.

Here then was a sinus leading from the divided extremity of the artery to the surface, eking it out as it were, and rendering one continuous tube to the surface. The ligature being removed, nature was ready to fill speedily that portion of the tube from the surface to the point where the ligature was applied, with granulations, and experience had taught me that it was only necessary to avoid any injurious loss of blood, and that nature would in a few days close the sinus. The force of the current of blood in an artery is overrated, for very slight pressure upon the bleeding mouth of an artery is suffieient to control bleeding. The ligature was removed, and a light compress of dry lint applied, and confined by adhesive plaster; being careful, however, not to make too great pressure, lest the granulating process might be interrupted. It was my intention to dress every twelve hours, or as often as the lint became moist by the discharge, but being called away, and my directions not being understood, the dressings were allowed to remain and bleeding occurred. Strong compression was made, but notwithstanding, bleeding occurred again. On removing the dressings I found that absorption of the granulations was going on in consequence of undue pressure. I then explained the principle upon which I relied for a cure; which is, that very light pressure with dry lint in cases like this, is sufficient to control bleeding, if renewed often, at least as often as it becomes saturated by the discharges, and that the bleeding vessel will be closed by granulations. This course was carefully followed, and no more bleeding took place. It was the unhesitating opinion of several physicians who saw this case that the patient was saved by avoiding a second operation; for the patient was seized with erysipelas, which was very prevalent, and of a bad character at the time; just at the period when the adhesive process should have been going on, had the artery been tied a second time; and from the severity of his symptoms there is little doubt that bleeding would have occurred again.

I could mention many other cases of smaller arteries; but this case is sufficient to illustrate the principle, which is all I have aimed at.

Case 4th.—Mr. P., while defending himself from an attack, received a wound in the hand from a dirk, which evidently divided the deep palmar arch. I saw him five days after the accident, and was told by him that profuse hæmorrhage followed the wound. A physician was called, a compress was applied, which arrested the bleeding for the time; but almost thirty-six hours afterwards, it occurred again. The compress was renewed; but twenty-four hours afterwards the hæmorrhage returned, and continued to return regularly every twenty-four hours, up to the time I saw him. I advised him to enter the hospital, as he was evidently suffering from the loss of blood, and would be in danger, if not relieved. Believing that compression could certainly secure an artery of this magnitude, I made a graduated compress, and filled the palm, so that the bandage could have full effect, at least as much as the parts could bear without endangering sloughing. The watchman in the hospital was directed to see him often; and at the usual hour bleeding occurred, in spite of the compress. I was called, but hæmorrhage ceased before I could remove the dressing. The compress was renewed, with more force; in fact so great as to induce a slight degree of sloughing, but hæmorrhage followed after the usual interval, notwithstanding. The hand was then left without dressing, and appeared to be the better, for it was relieved from a source of irritation, and no bleeding occurred until the usual hour, when it was arrested by the spontaneous formation of a clot, with as little loss of blood as when the compress was applied. The case had now become dangerous, and the radial and ulnar arteries were tied at the wrists, and the patient recovered.

This case illustrates two principles: first, that when an artery is wounded, and the bleeding is arrested externally, but allowed to go on internally, filling the tissues, and forming a clot around the mouth of the artery, larger than can become organized, hæmorrhage is likely to occur when the clot dissolves, as it is bound by the laws of nature to do; secondly, that when the first effort of nature fails, and the clot gives way, there is very little probability of a second clot becoming organized. Strong compression will never effect it; in fact, I believe it never occurs, but granulations may shoot out from the surrounding cellular tissue, and enclose or cap the mouth of the vessel, and thus effect a cure. Strong compression would of course interrupt this process, but where secondary hæmorrhage occurs, and the wound is tolerably healthy, a little lint may be pressed down upon the mouth of the vessel, and gentle pressure made, such as will not interfere with the granulating process, and in almost every case where minor arteries are concerned, a cure will be effected.

CASE 5,—Mr. M, a mechanic, had an altercation with a fellow workman, who assaulted him with a narrow chisel, and struck him in the fore arm, about two inches below the bend of the arm, and cut the ulnar artery, where it lies under the bellies of the flexor muscles of the wrist and hand. An old and experienced physician was called, who found no difficulty in controlling the bleeding externally, but a large quantity of blood was extravasated, in the inter-muscular spaces, and when this began to dissolve, bleeding occurred again. and continued until fresh clots were formed. This recurred several times; when he was admitted into the hospital, exhausted from loss of blood, and the arm tense from the extravasation of blood beneath the facia. The brachial artery was tied, and a free incision made in the arm to discharge the blood and relieve the tension; but the patient being exhausted, gangrene took place. Amputation was resorted to, and the patient narrowly escaped death with the loss of an arm. I do not mention this case because it is extraordinary or rare, but because it is common, and because the best physicians, and surgeons too, (*closet surgeons*) are too apt to temporize and hope for a favorable result, when there is no hope without prompt action. In this case it was proper to disgorge the parts as much as possible of the extravasated blood, and then make decided pressure over the mouth of the divided artery, such as to secure it from throwing out any blood, and continue it until a coagulum had formed in the artery; after which moderate compression should have been continued, sufficient to support the coagulum, and prevent the adhesive process from being disturbed by undue motion of the muscles. But if this course had failed, and hæmorrhage had occurred the next day, or the second day, the artery should have been tied, and no reliance placed upon a second clot. The principle may be applied, so far as dressing is concerned, in penetrating wounds in fleshy or muscular parts. It is commonly the case that a wound of this character, which is a mere trifle in its superficial aspect, is drawn together in the most scientific manner, so as to arrest the bleeding, while the small deep vessels are throwing out blood, which being deprived of an external escape, makes its way between the muscles and fascia, and perhaps eventually causes extensive suppuration. Such cases should rather be left to nature than treated in this manner. Compression should be made in the course of the wound to prevent, if possible the extravasation of blood in the tissues, and to secure a perfect co-aptation of the deep parts; this being effected, there can be no objection to closing the orifice, although it will be found that if the deep parts unite favorably, no great skill will be required to close the small orifice in the integuments.

In penetrating wounds of the chest, there is too great a fondness for hiding the mischief by applying adhesive plaster to the external wound. The immediate danger of penetrating wounds of the chest is hæmorrhage, and the remote is inflammation. If air is admitted into the chest, and the lung thereby allowed to collapse, hæmorrhage will be arrested (unless the large vessels are wounded) with as much certainty as the placental vessels from the contraction of the uterus, and no one will pretend that air in the cavity of the chest is a very powerful cause of inflammation, while all must admit that blood to any considerable amount is almost certainly fatal if left to nature. In penetrating wounds of the chest when the lung is wounded, even when the hæmorrhage is not immediately dangerous, it is proper to admit air and allow the lung to collapse.

Case 6.—A Spaniard was admitted into the hospital, having received a wound in the left chest, between the fifth and sixth ribs, which penetrated deep into the lung. The external wound had been very nicely drawn together, but bleeding continued, both into the cavity of the chest, and into the bronchia. Various remedies were used, such as are usually resorted to in internal hæmorrhage, but with no effect. The dressings were renewed —a free ingress of air allowed. (the wound being large,) when the lung collapsed and bleeding ceased. A large quantity of blood had escaped into the cavity of the chest, and the external wound being open, it was discharged by means of a large cupping glass which answered the purpose admirably. Pleuritic inflammation followed, but did not run high, and was evidently caused more by the coagulated blood, than by the air. The patient gradually recovered, and the lung resumed its functions perfectly.

It is evident that had the external wound been kept closed, this patient would have died, probably from hæmorrhage, or if he had escaped immediate death, he would have died from inflammation, which the blood in the chest must have necessarily produced.

These few cases I have selected from hundreds I witnessed, not because there is any thing remarkable in the cases, but merely to illustrate certain principles.

Art. VI.—*Remarks on Inflammation and Fever.* By Charles McCormick, Assistant Surgeon, U. S. Army.

There are no two subjects of greater importance in medicine than fevers and inflammations. In nine-tenths of all the maladies that afflict the human family, these are the chief forms of diseased action a physician is called on to combat, and whoever is most successful in managing them, will prove most successful in general practice.

A history of fevers and inflammations is almost a history of medicine.

The greatest evil has been caused by referring almost every disease, even fevers, to inflammation. This is erroneous in the extreme, as are also, the opinions which those persons who do this, hold and promulgate of inflammation itself. In reality, we would be nearer the truth in saying all inflammations are fevers, variously modified, than that all fevers are inflammations; and in practice it proves extremely judicious and beneficial so to consider them. When they said fever was inflammation, it was wrong. When they said they were the same, (identical) they were nearer right, but not in the way they meant, because they meant fever could not exist without inflammation, which is by no means the manner of their similitude, for it consists in the very opposite fact, viz: that inflammation cannot exist without fever, or those primary morbid impressions on the nervous apparatus, which are in fact the essential cause of fever, and without which, neither it nor inflammation can occur.

It is of the greatest importance that we should search out and understand, accurately, the true seat of fever, and, if possible, its essential nature. In order to accomplish this with accuracy and exactness, it is necessary, as in the examination of any other subject, to observe closely all its phenomena, all the circumstances that favor its origin, or from which it appears to spring, its early and remote effects, as well its commencement, as the changes and ravages it produces when suffered to run its course until it terminates finally in death.

By pursuing a proper system of examination, we can, I think, as certainly arrive at a knowledge of the laws that govern the action of the imponderable causes of fever, malaria, &c., on the animal system as accurately as has been done in discovering and laying down the laws that govern heat, light, electricity, &c. Now, it has been discovered under what circumstances these imponderable substances or agents are generated, and further, we are acquainted with most of the laws that govern them, or by which they are controlled, and influenced after having been generated. Here, as in a case of vitality, we may see and know its seat, manifestations and laws, but remain in profound ignorance of its ultimate and essential nature; so may we hope in fever to discover its seat, manifestations and laws, although we may fail to know what precise condition, change or injury, it produces in the great system to which we trace it. If we can trace it to its seat, and obviate its effects for all practical purposes, we understand it fully. Itis only then by applying the same methods of observation, and a similar mode of reasoning to the case of fever, that we could, in an attempt to investigate any other natural phenomena or causes, hope to arrive at true and exact results. In order to assist us in referring and tracing to their causes the various phenomena fever presents, we must, in the investigation, avail ourselves of the information we can derive from the action and effects of various natural objects (medicinal remedies) as is done constantly in chemistry by means of chemical tests; and, (what is to this case of more immediate analogy,) that which is done in medicine where we are influenced in some cases of difficult diagnosis by the effects of remedies.

Post mortem examinations have heretofore, and it is more than probable they will hereafter, fail to discover the proximate cause or seat of fever. Has dissection ever revealed to us the precise condition of the nervous apparatus, on which chorea, epilepsy, and tetanus depend; or can we after death, by the aid of the knife distinguish between the brains whose living manifestations in the one case were those of genius, in the other of idiocy. And such is the case with most of the diseases that are, by all alike, referred to the nervous system. The fact of finding after death, in many cases of fever, no organic lesion sufficient to account for a fatal termination, is also strong corroborative evidence that its seat is in that apparatus whose injuries it is so difficult to trace after death, *and on a due exercise of whose functions life itself depends.* I think it can be shewn that all the different local inflammations found, and which have been alleged by different authors, to be the proximate causes of fever, are secondary effects themselves, only resulting from the action of the cause of fever (malaria, &c.) on the nervous apparatus, reflected on the vascular system. It is about as philosophical to examine after death, the bodies of those who have died of fever, in search of its proximate cause, as it would be to examine a tree, house, or animal, destroyed by lightning, in search of the intimate, essential nature of electricity. In all such cases, all that our examinations can teach us, are, the effects produced by the action of the cause, but the ultimate effects reveal nothing of the intimate or essential nature of this cause.

The great benefits that have resulted from post mortem examinations have been to make known to us the effects produced when fever has been suffered to run its course; or when it has run its course, modified only by a treatment which cannot cut it short, and but seldom interrupts it. It makes known to us what local inflammations we may expect, and thus prepares us more readily to detect them when they supervene, and consequently to treat them with more hope of success. There is, however, one method of treatment, which, in a majority of cases of fever, cuts short and arrests at once the febrile movement, and speedily restores the healthy functions of the different organs. A correct knowledge of the therapeutical effects of the remedies possessing this power, will also be of great avail in our inquiries into the intimate and essential nature of fever.

Fever, from whatever cause it originates, or whatever type it assumes, is that general morbid condition of the system, resulting from a primary morbid impression made upon the nervous apparatus, which causes derangement of function, or other injury therein, according to the force of the cause producing it, or to the diminished vital resistance offered to its action by its subject.

Immediately following this primary impression, we observe a state in which diminished action is apparently present—a mere pause, a calm usually before the coming storm, which is generally followed by a state that speedily puts to flight all ideas of debility; but at times this state of oppression continues, and the system recovers but slowly and imperfectly, from the shock the nervous apparatus has sustained from the malaria, &c.; or this shock has proved, in other cases, too powerful, and the functions of life are completely overwhelmed, either from the force of the cause, or from diminished vital resistance. At the outset the usual play of the nervous function that maintains healthy action is interrupted; the healthy play of the function being thus interfered with, the phenomena over which its action presides must consequently be deranged, and hence arise the early symptoms of fever. The deranged phenomena must, and will vary according to the nature of the cause, and the condition of the vital resistance of the subject, and their origin cannot with accuracy be referred to any other system or apparatus. This derangement of the nervous function continues as long as the fever, and it is, in reality, the *fons et origo* of the disease, without which it cannot exist, and consequently where it does exist, there is fever; and again, as soon as it is remedied or removed, the fever ceases. Herein consists the real pathological difference between fever and inflammation, viz., that, in the former, the chief morbid action not only commences in the nervous apparatus, but continues paramount therein, the vascular system during its presence slowly becoming implicated and less deeply; whereas, in inflammation the nervous apparatus, although primarily implicated, is far less so, and the morbid actions pass more rapidly into, and involve the vascular system more deeply, and more permanently; in the former case, that condition of the vascular system, called congestion occurs most frequently; in the latter, that known as inflammation. But the two may be present in the same case. Dysentery is an example of fever with inflammation—the winter fever, or pneumonia, or pleurisy, with bilious fever, is an example of inflammation with fever. The latter occurs very commonly in this section of country (Fort Gibson, Arkansas); almost any inflammation that occurs here, will, sooner or later in its course, become complicated with fever; an incised, complicated, or lacerated wound, colic, cholera morbus, &c. &c. In all such cases, the treatment will be rapidly successful, if the appropriate treatment for fever, (sedatives or nervous alteratives) be combined with proper antiphlogistics; whereas, without such combinations, antiphlogistics will prove tardy, uncertain, and imperfect.

In this view of the case then, the nervous apparatus being the locale on which the cause of fever first impinges, and fastens itself, it cannot be regarded as the result of local inflammation, or as a local disease, for this tissue pervades all others. It is, therefore, a general disease, one pervading every tissue. As Dr. Fordyce describes it; "Fever is a disease that affects the whole system; it affects the head, the trunk of the body, the extremities; it affects the circulation, the absorption, and the nervous system; it affects the body, and likewise the mind. It is, therefore, a disease of the whole system, in every kind of sense. It does not, however, affect the various parts of the system uniformly and equally; but on the contrary, sometimes one part is affected more in proportion to the affection of another part." At the same time that we deny fever to be the result of *local* inflammation, we deny it to be the result of *inflammation at all*, or in any way connected with inflammation than that the different organs are prone to congestions, and inflammations during the progress of fever. We have described fever as consisting in functional, or other injury of the brain and nerves, directly induced by malaria and other causes. If it were induced by any inflammatory action of the nervous apparatus, we should find traces of it after death, and no one but Dr. Clutterbuck and his followers will

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maintain that any such state occurs, and they only in reference to a part of this system, the brain. If, as Dr. Cullen supposed, malaria acted as a sedative, how shall we explain the violent action of the heart and arteries, so commonly met with; and above all, how shall we explain the powerful and almost *antidotal* effect of this class of remedies, (sedatives) in relieving the very condition he views as resulting from the action of an agent, in his opinion possessing this very action. If, then, it neither universally exalts nor diminishes the action of the nervous apparatus, but at one time we find it exalting, at another diminishing the powers of life, all that we may be justified in saying of it is, that it deranges the vital functions, and produces results ascribable to what we would term a morbid irritant. Raving delirium, and the moribund commencement of algid or congestive fever, are opposite effects, produced by the same cause, malaria. The one *may* be regarded as stimulating, the other as sedative; they are both evidently deranged actions resulting from a morbid impression or irritation, acting immediately on the cerebro-spinal, or ganglionic, or both systems at the same time, through the skin, lungs, &c.; sometimes it acts more powerfully on one division of the nervous apparatus, sometimes on another; sometimes the impression being more intense, at others less so; in the former tending more to congestions, (as in congestive fever,) and in the latter to inflammations, (as in synocha.) It is the nature of the cause, whether contagion, infection, atmospheric viscissitudes, &c., (its action at one time being exerted on one part of the nervous apparatus, and at another time on some other), the different degrees of its concentration, and of the vital resistance offered to it, that cause all the different varieties of fevers described by authors.

The condition of the blood that has been assigned by Dr. Stephens as the cause of fever, is surely secondary to the nervous derangement; for as this system presides over sanguification, it is only reasonable to suppose that it too (the blood) must suffer change where the chief agent that governs its elimination is deranged; and, moreover, if this were the cause of fever, it should always be present at the commencement; whereas this condition is only *obvious* in the latter stages, or in certain severe forms of fever, unless to a limited extent, and which can be satisfactorily explained by recalling the fact that the impression on the brain and nerves occur first, and, according to varying circumstances that determine its action, either passes off, or becomes greater and greater, until a free development of its injurious effects occur; and, from the close relation between the nervous apparatus and the blood, its alteration or injury should naturally be extended to a greater or less extent, and we find that such is the case according to the force of the cause or diminished vital resistance. When the moving power of the blood is diminished, the circulation is correspondingly slow, and not only congestions occur, but the change which in health takes place during the passage of the blood through the lungs, is greatly diminished by the slow and laborious circulation, and this itself is a direct cause of change in the blood from its healthy condition; it must always take place when the mass of blood is disproportioned to the moving power (innervation) and where the cause of fever acts violently, it produces this disparity.

Fever can be cut short, and so can inflammation, at its commencement, but this latter only at its commencement, before the action has passed from the nervons to the vascular tissue, for after this the series of actions must be accomplished which have been set up in the vascular system in response to the morbid impression on the nervous apparatus. The first stage of inflammation is irritation; if this be properly treated by sedatives, opium, blood-letting, &c, the further phenomena of inflammation will not supervene; if, however, it be suffered to proceed, the action which in this state (irritation) is simply passing, or about to pass, from the nervous to the vascular system, at length becomes transferred thereto, in a great measure; and when this occurs, the disease cannot be cut short for the reason above given. In its turn the inflammation reacts on the nervous system, and proves an irritant by the morbid impression it communicates thereto; as, in purely local inflammation the nerves of the part first become implicated; inflammation of the organ follows; this local condition acts on the general nervous system, and the phenomena of fever become developed. In this condition we invariably find that a proper combination of sedatives or nervous alteratives, with our antiphlogistic remedies, will prove highly beneficial; probably because, by a proper use of opium, (the great sedative,) we can obtund the sensibility of the brain and nerves, so to obviate the primary as well as the secondary effects on the nervous apparatus, without carrying it so far as to interfere with the due performance of their ordinary functions.

In order to determine the seat of fever, we must, by the aid of physiology, endeavour to analyze the symptoms, refer each derangement of function to its appropriate organ, examine by the same means, the nature of the cause inducing the disease, and, finally, endeavonr to discover, by the therapeutical effects of remedies, the seat, and if possible, the nature of the malady. If we fail in this last, we may still succeed in discovering the seat of the disease, and the remedy therefor. It may be possible that the only knowledge we may or can acquire of the nature of the disease will be derived from a knowledge of the manner in which the cause producing it acts, the condition it produces, and the action of the remedy, by which it is cured, upon this cause, and upon the condition it produces in the organ it primarily affects; this will be sufficient, as our knowledge of fever will only then be on a par with what we know of many other subjects that are generally considered to be well understood. We shall then know its seat, the phenomena that accompany it, its effects, and, above all, its remedy. We know that water freezes—we know how ice is formed—the phenomena that accompany its formation—and its properties when formed; in short, we know how it is produced, and how to destroy it; but who knows the intimate nature of the cause that produces the effect. We know the laws that govern the generation and distribution of heat, but nothing of its essential nature. If we know how fever is produced, and how to destroy it, we know all that is useful, and with this we may be satisfied.

Whatever share we may attribute to the nervous function (innervation) in the various phenomena of animal or organic life, none will deny the great controlling influence it possesses over every function of the human body. When we consider, and it is a fundamental starting point, that all the manifestations of our feelings, and intellect—all sensation, and motion—are the results of the action of the nervous system; that it presides over digestion, absorption, respiration, circulation, nutrition, calorification, and secretion; that no impression can be made, no act performed without its controlling agency: we shall be prepared to appreciate fully the important part it plays in the cause and phenomena of fever, *as well as in every other act or motion in the animal body.*

It is by means of this apparatus that an animal experiences feeling or has the perception of an impression. Sensibility, in its general acceptation, means the property possessed by living parts of receiving impressions, whether the being exercising the property has consciousness of it or not. To the first of the cases Bichat gave the epithet *animal*, to the second, *organic*. Every impression, therefore, made upon the body, whether we are conscious of it or not, must be felt or perceived by the apparatus endowed with this power, viz., the cerebro-spinal, and ganglionic systems.

On examining the structure of the nervous system we find it consisting of a central and peripheral mass, connected with each other by means of nervous chords. We find it pervading, to a greater or less extent, every tissue of the body, and distributed in an especially free manner to all the arteries of the various viscera; and wherever, and in what manner soever spread out, is each part endowed with a peculiar faculty or function, as that of vision in the eye, olfaction in the nose, audition in the ear, touch, taste, and according to the Germans, of a visceral sense in the tegumentary membranes. It is through the medium of the nervous system that man is placed in relation with the world around him; it is, therefore, by and through this system, that all impressions are made on him, whether beneficial or injurious; and all the actions following these impressions, arise and are reflected from this system—each impression producing a resulting action corresponding to it—modified by the force of the impression and the state of the vital resistance or endowments at the time the impression is made.

The infinity of causes acting immediately on the nervous apparatus gives rise to a correspondingly infinite variety of phenomena. Among the most obvious causes that all admit as acting or creating impressions immediately and solely by the nervous system are also such as are directed to, and infringe upon our senses. Everything that thus infringes on the brain and nerves give rise to results corresponding to the peculiar impression it produces; as in the case of the optic nerve, the action of each object upon it produces, as often as it infringes on it, precisely the same idea of colour, form, or size; a green body always inducing the idea of green colour; a square body of square form; and again, each peculiar tint of colour. and each peculiarity of size and form produces correspondingly peculiar impressions. Such is the case with all our organs; they are brought into action by an almost numberless variety of impressions, and give rise to phenomena answering to each. But this apparatus is not merely endowed with the faculty of receiving and radiating through the system all the various impressions made upon it—it is not only the sentinel that guards the citadel from without, but the active agent and operative within.

Innervation, the function of this system, holds under its influence, in a more or less direct manner, all the phenomena of life. We find it has a more direct influence over the rest of the organization, as this system is more developed in the animal.

The nervous influence is not limited to the solids; the fluids, and especially the blood, experience its influence.

Nutrition is under the influence of the nerves, for when the nerve of a part is injured, the nutrition of that part is interfered with. Digestion is materially interfered with on division of the par vagum, and from this the stomach only receives a part of its nervous influence. When the supply of innervation is cut off from the lungs by dividing the nerves of this organ, asphyxia and death speedily come on.

The influence of the nerves on secretion is seen in many nervous diseases; in hysteria for instance, there is copious secretion of pale, limpid, and watery urine. The same secretion is known also to occur under the passion of fear. Section of the nerves of an organ suspends in a great measure its secretion. When the innervation of an organ is interfered with, its functions are seriously impaired. It is extremely difficult to cut off from any organ its full supply of nervous influence, and until the whole, or a greater part, of it is cut off, the function of the organ will be carried on to some degree.

All the sympathies of the body are kept up and maintained by this apparatus, and the influence of one organ on another is transmitted through the nerves.

We know that the vital endowments greatly control and modify the chemical actions that occur in the living body, and it may yet be found that the nerves of a part possess a peculiar endowment which controls the action of the organ, and gives a peculiar impress or bias to the chemical affinities, which cause it (the organ) to abstract certain principles from the blood.

The causes that act in producing fever are heat, moisture, cold, fatigue, violent emotions, excess in eating and drinking, marsh, and human miasm, contagion, bruises, and other injuries. All these different causes, by their action on the human body, are capable of producing fever, and each cause its peculiar variety, form, or type. As in the case of different causes acting on the organ of sense, so in these, certain causes produce certain phenomena. The contagion of small pox give rise always to a peculiar set of phenomena which steadily follow its application, and are only modified by the state of integrity or vital resistance of the brain and nerves, or the greater or less concentration of the poison, or the longer or shorter period of exposure to it. The poison of scarlatina produces constantly a train of phenomena peculiar to it, and only subject to the same variations, under the same influences of greater or less concentration, greater or less vital resistance, and longer or shorter exposure; and so with remittent, typhus and yellow fever.

Some of these causes act by direct impression on the nervous apparatus, as cold, heat and moisture; others through the circulation. In this latter case, however, the blood only acts as the carrier of the poison in a way similar to its action as a carrier of oxygen, and then, even in this case, makes a direct impression on the nervous apparatus. If the poison possessed the power of changing the nature of the blood (beyond its simple addition thereto) this state would only act as an additional irritant, and would tend greatly to aggravate and increase the morbid phenomena resulting therefrom.

The living animal body consists of tissues possessing various vital endowments and physical properties. The former, comprehending the manifestations of the mind, sensation, and motion, are given to it and evinced, as far as we can judge, by means of the *nervous system;* the latter, weight; elasticity, contractility, &c., are purely physical, and the result of the peculiar form and structure of each individual part. It is among the vital endowments and the structures on which they depend, or whence they originate, that we must seek for the seat of all impressions, whether morbid or healthy. The brain, a part of this system which is found to be most highly developed in man, cannot be otherwise regarded than as the organ of the mind. This is proved in the most conclusive and satisfactory manner by pathology and phrenology. All our thoughts and ideas are consequently the result of the function of this most elevated portion of this system; any injury, mediate or immediate, done to this portion, according to its violence, either impairs or altogether effaces the manifestations of the mind. The perception of food received into the stomach results from an impression on the nerves of this organ; an increased afflux of blood, and the secretion of gastric juice results therefrom; and, accordingly, we find that whatever depresses or debilitates the nervous power, materially interferes with, or impedes this process. The blood, the natural stimulus of the heart, creates an impression on the nerves, on which a healthy responding action follows. This is a normal impression as long as the blood continues healthy or unchanged; the moment it changes, becoming morbid by taking up or retaining any foreign substance, a morbid impression is made, and irregular actions supervene—the rays of light infringing upon the eye, the waves of air on the ear, the contact of solid fluid or æriform bodies with the tegumentary membrane; in short the impressions made on all our senses, are by all referred to the nervous apparatus. Take this in connection with the facts; that all those organs most abundantly supplied with nerves, such as the skin, mucous membranes, heart, &c., are endowed with this faculty of sensibility in the exact ratio of nerves distributed to them; being found more highly exalted in those most abounding with nerves, and gradually diminishing until it is found nearly extinct in those wherein it is difficult to trace them, and in which diseased action alone makes manifest their presence. Remember that an injury done to the nerves of a part, impairs its nutrition; that when the nerves of an organ are divided, its functions are seriously impaired, as on division of the nerves of the lungs, asphyxia supervenes; and the conclusion seems inevitable that all sensation and all motion are the peculiar properties of the nervous tissue, and that all the other tissues possess these properties of sensation and motion, solely on account of and by the nerves entering into their composition.

A view of the therapeutical effects of the chief febrifuges will serve to give concluding confirmation to this view of the nature, seat, and remedy of fever. The chief remedies for fever, are the class of sedatives or nervous alteratives, and of the chief are opium, Peruvian bark, and their alkaloids. Every person must be familiar with the fact that a full dose of either remedy, given before, or during a chill, will altogether prevent, or diminish the force of the fever, and cut short the cold stage. Now the action of these remedies, is direct upon the brain and nerves; and on other parts, only through their influence on the nervous apparatus; this class of remedies differing from others, not in acting on the nerves, but in soothing irritation and allaying pain. The articles seem either to neutralize (as an antidote does) the effects of the poison, to change in some way the action, or assuage irritation leading to morbid action. Now as the poison is clearly not present in many cases, such for instance, as in cases of traumatic fever; and as the remedies possess the same power over them as over miasmatic cases, it is fair to infer, that their action is sometimes at least, that of allaying an irritation. When this irritation is allayed, the fever depending on it ceases. The active exercise of an organ in a normal state, creates waste and consequent increase of nutrition therein. The morbid action caused by irritation, produces waste and imperfect nutrition; the waste begins with the nutrition and is relative thereto; and it may be, that the waste of substance is the only condition present. Now if any cause of irritation be present, the remedy would seem to be, to remove the cause, allay the irritation, and supply the waste. That these remedies are capable of allaying irritation, none will deny, and according to Professor Liebig's analysis, it is extremely probable, to use his own words, that those articles, the alkaloids of opium and Peruvian bark, are food for the brain. Beclard has said, "that life consists of the mutual action of the blood on the nerves, and the nerves on the blood." In a state of health they are in exact relation to each other; the bulk of the body in motion, corresponds to the force (innervation) maintaining the motion; in disease this relation is lost very frequently. The power of innervation being diminished by morbidly deranged action in the brain and nerves, the blood itself becomes an additional source of irritation, both from having lost its relative proportion to the moving force, and from the imperfect changes that occur in it; the secretion being greatly diminished, or altogether suspended. Bleeding restores this relative propôrtion, and the use of alkaloids quiets irritation, and changes the irregular action of the moving force, and probably repairs the injury by becoming speedily assimilated to this structure on presenting the elements ready formed. However this reasoning may be, the fact stands, that bleeding allays increased and irregular action when it is required; opium and quinine act on the nerves, quieting irritation and subduing fever whenever present, and from what cause soever originating.

The power of quinine to control, arrest, and even to prevent fevers, may justly be compared for efficacy with the power of pure vaccine matter to prevent the small pox; and in truth I doubt greatly if the general introduction into practice of the administration of quinine, in large doses, freely in all cases of fever, will not prove a greater blessing to mankind than the discovery made by Dr. Jenner has done, or ever will do;—small pox being of less frequent occurrence than the other forms of fever.

We will now take a view of the symptoms which are earliest observed after the action of the cause, or which are obvious at the outset of fever, and necessarily indicate the apparatus, whence by the morbid impression of malaria, &c., they are made manifest. These are, general *malaise*, yawning, languor, lassitude, a feeling of debility, and a depression of the vital force, a sense of cold, with or without trembling, usually referred to the spine, especially the small of the back, frequently accompanied by pain in the head, spine and extremities, at times almost insupportably severe, more generally in the small of the back; there are rigors, paleness, lividity of the extremities, diminished temperature, difficult respiration, anxiety, nausea, and vomiting. Sometimes all sensation is obliterated—there is blindness, deafness, speechlessness, and stupor; a stunned and torpid state of the mental faculties, and in the severer cases the power of deglutition is gone, and even boiling water fails to excite sensation. Again there is excessive sensibility, restlessness and sleeplessness, the functions of the optic, olfactory, and auditory nerves being painfully excited by ordinary normal impressions; and so, also, with the manifestions of the different organs of the brain. There is difficulty of articulating, with stupid countenance, and staggering as if the patient were drunk; general or local tremors, with or without delirium, the former at times resembling delirium tremens, and the latter the tremors of palsy; rigid spasms of the muscles, as in tetanus, epilepsy. &c.; jactitation, vertigo. and fainting.

These, then, are symptoms observed at the commencement of fever; they are always present at its outset in greater or less number, and *are signs that first make manifest the aetion of the cause of fever on the human subjeet.* Even the mildest cases are ushered in by general *malaise*, languor, lassitude, diminished temperature, and sense of chilliness. They all point to or indicate the first link in the chain of morbid action as commencing in the nervous apparatus.

After these initiatory symptoms are observed the following: in some new cases, the circulation seems to suffer but slightly; most generally, however, one of the two following conditions obtain; there is acceleration of the pulse; it is well developed, and made up of the following conditions—frequent, quick, strong, full, hard, rebounding, and wiry; there is throbbing of the large arteries, especially the carotid and temporal, with flushed face, eyes bright, watery and injected; heat of skin, and sometimes turgescence of the whole body; respiration strong, frequent and quick, with a sense of heat. Again, there is irregularity of the pulse; it is either slow or accelerated; in either case, however, shrunk, feeble and fluttering, easily compressed, and at times almost imperceptible, the skin is cold, livid and shrunk, especially the fingers, toes, and extremities, where the patient complains of intense pain, unless this condition is accompanied with insensibility; the circulation driven back on the internal organs; the eyes are dull, stupid and watery, the respiration slow, difficult, and laborious.

As occurs in the circulation, the animal temperature in some rare cases is but slightly changed. There is, however, most generally great variation in it; at times it is highly exalted; again is greatly deficient, and constantly, in nearly all cases varying from its natural standard. Such, too, is the case with the secretions; they constantly give evidence of great departure from ordinary healthy condition; sometimes excessive, at others greatly deficient; sometimes excessive in one or more organs, and at the same time deficient in others; in all, changed in their properties and appearances as seen in those of the skin, liver, kidnies and alimentary canal.

In short, after the appearance of symptoms which are clearly referable to the nervous system, there appear those which are as clearly referable to derangement in the functions of circulation, calorification, secretion, &c. and now it must be recollected that those symptoms, which have been named as referable to the nervous apparatus, *are not only present at the outset of fever, but continue, during its progress and course*, to a greater or less extent, variously modified and combined with those symptoms which are referable to the functions of circulation, calorification, and secretion, hereby showing that the primary impression made on the nervous apparatus was not transient and temporary, but permanent and abiding, manifesting itself by its appropriate signs, during the entire course of the fever; or, in other words, this impression acts on the brain and nerves, and its action continues as long as the disease remains. The abnormal impressions and changes, the morbid irritants produce primarily upon the nervous system, are irradiated and reflected throughout the whole system, in accordance with the laws governing the reflex actions of the cerebro-spinal and ganglionic systems. As the blood and nervous substance are in more immediate relation to each other, and act reciprocally, the one upon the other, it would seem to follow that the derangement in the nervous should first propagate itself to the vascular system; inasmuch as derangement in the one must cause, according to its extent, greater or less derangement in the other. The two great systems being involved, all the functions of the body that depend for their due performance on the healthy action of these, must also become implicated. Whatever organ, from its previous predisposed state, suffers most from the reflex action, will prove most prone to hyperæmiæ or congestions. These local hyperæmiæ or congestions, occurring when the circulation is excited, render the organs in which they happen liable to inflammatory action, or inflammation itself. When, however, the action of the morbific cause, instead of exciting fever, causes the immediate death of the individual, these local hyperæmiæ or congestions cannot be overcome, and the patient dies without any trace of inflammation; simple engorgement or congestion being alone discovered, produced by a retreat of the circulation from the surface, and its concentration on the heart and internal organs. The heart is the chief agent in capillary circulation; its action, as well as that of respiration, is dependant on nervous influence; and as in this state, on account of the depressed condition of the nervous substance and function, it is of itself incapable of exercising the necessary vigor or force to keep the blood in motion, and maintain the balance of the circulation. Hence result the phenomena observed in algid or congestive fever.

Even Broussais, the great advocate of inflammation as the cause of fever, admits, speaking of the cause of fever, that "at its highest degree of intensity it acts violently on the nervous system, paralyses its energies, and kills in a few moments, without permitting any reaction." And surely all who have seen much of fever must admit the truth of the remark. It appears, therefore, that in some cases the cause acts with such force that the shock immediately proves fatal; and is it not fair to infer, that whatever be the result of the impinging on the human body, of the cause of fever, (malaria, &c.,) be that the destruction of the individual, or the excitement of fever, the cause acts upon the same apparatus, the only difference in its effects, as before stated, resulting either from the concentration of the cause, or diminished vital resistance on the part of the subject.

From all that has been said, it would appear:

1st. That the causes of fever first impinge upon, and make their first impressions on the nervous apparatus.

2d. That all the *primary characteristics* of fever are evinced by this system..

3d. That throughout the course of fever, this system continues more or less affected, not resuming its healthy functions until convalescence is established.

4th. That all the various local derangements of function, congestions, and inflammations of the different organs, supervene, or are subsequent. That the balance or relation between innervation and circulation is lost; that the blood and other fluids become altered or morbidly changed.

5th. That in some cases, at least, death is the immediate result of the injurious action on the brain and nerves, no organic lesion existing to account for it.

6th. The nature or precise character of the fever, or rather, variety of fever, depends on the nature of the cause.

7th. Those articles possessing most power and influence in subduing fever, are all such as act most obviously and powerfully on the brain and nerves.

Part Second.

REVIEWS AND NOTICES OF NEW WORKS.

I.—*Researches into the Physical History of Mankind.* By JAMES COWLES PRITCHARD, M. D., F. R. S., M. R. I. A., &c. &c. &c. Third Edition. London: Vol. 1, 1836: vol. 2, 1837: vol. 3, 1841: vol. 4, 1844. [Concluded from the September number.]

In the second volume our author passes to a survey of all the known tribes of Africa. He does not attempt to distribute them according to the race, but proceeds in the order of their geographical positions.

Before examining the people, it will be important to take a brief survey of the physical structure of the continent. "It may be considered as consisting of two great mountain regions or table lands of very unequal extent," which include between them a vast intervening space of sandy desert, which extends across the whole continent from Egypt to the Atlantic shore—"an ocean of sand, interspersed with green islands or oases, separating the region of Mount Atlas, from the great highland of Central Africa; of which the Mountains of the Moon form the northern border. The highlands of Atlas are less completely separated from Europe by the narrow Mediterranean, than from the central highlands of Africa, by the great Sahara."

Atlantica is that part of Africa, lying between the Great Desert and the Mediterranean, including the highlands of Atlas, and the states of Morocco, Algiers and others; and it declines towards the eastward into the sandy plains of the lesser Syrtes.

The highlands of Central Africa is still an unexplored region. It seems that this great plateau, or vast aggregate of mountains, extends in length about two thousand miles, in an oblique direction, from lat. 20° south, to 10° of north lat.; its northern border being at the Great Desert, and the southern extremity reaches to the cape of Good Hope. It has been compared to the great table land of South America and the steppes of Asia; and was supposed by Lacépède to include the most elevated lands on the globe. The lofty Jibbel Kumri, the Abyssinian, and Kong mountains are offsets from this great nucleus. On every side great rivers flow down its declivities through defiles; and it send its tributes to the oceans through the broad channels of the Nile, the Jolliba, the

Camoens, the Zaire, the Zambesi, and the river of Zanzibar, Loando, and cape Negro.

The low countries of Africa, which, in many places border the highland regions, are partly fertile valleys, but mostly extensive seas of sand. The principal fertile regions are the extensive districts watered by the Niger and other streams, and the Biledulgerid or Land of Dates, which is a verdant zone extending along the southern border of the greater Atlas.

"The Great Sahara is a vast region of sand, traversed by chains of rocky mountains; a sterile and desolate wilderness, interspersed, however, by innumerable oases or islands of verdure, which exist whereever water springs forth from the soil, and irrigates small surrounding tracts, shaded by groves of palm-trees, and affording places of refuge and safety to caravans and travellers perishing with thirst." Sometimes these oases become, like islands of the ocean, the abodes of fixed inhabitants, who acquire peculiarities of character and language, and display in their physical traits the effect of external agencies. Such are the oases of Fezzan, of Sewah, which contained the celebrated temple of Jupiter Ammon, and those of Gualata and Agades.

The inhabitants of Atlantica have been from time immemorial a race called Berbers, from which Berberia or Barbary. The early settlements of the Phœnicians in Africa, being mainly with a view to traffic, consisted of a small number of persons, who intermarrying with the native Africans, blended the races, so that we find all the cities and governments established by foreigners, were composed of a mixed-blooded population. Even in Carthage, the "Tyria Bilingues" spoke a native language in addition to their own, the Punic or Phœnician. There is reason to believe this other language to have been the Berber or Lybian language, and that this race constituted a considerable proportion of that empire. The great Numidian kingdom, whose name holds so conspicuous a place in Roman and Punic history, whose kings were the respected allies or dreaded enemies of the Scipios and Cæsars, among whose children we find the distinguished Juba, "the best of all royal historians," who was also a great navigator, and who discovered the Canary islands, was likewise of this race.

Among the modern people who belong to this race, are the Berbers of North Atlas, the Shuluhs or mountaineers of Morocco, the Kabyles of Algiers and Tunis, the Berbers of the interior, behind Tunis, the Tuaryk and Tibboos of the Sahara and its borders. They include people of various complexions. The Moors of Barbary are handsome, and have the same colour as the Spaniards of the opposite coast, and as the Sicilians and Italians. The inhabitants of the highlands of Mount Atlas are also fair. Among the Tuaryk of the southern part of Atlantica, and parts of the desert, and the Tibboos of the desert, we find the colour varying in relation to their position, from brown to the deepest black, and some of the tribes having the same language have, besides the colour, the hair of negroes. In the mode of life, the tribes differ greatly; some live in cities and are commercial; some lead a pastoral and nomadic life in the plains and deserts, and some dwell in caverns, in the snowy mountains of Atlas; living by plunder and rapine, and their exploits give origin to traditions and legends which terrify the people of the plains.

The land of Negroes lies in that part on North Africa, a few degrees north of the line, including the extensive tract lying south of the Great Desert, and extending south to the northern border of the great plateau of Central Africa ; and extending from the Abyssinian mountains on the east, to the Atlantic on the west, along which it extends in breadth from Senegal river to the bight of Biafra, and including the Kong mountains and the highland of Senegambia. This extensive region includes great nations of people, all of whom have the negro characters more or less strongly marked, but rarely to the extent in which they are found in some other tribes. Neither Phœnician nor Roman culture ever penetrated to this remote tract, but the enthusiasm of the propagators of Islam have converted the people to that faith, and given an impulse to civilization. They now practise agriculture, as well as the necessary, and some of the ornamental arts ; they live in great cities and pursue commerce. The Marabouts give instruction, and "the schoolmaster is now abroad," in tracts where previous to the introduction of Mahomedanism, the lonely and predacious savage wandered in quest of plunder.

In he western part of this region are found the Mandingoes and Bambarrans, Fulahs, the Ioloffs, the Inta race, including the Ashantees and others. As we go towards the interior, and from thence eastward, we find in the immense regions of Soudan, Bornoo, &c., various great nations, those of Tombutum, Bornoo, &c. Among the people of these nations, there is a great variety in every respect. In some, as the Fulahs, the complexion is red ; in the Inta race the hair is long and flowing, while the prevalent type is decidedly that of the negro. These nations present a mixture of civilization and barbarism : wherever Mahomedanism has been introduced, the people are, to a considerable degree civilized ; as among the many branches of the Joloffs, Mandingoes, &c.; whereas the pagan nations, or pagan branches of Mahomedan nations, are in the lowest state of barbarism.

The same characters are found among the nations of Kordofan, and the countries lying south of it. The people of Sennaar, lying northward of Abyssinia, have a yellow or red colour, and are barbarous. The Pagan negroes or Shangallas, of the western borders of Abyssinia, are extremely savage. Bruce says of them, that they still have the characters given them by Ptolemy, and "are still Rizophagi, Elephantophagi, Acridophagi, Struthiophagi, living on roots, elephants, locusts and other wild things. During the fair portion of the year, the Shangalla live under the shade of trees." Where the trees are thickest, and the water in the largest pools, there the populous nations dwell, who have often defeated the royal armies of Abyssinia."

The nations bordering on the south of Abyssinia are the Gallas, the Danakil, the Sùmàli, and the Hazorta. Most of these nations are savage pagans, pursuing a pastoral life and devoted to robbery. The Gallas are copper-coloured, with long hair. The Sùmàli are very black, with fine features, and their soft hair is allowed to flow in ringlets about the neck.

In the extensive region lying between Abyssinia and Egypt, we find Nubia and other countries inhabited by tribes, mostly of mixed blood. The Nubians vary in colour from jet black to mahogany colour, but without the slightest trace of resemblance to negroes. The features have the European cast, the nose beautifully curved, eyes deep set, the hair curled but not woolly. Most of the races are mixed with the Arab.

Nubia and Egypt taken together, constitute an extensive tract lying along the Nile from Sennaar and Kordofan to the Mediterranean, and limited to the west by deserts, and east by the Red Sea. This region has always been an interesting one. The great nations of antiquity generally inhabited the broad and fertile vallies lying along large rivers; and such regions were always the birth-places of civilization, the scenes of the foundation of cities, and of the invention of the arts which embellish human life. Accordingly in ancient times there arose along the course of the Nile, great nations, and the city of Thebes, Memphis, Meroë, Cairo and others, the homes of the Pharoahs, the Cleopatras, the great Rhamses, and Candaces. There arose also the wondrous pyramids, and the magnificent temples of Uxor, Karnac, Dendera, and Epsamboul, and the thousand others, excavated in mountains, or raising their awful piles towards heaven, inscribed with mysterious hieroglyphics and emblems, with zodiacs, or with huge figures, and sacred symbols, which shadow forth the powers of nature. Here were held the allegorical and mysterious rites of the gloomy worships of Isis and Osiris. Here long before Troy was peopled, the shape of the earth was known; and the laws which govern astronomical phenomena were known, while Greece was yet a wild, inhabited by savages.

Recent reasearches, particularly those of our distingushed countryman Dr. Morton, leave no doubt that the people who were thus highly enlightened were principally of the Iranian race, but there are some peculiarities observed, particularly in the position of the meatus auditorius externus, which instead of being placed on a level with the alæ nasi, are situated on a level with the eye.*

The complexion seems to have varied, from tawney-red to black, the hair black, flowing, curled or sometimes frizzled. The face characterized by a peculiar turgid habit, with flabby cheeks, and short chin.

In South Africa, as we proceed from the line, we find on the eastern coast, the extensive regions of Zanzibar and Mozambique, and, in the interior, the countries of Monomoegi and Monomotapa which extend to 20° South; and on the western coast, regions extending from the Bight of Biafra to the borders of Great Namaqua-land; a space of about 1800 miles, inhabited by numerous great nations, as those of Loango, Congo Angola, Benguela, &c. These great lines of coast, as well as the interior, as far as is known, are inhabited by powerful negro empires, the people of which closely resemble each other. Many of them tattoo their skins in regular figures, and file their incisive teeth to sharp points. They are mostly pagans, and very savage; but it is said that some in Loango profess Judaism. It is also said that in the elevated plateaus in

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* The learned researches of Dr. Morton show, that this deviation is more apparent than real. This appearance seems to be due to two circumstances; to the "remarkable vertical length of the upper jaw in some heads, in which it is manifest, that the ear would possess a remarkable elevation in respect to the maxillary bones, without being nearer the top of the head than usual;" and in cases when the bony meatus presents no deviation from the usual relative arrangement of parts," the auricular cartilages are prolonged upwards so as to give an appearance of elevation to the parts. vide Crania Ægyptica p. 26.—

the Interior behind Mozambique, there are highly civilized nations, and the relations have received the sanction of Arrowsmith and Bowdich. Behind the empire of Kong, in the elevated plains of the interior, dwell powerful hordes of fierce nomadic warriors, the Jagas, who hold a conspicuous place in the history of the Portuguese settlements there.

Africa is generally supposed to terminate toward the south, in a point; but instead of this, we have a broad front of coast presented to the southern ocean, and a parallel of latitude passing through De Lagoa bay will cut off a great region, entirely ultra-tropical, and inhabited mainly by two races.

The eastern part of this region, extending from De Lagoa bay, to the borders of the Cape Colony, and back into the interior, or even across the continent in some places, is inhabited by the various tribes or nations of Kaffirs. These people resemble in their features, the Europeans much more nearly than they do negroes. Their beards are full, the complexion is generally a clear yellowish brown, their language is "full toned, soft, and harmonious. These people when compared with the destitute savages inhabiting the insulated hamlets of central Negro-land, may be considered as enlightened. They possess some arts, such as the manufacture of copper implements and ornaments; and the rudiments of civil culture. They live in large communities, cultivating grain, &c., in fields enclosed by hedges of a prickly shrub. The Bechuanas are the most highly civilized of these tribes; and besides bread stuffs, raise sugar, and tobacco, and manufacture razors and knives of fine iron, build houses of masonry, and ornament them with pillars and mouldings.

The extremity of the African continent, south of the region inhabited by the Kaffirs, is peopled by the Hottentot races. Differing as they do, most widely, in physical peculiarities, and by a language so different from those of any other African races; and inhabiting the extreme point of a continent, they present a singular phenomenon, worthy of investigation. It has been supposed that Africa has been peopled from the North, and that successive tribes have pressed upon each other toward the south, and that the Hottentots are to be considered as the remnants of the race which first entered the continent. The Kaffirs even in recent times, have encroached considerably upon their territory, as may be shown by pointing to the Hottentot names of the rivers of Kaffir-land, and the Hottentots still regard them as intruders. The peculiarities of this race, have already been adverted to. They live in little hamlets or kraals, of moveable huts composed of a few sticks, and covered with mats, and lead a pastoral life. The Gonaquas, who were the most civilized and wealthy tribe of this race, is now extinct. The Bushmen tribes are beyond comparison, inferior to any other African race, and in the wretchedness of their physical and moral condition, they can only be compared with the miserable savages of the Australian islands.

In concluding this brief notice of Dr. Pritchard's Ethnographical survey of Africa, we will point the bearings of some of the facts presented, as illustrative of the relations of physical diversities, and their dependence on physical agencies. We will more especially consider the relations between the colour and other varieties of the races, and climates, latitude, and elevation of the regions of which they are the immemorial inhabitants; and as our reader, will probably be familiar with corresponding facts in regard to Europe, we may institute a comparison between the physical characters of the people living in different latitudes on the two continents, which taken together reach in a line nearly from one pole to the other.

Beginning in the extreme north of Europe, and proceeding south we shall meet with nations whose predominant characters are the following:

1st. The Norwegians and Swedes are generally tall men with light-colored hair, and gray eyes.*

2d. The people of northern Germany, Denmark, Finland and a part of Russia, as well also as many of North Britain, are xanthous. These people too, have had the same characters since first described. Silius Italicus called the Hollanders "auricomi Batavi," the golden haired Batavia; and Linnæus gives the Finns the character of yellow haired.*

3d. In the latitude of France, and north of the Pyreno-Alpine line, the prevalent colour of the hair is chesnut brown, eyes hazel, and skin moderately fair.

4th. In the level countries, forming the margins ef the Mediterranean basin and belonging to Europe and Africa, and in the intervening islands, the people have black hair, black eyes, and a darker brunette complexion.

5th. The next region to the south is that lying south of Atlas, the natives of which are the "gentes subfusca coloris" of Leo Africanus, having a light brown colour, which varies even to black.

6th. The intra-tropical regions are inhabited by nations, of which, if we except the highlands, the predominant colour is black.

7th. To the southward of the tropic of Capricorn, we again come to the copper-coloured people of Caffraria.

8th. Towards the Cape of Good Hope reside the tawney Hottentots, scarcely darker than the Mongols, whom they resemble in many other particulars besides colour.

In ascending high mountains we perceive the same kinds of modification, in the beings inhabiting regions at different elevations, as strike us in going from the Equator towards either pole; vegetation, at each step in the ascent, assumes a more northern aspect, indicating that the influence of physical agencies vary in the same way as if we were directing our course to higher latitudes. Man appears not to be altogether exempt from the same kind of variations, and as we proceed upwards in mountainous tracts we find the permanent inhabitants departing in physical characters from those inhabiting the level low-lands, at the base. The Swiss inhabiting the high mountains above Lombardy have light complexions and sandy or brown hair; but as we descend into the Milanese we find the peasants having black hair and eyes, and "with strongly marked Italian and almost oriental features. In the elevated parts of the Biscayan country, instead of the swarthy complexion and black hair of the Castilians, the natives have a fair complexion with blue eyes, and flaxen or auburn hair. And in Atlantica, while the Berbers of the plains are of a brown complexion with black hair, the Shuluk mountain-

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* Linnæus defines them as "Gothi corpore proceriore, capillis albidus, oculorum iridibus cinereo-cœrulescentibus." Linn. Fauna Suecica.

† Fennones corpore toroso, capillis flavis prolixis, oculorum iridibus fuscus." Fauna suecica,

cers are fair, and the inhabitants of the highlands of Mons Aurasius are completely xanthous, having red or yellow hair and blue eyes.

Even in the intertropical region, high elevations of surface, as they produce a cooler climate, seem to occasion the appearance of light complexions. In the high-lands of Senegambia, the light copper-colored Fulahs are found, surrounded on every side by the negro nations inhabiting low districts ; and on nearly the same parallel of latitude, but at the opposite side of Africa, are the highlands of Enarea and Kaffa, the inhabitants of which are said to be fairer than the natives of southern Europe. The Galla and Abyssinians are also much fairer than the inhabitants of the low countries in their immediate vicinity.

An equally decided relation exists between other physical characters of the races, and the local and physical circumstances, which have operated through long periods of time as modifying influences. We have already remarked that the races having the negro character in an exaggerated degree were generally inhabitants of low and often unhealthy regions, near the shore of the sea, or along great tropical rivers ; such are the wretched Papels and Bulloms. The high table-lands of Africa are, as far as they are known, the abodes of wandering tribes, like the Kaffirs, which recede considerably from the negro type. "The Mandingoes are, indeed, a negro race inhabiting a high region, but they have neither the depressed forehead nor the projecting features considered as characteristic of the negro race.

"We may further remark, and perhaps this observation is fully as important as that of any other connected fact or coincidence, that physical qualities of particular races of Africans are evidently related to their moral and social condition, and to the degree of barbarism or civilization under which they exist. The tribes in whose prevalent conformation the negro type is discernable in an exaggerated degree, are uniformly in the lowest stage of human society ; they are either ferocious savages, or stupid, sensual and indolent." "On the other hand, wherever we hear of a negro state, the inhabitants of which have attained any considerable degree of inprovement in their social condition, we constantly find that their physical characters deviate considerably from the strongly marked or exaggerated type of the negro."

It is very often asked why it is, that if the color of negroes depends upon physical influences, that other people settling in Africa do not likewise become black ?

In order to answer such a query, it is but proper to examine to what extent changes are known to be produced ; and then to see why these changes are not now observed to take place to any considerable extent in Europeans who settle there.

That remarkable deviations have taken place in races, since their settlement in particular parts of Africa, we have the best evidence in the following cases :

1st. In the variations in the characters of the different Lybian tribes, according to the region inhabited by them as already shown.

2d. "The Arab tribes, who emigrated into Africa eleven or twelve hundred years ago, have undergone a very considerable change in their physical character," and retaining as they everywhere do, their primitive type, and their ancient manner of life, their physical characters have undergone striking changes; and there are several black races in Africa, which are the genuine unmixed descendants of Arabian emigrant tribes. It must be remembered, too, that the parts of Africa which these tribes inhabit are not negro countries, but various tracts of Atlantica and the Sahara, and the borders of Egypt and Nubia."

3d. "There are no authenticated instances, either in Africa or elsewhere, of the transmutation of other varieties of mankind into negroes. The experiment has never been tried; for although Europeans and Asiatics have settled, and all their descendants have dwelt for generations on the soil of intertropical Africa, they have never adopted the manners and mode of life of the aborigines." It appears, too, that when negro nations leave their original barbarous mode of life, and change their habits and customs, their appearance undergoes considerable variations. The Barabra of the Nile, already mentioned, are an example; for though the unmixed descendants of the Kordofan negroes, they now display physical characters which deviate considerably from the negro type. The same remarks apply to the Funge, the conquerors of Sennaar, who, though they are descendants of the Shilukh negroes, have no longer the genuine characters of the negro race.

The third and fourth volumes are devoted to the ethnography of Europe and Asia. Beginning with the Allophyllian or aboriginal races, the mound-builders of Europe, who preceded the Celtic and other races which have in succession occupied parts of Europe, our author goes on to deduce from all the vast collection of historical facts, and from physical, moral and intellectual relations, the history of the origin of each race, and a rich treat does his work offer to those who have taste and time to read it. But the researches contained in these valuable volumes, have not that close and immediate relation to our science, that would justify me in entering into a detailed account of them. In general, the diversities of the races inhabiting these continents, are not marked by those abrupt and wide deviations which are so remarkable and instructive, in the instances already taken from the previous volumes.

There are, however, instances, particularly in Asia, of the same kind and degree of diversity in certain races.

Among the Hindoos, for example, some of the branches as are perfectly black, but agree in all other respects with the fair brahmins, having straight hair and fine features; and among these black Hindoos, we have examples of most noble intellects, of which the celebrated Ram-Mohun-Roy, may serve as one.

The black colour of a portion of the Hindoo race has led many to assign to them an African origin, and our author once inclined to this theory, but long since has ceased to regard it as probable, as there is no other reason than the colour to sustain it; whereas, the general character, language, and the testimony of ancient writers, may be regarded as sustaining the opinion that they are pure Hindoos, whose colour has been modified by physical influences. We learn, too, that the great diversity of complexion in the different races of the Hindoos is not a matter of accident, but "bears a relation to the climates of countries which they inhabit," and "that the development of individual varieties is promoted by the influence of particular external agencies, and takes place chiefly in particular climates. Thus, the light and sanguine complexion appears to be the character of many, but not of all, in the northern and hilly countries of Hindostan, and the higher castes who are protected from the influence of the climate are generally fairer, that is, the children at birth, or soon after, are generally fairer than among the people of low castes, who are exposed." In Malabar, and some other parts of India, there are black Hindoos, "and even some black brahmins of high caste."

Here, for the present, we will take leave of this valuable work; but it would not be doing justice to its great merits, to neglect this opportunity of urging its claims to the attention of our profession. This work is, unquestionably, one of the noblest monuments of learning and research, erected by the intellect of man, in the present epoch of literature, which it remains for future generations fully to appreciate; and it will be sought after, and valued as a classic treasure in the distant future, when the present time comes to be considered as remote antiquity.

W. M. C.

II.—*The Anatomy and Diseases of the Breast. With numerous Plates.* By Sir Astley Cooper, Bart., F. R. S., Sergent Surgeon to His Majesty; Consulting Surgeon of Guy's Hospital; Lecturer on Anatomy and Surgery, &c. &c. &c. To which are added various Surgical Papers, now first published in a collected form. Philadelphia: Lea & Blanchard: 1845.

The name of Cooper is indissolubly connected with the progress of modern Surgery. Whether at home or abroad, as a lecturer or a writer, Sir Astley is everywhere, and in either capacity, regarded as the highest authority in surgical science, to whom we can appeal. The history of his professional career, whilst it is well calculated to stimulate and sustain the efforts of the young surgeon, furnishes, at the same time, a happy illustration of what unceasing application and a steady determination can accomplish. His work on the anatomy and the diseases of the breast stands, and will stand, as a monument to his genius for accurate observation and great research. As one of the last productions of his fruitful mind, this work will command everywhere that consideration and weight which the medical public has long since attached to the name of Cooper. But any eulogy from us, at this time, when growing years will but increase the measure of his fame, cannot add to the solid glory which rests upon the production that he has left behind him. Well might Sir Astley have exclaimed when finishing this—his last work—*non omnis moriar*—"by my deeds I shall live." All the facts and observations contained in this work are the results of personal conviction and direct experiment.—They are not the idle theories and baseless speculations of a man ambitious to write a book, but, on the contrary, the fruit of great toil and a careful study of the structures described. The chief merit in Sir Astley's works lies in the *practical* turn which he imparts to every subject that he undertakes to investigate. He studies and treats of things; with him the name is never mistaken for the substance.

In his "Introduction," he says—"In preparing this work, I have restricted myself to describing from my own preparations only,—and if every author in our profession would adopt this plan, and merely write on what he is capable of demonstrating, preserving and exhibiting to others, the medical world would not be overwhelmed with those crude opinions, theories and conjectures which, according to the present system of quoting all that has been written, are sure to compose the greater part of the works that issue from the press."

After a few "general observations" upon the changes which take place in the breast of the human female during gestation, and preparatory to nursing the infant, Sir Astley goes on to describe the situation of the mammæ, on the anterior and lateral parts of the chest, in what he calls the mammary region; where, when the child is drawing nourishment from the breast, the mother's eye can rest constantly upon it. It requires no effort of the fancy to suppose, that the sight of the infant acts as a stimulus to the mammary gland, increasing the secretion and flow of milk to the breast, thus beautifully illustrating the influence of moral impressions upon the functions of an organ. More than half of this magnificent volume is devoted to a minute description of the anatomical structure of the breast; each part illustrated with well executed plates, which were taken from actual dissection. We must pass over this portion of the work, not that it is dull or uninstructive, but because, in a short paper, we should be unable to enter into details, without which we must necessarily mutilate one of the clearest and most elegant anatomical papers of modern times. Surely every medical man who desires to have accurate information on the anatomy of such an important organ, will consult the text. We now propose to notice Sir Astley's remarks upon the "*Diseases of the Breast*;" a subject full of interest not only to the mere surgeon, but likewise to the general practitioner.

The female breast is liable to take on diseased action in common with other structures; besides a few, peculiar to the part, and almost exclusively belonging to this organ. This might be inferred, *à priori*, when we recollect that many of the elementary tissues enter into its composition. To be enabled to understand and treat the diseases of the breast, we must bear in mind, not only its general anatomy, but the precise order in which each tissue, fascia, gland, duct, etc., is arranged; then we can venture to pronounce, after duly considering the history and progress of the case, with some degree of certainty, upon the true nature of the affection.

The diseases of the breast may be acute or chronic, simple or malignant, curable or incurable; how important, then, a nice diagnosis in all cases of the kind, because the treatment must be based upon the specific character of the morbid change. The diseases of the mammæ are liable to assume a chronic form, especially if medical or surgical aid is not sought for in the early stage of the attack. Among the uneducated classes of society, most of the structural degenerations of the breast are regarded as *cancerous;* and no sooner is the unfavourable diagnosis announced, than a swarm of nostrum-mongers and quacks volunteer their services, and promise to effect a radical cure.

The disease, which in the beginning is simple engorgement of the glandular structures, and consequently exempt from danger, assumes, under the repeated application of violent irritants and escharotics, a malignant character—thus confirming the original diagnosis—and rapidly advances to an incurable stage. Besides, a familiar acquaintance with those morbid changes of structure to which the breasts are peculiarly exposed,—"is frequently a source of great security and happiness to those afflicted with such diseases, as well as of great satisfaction to the surgeon. I have scarcely, says Sir Astley, witnessed a stronger expression of delight than that which has illumined the features of a female—perhaps the mother of a large family, dependant upon her for protection, education and support,—who, upon consulting a surgeon, for some tumour in her bosom, and expecting to hear from him a confirmation of the sentence she had pronounced upon herself, receives, on the contrary, an assurance that her apprehensions are unfounded.

Pale and trembling, she enters the surgeon's apartment, and baring her bosom faintly articulates, "sir, I am come to consult you for a cancer in my breast;" and when after a careful examination, the Surgeon states, he has the pleasure of assuring her that the disease is not cancerous—that it has not the character of malignancy—that it is not dangerous, and will not require an operation; the sudden transition from apprehension to joy brightens her countenance with the smile of gratitude; and the happiness of the moment can hardly be exceeded, when she returns with delighted affection to the family from which she had previously considered herself destined soon to be separated by death, with the alternative only of being saved by a dubious and painful operation."

We have made this quotation, because it displays that generous sympathy for the afflicted, which does honor to the heart of the great English surgeon. With such interest in the happiness and welfare of his patients, Sir Astley must, of necessity, be successful in his attempts to alleviate human suffering; for the desire to save the life of a fellow-student, when about to perish from a profuse hæmorrhage, first led our author when a boy, to turn his attention to the science upon which he subsequently reflected so much credit.

He divides the diseases of the breast into three classes: First, into those which form common inflammations, both acute and chronic; secondly, those which arise from peculiar or specific action, yet not malignant nor contaminating to other structures; thirdly, into such as are not only local, malignant and specific in their actions, but which are connected with an unhealthy or cachectic state of the constitution.

In the *first* class, are comprehended acute inflammation of the breast; as milk abscess; chronic inflammation characterised by indolent swellings, and after a time, ending in an abscess; and thirdly, a lacteal tumour, caused by a chronic inflammation in one of the lacteal tubes. Several species of tumours are included in the second class, as follows: The 1st, the hydatid; 2d, the chronic mammary tumour; 3d, the ossific; 4th, the adipose; 5th, the large and pendulous breast; 6th, the scrofulous; 7th, the irritable breast; 8th, the ecchymosis of the breast. The malignant diseases, such as schirrous and fungous tubercle, are included in the third class. The distinguished author then proceeds in the text to describe the symptoms, both external or local, and internal or constitutional, which attend the progress of each of the above diseases; adding, also, numerous plates by way of illustration. Of course, the morbid, the internal structure is most carefully described and represented, so as to enable every one to distinguish for himself between the various affections seated in the female breast.

With that true candour which belongs to a great and magnanimous soul, Sir Astley acknowledges that he has often been mistaken in his diagnosis. Some of the surgeons of the present day commit errors ; but they either lack the candour to confess, or the sagacity to detect them. Let such strive to emulate our author.

The propriety of opening mammary abscesses, caused by acute or chronic inflammation, has been doubted by some surgeons. On this point, Sir Astley says, if the abscess be quick in its progress, and seated in the anterior part of the breast, and the sufferings not very severe, it is best to leave it to nature ; but if, on the other hand, the abscess be deeply seated, its progress slow, the pain intolerable, with a high degree of irritative or symptomatic fever, causing loss of rest, with wasting of flesh, cut down upon and discharge the matter.

When an abscess is deeply seated, and discharges itself through several sinuses, the best treatment is to throw into the sinous openings diluted mineral acids ; such as the sulphuric, with rose-water. That hardness and induration which sometimes persist after the contents of an abscess have been discharged, may generally be dissipated by the steady application of the emplast. ammoniac. cum hydrarg., or the use of the iodine ointment. In such cases, when, as is generally the case, we have reason to suspect a scrofulous taint, the hydriod. potass. internally, will enhance the efficacy of the local remedies. The *lacteal* or *lactiferous* swelling is caused by an obstructed lactiferous tube, usually happening soon after the birth of the child, and which is followed by an immense accumulation of milk, arising from a complete closure of the orifice of the duct. This species of swelling may be recognized by a distinct fluctuation—absence of inflammation—a sense of painful distension; greatly aggravated by the rush of milk to the obstructed part, when the infant attempts to nurse. When punctured with a lancet, the milk flows, soon to reaccumulate, and present the same swelling and distinct fluctuation as before. If the accumulation is permitted to go on, ulceration may take place, at some point, in the tumour, generally near the nipple, and the milk be discharged. To cure this affection, the mother should either wean the child, when a small puncture will give a free exit to the milk, the secretion of this fluid ceasing, no farther accumulation takes place ; or, in case she refuse to give up the tender offices of the nurse, we must be content to make a larger puncture in the tumour, and allow the milk to escape whilst the infant is nursing. We come next in numerical order to the "*hydatid disease of the breast*," of which our author makes four species :—three simple, and one malignant, in character. The three former varieties of the hydatid disease, may be recognized by the gradual swelling of the breast ; hard but free from pain and tenderness, also no signs of fluctuation. It may thus continue gradually to enlarge for years until the mamma weighs as much as nine or ten pounds. Sir A. saw one weighing nine pounds. We have said that the breast is hard ; this expression should be qualified by stating that, after the lapse of a long time, one or more points of fluctuation may be detected in the breast.

The superficial veins swell and assume a varicose appearance ; and, although the breast may attain the size already specified, still, with occasional exceptions, it is exempt from pain. The tumour remains very pendent and moveable; sometimes a part, then the whole of the gland is involved.

The disease still persisting, the point at which fluctuation is detected, gradually takes on inflammatory action; finally, a quantity of glairy serum escapes through an opening effected by ulcerative absorption. The constitution and general health of the patient do not experience any disturbance until the commencement of ulceration. The "hydatid disease" may be distinguished from inflammatory engorgement of the breast, by the absence of pain, either with or without pressure, and the perfect good health of the patient.

The diagnosis may be established beyond question by pushing a lancet into one of the fluctuating tumours, when the evacuation of a "clear serum, instead of purulent fluid," announces to the surgeon the true nature of the disease.

In ordinary cases, we may puncture the bags, and this will suffice without local or general treatment, as the health remains unaffected; but when the bags are numerous—the breast immensely large and pendulous—drawing upon the surrounding parts, and when the patient is rendered miserable from the constant apprehension of a *malignant* disease, Sir. A. says the surgeon will do well to remove the affected breast. Rarely are both breasts affected with this disease, at the same time. It is purely a local affection, and consequently not likely to return after an operation.

The symptoms, progress, and morbid structure of the diseased parts are illustrated by several interesting cases, which we can not pause to notice.

We should next speak of "Chronic Mammary Tumour," but these are so simple in their "nature and so entirely free from any malignant contamination, and withal, scarcely demanding the attention of the surgeon, except when consulted as to their specific character, that we shall simply mention the affection in this connection, and pass on to the consideration of the "cartilaginous and ossific tumour." In certain chronic and specific inflammations of the breast, a gelatine resembling that forming the basis for bone in the fœtus, is occasionally effused; "it becomes vascular, and resembles cartilage in its yellow whiteness, in its compactness and elasticity, and still more in its becoming the nidus for bone; for as the blood-vessels and absorbents enter it, the latter remove portions of it, whilst the former deposit in the interstices produced by the absorbents, the more solid material of bone, viz.; the phosphate of lime; and when the tumors composed of this structure are steeped in an acid, the phospate of lime is formed, but the cartilaginous or gelatinous basis is removed." This particular form of disease is illustrated like the preceding, with cases and plates.

Scrofulous swelling of the breast is analagous to the same disease in the other glands of the body. They may be readily recognized, and require the usual treatment for that constitutional affection. Of the *irritable tumour of the breast*, a good deal has been said, and but a little that has been satisfactory. Although a disease less frequent than some already noticed, still it is so painful, and requires such prompt measures, that we shall give the views of the author for the benefit of our readers.

The breast may become irritable without any perceptible swelling; sometimes it is accompanied with a tumour of a specific growth. The disease is mostly witnessed in females between the ages of sixteen and thirty years: Sir Astley has never seen the disease prior to the age of puberty. When the glandular structure of the breast is the seat of the affection, little or no swelling is to be observed; it remains painful for several hours after a manipular examination. In addition to the pain of the breast, the patient experiences uneasy sensations as far upwards as the shoulder, and backwards to the axilla; sometimes reaching to the inner side of the elbow, and even affecting the fingers; the pain also reaches, on the same side, to the hip. The patient finds it impossible to repose on the affected side; they complain now of heat, now of cold, in the breast; the pain may be compared to that of the tic-doloreux, shooting with electric speed from one part of the breast to the other—sympathetic vomiting is excited; just prior to the eruption of the menses, the sufferings are augmented, to abate considerably during, and partially decline after, the disappearance of this discharge. The integuments of the breast present no marks of inflammation or discoloration; sometimes a part of one, then the whole, of both breasts, is affected. These distressing pains may persist for years without much intermission, yet the breast presents no other marks of malignant disease. Now and then, one or more small moveable tumours, the size of a pea, may be found seated in the mamma; they never suppurate; sometimes disappear spontaneously.

When these little bodies are removed and dissected, they are found to be composed of a "solid and semi-transparent substance, with fibres interwoven into it, but without any regularity." They are destitute of nervous filaments; they are of a cellular, rather than glandular structure.

Hence, similar painful tumours, may be produced in the cellular tissues in other parts of the system.

The diagnosis is easy; the pain, the tenderness, and the unmitigated agonies, which frequently attend a manual examination of the parts, "distinguish it from the hydatid, the chronic mammary tumour, and the schirrous and fungous tubercles."

"The disease is liable to be developed in the delicate and irritable," in those endowed with great excitability with a correspnoding deficiency of tone in the muscular system. In this affection the menses are irregular, abundant or deficient, with or without regular returns. In the treatment of this affection, the indications are obviously to lessen the irritability, lull the local pain, and increase the tone of the general system, at the same time, acting with special reference to the derangement of the catamenial discharge. As a local application, our author prefers a plaster, composed of equal parts of soap cerate and extract of belladonna, or a poultice with solution of belladonna, and bread.

Besides the above, it is recommended to wear-oil silk, or hare-skin next to the breast, as a means of exciting perspiration, thus soothing and allaying the pain. A few leeches may be applied to diminish the vascular excitement of the part; but too much depletion will increase the debility, and augment the excitability.

As constitutional remedies, Sir Astley advises the internal use of the submurias. hydrarg, in conjunction with opium or conium mac. with an occasional aperient.

To lessen the irritability of the part, the medicines after the following formula may be used:

℞ Extract Cinii.
Extract Papaveri *aa* grs. ii.
Extract Stramonii.
Extract Semenibus. gr. ss.
M. ft. Pilulæ.

One of the above pills may be ordered three or four times per diem. The courses are to be regulated by the various martial preparations, either with or without the aloetic pill. The knife is never called for in this disease. Sir Astley has said nothing of some of our best alteratives in the treatment of this affection; such as the guiac. sarsaparilla, iodine and its various preparations. The use of such must be left to the discretion of the physician.

Other interesting papers, besides those already noticed, are embraced in this volume. They relate chiefly to capital surgical operations, which were performed by our author years ago. These surgical feats, achieved forty years since, made quite a noise in the scientific world at the time; now they are executed almost daily in every section of the country, and by the humblest members of the profession without surprise to any.

So much for the advancement of medical science; so much for the good of humanity!

In closing our rather extended notice of the last finished production of the great English surgeon, we should not withhold our praise from the enterprising publishers, Messrs Lea and Blanchard of Philadelphia, to whom the profession in this country is so much indebted for bringing out in a cheap and elegant style, many of the best works of the day. The manner in which this volume is finished does not detract from the matter it contains; certainly the highest praise we could bestow, after what has been said.

Sir Astley in our estimation, does not always write good English; and as his works are destined to live, we think it a great pity that he did not pay more attention to style and diction. A. H.

III.—*Elements of Materia Medica and Therapeutics.* By John P. Harrison, M. D. Professor of Mat. Med. and Therapeutics in the Medical College of Ohio. 2 vols. 8vo. Ledyard & Copperthwait: Philad., 1845.

This is a book which with many things to recommend it, has likewise a goodly share of those which are objectionable. In reading it, every one will be pleased to find in the practical parts of the work much that is accurate and instructive, and some of the theoretical views are interesting and no doubt correct; among the principal defects of the work we cannot overlook the inflated and stilty style in which the author indulges to an extent altogether inappropriate to his subject, and which rather obscures his ideas and renders its perusal painful.

The first part of the first volume is occupied with the consideration of the general principles of Materia Medica and Therapeutics, the *modus operandi* and classification of medicines. In this part, our author makes war upon the classifications adopted by the principal medical writers, and adopts one, of which, he assures us, "the superiority lies in its easy comprehension:" but at the same time goes on modestly to admit that he does not assume to "have attained such a degree of perfection in this matter, as to exclude all efforts at alteration and emendation."

He thinks that Pereira, Thompson, &c. have described too large a number of medicinal substances, and asks if "it is not apparent that such redundancy is both unscientific and injurious? derogating from that simplicity, &c. which should ever characterize all our researches into the sanative efficiency of the articles employed in the practice of medicine." We offer our congratulations to the scientific far-west for this triumphant vindication, by one of her sons, of the claims of science, by refuting the ideas of former therapeutical pretenders.

In reference to the question respecting the modus operandi of medicines, our author leans exclusively to the view that medicines operate only by impressions made through the medium of the nervous system; and asserts that

"The very minute quantities of medicinal bodies which can be made to effect an entrance into the circulation affords disproof of their modus operandi, through that medium."

He starts out too with the sweeping assertion

"That whenever any foreign matter reaches the circulation, it is exclusively under the dominion of the peculiar power attached to all living bodies. Neither molecular composition nor decomposition are under the control or direction of the laws of chemical affinity and repulsion."

These two propositions imply in the first place a most sovereign contempt for the science of organic chemistry, and secondly a perfect confidence on the part of the author that he knows all that can be known in reference to the physical relations, properties and effects of medicines. Admitting fully the agency of the nervous system in receiving and propogating impressions produced by medical substances, and aware that their absorption or admission into the circulation may not be invariably essential; we cannot but think the author has viewed the subject with a partial, if not with a prejudiced eye; and has refused a due consideration to the experiments of Magendie and other late physiologists, in relation to absorption.

Our readers may perhaps desire to know something more of the *scientific* classification of our author. He has six divisions with corresponding subdivisions, viz: 1st. class, Alteratives; Orders, *a*, anti-inflammatory, *b*, anti-cachectic, or invigorating. 2d. class, Evacuants; Orders, *a*, blood-letting, *b*, emetics, *c*, cathartics. 3d. class, Incitants, or Excitants; Orders, *a*, stimulants, *b*, antispasmodics, *c*, tonics, *d*, astringents. Class 4, Secretants; Orders, *a*, diaphoretics, *b*, dureties, antilithics, *c*, expectorants, *d*, emmenagogues, *e*, anthelminties. Class 5, Narcotics, Anodynes. Class 6. Derivatives, Revulsives; Orders, *a*, baths at various temperatures, *b*, frictions, *c*, rubefacients, *d*, epispastics, *e*, pustulants, *f*, supperatives. *g*, cauterizing, counter-irritants.

Now let us see some of the Doctor's scientific consistency. In his first class, he includes under the head alterants, an order which he calls *anti-inflammatory*, therefore we have his authority for supposing that the *anti-inflammatories* are alteratives; but in another place (vol. 1, p. 144,) he says that "the Italians talk of the contra-stimulant property of tartar-emetic; but I attribute its virtues as a remedy in pneumonitis and other phlegmasia, to its *alterant* rather than its *contra-stimulant* power." From this we learn that contra-stimulants are not alteratives, but that anti-inflammatories are; and we hope ere long to see another work from the learned author which may enlighten the profession generally, respecting the means of discriminating, in a general way at least, between *anti-inflammatories*, and contra-stimulants; for we have no idea of seeing such important ideas monopolized by any single individual in the profession.

Proceeding from alterants in a survey of his classes, we find that he does not deviate from the beaten tract until he comes to secretants, and here he only does so by the addition he makes to that class, of the order of anthelmintics—anthelmintics are then secretants: this *scientific* discovery induced us to pursue our researches into the second volume, p. 503, when we found this classification explained further; as follows.

"An anthelmintic or vermifuge is a remedy that dislodges worms from the digestive tube. They may operate to their result by a direct purgative action which sweeping out the contents of the alimentary canal carries the worms onward and forces them through the rectum. Or a vermifuge may act by mechanical irritation of the parasitical animals, and thus destroy them, or the worms may be killed by the deleterious agency of the substances taken to remove them."

Nothing is said in this place about secretion.

Our author goes on, however, afterwards to state that the existence and propagation of *entozia*, (we hope he does not mean entozoa) are closely associated with a perverted state of the secretion of the intestinal tract; and we inferred from his remarks and classification, that all anthelmintics are substances which, by inducing a modification of these secretions put a stop to the *existence* and *propagation* of these troublesome creatures. Now we would be glad to be out of the horns of the dilemma, and to learn how anthelmintics act, whether by purgation, irritation of the worms, deleterious agency of substances upon them, or as *secretants*

Our author goes on a little farther to speak of *invermination*, or the *genesis* of *entozia* and is "fairly invited to expatiate on the wide field of equivocal generation"; but "declines this inviting excursion, for our limits do not allow of any indulgence in such almost impalpable speculations." He could not, however, lose the opportunity of launching out on "*Acarus crossii*"; curious fancies on the origin of men, birds, beasts, fishes, and all creeping things. Chemistry sufficiently bold and aggressive in its attempts to explain the processes of the living economy" and the "Vestiges of the natural history of creation" and gives the *coup de grace* to all "effusions of a vain philosophy," by a simple reference to "the great, pervading, comprehensive law of germs" which, "though apparently inoperative in a few obscure instances of animalcular formation, satisfactorily explains the origin of organized living structures." Now we can see no sufficient reason for such flights of fancy when the only end to be arrived at was to indicate the well known and long established principle that worms are seldom the immediate cause of the symptoms commonly attributed to them, and that they are in a vast majority of instances falsely charged with generating disease." But it is only one of the author's examples illustrating "that simplicity and directness which should ever characterize all our researches," &c.

These are two instances in which the author has deviated from the common place classifications, and the profession will be the best judges as to the extent of the obligations under which he has laid our science by these changes.

Leaving worms and vermifuges, we will glance at some of the author's doctrines in reference to the action of some particular remedies. Take tartar emetic, for example. He goes on to tell us that

"The thoroughness of the depression produced by it transcends that created by any other remedy of a nauseating kind. It subdues and overpowers the vital forces with a steadfast onward and resistless energy, which has often terminated in a rapid extinction of life." "The reduction of activity in the vital phenomena brought about by vomiting is often sudden and great. The catharsis provoked by this salt, is an additional source of the depletion accomplished by its action on the digestive tube." The local effects of tartarized antimony, as far as the mucous coat of the stomach and bowels is liable to feel its disturbing and irritating impressions, are peculiarly energetic" "Twenty grains, retained, brought on sudden and fatal prostration." "Six grains retained in one case nearly proved fatal." "Inflammatien of the stomach has resulted from a few grains of tartar emetic." "A stout woman died of gastritis after taking six grains of the medicine."

It appears to us that these extracts would rather incline to the belief that tartar emetic is, often at least, a *violent, perturbating remedy.* And as our author says that "*violent* and purturbating medication has destroyed many lives in fever;" we would hardly expect to hear of his recommending the remedy in such cases. But to the contrary we find him going off at a tangent, and saying,

As an emetic, tartarized antimony is possessed of great therapeutical efficacy in the commencement of *essential*, or *idiopathic fever*, especially in *autumnal bilious fever.* And in the common substitution of purgatives for emetics, in the beginning and early stages of fever, we cannot but consider that sound therapeutics has sustained an injury." "The controlling agency of antimony over fever is universally admitted."

We cannot withhold our dissent from this opinion respecting what our author calls idiopathic fevers, or any other class of fevers, in which the gastro-intestinal mucous membranes are generally irritated, either primarily or consecutively. Opportunities are unfortunately too often presented in this country, of seeing the results of every variety of treatment in fevers; and of all others the emetic practice is certainly the most disastrous in its consequences. Every one must admit the great efficacy of this remedy in parenchymatous inflammations, but in the generality of our fevers it is certainly not a safe remedy. It is not unusual in the country, where this remedy is in popular use in fever, to see a patient sink rapidly under its effects; and the induction of colliquative discharges, and congestive conditions consequent upon its use are by no means of rare occurrence.

Here we must leave this work by recommending it to the notice of the profession; for if we regard it as in some points objectionable, there are many things in it to recommend it; and those who read it attentively will, we have no doubt, be repaid for their trouble.

We are indebted to the publishers for the opportunity of perusing it.

W. M. C.

IV.—*Lectures on the Principles and Practice of Physic; delivered at King's College, London.* By THOMAS WATSON, M. D., Fellow of the Royal College of Physicians, &c, &c. 2d American, from the 2d London Edition, Revised with Additions, by D. FRANCIS CONDIE, M D. Philadelphia: Lea and Blanchard, 1845. pp. 1060.

There could be no better evidence of the high estimation in which this valuable work is held by the profession, than the rapid succession of editions through which it has passed both in Europe and America. It occupies the very first rank among modern works on the principles and practice of physic, and has received the almost unqualified commendation of the Medical Periodical Press, wherever it has been read. We had the pleasure of giving the first edition a brief notice in the first number of our Journal, (1844;) and now, since it has been reviewed in all the medical periodicals of the day, and indeed, is in the hands of nearly all reading physicians, we would deem it an act of supererogation to enter into any thing like a critical analysis of its multifarious and most valuable contents. We cannot omit the occasion, however, to recommend it warmly to our immediate readers in the Southern states; not that they may expect to find in it an able exposition of the nature, causes, and best treatment of the most serious prevalent diseases in this region, for such is not the fact; but because it abounds in sound principles, and contains an excellent summary of the latest discoveries and most important improvements in medical science. We may study with profit the elements and principles of medicine as taught by the profound and erudite physicians of London and Paris, for *mutatis mutandis*, they will be found useful in all places; but, as we have ever contended, the region in which we reside, differing as it does so materially and in so many particulars from theirs, gives a peculiar *caste* to a great many diseases, and demands a corresponding modification in their treatment. Say what you will about the human constitution and its diseases being the same in all places, and requiring the same plan of treatment; we do not believe a word of it. The Englishman in London, is not the same as the Englishman in New Orleans; nor will the remedies that cure his pneumonias, his rheumatisms, and his bowel complaints, there, be equally successful here. If the various disorders of the human system are entitled to definite names, and are capable of being classified, the greatest portion of them may prevail throughout the habitable globe; though evidently greatly modified by climate, locality, habit of life, &c. If we wish to learn the proper management of Southern diseases, we must study the experience and observations of Southern physicians. Nay more; we must see them ourselves. The great mind is yet to appear in the southern portion of the Mississippi Valley, familiar by long and extensive experience with its various modifications of diseases, and embued with all the science and learning of the profession, that is to give us a work on the theory and practice of medicine, adapted to the region and to the time. If the common impression, that *circumstances and the occasion make the man*, be true, we may hope to see such an one spring up even in our day and generation. A spirit of enquiry and reflection is already awakened in the South, which we hope and trust may lead to the most auspicious results.

But to return to Dr. Watson. He appears to be perfectly conversant with medical knowledge, both ancient and modern ; his work affords a comprehensive glance at the whole fabric, and may be studied with profit by the physicians of every clime. It is an admirable text book for the student, and a useful companion for the practitioner. It is especially as regards diseases somewhat uncommon, that this work will be found convenient and useful to the practitioner. As a book of reference it will afford him the best information of the day respecting these, though it is sadly deficient in regard to *our* most formidable diseases. Nothing is said of yellow fever ; nor of our congestive fever. Dr. Condie has supplied valuable notes on bilious fever and chronic diarrhœa. Dr. Watson's lectures on diseases of the lungs, the heart, the nervous system, and dropsies, are very valuable. Dropsies are very common in this region, and much valuable information may be gathered from Dr. Watson in regard to their pathology, without a proper knowledge of which, our treatment must be empyrical.

We would here conclude our notice of this valuable work, but cannot forego the pleasure of laying before such of our readers as may not have access to it, the following interesting extract on the nature of contagion and the communication of disease .—

" To say that a febrile disorder, is contagious; is the same thing as to say that it is produced by an *animal* poison. Now there are many poisons, very deadly poisons too, which cause diseases that are not communicable from person to person. That particular poison, the *malaria*, is of this kind.

" Of the inorganic poisons some are taken into the blood, and emerge again from the body, unaltered, with one or more of the ordinary secretions ; chiefly with the urine. They may induce changes in the body as they pass ; and if these changes be salutary, the substances so inducing them become medicaments. If the changes be destructive or injurious, they are strictly poisons.

" Other of the inorganic poisonous substances do not find so ready an exit from the body, They enter into permanent chemical union with the constituent tissues of particular organs. In this way, to use the words of Liebig, they deprive the organs of the principal property which appertains to their vital condition, viz: that of suffering and effecting transformations. If the organs of which the functions are thus destroyed be vital organs, these poisons are fatal.

" But the animal poisons, those at least with which we are now concerned, act in a totally different manner. They effect changes in the blood, whereby they are themselves abundantly multiplied or reproduced ; and the eruptive disease that ensues seems to be the mode provided by nature for the escape or the expulsion of this newly-formed morbid matter from the system. This is the old fashioned humoral pathology, founded on bold, unproved speculation ; and it is most curious to see these very doctrines, which had sunk into such universal discredit and contempt, now again assuming their places, as scientific truths, upon the secure basis of organic chemistry. A wonderful specimen this of the sagacity of the older physicians—of the despised wisdom of our forefathers.

"The ancients attributed various disorders to a fermentation of the animal fluids. The cause of fever, according to Hippocrates, was some morbid matter in the blood. This matter, by a process of concoction, was brought, in a certain number of days, into a state in which it was ready for expulsion from the body. It was then thrown off by hæmorrhage, by sweats, by alvine discharges, or deposited upon the surface in the form of cutaneous eruption; and these eruptions or evacuations constituted the crisis of each fever.

"The doctrine thus enunciated by the Father of Physic, is very nearly the same with that which Liebig is teaching in the nineteenth century. This distinguished chemist ascribes the phenomena which succeed the introduction of certain animal poisons into the blood, to a certain process exactly resembling fermentation. Let me try, in a few words to expound his views on this deeply interesting subject.

"You know that the brewer excites the fermentation of his sweet-wort by adding to it a small quantity of yeast. Wort is an infusion of malt, and contains sugar and gluten, with other vegetable matter in solution. Yeast is putrefying gluten; and its compound particles are, therefore, in a state of intestine motion or transposition. When placed in contact with sugar in solution, it has the property of communicating a similar intestine motion to the elements of the sugar, whereby they arrange themselves into new and simple forms, namely, into alcohol and carbonic acid. If there were no gluten in the wort, this would be the whole of the process, during which the added yeast disappears. But the decomposition or fermentation of the sugar reacts upon the gluten in the wort, and converts it gradually into yeast, which mingling with the liberated carbonic acid, rises and floats upon the surface of the fermenting liquid; so that, when the process is completed, there has been produced thirty times as much yeast as was originally added to the wort.

"Now this is but a type of what happens in other fluids under analogous circumstances; and it may be laid down as an abstract proposition in Liebig's, or rather his translator's words, that 'a substance in the act of decomposition, added to a mixed fluid in which its constituents are contained, can reproduce itself in that fluid, exactly in the same manner as more yeast is produced when yeast is added to liquid containing gluten.'

"Thus the virus of small pox (which virus is formed out of the blood,) causes such a change in the blood as gives rise to the reproduction of the poison from the constituents of that fluid; and whilst the process is going on, the natural working of the animal economy is disturbed—the person is ill. The transformation is not arrested until all the particles of the blood which are susceptible of the decomposition have undergone the metamorphosis.

"Leibig shows that similar processes may take place in mixed fluids; (and therefore in the blood,) without the regeneration of the added substance, just as the fermentation of a solution of sugar is effected by the addition of yeast, without any reproduction of the yeast, if there be no gluten in the saccharine solution. In such cases, the disease which accompanies, or results from the transformations that occur in the blood, is not contagious ; the poison is not renewed. It is thus apparently, that certain miasms produce disorders which are not communicable from person to person.

"In order that a specific animal poison should effect its own reproduction in the blood, and excite that commotion in the system which results from the formation and expulsion of the new virus, it is requisite that a certain ingredient (analogous to the gluten in the brewer's sweet-wort,) should be present in the blood, and this ingredient must have a definite relation to the given poison.

"If this ingredient be indispensably necessary to life, the poison which transforms and destroys it, is inevitably a fatal poison. May not this be the *modus operandi* of the poison of hydrophobia? Again, if this ingredient be wanting, no reproduction of the poison takes place, nor, of course, any of those symptoms which are consequent upon such reproduction. The poisonous qualities of the animal substance are not developed. It ceases to be a poison. And this ingredient, if naturally present, is exhausted and destroyed, for a while at least, by the operation of the poison, Hence, for a while at least, the same disease cannot be again produced by the agency of that poison. Supposing the ingredient to be one which is not essential to the composition of the blood, and to have been thus destroyed and exhausted, it may never be replaced ; or it may be replaced only after a long interval. In some persons it may never exist at all ; or it may exist at certain periods of their lives. It may even be acquired by unnatural or peculiar modes of living. All this is not only very possible, but probable. A certain number of peculiar circumstances do certainly exist in the blood of some men, which are absent from the blood of others. In childhood and in youth, the blood of the same individual contains variable quantities of substances which are not to be found in it at other periods of life.

"This theory of Leibig's offers, then, a reasonable explanation, the only explanation, indeed, that I have ever met with, of the curious facts, that contagious disorders furnish a protection, temporary or permanent, against their own return ; that they have a tolerably definite period of incubation, and run, for the most part, a determinate course ; that some persons are less susceptible than others of these animal poisons, or not susceptible at all ; and that the same individual may be capable of taking a contagious disease at one time, and not at another. Moreover, the light supplied by this theory gives distinctness to our conceptions respecting certain deviations from the regular course and type of these diseases ; which deviations are not uncommon. Thus the symptoms which precede and usher in the eruption are sometimes slow, halting, and irregular in their progress ; appear, and then recede, and reappear, so that we are in doubt what is to happen, until the disease declares itself in its decided and authentic form. We may suppose this to depend upon some tardiness or interruption of the process, whereby the virus is (to use the ancient term) concocted. Again, the series or combination of symptoms that marks the specific disease is sometimes, as I stated before, *incomplete*. We have the eruption of measles without the catarrhal symptoms ; the sore throat without the rash of scarlet fever. And experience has found that where the malady is thus imperfectly developed, the protection it confers against its recurrence is all incomplete. To explain this double failure we may reasonably infer a corresponding defect in the series of changes which a poison tends to produce in the mass of the blood.—Glandular enlargements and chronic abscesses are frequently *sequatæ* of these exanthematous disorders. They may be considered to represent the dregs of the reproduced virus, which has been imperfectly eliminated from the system by the usual channels.

"Such is a brief exposition of Liebig's ingenious theory. Do you ask whether I adopt it, with implicit credence in its truth? I answer by no means." Respecting the points so curious, it is scarcely possible to refrain from speculation altogether. These views can be recommended by the authority of a consummate chemist. They furnish a plausible explanation of the main facts of the case, namely: that the disease is produced by an animal poison; the specific virus increases prodigiously in quantity within the body during the progress of the disease; and that the susceptibility of its influence in that individual, is thereby somehow exhausted. I entertain the theory, therefore, until a better one is propounded. It has this incidental merit, that it involves no risk of practical error."

We must here close our imperfect notice of this valuable work, and again recommend it to the student and practitioner. F.

Part Third.

EXCERPTA.

1.—*Report on the Progress of Human Anatomy and Physiology in the years* 1843-4. By JAMES PAGET, Lecturer on General and Morbid Anatomy and Physiology, and Warden of the Collegiate Establishment, at St. Bartholomew's Hospital.

The purpose and the general plan of the following Report are similar to those of the last. The works noticed are, with a few exceptions, those published between the first day of October 1843, and the last of September 1844. It is, for many reasons, larger than that of the last year; chiefly because more progress has been made in the sciences of which it treats, and many of the subjects in which that progress has been effected are too important to be lightly passed by, and too difficult for a brief account of them to be clear.

CHEMICAL COMPOSITION OF THE BODY.

Elementary constituents. The researches of MM. Devergie and Hervy are known, which appeared to prove the existence of small quantities of copper and lead in the tissues of the human body. MM. Flandin and Danger, and the commission of the Academy of Medicine, contradicted these observations;* but M. Barse, in a paper, read before the Academy of Sciences of Paris, in August 1843, confirmed them by finding both the metals in the bodies of two persons to whom they could not have been given for poisons. The researches of Signor Cattanei di Momo seemed to prove that the metals do not exist in the bodies of new-born children or infants; and, now, M. J. Rossignon has given a probable account of the sources from which the bodies of adults derive one of them—the copper. He has found this metal in the blood and muscles of the dog, and in many articles of vegetable and animal food, e g. gelatine from bones, sorrel, chocolate, bread, coffee, succory, madder, and sugar. The ashes obtained from starch-sugar yield 4 per cent. of copper; those of gelatine 0·03 per cent.; those of bread 0·005 to 0·008 per cent.†

Proteine compounds. The most valuable contributions made this year to the science of animal chemistry are the works of Professor Mulder.‡ By his continued investigations he has discovered two new proteine-compounds of great importance, a binoxdyde and a tritoxyde (bi-oxy-and tri-oxy-proteine). The former contains two, the latter three equivalents more of oxygen than pure pro-

* Bulletin, Fevr. 19, 1839.

† Medical Gazette, Dec. 1, 1843, from the Gazette Medicale.

‡ Proeve eener algemeene Physiologische Scheikunde; Rotterdam, 1843, 8vo. The part of this work which relates to all the proteine-compounds is translated by the writer in the Medical Gazette, Sept. and Oct, 1844; and a more complete account of the two compounds above mentioned is given in the same journal, Feb 9, 1844, by Dr. G. Bird, from a paper in the Annalen der Chemie und Pharmacie, Bd xlvii, p, 300.

teine does. One or both of these compounds exists ready-formed in arterial and all inflammatory blood; in the buffy coat; in pus; in false membrane; in vitelline substance; in cooked meat. It is probable that they are both formed by the oxydation of fibrine (or, at least, of that compound of proteine which appears as fibrine when blood coagulates), as often as the blood passes through the lungs. When blood coagulated by heat is digested with water, much of the trytoxyde of proteine is dissolved; and it appears to constitute a large portion of the *serosity, muco-extractive matter, and extrait de viande* of different authors. Mulder considers also that it is in its combination with proteine in these oxydes that oxygen is conveyed to the systemic capillaries (see page 253), where it is consumed, and whence, in place of the oxy-proteine, fibrine returns to the lungs in the venous blood.

Both compounds exist in excess in the blood during inflammatory diseases. They form the greater part of the buffy coat, and of false membrane. When a buffy coat is digested in water it is divided into two portions: one soluble, of which the greater part is hydrated tritoxde of proteine, (and it is this which M, Bouchardat supposed to be gelatinous matter,) the other insoluble, consisting of binoxyde of proteine with fatty matter. The composition of false membranes is the same with the addition of gelatine. In pus the tritoxyde of proteine is what has been described under the name of pyine; a chief portion of vitelline substance is a sulphuret of the binoxyde.

The same compounds which are thus ready-formed in the living body may be, in several ways, artificially obtained. Both of them are formed by the oxydation of fibrine when it is boiled in water;* the tritoxyde alone is similarly formed from albumen. It is this tritoxyde which has been supposed to be gelatine obtained by the decomposition of albumen or fibrine when subjected to long boiling. The hydrated tritoxyde may be obtained by forming a chlorite of proteine (by passing a current of chlorine through a solution of albumen) and decomposing this with ammonia. The binoxyde may be obtained by the decomposition of the bisulphide of proteine which forms a chief constituent of hair. Lastly, when fibrine is partially dissolved in very dilute hydrochloric acid, the precipitate obtained by adding ammonia (the *albuminose* of Bouchardat) is an anhydrous tritoxyde of proteine; and the portion which is not dissolved by the acid is, probably, binoxyde of proteine. The formula of the binoxyde given by Mulder is C. 40, H. 62, N. 10, O. 14; that of the tritoxyde, C. 40, H. 62, N. 10, O. 15.†

M. Wurtz‡ finds in the products of the putrefaction of fibrine exposed for eight days in summer to the air, albumen carbonic acid, acetic acid, *butyric acid* and ammonia. He considers that the production of the last named acid indicates that fibrine (and, perhaps, the other proteine compounds) may be transformed into the neutral fatty bodies which are so intimately related to the volatile fatty acids.

He has also succeeded in removing from albumen of white of egg, all the inorganic matters through its combination with which it has been supposed to derive its solubility in water; and this without altering its solubility or its other essential chemical characters.

Gelatine. Mulder's analyses from which he deduced his formula for gelatine (C. 13, H. 20, N. 4, O. 5), have been confirmed by Van Goudoever, who has also analysed gelatine which had lost its power of gelatinizing by long-continued boiling, and has found its composition to be C. 49.5, H. 6.56, N. 17.36, O. 26.58, or, in equivalents, C. 52, H. 82, N. 16, O. 21. He hence deduces that it is changed into a compound in which four equivalents of gelatine are united with one of water, (4 (C. 13, H. 20, N. 4, O. 5) + aq.); a compound analogous to one

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* It is probably by a similar oxydation that we may explain the observation of Scherer, that moist fibrine when exposed to oxygen absorbs more of it than it gives off of carbonic acid.

† For these formulæ, the equivalents of the elements are considered to be O. 100, C. 76.437, H. 624, N. 88.36.

‡ Report of the Academie des Sciences, seances du 15 et 20 Avril, in the Gazette Medicale; and in the Ann. de Chimie et de Physique, Oct. 1844

of those of gelatine with chlorous acid, discovered by Mulder (4 (C. 13, H. 20 N. 4, O. 5) + Cl. 2, O. 3).*

Fatty Principles. Mulder has also discussed, at some length, the chemical, physiology of the fatty matters of both plants and animals. He adopts the opinion of Berzelius and Redtenbacher† that the basis of neutral fats is an oxyde of a radical, which they name Lipyle, and of which the formula is C. 3, H. 4, that of the oxyde being C. 3, H. 4, O. With stearic, margaric, and elaic acids, this forms stearine, margarine, and elaine ; and glycerine is not present (as such) in the fats, but is formed in their saponification by the union of two equivalents of the oxyde of lipyle, with three equivalents of water.

After suggesting the modes in which the several kinds of fat may be transformed in their passage from plants to animals, and from the body of one animal to another, he considers whether the neutral fats taken as food are conveyed, unaltered, into the tissues in which they are deposited, or are saponified, and again reduced into neutral fats by being made to combine with lipyl-oxyde in the body. He supposes they are saponified by the alkali of the bile and chyle ; and that glycerine is, at the same time, formed, and carried with them into the blood. The question, in this case is, how is the neutral fat reproduced ? for glycerine will not unite again with the fat-acids to form neutral fats. He supposes that the change is affected by the union of the fat-acids with nascent lipyl-oxyde, and that this is produced in such circumstances as, if more oxygen were present, might give rise to the formation of lactic acid. For when lactic acid is sublimed, a white substance is obtained which has the composition of the binoxyde of lipyl (C. 3, H. 4, O. 2), and of which two equivalents, when it is in contact with water, absorb one equivalent of water, and form lactic acid. And, for explanation of the way in which these changes may be brought about, he refers to the researches of Lehmann§ on the influence of mixtures of fat and albumen in inducing chemical changes in other principles.||

Spontaneous decomposition Dr. Helmholtz¶ has published some interesting experiments for the determination of the conflicting views respecting the nature of the so-called spontaneous decomposition of dead organic substances. The question has, for some time, been, whether these are due to the development of microscopic organisms, to which the substances decomposed serve as nutriment, and the secretions of which are the products of the decomposition ; or whether the process is one of mere chemistry.

In the first place the author confirms the fact already known (but which Liebig tried to explain away) that these decompositions do not take place in substances which have been heated to the boiling point, and which are only supplied with air that has been exposed to a red heat. He found that azotized organic substances, derived from the animal proteine- and gelatine-compounds, remained unaltered for eight weeks in summer, though freely supplied with air, provided that air had all passed through a red-hot glass tube before coming to them. *They consumed the oxygen:* but no process of fermentation or decomposition could be detected : yet it soon commenced, with the attendant development of infusoria, when only a small quantity of air which had not been heated was introduced.

With other spontaneously-decomposing azotized compounds, these peculiarities are not observed. Hydrocyanic acid and urea decompose as rapidly at the boiling heat and in closed vessels, as they do when exposed to the air. Morever, when urea decomposes in closed and heated vessels in which it is mixed with

* Physiologische Scheikunde, p. 351, where also is a suggestion of the mode in which gelatine may be formed from the proteine-compounds of the food.

† Annalen der Chemie und Pharmacie, August 1843.

‡ The formulæ given by Mulder are here adopted, in which water is considered as H. 2, O,

§ In Simon's Beit. zur Phys. und Path. Chemie Bd. i, p. 73.

|| Physiol. Scheikunde, p. 262-82.

¶ Muller's Archiv. 1843, Heft v.

other organic substances, neither fermentation nor putrefaction is excited in them by its decomposition.

Again, organic substances do not ferment or putrefy when no oxygen is supplied to them except such as has been immediately derived from water decomposed by electricity. So that it appears that neither oxydation, nor the spontaneous decomposition (resembling putrefaction) of urea, nor the chemical movement produced by the electric current, nor the presence of any of those constituents of the atmosphere which remain undecomposed by a red heat, is capable of exciting the fermentation or putrefaction of organic substances. Since, however, the presence of some part of the atmosphere is necessary for it, it can be only by one or both of the two remaining constituents that it is excited, and these are, the exhalations of putrefying substances, and the germs of organic beings.

To determine which of these two is the real excitant of the process, Helmholtz separated them. He filled a vessel with fluid capable of putrefaction, but heated and excluded from the air, so that it would not putrefy spontaneously, and then he introduced into it another fluid, also capable of putrefaction, and which had not been heated nor excluded from the air; introducing it, however, by endosmosis through a bladder which even the smallest organic germs could not pass through. The fluid thus introduced excited putrefaction in that within the vessel, and the process went on as quickly as it would if the first fluid introduced had been exposed to the air. But its results were different from those of open putrefaction; the organic fluids though they developed gases and had a putrid smell, remained clear; the portions of flesh placed in them remained firm, though putrid; and no infusoria were produced. Experiments of the same kind did not succeed in exciting vinous fermentation. Although endosmosis took place, yet no gases were developed, and no yeast-vegetables were produced.

It appears then, 1st, That for putrefaction of animal substances (at least of the proteine- and gelatine-compounds), the most essential condition is the presence of putrefying particles of similar substances: though perhaps it may also be excited by a vital process. 2d. That the putrefaction of these substances differs from the spontaneous decomposition of other azotized compounds, by its capability of propagating itself. 3d. That it presents the most favorable condition for the development of living organisms; and, that this, though not essential to the process, modifies its results. And 4th. That the vinous fermentation is connected with the access of some solid body, which may be excluded by the interposition of a bladder, and which can only be supposed to be the *yeast-plant.* Of this yeast-plant, analysis confirming, though independant of, each other, have been made by Mulder* and Schlossberger†. Carefully-purified yeast yields two distinct substances, of which one, comprising the cell-contents, and extracted by potash or acetic acid, resembles the proteine-compounds; and the other, comprising the cell-membranes, might be classed with the amylum or cellulose compounds.

As one of the products of spontaneous decomposition, Dr. Zimmerman‡ has illustrated the formation of the the triple phosphate in numerous putrefying organic substances besides the urine. The crystals form much less abundantly, or not at all, when those compounds are removed from the several substances which, in their decomposition, may yield ammonia, such as proteine-compounds, mucus, pus, &c.

BLOOD.

Coagulation. An inexplicable case, in which the complete coagulation of the

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* The analysis of Mulder is stated by Schlossberger to be in the first part of his Physiological Chemistry. This refers probably to the German translation, which was published later than the Dutch original, in which I find no such analysis.

† Annalen Der Chemie und Pharmacie, August, 1844,

‡ Casper's Wochenschrift, Oct. 21, 1843.

blood did not take place till fifteen days after its abstraction, has been published by Dr. Polli,* whose former researches on the blood were given in the last Report, and who adduces this case in proof that there is no blood which becomes putrid before it has coagulated. The patient was a man 37 years old, with acute pneumonia. The blood of a first bleeding was drawn into a block tin vessel, and set in a temperature between 46° and 52°. It remained liquid for eight days, the blood-corpuscles having settled to the bottom, with the liquor sanguinis floating above them, exactly like serum pressed from a clot. On the ninth day, a membranous pellicle began to form on the surface of the fluid, and this becoming thicker, and increasing in tenacity and consistence, acquired at last all the characters of the most genuine buffy coat. The serum began to ooze from the clot on the fifteenth day: and on the clot being now taken out of the vessel, it was found that the upper four-fifths of it consisted of buffy coat, and only the lower fifth of clot colored with corpuscles. The serum which continued to be expressed was perfectly transparent, and the blood did not show signs of putrefaction till a month after it had been drawn from the body. (A small quantity drawn from another pneumonic patient and placed under the same circumstances, was completely coagulated in two hours, and was quite putrid in fifteen days). In fifteen bleedings of the same patient in the following eight days, the blood drawn gradually lost its indisposition to coagulate, the whole process being completed each time in twelve hours or less. The patient recovered.

Colour.—Some experiments by Scherer† both confirm the opinion of Nasse,‡ that the change from the arterial to the venous colour of the blood depends in great measure on the form of the blood-corpuscles, and explain most of the observations of Dr. Stevens on the effects of distilled water and salts upon the blood. Their general conclusions are: 1. That when fresh-stirred and bright-red ox-blood is mixed with distilled water, it acquires a dark-red colour, and its corpuscles, by imbibing water, become spherical, and at last vanish. But, 2. That if, after the change has begun, and not gone far, a concentrated solution of a neutral salt be added, the blood-corpuscles again acquire their natural form, and the bright-red colour is restored. 3. That when oxygen is passed through blood darkened by the addition of distilled water, it is not changed in colour, and the blood-corpuscles do not reappear; but that the same kind of blood, mixed with a small quantity of milk, or oil, or finely-powdered chalk, or gypsum, soon regains its bright-red colour. 4, Again, by the long-continued contact of concentrated saline solutions with the blood-corpuscles they become jagged and decomposed, and the blood becomes black; and those which have been reddened by the action of salts, become black again on being expanded by the imbibition of water. 5. By adding carbonic acid to bright-red blood, its corpuscles change their biconcave for a biconvex form, and at the same time its colour changes from red to black. So that there are always changes in the shape of the blood-corpuscles, coincident with the changes in the colour of the mass of blood; whenever they are dilated, as by distilled water or carbonic acid, the dark colour is produced; whenever they are contracted into the biconcave form, the bright-red colour is restored.

Mulder, also,§ espouses the opinion of the changes of colour in the blood

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* Gazzetta Medica di Milano, Gennaio 20 1844—On the blood corpuscles; the discussion between Dr. Carpenter and Mr Wharton Jones, whether it be by the red or by the pale corpuscles that the albumen is to be supposed to be transformed into fibrine, is continued in the three last preceding Nos. of this Review. I must be content to refer the reader to their several papers: and as to Mr. Addison's work 'On the Actual Process of Nutrition in the Living Structures,' and to his papers in the Prov. Med. and Surg. Trans. for 1843, and in several recent numbers of the Medical Gazette. for a further account of his views and observations respecting the apparent transformations of the pale corpuscles of the blood into pus corpuscles. mucus-corpuscles. tissues, &c.

† Henle and Pfeuffer, Zeitschrift, &c., and Oesterr. Medic. Wochenschrift, Nov. 4, 1843;

‡ Handworterbuch der Physiologie; Art. Blut.

§ Verslag van de Vertiende Vergadering van het Nederlandsche Instituut in 'Het Instituut,' 1844, No. iv, and Physiologische Scheikunde, pp. 361 77.

being immediately due to physical rather than to chemical changes of the corpuscles, and has added many facts to those just quoted in disproof of the opinion of Liebig. that the changes are due to the alternate production of the carbonate of the protoxyde, and of the perexyde, of iron in the blood-corpuscles, as they pass alternately through the systemic and pulmonary capillaries. His chief facts are—1. That the elementary composition of the colouring matter is the same, whether obtained from arterial or from venous blood, viz., C. 44, H. 44, N. 6, O. 6. Fe. O. 2. That the change from dark to bright blood is effected as completely by the agency of a neutral salt as by oxygen. 3. That if the iron were present in the blood as an oxyde, (and especially as a peroxyde) it should be easily extracted by weak acids; but he has found that well prepared hæmatine may be digested in diluted hydrochloric or sulphuric acid for several days without the iron in it being in the least diminished. After being so treated he has obtained, after incineration, the regular proportion of 9-49 per cent. of oxyde.* If strong sulphuric acid be poured on dried blood, or dried pure hæmatine, and kept on it for some days, and then water be added, hydrogen is evolved, and sulphate of peroxyde of iron is found in the solution, which could not happen if the iron had been at first in the form of peroxyde.† 5. The iron may thus be all extracted from the blood, or from hæmatine, (though not, as some say, without affecting the colour,) and the other constituents may be obtained in a separate form. Numerous analyses of this constituent, by Van Goudoever, regularly yielded the same equivalents of the elements, viz., C. 44, H. 44, N. 6, O. 6; but if the iron had been united with this in the form of Fe. 2, O. 3, and in the proportion of one equivalent to two, there should have remained only four and a half equivalents of oxygen.

Mulder concludes, therefore, that iron is present in hæmatine, as iodine is in sponge, or sulphur in cystine, or arsenic in cacodyl. His notion of the mode in which the changes of colour are effected is, that when the corpuscles of the venous blood are exposed in the lungs, oxy-proteine is formed by the oxydation of the fibrine proteine of the liquor sanguinis, or, perhaps, by the oxydation of the outer layer of the cell membrane of the corpuscles. If formed in the liquor sanguinis, its peculiar plasticity would lead to its being deposited in a thin layer on the corpuscles. In either such case, the dark corpuscles, would after respiration, be invested by a thin layer of white and imperfectly transparent oxy-proteine, or buffy coat, through which they would look bright-red, as dark blood does when contained in a vessel of milk-white glass. But, in the systemic capillaries, the oxy-proteine may be consumed in nutrition, and the darkness of the corpuscles will then again appear unveiled.

Moreover since it appears that, in the biconcave form, the corpuscles by reflecting more light, are always bright, and in the biconvex form always dark, it may be that in the arterial blood they are not only *buffed*, but also *cupped*, by the oxy-proteine, [by the plastic properties of which, moreover, it is easy, on this pretty theory, to explain the ready adhesion of the corpuscles in inflammatory blood.] Diluted acids, which make bright blood dark, may do so by making the outer layer of the corpuscles transparent, as they do fibrine before dissolving it; and concentrated solutions of neutral salts may make it bright by making the same layer contract.

Chemical composition. M. Figuier‡ has suggested an easy method for the

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* Liebig adduces the possibility of extracting iron from dried blood as one of the proofs of its being in an oxydized state; but Mulder says this iron must have been extracted from some other constituent of the blood; for others, besides the globules, even pure serum, contain iron.

† When the blood or its colouring matter has been exposed to the air or prepared in it, the iron must always, according to Liebig's view, be in the state in which he supposes it to be in arterial blood.

‡ Report of the Academie des Sciences du 8 Juillet 1844. and, in full, in the Ann. de Chimie et de Physique, Aout, 1844.

rough analysis of the blood. By adding to one volume of defibrinated blood two volumes of a solution of sulphate of soda, of sp. gr. marking 16° to 18° in Baum's areometer, the corpuscles will separate, (as Berzelius showed,) and may, with hardly an exception, be all collected on a filter. Thus their quantity may be estimated, as that of the fibrine may [very roughly] by what is obtained by whipping. The quantity of albumen may be estimated by boiling the serum ; and the water, by evaporating a separate portion of blood.

Ashes. Enderlin* has carefully analysed, in Liebig's laboratory, the ashes of the blood. Their solution in hot water formed a very alkaline fluid, which, in all cases, contained alkaline phosphates and sulphates, chloride of sodium, and, sometimes chlorides of potassium. But, from various tests [which I have repeated, and found exactly true,] he proves that—1. The alkaline reaction of the ashes cannot be due to an alkaline carbonate, for both the ashes and the precipitates from their solution by nitrate of silver and chloride of calcium, may be dissolved in acids without the development of gas. 2. The alkaline reaction cannot depend on the presence of caustic alkali ; for then the solution could not be, as it is, neutral after the addition of a solution of neutral chloride of calcium. 3. The absence of alkaline carbonates and of carbonate of lime in the ashes of the blood, proves that its albumen cannot be in the form of a salt (albuminate) of soda ; and furnishes additional evidence that there are no alkaline salts of lactic, acetic, or fatty acids in the healthy blood ; and, lastly, proves that the blood can contain no alkaline carbonate. 4. The alkaline character of the blood-ashes and of the blood itself, must therefore be due to the phosphate of soda ; and the presence of the tribasic phosphate of soda in the ashes proves, according to Enderlin, that it must be in the same form (3 NaO., P. 2, O. 5—that of the basic phosphate of soda of earlier chemists,) in the blood itself ; for this salt alone remains tribasic after a red heat—the common phosphate of soda would yield pyrophosphate after incineration. He shows also that this view of the alkaline nature of the blood is consistent with the phenomena of respiration, and all other facts : especially, solutions of both the basic phosphates of soda are distinguished, as the serum is, by readiness to absorb large quantities of carbonic acid :

The quantitative analysis of the ashes showed that, in 100 parts from human blood, there are :

Tribasic phosphate of soda,	22·1
Chloride of sodium,	54·769
" potassium,	4·416
Sulphate of soda,	2·461
Phosphate of lime,	3·636
" magnesia,	0·769
Oxide of iron, with some phosphate of iron,	10·77

It follows from these analyses that the albumen in the blood is not in the form of an albuminate of soda, nor of a combination with carbonate or bicarbonate of soda, but is in combination with the alkaline tribasic phosphate and chloride. The former salt possesses, in a high degree, the power of dissolving proteine-compounds and phosphates of lime ; and it is probable, therefore, that it is the solvent of both these constituents of the blood.

* Annalen der Chemie und Pharmacie, Mars und April, 1844. The same paper contains analyses of the blood-ashes of the calf, ox sheep, and hare, confirming rhe above conclusions, and numerous miscellaneous observations on the chemical characters of the proteine-compounds. Other analyses from these papers are reported under the head of Saliva and Fœces. They contain also an analysis of the ashes of oxflesh, which the author finds idential (in quality) with those of blood, confirming thereby the analyses of Playfair and Bockman, who found a similar and even closer identity of composition between the complete blood and flesh.

Milky serum. Dr. A. Buchanan,* by experimental bleedings, has confirmed the fact of the frequent or general occurrence of milky or opaque serum in the blood of healthy persons, after taking food. The serum, he says, becomes turbid about half an hour after taking food: the discoloration increases during several hours, attains its maximum in about six or eight hours (after a full meal by a healthy person,) and then becomes gradually clearer, till its limpidity is restored. The opaque serum is generally milk-white, sometimes cream-yellow, or yellowish-brown, like thin oatmeal gruel; or it merely loses its limpidity, and is like weak syrup. It always contains solid white granules, smaller than the blood-corpuscles (spherical or irregular in form) which are suspended in it, and which will rise in a white cream to the surface, either spontaneously or after the fluid has been saturated with common salt. The cream thus obtained is soluble in caustic potash, but insoluble in ether and alcohol; and is considered by Dr. R. D. Thomson as probably a proteine-compound.

GENERAL ANATOMY AND PHYSIOLOGY OF THE TISSUES.

Tendinous tissues.† S. Pappenheim‡ has described the nerves of several of these tissues. In the periosteum, whether covering the drafts or the articular ends of bones, he finds them very numerous, lying especially in the outer surface of the membrane, in company with or upon the arteries, and having terminal loops. In the ligaments, there are nerves, which, after ramifying in the cellular tissues covering them, penetrate with the processes of that cellular tissue, in company with or upon the vessels, into the substance of the ligaments and end in them with plexuses and loops. Nerves may be found in like manner in all capsular ligaments; and it may be expressed, as a general rule, that all ligaments which receive vessels receive also a small number of nerves, though it is but one or two primitive fibres. In tendons, nerves can only sometimes be traced: and Pappenheim has never traced them into human tendons. He supposes that here also the rule of nerves coexisting with arteries may hold; [but here he is wrong, for arteries may be injected in the toughest tendons;] he believes that he has proved the existence of both sympathetic, sensitive, and motor fibres, in all nerves of the fibrous tissues which he has yet examined.

Serous membranes. Reichert§ describes (what Henle doubted) an epithelium on the interior of the tendinous and sub-cutaneous bursæ, like that lining arteries and the true serous membranes. He has also|| explained the error of his and Remak's observation of a supposed layer of cells within the epithelium of the blood-vessels.¶ The appearance is due to the formation of artificial vesicles by the action of water. It is often produced on the surface of serous membranes, gland-ducts, &c.; and is always likely to lead to error.

Valentin** has related some interesting experiments on the properties of animal membranes as *filters.* A solution [or suspension] of albumen so diluted that it appeared a homogenous fluid under the microscope, when filtered through some horses' pleura previously dried, was separated into a more diluted fluid which passed through, and a more concentrated one which was retained upon the filter. A similar division was similarly effected in pure serum which had been repeatedly filtered through paper. Saline solution passed through unaltered. [Probably it may be added that different serous membranes filter with different degrees of fineness, and that on this depends the differences of the

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Transactions of the Glasgow Philosophical Society, March, 1844; Extract in the London and Edinburgh Monthly Journal of Medical Science, July, 1844; and in the Medical Gazette. Oct 4, 1844. The examinations are confirmatory of Mr. Gulliver's. (Gerbers Anatomy, Appendix, p. 22:) and of the general opinion, that the opacity of the serum is due to the admixture of chyle.

† On the structure of the Fibro cellular tissue, see Reichart's observations on the description of the nerve-fibres.

‡ Muller's Archiv. 1843, Heft v. § Ib. 1844. Jahresbericht, p. 229. || Ib. p.289.

¶ See Report. 1842, p. 39. ** Lehrbuch der Physiologie des Menschen, Bd. i, p. 601.

fluids found in them after death. The fluid of the cerebral ventricles, for example, though having no characters of a true secretion, is peculiar; and, unlike the fluids of other serous membranes, is not tinged when the serum is colored by madder, and is very rarely discoloured in jaundice. It appears to be a fluid more finely filtered from the liquor sanguinis. Again, under the increased pressure from congestion, whether passive or active, the filtration of the fluid through the blood-vessels and serous membranes will be less fine: hence the general occurrence of soft jelly-like masses and thin strings of fibrine in the fluid effused in ascites from extreme obstruction of the circulation: hence, also, a probable explanation of what Bishoff* has remarked, that the abdominal cavity in rabbits and bitches at the time of heat often contains some pellucid fluid which almost all coagulates when left at rest. These facts also coincide with the observations of Mr. Robinson† on the effects of obstruction of the renal circulation in producing effusion of parts of the blood into the urinary tubules: and with some of his recent illustrations‡ of the general effects of the increased lateral pressure of the blood on the walls of the minute vessels.]§

Cuticles,‖ *Mucus, &c.* Reichert,¶ from his own and Bidder's observations, denies that the epithelium is ever so shed from the digestive canal, in or after any act of digestion, as to leave any portion of the subjacent mucous membrane uncovered or *raw*. When it has appeared so, the epithelium which remained has probably been washed off after death. In connexion with the same subject are some experiments by Oesterlen** which have proved the influence of the layer of mucus which lines the digestive canal, in retarding both the imbibition of fluids inclosed within the canal and their permeation by endosmosis. The passage of fluid into or through the mucous membrane of the intestines was, in many cases, more than twice as rapid when the mucus had been removed as while it was yet adherent.

Mr. Queckett†† has detected a double movement of the ciliæ on the gill-rays of the mussel. Besides their commonly observed curved or lashing movement in a vertical plane, each row presents a slight movement of the ciliæ on themselves, each cilia turning on its own axis through the space of a quarter of a circle, with a movement like that of the *feathering* of an oar in rowing. It is almost certain that without a movement of this kind, it would not be possible for any ciliæ to propel fluid in any determinate direction, or to propel, as they do, separate particles, such as epithelium-cells, to which they are attached.

Against the opinion of the transformation of young epithelium-cells into pus-corpuscles in inflammations, Dr. Buhleman‡‡ states that in coryza and bronchial catarrh, the discharge of very numerous exudation-corpuscles takes place without ever being preceded by the separation of an unusual quantity of epithelium. Neither can there be found, in the first stages of these diseases, any of those bodies which are supposed to be intermediate between epithelium-cells and pus-corpuscles; nothing more than fully-developed epithelium-cells are to be found scattered in the abundant quantity of exudation-corpuscles; and these latter, as the disease becomes chronic, without any important change in the quantity or character of the cast-off epithelium, gradually passing through the intermediate stage of *mucus corpuscles*, assume the characters of pus-corpuscles.

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* Muller's Archiv, 1844, Jahresbericht, p. 120.

† Medico-Chirurgical Trans. vol. xxvi, p. 51. See also the similar observations of Dr. H. Mayer, on the effects of Congestion of the vessels of the Digestive Canal, in Schmidt's Jahrbucher, Mai 1844. ‡ Medical Gazette, June 28, 1844.

§ The observations included in brackets and most of those in the notes are the author's. See, further, Krause's account of Epidermis, with that of the skin.

‖ See, further, Krause's account of Epidermis, with that of the skin.

¶ Muller's Archiv, 1844, Jahresbericht, p. 121.

** Beitrage zur Physiologie des gesunden und kranken Organismus, Jena, 1843, p. 241, e. s. †† Medical Gazette, May 3 1844.

‡‡ Beit. zur Kennt. der kr. Schleimhaut des Respirationsorgane; Bern 1843, p. 40.

*Bones.**—Dr. Daubeny† has established the truth of the much-doubted‡ opinion of Morichini and Berzelius, that fluorine, in the form of fluoride of calcium, is contained in recent as well as fossil bones and teeth. It appears to exist in recent bones in about a quarter of the proportion in which it exists in fossil bones; but the proportions in different specimens of both kinds are variable. The professor ascribes the failure of those who have not detected the fluorine, except in fossil bones and teeth, to the tenacity with which it is retained by animal matter, and to its being carried off with the carbonic acid evolved at the same time too rapidly to act upon the glass submitted to it. He therefore, before submitting the bones to the action of strong sulphuric acid, burns away all the animal matters, removes the carbonic acid by dissolving them in hydrochloric acid, then throws down the earthy phosphates by caustic ammonia, and dries them.

The experiments have also been fully confirmed and extended by Mr. Middleton,§ who has also detected fluorine in several kinds of deposits from water, and ascribes its comparative abundance in fossil bones (as MM. Gerardin and Preisser did‡) to the filtration of water impregnated with it.

Muscles, structure of. Dr. F. R. Will ‖ maintains that the tranverse striæ of animal muscular fibres are due to the fibrils (which, in their natural relaxed state, are, he believes, uniform and cylindrical,) being thrown, in contraction, into undulations or zig-zag flexures, His arguments for the natural cylindrical form of the filaments are, chiefly, the usual straightness of the longitudinal lines between the fibrils whether contracted or relaxed; the occasional appearance of unmarked fibrils, protruding from the end of a torn fibre; and the frequency of uniform cylindrical filaments in muscle which has been long macerated. And his arguments for the zig-zag condition, or the undulation, to which he ascribes the appearance of transverse striæ, more or less contracted on the fibrils, are the following:

1. When the primitive fibres of the muscles recently taken from dying insects, contract under water, the transverse striæ, which at first were wide apart, are approximated; and in every such contraction, the clear as well as the dark tranverse lines became narrower, the elevations at the margin of the fibre become more prominent, and the constrictions between them become deeper and narrower. This could not happen if the contraction depended on the formation and widening with flattening of beads, or varicose enlargements of the filaments: for, in this case, the shadows (dark transverse lines) thrown by the beads or enlarged parts, should become broader and more intense as the beads become larger; whereas, in increasing contraction they become more intense and narrower, as the zig-zag angles become more acute.

2. The longitudinal striæ separating primitive filaments, remain straight even when the filaments are most contracted; whereas if the filaments become beaded, these lines should indicate that form, in the same manner as it is seen at the edge of the fibre where side views of the zig-zag filaments are obtained.

3. In repeated observations of the muscles of crabs, some of which were partially dried, and others macerated in water, it was found that, in the former, the individual filaments which projected from the torn ends of fibres were, at their extremities, bordered by two perfectly straight lines, which, as they were traced down towards the mass of the fibre, became slightly undulated, then zig-zag in obtuse angles, and then in more and more acute angles, till they could not be recognized as continuous lines. In the macerated muscles, on the contrary, there was scarcely any trace of transverse striæ, and the filaments appeared in almost every part bounded by straight lines.

4. The appearance of the transverse striæ, and of rows of spheres may be imitated by thin cylinders of white wax, or a similar substance, moulded in undulations or zig-zag flexures, and examined through a tube as they are held

* On the development of Bone, see the Section on Development.
† Philosoph. Magazine, Aug. 1844. ‡ See last Report.
§ Philosoph. Mag. July and Oct. 1844. ‖ Muller's Arch 1843, Heft iv.

against the light, the angles next the observer appearing darker and breader than those next the light; and the same appearance is produced by the coarsely-undulated surface of many muscles and tendinous tissues.

[This last argument is of little weight; and the facts stated in the first and second are just as well explained by supposing, not that the fibrils present a series of varicose bead-like enlargements, but that they are formed by a series of discs, the lines of union of which make the dark transverse striæ; or, as Mr. Erasmus Wilson* has better expressed it, by a linear series of minute cells, flattened at their apposed surfaces, and so compressed longitudinally as to leave no indentation on the surface, thus constituting a uniform cylinder divided by transverse septa, which are formed by the adherent surfaces of contiguous cells. Will's third argument deserves attention; but as the observations were made on dried and macerated fibrils, no conclusion can be safely drawn till they are confirmed by observations on muscles in the natural state.]

Valentin,† who has long described the relaxed muscular fibril as a uniform cylinder, confirms, generally, Will's account, though he cannot determine whether the striated appearance of the fibrils is due to their becoming varicose, or to zig-zag flexures, induced by contraction. He also still holds to his belief‡ that the fibres and fasiculi in the fully-contracted state are inflected in zig-zag lines, with angles of from 80° to 120°. He mentions, for demonstration of this opinion, his observations on the exposed laryngeal muscles of the frog. His account is almost precisely like that of Hales,§ by whom the zig-zag arrangement of fibres was first observed in the abdominal muscles of the frog; and in further evidence of this arrangement being assumed in contractions, he adds, that as often as it is seen (and it may take place seventy-four times in a minute) the small arteries and nerves between the fibres become tortuous (through the approximation of their extremities), and straighten themselves when the contraction ceases.

Muscles. Force of contraction. Valentin‖ has also described a very ingenious *myodynamometer* for testing the force of contraction in the muscles of frogs and small animals; the force being estimated, not as in Schwann's experiments by weights in a scale, but by the tension of a bow-spring. From numerons experiments with it he has deduced (besides confirmations of the results of Schwann,) 1st. That when, after death, all the irritability has ceased, the muscular fibres tear with a far less weight, that they were previously able, when galvanized to draw. 2. That by too frequent and rapid irritation the irritability of the muscles is so exhausted, that it is for a time reduced to zero; but that it collects again, though, to a less degree, the longer the animal has been dead, and the oftener its irritability has been exhausted. 3. That by repeated equal irritations, the strength of the muscles (in beheaded frogs) decreases in a regular and corresponding ratio, losing the same amount in each successive period of time. 4. That after tying the femoral artery or vein, or dividing the sciatic nerve (in frogs) the full strength of the muscles remains unaltered for several (in one case as many as twelve) days.

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* Report of the Royal Society, Philos. Magazine, Aug. 1844. What I have quoted from the description expresses almost exactly what has appeared to me to be the structure of the muscular fibril, especially in the large fibres of the eel; except that I doubt greatly whether the component portions of the fibril can be trnly called CELLS; they have, rather, the appearance of solid transparent particles. Neither have I ever seen any thing to indicate a varying density of the contents in each successive set of four cells such as Mr. Wilson describes; perhaps this appearance was due to the fibres or fibrils being thrown into the coarse zigzag flexures, described by some as produced in full contraction of the muscle.

† Lehrbuch der Physiologie des Menschen, Bd. ii, p. 33.

‡ See Report for 1842, p 30. § Statical Essays, vol. ii, p. 59.

‖ Physiologie, Bd. ii, pp. 176-92. See, in the section on nerves, the application of similar experiments by Matteucci to determine the force exerted by nerves in exciting muscular contraction.

Rigor mortis. Dr. Gierlichs,* of Bonn, has confirmed what I stated in the last Report (p. 6), respecting the riger mortis of the heart, and the general rule of the rigor mortis setting in as soon as a muscle has ceased to be irritable by stimuli. In frogs, in which the rigor is often not established till three or four days after apparent death, the hind-legs do not become rigid till from six to twelve hours later than the fore-legs; and they were often irritable to galvanism when the fore-legs were already quite rigid. Various means which exhaust muscular irritability, such as poisoning by strychnia or hydrocyanic acid, accelerate the accession of the rigidity. After injections of potash into the blood, the rigor takes place with unusual rapidity, and is very marked. This affords an additional evidence against the notion, of rigidity being dependent on coagulation of the blood or effused liquor sanguinis, for the alkali would retard that process; and a corroborative fact is, that if blood be repeatedly drawn from a dog, and each portion that is abstracted be injected again after the removal of its fibrine, the rigor mortis takes place as usual. No destruction of nerves appears to affect it, unless the muscles have been so long paralytic that their nutrition has been affected. When one of the crural arteries of a frog was tied, the rigor mortis ensued several hours later in the corresponding limb than in the other. Its occurrence bears no apparent relation to the loss of heat.

Valentin,† also, considers that the rigor mortis may be assumed to affect the involuntary muscles, and relates an experiment to prove its occurrence in the digestive canal. If a portion of intestine from an animal just slain be tied close at one end, and at the other be tied round a graduated tube, and be filled with water, some of which also rises into the tube, it will, after some hours, slowly contract, and, pressing the water from its cavity, will elevate considerably that which is in the tube.

CIRCULATION.

Anatomy and Physiology of the Heart. Kirschner's article in the last published part of Wagner's Handworterbuch‡ contains all the results of his latest investigation of the actions and anatomy of the heart. The parts which are of most interest from their novelty are briefly as follows: 1. The contraction of an auricle begins at the entrance of the great veins and thence proceeds to the base of the auricle. This is evident, if the auricle, while acting be looked at *from the side.* 2. The contraction of a ventricle begins simultaneously in every part, the whole cavity draws up uniformly to the origin of the artery: this has hitherto been overlooked because such large hearts have been usually examined, that it was impossible to take in the whole ventricle at a glance; but it is evident when hearts as small as a rabbit's are examined while acting slowly. 3. The auricles never completely empty themselves in their contraction.§ 4. If fluid be injected into either side of the heart through any of the great veins, (the other veins being tied and the apex of the heart suspended loosely in its natural position), the apex is always moved backwards as the cavities become fuller. 5. If the injection be made though one of the left pulmonary veins, the apex of the heart, at the same time that it is moved backwards, rotates from left to right; if

* Medic-Correspondenz blatt Rheinischer Aerzte, 1843, and Schmidt's Jahrbucher, Mai 1844. † Physiologie, Bd. ii, p. 86.

‡ A part of the contents of this article [Herzthatigkeit] was published before the last year, but not in a connected form, and they are scarcely at all known in England. The description of the valves and their cords is singularly accurate, though, if I may so speak, too diagram like; and the arrangements are so nearly constant, that they must be of prime importance in securing the due action of the valves. The article contains, also, a good account of all the points discussed concerning the heart's action; and an ingenious theory of it by Kurschner, which is omitted here only because there is not space even for all the facts that should be noticed

§ [The ventricles probably do empty themselves. If a heart, in which the contraction of the rigor mortis is very marked, be divided transversely, it will often be found that the opposite inner walls of the left ventricles are in contact, and its cavity completely obliterated; indeed, the form and position of the fleshy columns appear to be specially arranged for this end.]

the injection be made through any of the other veins, the rotation is from right to left. 6. When after filling the cavities, the fluid is drawn back by raising the piston, the heart returns to its previous position, being made to pass through exactly contrary movements, apparently by the elasticity of the tissues disturbed in the previous distension. 7. It is probable, therefore, that the alternate filling and emptying of the heart's cavities during life, which these injections are intended to imitate, contributes materially to its natural movements of receding from the wall of the chest and rotating its apex from right to left in the dilatation of the ventricles, and of tilting forwards and rotating its apex from left to right in their contraction. 8. In the auriculo-ventricular valves [to give only such a general description as may apply to both] one may observe besides their two or three chief divisions or lobes, the arrangement of which is always the same, as many smaller and less regular *intermediate lobes* connecting the adjacent edges of the larger ones. Each chief lobe may be divided into a thicker middle or *nucleus-portion*, and a surrounding thinner and dentated *marginal-portion*. 9. The number of fleshy columns projecting from the ventricular wall is always equal to the number of chief divisions of the valve. The tendinous cords attached to the outer surface of each chief division are always derived from two fleshy columns (or in the right ventricle from one column and the septum): the cords attached to each intermediate division or lobe of a valve are all derived from one column. Consequently on each column there are, usually, three groups of tendinous cords, of which the two outer groups belong to the halves of two adjacent chief divisions of the valve, and the middle one to a smaller intermediate division. 10. The tendinous cords may be divided into three classes: namely, *tendons of the first order*, of which two or four, of considerable size, and from different columns, go to the attached margin of each division of the valve, and are there fixed to the muscular tissue of the ventricle; *tendons of the second order*, which are of smaller size, and of which two or three are given from each column (directly or through the medium of one of the first order) to each chief division. They are attached to the outer (or back) surface of the valve in such position that their points of attachment lie in straight lines, which run parallel or slightly converging from the attachments of the tendons of the first order towards the apex of their division of the valve. Both these kinds of tendons may be seen on looking at the back of the valves: but if any division of a valve be spread out, then much finer *tendons of the third order* come into view, which are given off from the preceding, and are attached to the thin and looser free marginal portions of the division. Their points of attachment to the back of the valve usually lie in straight lines drawn from the tendons of the second order, which give them off, to the very edge of the valve; and their number depends on the breadth of the free marginal portion. Where two divisions of a valve meet at their attached margins, a tendon of the first order is always fixed at their point of union, and always gives off tendons of the third order to the adjacent margin of each division; so that when the ventricle contracts the two adjacent divisions must be brought together by the influence of the one set of tendons with which they are both connected. 11. The valves are composed (besides their proper tissue and the endocardium) of the continuation of the tendinous cords which usually spread out like palm-leaves and are interwoven, and of muscular fibres, of which a certain number may be traced, (especially after several days soaking' in cold water), passing from the adjacent wall of the auricle into the interior of each division of the valves, and connecting themselves with the ends of the tendons of the second order in the central portion of the valve. 12. These muscles may be supposed to have the office of keeping the valves tense when, in the contraction of the ventricles, the auriculo-ventricular rings are reduced in size, and the fleshy columns are gradually brought nearer and nearer to each other: but neither they nor the columns can have any share in raising the valves to close the orifice; this must be effected by the blood. When the ventricle contracts the columns contracting with it fix, through the tendons of the first order, the attached margins of the valves;

then the blood pressing on their outer surface unfolds them and spreads them before the orifice, into the form of a cone which gradually elongates and becomes narrow, and is flattened as the ventricle empties itself. 13. The purpose of the complicated but regular arrangement of the cords must be to secure the strength of every part of the valve when pressed by the blood. Probably, when unfolded, the dentated edges and the marginal portions are distended and fit into each other. The size also of the valves which appears to be more than enough to close the orifice, reduced as they are in the contracted state of the ventricles, makes it probable that they are not completely unfolded at once in the contraction of the ventricles, but that as the size and position of the ventricle and the size of the orifice change, so different portions are successively unfolded and brought to resist the varying pressure of the blood.

Valentin* has supplied, by laborious investigations, numerous valuable data for the study of the circulation, and has also confirmed most of those already established by Poiseuille and others. Among his deductions are, 1, that in health the quantity of muscle in the right ventricle of man, many mammalia and birds, is equal to one-half of that in the left; 2, that the quantity in the right auricle is about equal to 2-3ds of that in the left; 3, that the muscular power of the right auricle is to that of the left as the square root of the muscular power of the right ventricle to the square root of that of the left; 4, that, in man and the higher mammalia, the absolute force exerted by the left ventricle is equal 1-50th of the weight of the body; by the right venticle equal to 1-100th of the same; 5, that the average quantity of blood discharged by the left ventricle is five ounces [an estimate, for which the examinations are insufficient to establish it as certain, but which agrees much better than the old one of two ounces with the time in which the blood makes the round of the circulation; for according to this estimate, the blood may all pass through the heart in from 41 3-4ths to 62 2-3ds seconds.] 6. That a pressure equal to that of a column of mercury from 1-12th to 1-3d of an inch is sufficient for the effectual closure of any of the valves; so that even the weakest action of the heart is, probably, enough for it, provided they be healthy.

To prove that the impulse of the heart depends [but it can be only chiefly dependent] on the contraction of the ventricular fibres, Valentin† cut off the apex of the heart, in several cases, so that the resistance of the blood and the great vessels and the supposed consequent recoil, were prevented. Yet the tilting movement was observed, as much as when the heart was entire. In evidence, also, that the first sound is due to the tension of the auriculo-ventricular valves, he says, that if a portion of a horse's intestine be tied at one end, be moderately filled with water, *without any admixture of air*, and have a syringe containing water fitted to the other end, the first sound of the heart is exactly imitated by forcing more water in. It may be distinctly heard with the stethescope applied near the tied end of the intestine, at the instant of the water making it tense.

Arterial circulation. Some measurements of arteries for the comparison of the respective areas of trunks and their branches, by Dr. E. Hazard, have been published by Dr. Horner.‡ The mode of measurement and other circumstances are not stated; but the results are, on the whole, confirmatory of those which I obtained,§ namely, that generally the joint area of branches is rather greater than the area of their trunk.

Dr. Spengler,|| of Etville, has been making experiments on the force of the current of arterial blood, and its variations in the several acts of the heart and respiration, the results of which give strong confirmation of nearly all those obtained by Poiseuille and others. He has used a new apparatus which is, perhaps, less liable to error than the hæmadynamometer, and which can be so adapted to an artery that the amount of lateral pressure exercised by the blood

* Lehrbuch der Physiologie, Bd. i, pp. 415-30-36-43, &c.
† Lehrbuch der Physiologie, Bd. i, p. 427, &c.
‡ Special Anatomy and Histology, vol. ii, p. 172; Philadelphia, 1843.
§ Med. Gaz. July 8, 1842. || Muller's Archiv. 1844, Heft 1.

upon its walls may be ascertained without obstructing the current. He has thus obtained the first experimental evidence of what Dr. T. Young and Weber calculated, and the experiments of Poiseuille made almost certain, namely, that the pressure which the current of blood exercises at any part of the arterial system is equal in all directions.

Valentin* has also abundantly confirmed the results of the same experiments; and, from numerous micrometric measurements of the diameters of several arteries and of the thickness of their walls, he concludes, that the thicknesses of the walls of any two systemic arteries are in direct proportion to the square roots of the absolute (*hydrodynamic*) forces under which the blood flows in them;† and that the thickness of the wall of the pulmonary artery is to that of the aorta, as the square root of the force of the right ventricle is to that of the force of the left; i. e., as already stated (p. 261) as $\sqrt{1} : \sqrt{2}$.

Capillary circulation. Valentin§ has estimated the velocity of the capillary circulation in many careful microscopic examinations of frogs' feet during breeding time, and has found the average to be between 1-5th and 1-3d of a Paris line per second; or from ·938 to 1·4 English inch per minute. In the small veins it is about 1-8th faster. [These results agree nearly with those of Hales, who stated the velocity at an inch in a minute and a half: and more nearly still with those of E. H. Weber, who found it 1¼ inch in the minute.]

Venous circulation. Valentin§ has also, by an apparatus like that used by Poiseuille for estimating the dilatation of the arteries in their pulsation, determined the amounts of the dilatation of the veins near the chest in the act of expiration, and of their contraction in inspiration. In the external jugular vein of the dog the average dilatation is equal to 1-12th of the circumference of the vein; the enlargement of the portion of vein (an inch and a quarter in length) which was enclosed in the apparatus, was between 1-10th and 1-11th of its previous dimensions.

Circulation in the lungs. Mr. Erichsen,|| in an essay to prove that the real cause of death after the sudden introduction of air into the veins, is the difficulty of the passage of frothy blood through the pulmonary capillaries, relates this experiment; a pressure equal to that of from one and a half to two inches of mercury is sufficient to drive bullock's blood, deprived of fibrine, through the capillaries of the lungs of a dog recently killed; but if the air be previously blown into the pulmonary artery it will require a pressure equal to that of from three to three and a half inches of mercury to force similar blood through the the same set of vessels. The pulmonary circulation being thus arrested, the left ventricle receives an insufficient supply of blood, and respiration ceases in consequence of the defective quantity of arterial blood sent to the nervous centres. But, for some time after respiration has ceased and animal life has nearly ceased, the heart continues to act regularly and forcibly; nor do its right cavities ever become so distended as they are in ordinary asphyxia, unless the air have been forcibly blown into the veins.

RESPIRATION.

Respiratory movements. In a fourth memoir MM. Beau and Maissiat¶ have completed their account of the respiratory movements.

That which is chiefly interesting (if not accurate) in this as in their former memoirs, is the account of the action of the muscles. They, first, rigidly repre-

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* Lehrbuch p. 456.

† [If this be true, it will follow, since the hydrodynamic forces in different arteries are directly proportionate to the areas of sections of those arteries, or, to the squares of their diameters, that the thicknesses of the walls will be directly as the diameters; but this appears improbable, although the few measurements made may show that the rule is true for the several parts of the aorta and the innominata.]

‡ Physiologie, i, p. 468. § Ib. p. 501.

|| Edinb. Med. and Surg. Journal, Jan. 1844.

¶ Archives Gen. de Medecine, Nov. 1843. See last Report.

sent the oblique and transverse muscles of the abdomen acting together in complex respiration, as the expiratory muscles of the abdomen; like the triangularis sterni and intercostals which they regard as the corresponding expiratory muscles of the chest. In the costo-inferior type of respiration, they act by depressing the ribs; in the costo-superior, by drawing in the abdominal walls, and pressing the abdominal viscera against the diaphragm; and in very deep and forcible complex expirations in either type, they act in both these ways. The authors further represent the *recti abdominis* as flexors of the thorax on the pelvis; *not* expiratory muscles; and they add that the fibrous transverse septa serve as bonds for the muscular fibres, which, but for them, might often be separated by the eccentric pressure of the abdominal viscera in expiration. The septa are, besides, supposed to be stretched in deep inferior costal inspirations; and to act by their elastic recoil with other elastic parts in the following expiration; [the recti abdominis may have no part in complex expirations in dogs, in whom the authors examined them, and found them tranquil during cries; for, in dogs, they commonly pass straight from the thorax to the pelvis; but in men, in whom these muscles are usually arched forwards, they may be felt contracting in all forcible complex acts of expiration. By their contractions they probably serve in two ways: 1, by straightening themselves so as to reduce the size of the chest and press up the diaphragm through the medium of the abdominal viscera; and 2, by acting, by means of their transverse intersections, upon their own sheaths which they make tense and fitter to afford fixed points for the action of the oblique and transverse muscles. There can hardly be such an elastic contraction of these bands as is assumed; their tissue is not one which would recoil quickly after being stretched, nor is there any of the inspiratory forces (especially among those admitted by the authors) which would be capable of stretching it, in order to its recoiling.] The *sacro spinalis*, *longissimus dorsi*, and *transversalis colli* are described as extensors of the trunk; not expiratory. The *quatratus lumborum* as the continuation of the infra costales; a lateral flexor of the spine; probably not expiratory.

At the end of their memoir, the authors enumerate the several muscles they have described, according to their functions, as follows: [and I have added signs of doubt when their conclusions appear improbable]. *Non-respiratory*—Sacro-spinalis, rectus abdominus (?), serratus posticus superior (?), quadratus lumborum. *Doubtful*—Levatores costarum [inspiratory?], serratus posticus inferior [expiratory]. *Inspiratory: directly* and *ordinarily*—Diaphragm and scaleni, the one or the others predominating according to the type of the respiration: *direct* and *extraordinary*—sterno-mastoid, pectoralis minor, lower fourth of the pectoralis major, serratus magnus: *indirect*—trapezius and levator anguli scapulæ. *Expiratory direct and ordinary*—in forcible expiration, intercostals (?). infracosteles, triangularis sterni, latissimus dorsi, transverse and oblique abdominal, pyramidalis: *direct and extraordinary*—upper three-fourths of the pectoralis major: *indirect*—trapezius, sphincter ani, levator ani, coccygeus.

Capacity of respiration. Many very interesting and practically important results have been obtained by Mr. Hutchinson,* with his *spirometer*, an instrument by which the capacity of respiration is measured by the quantity of air expired in a full and forcible expiration. Among these the chief is the fact of the existence of an intimate relation between this capacity and the height of the individual examined. In 1088 healthy men from five to more than six feet in height, he found the capacities of respiration as follows: in men of 5 feet, 135 cubic inches; of 5 ft. 1 in., 177 c. i.; of 5 ft. 2 in., 173 c. in.; ft. 5 ft. 3 in.; 184 c. i.; of 5 ft. 4 in., 193 c. i.; of 5 ft. 5 in., 208 c. i.; of 5 ft. 6 in., 204 c. i.; of 5 ft. 7 in., 224 c. i.; of 5 ft. 8 in., 220 c. i.; of 5 ft. 9 in. 229 c. i.; of 5 ft. 10 in., 246 c. i.; of 5 ft. 11 in., 254 c. i.; of 6 ft. 255 c. i.; of upwards of 6 ft. 260 c. i. These numbers are such that it may be generally stated that for every additional inch of height from 5 to 6 feet, eight additional cubic inches of air, at 60°, are

* Lancet, July 27 and Aug. 3, 1844.

given out by a forced expiration. And the results of the examinations are so nearly uniform that disease may be suspected in any man who cannot blow out nearly so many cubic inches as the average of those of the same height, even when, by external measurement, his chest appears to be of full size. Indeed, in general, the size of the chest affords no good indication of the capacity of expiration. The only exceptions among healthy men to the general rule of the direct proportion between the height of the body and the capacity, are in the cases of fat men whose capacity is always low.*

In the ordinary respiration of men, from seventeen to thirty-three years old,† Valentin has calculated from the watery vapour contained in the saturated expired air, that the average quantity of air expired in a minute is 400 cubic inches; the extremes under varying circumstances, being 234 and 686 cubic inches; and the average quantity in one ordinary expiration 31·1 cubic inches, the extremes, in very tranquil and somewhat hurried respiration, being 11·4 and 74 cubic inches. [Mr. Coathupe's estimate of 20 to 25 cubic inches is probably better, for it was drawn from the results of respiration continued during a longer period and with less restraint than those of Valentin's].

Force of respiratory movements. From another set of experiments with an instrument something like a hæmadynamometer with a mouth-piece, Valentin‡ deduces that the force exerted in tranquil inspiration and expiration is equal to the pressure of a column of mercury from ·13748 to ·3937 of an inch high; and the force of the same acts when violent is equal to the pressure of a column from ·7874 to 1·5748 high.

Mr. Hutchinson§ finds that the full expiratory force of a healthy man is generally about 1-3rd greater than his inspiratory force. Taking the general rule, among 1200 persons, of various classes, the inspiratory force increases pretty regularly from those of 5 feet high to those of 5 feet 9i n., and then decreases. If the power of the respiratory muscles be a fair measure of the power of the whole muscular system, the men of this latter height might, therefore, be regarded as the strongest. But in four classes of men picked for active service, Mr. H. found the respiratory force greatest in those of 5 feet 7 in.||

Changes of the air in respiration. The whole of this subject has been very carefully examined by Valentin and Brunner,¶ operating on large quantities of quietly respired air: and their results are probably more nearly deserving of implicit confidence than any hitherto published. They are, briefly, as follows: 1. The expired air has always, (even in widely varying external temperatures), a temperature of from 97·25° to 99·5° F.; most frequently the latter temperature. 2. It is always saturated with watery vapour. The quantity of vapour exhaled from the blood in the air passages may therefore be estimated by subtracting the quantity contained in the atmospheric air inspired, from the quantity which (at the same barometric pressure) would saturate the same atmospheric air at the temperature of 99·5°. And, on the other hand, if the quantity of watery vapour in the expired air be estimated, the quantity of the air itself may from it be accurately determined, being as much as that quantity of watery vapour would saturate at the ascertained temperature and barometric pressure. 3. The chemical changes are due to the simple diffusion of gases taking place between those of the atmosphere and those of the blood. The nitrogen is unchanged. The volumes of oxygen absorbed and of carbonic acid exhaled from the blood are determined by the established laws of the diffusion of gases, so that for 1 volume

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* It was this observation (which has also been made by M. Bourgery, see last Report, p. 11,) that thin men have the greatest capacity of respiration. which first led Mr. Hutchinson to the discovery of his law. † Lehrbuch, p. 452.

‡ Ib. Bd. i, p. 525. § L. c.

|| The rules established in these valuable papers are especially applicable to the examination of men for military or other active services. They contain numerous other facts of considerable importance in practice, which, however, can hardly find a place in a report of this kind. ¶ Lehrbuch, Bd. i, p. 457, e. s.

of carbonic acid exhaled, 1·17421 volume of oxygen are absorbed; or by weight for one part of carbonic acid, 0·85163 of oxygen. Now one part by weight of carbonic acid contains 0·72727 of oxygen;* therefore, for each part of carbonic acid which is discharged in respiration there is an excess of 0·12436 of oxygen which enters the blood and is disposed of otherwise than in forming the carbonic acid excreted from the lungs; or, by volumes, for each one of carbonic acid, an excess of 0·17421 of oxygen. Hence, if it be known how much carbonic acid a man has exhaled from the lungs in a given time, we may reckon how much oxygen he has in the same time absorbed. Valentin and Brunner have determined that, in a medium of temperature and atmospheric pressure, they each on an average of six experiments, breathed 562·929 litres of air in the hour, and each in the same time expired 635·8565 grains of carbonic acid containing 173·414 grains of carbon. From this and from their respective diffusion-volumes the hourly consumption of oxygen may be calculated at 541·5 grains; and these results agree very nearly with those obtained by Andral and Gavarret.† They show that in each hour 69·0575 grs. i.e. 541·5—(635·85—173·414) of oxygen are absorbed which are not employed in forming the carbonic acid of the expired air.‡

But, notwithstanding these exhalations of carbonic acid, M. Boussingault,§ from 142 analyses of large quantities of the air of Paris, has confirmed the conclusion already generally received, that the quantity of carbonic acid contained in the air of large towns, is not above the average. The average quantity which he found was 3·97 volumes in 10,000. From the quantity of combustibles consumed, in food, fuel, and lighting, in Paris, he estimates the daily produce of carbonic acid at 115,932,871 cubic inches; and the speedy diffusion of such a quantity is not surprising, when it is added that the surface of the ground "within the walls" of Paris measures 1354,229,533 square inches; so that if the whole of the carbonic acid produced in twenty-four hours, were produced in an instant, it would form a layer on the surface less than an inch in thickness.

To this it may be added, on the authority of Mulder,|| that the results similar to the above, which were obtained by De Saussure, as to the quantity of carbonic acid contained in the atmosphere at various times and places, have been confirmed by Verver, whose experiments, like those of Boussingault, were performed by means of Brunner's aspirator. He says also that he has instituted examinations to determine the quantity of ammonia in the atmosphere, and that it is so extremely small, that it is not possible that plants should derive their nitrogen from it; it is not more important, as a constituent of the atmosphere in its relations to the organic kingdom than many other of the innumerable substances that are exhaled into the air, and brought down again with the rain.¶

* These numbers represent the proportionate diffusion-volumes of the two gases, calculated according to the law of their being inversely as the square roots of their specific gravities The results of the experiments were so nearly the same that the differences may be safely referred to errors of analysis † See last Report, p. 11.

‡ The recent observations of M. Gay Lussac (Annales de Chimie et de Physique, Mai, 1844) directed against the experiments of Magnus, only prove inconsistencies in the results of those experiments. It was always sufficiently evident that the quantities of gases supposed by Magnus to exist in the blood could not be right; but nothing has disproved either the fact that such gases do exist therein, or the theory that the carbonic acid is formed in the systemic circulation; and if further evidence were necessary, it is abundantly furnished by the results of Valentin and Brunner's experiments, which show that the proportions of carbonic acid and oxygen that are interchanged are determined, not by their chemical equivalents, but by their diffusion-volumes. It is hardly possible that this should happen unless the carbonic acid were already formed and dissolved in the blood when it arrives at the pulmonary capillaries.

§ Ann. de Chimie et de Physique, Mars, 1844, t. 85.

|| Physiol. Scheikunde, pp. 113, 160.

¶ Mulder considers that the real source of the nitrogen of plants is in the ammonia formed by the combination of the nitrogen of the moist atmospheric air, contained in the porous earth, with the hydrogen given off from the decaying organic compounds. By a similar process, ammonia is formed by the decomposition of water and atmospheric air in all porous bodies, provided they are moist and exposed to the air at a certain temperature: a similar production of ammonia from water and atmospheric air is a part of the process by which nitre is formed in many natural nitre-caves.

But if this be true, there must be differences in the quantity of ammonia in the atmosphere of different places, which is very improbable. For Dr. John Davy* has found traces of its presence in several samples of rain-water collected at Ambleside; [and in many fogs in England, it may be detected by the action of the air on slightly reddened and moistened litmus paper. In London fogs it is evident to the nose and eyes; and, as in these, so about Ambleside, it is probably brought down with soot, for Dr. Davy describes a layer of carbonaceous matter, like particles of soot, often covering wide extents of the surfaces of the Westmoreland lakes. Such layers are abundant enough on the surface of water in London; after a night of still frost, great quantities of soot might be swept from the surface of the ice. May we not suppose that the minutely divided carbon which floats in the atmosphere, is constantly disinfecting it by absorbing not ammonia alone, but many other gases, and holding them in its pores till it falls, or is washed down by rain, and yields them to be decomposed by plants?]

It would be beyond the limits of this Report, to enter far on the subject of the purification of the atmosphere from the changes produced by respiration. It must suffice to refer to the papers of Prof. Draper†, Dr. Gardner,‡ and Mr. Hunt.§ (especially to that of the first,) on the decomposition of carbonic acid by plants, under the influence of those rays alone of light, which occupy the most luminous part of the spectrum, the orange, *yellow*, and green rays; and to the observations of Wohler|| and Morren,¶ on the removal of carbonic acid, and production of oxygen, by certain of the minutest and most abundant infusoria.

ANIMAL HEAT.

An extensive series of observations has been made by M. Roger** on the temperature, of children in health and various diseases.

In nine examinations of infants from one to twenty minutes after birth, the temperature (observed in these and in all the other cases, in the axilla,) was from 99·95 to 95·45. Immediately after birth the temperature was at the highest; but it quickly fell to near the lowest of those above stated; but by the next day, it was again completely or nearly what it was before. The rapidity of the pulse and of respiration appeared to have no certain relation to the temperature.

In thirty-three infants of from one to seven days old, the most frequent temperature was 98·6; the average was 98·75; the maximum (in one case only,) 102·2; the minimum (also observed only once,) 96°·8. All the infants were healthy. The frequency of respiration had no evident or constant relation to the temperature. A few of the infants were of a weakly habit; their average was 97·7, the others were strong, and their average temperature was 99°·534. The age of the infant (in his short period,) had no influence on its temperature; neither had its sex, nor its state of sleep or waking, nor the period after suckling.

In twenty-four children, chiefly boys, from four months to fourteen years old, the most frequent temperature was above 98°·6; the average was 98°·978; the minimum was 99°·15; the maximum 99°·95. The average temperature of those six years old or under, was 98°·798; of those above six years old, 99°·158. The average number of pulsations in the minute was in those under six years old 102; in those above that age 77; yet the temperature of the latter was higher than that of the former, or of younger infants. There was no evident relation between the temperature and the frequency of respiration; nor in a few examinations, was the temperature affected in a regular way, by active exercise for a short time, or by the stage of digestion,

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* Edinburgh New Philos. Journal, July, 1844.

† Philosophical Magazine, Sept. 1843, and many subsequent parts.

‡ American Journal of Science and Arts, Jan. 1844; and Edinb. New Philosoph. Journal, July, 1844.

§ Philosoph. Magazine, 1843-4, various parts.

|| Annalen der Chemie und Pharmacie, Bd. xlv, p. 206; and Schmidt's Jahrbuch, Oct. 1843, Bd. xl.

¶ In Mulder, Physiol. Scheikunde, p. 117; and Ann. de Chim. et de Phys,, Sept. 1844.

** Arch. Gen. de Medecine, Juillet, Aout, &c., 1844.

As already said in all the examinations from which these results were obtained, the thermometer was held in the axilla; comparative examinations proved that the temperature of the axilla, (though lower than that of internal organs,) was higher than that of any other part of the surface of the skin. Of the other parts examined, the warmest was the abdomen, then in succession, the cavity of the mouth, the bend of the arm, the hands, the feet; of which last, the average temperature, in four examinations was only 87°·35. (These results correspond sufficiently with those obtained by Dr. John Davy.)

In diseased states, (to the illustration of which the greater part of the memoir is devoted,) the temperature of the skin in children may descend to 74°·3, and may ascend to 108°·5. Its range of variation is therefore much greater than in adults, in whom M. Andral found it to vary in different diseases not more than from 95° to 107°·6.

Dr. John Davy* also has contributed some miscellaneous observations en animal heat. They were made with a thermometer placed beneath tbe tongue. In eight old men, and women, all, with one exception, between eighty-seven and ninety-five years of age, the temperature was 98° or 98°·5; therefore not below the average of persons in the like circumstances. But, two observations showed, that in exposure to external cold, the temperature was more reduced than in young persons, in one case to 95°, in the other to 96°·5. A few observations were also made on persons working in rooms at a temperature of 92°; in one case the temperature under the tongue was 100°, in another 100°·5; and in a third, with an external temperature of 73°, it was 99°. The same slight variations of the temperature of superficial parts in accordance with changes of external temperature were shown by repeated observations on a healthy man in the different seasons at Constantinople. By moderate exercise the temperature was raised, (but not above the general average,) on the surface of the extremities, and was not affected in the internal parts.

Some very interesting observations on the effects of hot moist air upon the body were made by M. Constantin James,† at the baths or stoves of Nero, called formerly Posidianæ, near Pozzuoli. The physiological part of his account is, that he had first to traverse a passage, leading to the hot springs, about seven feet high, and three wide, in which the temperature in the upper strata of air was 104° F. in the lower 91·4. After going along this for about fifty yards, the passage narrowing and winding, the temperature in the different strata became 109°·4, and 98°·6, and was very inconvenient; his pulse increased from 70° to 90 times in the minute. After a few instants, as he went on, his pulse became 120°, his temporal arteries beat forcibly, his respiration was short and panting, his body bedewed with sweat; he was obliged to stop every instant, and put his head to the ground, where he could breathe the least heated air. The temperature had now become 118°·4 in the upper, and 111°·4 in the lower strata, and the atmosphere was filled with dense vapour. Still descending towards the spring, the atmosphere became still more suffocating; his head felt bursting; he was utterly exhausted, and had nearly lost his consciousness; his pulse could not be counted. The temperature of the spring was 185°, while that of the atmosphere was 122°. Summoning all his strength, the experimenter made his way back through the passage, in which he had been for nearly a quarter of an hour, and of which the whole length was about 120 yards. On coming into the cold air he nearly fainted, and staggered like one drunk, till he was relieved by bleeding at the nose; but through the evening his pulse was 100, he was feverish, had ringing of the ears, and a kind of creeping in the limbs. After a good night's rest he was completely recovered. The water from the spring was clear, and not charged with any gas; neither was any deleterious gas mingled with the heated air.

M. James compares these observations with those made on rabbits and other animals, by M. Magendie. The chief results of these experiments were as

* Philosophical Transactions, 1844, p. 57. † Gazette Medicale, 27 Avril, 1844.

follows: two rabbits of the normal temperature of about 102°·2 were placed in two stoves, one at 212°, the other at 140°; the first died sooner than the second, but the temperature of each at the instant of death was the same, namely 111°·2. And the same experiment often repeated, showed that whatever were the ratio at which the heat was applied, the animal died when this increase of nine degrees was attained. In birds whose normal temperature [in some,] is 111°·2, i. e. the same as that at which mammals die, death ensues upon the same increase of nine degrees of temperature; they die when their blood is at 120°·2.

If when an animal is near dying from the effect of heat, an artery be opened, its blood is as black as that of a vein, it does not coagulate, and does not become bright on exposure. Exposed to dry heat, the animal loses much weight by evaporation before it dies; exposed to moist heat it loses no weight, but dies sooner. In the vapour-baths of Nero, M. James was almost suffocated in a temperature 112°, while in those of Testaccio, in which the air is dry, he was but little discomforted by a temperature 176°; and this has been confirmed on many animals. The quantity of weight lost by evaporation in hot dry air is directly proportionate, not to the temperature, but to the duration of the exposure, and the rate of loss is the same during all the times. After death, the lungs and heart are found in the contrary state to that seen after death from cold; they are empty from blood, and it is collected and extravasated at the surface of the body.

DIGESTION.

Saliva. From analyses, conducted on the same plan as those of the blood, (page 254), Enderlin* concludes that the saliva like the blood, contains no lactate, carbonate, or acetate; but that its alkaline reaction is due to tribasic phosphate of soda, which serves also as the solvent of the mucus and proteine-compounds. The analysis of the ashes obtained from a very large quantity afforded, in 100 parts:

Tribasic phosphate of soda (3 Na. O, P 2, O 5)	28·122
Chlorides of sodium and potassium	61·93
Sulphate of soda	2·315
Phosphate of lime, " magnesia, " iron	5·509

He believes, from this, that the saliva must take a very important part in digestion.

And it is but reasonable to connect these discoveries of the basic phosphate of the saliva and the acid phosphate of the gastric fluid with the fact observed by Schultz and Lehmann, and more clearly by Dr. Wright (see last Report, p. 12, and Lancet, 1842-3,) that the alkalinity of the saliva bears a direct proportion to the acidity of the gastric secretion. The observations made this year upon the relations of the phosphate salts in physiology may, probably, be regarded as the most promising of the year's discoveries.

Palate. A very excellent and comprehensive dissertation on the nerves of the palate has appeared from Dr. Hein† The conclusions of numerous experiments (which will be referred to again in the Report on the Nerves), were in many respects similar to those of Volkmann, The soft palate is supplied by four nerves. The mucous membrane of the anterior and upper surface and the subjacent glands receive sensitive filaments from the posterior palatine nerves of the second division of the trigeminus. The lower part of the anterior surface, and the surface of the anterior arch, are supplied by gustatory branches of the glosso-pharyngeal, which also gives gustatory branches to the middle part of the lower region. The whole posterior surface and posterior arch receive sensitive fibres from the pneumogastric and accessory. The second division of the trigeminus also sends filaments (by the middle palatine nerve) to the levator

* Annalen der Chemie und Pharmacie. Marz 1844.

† Muller's Archiv. Heft iii, iv, an inaugural prize essay; Heidelberg.

and azygos muscles; but these are probably centripetal nerves. Of the muscles, the tensor and azygos receive (besides these centripetal fibres) motor fibres from the pneumogastric and accessory; the tensor receives fibres of both kinds from the otic ganglion, and probably, the internal pterygoid branch of the third division of the trigeminus; the palato-glossus is similarly supplied with both kinds of fibres by the glosso-pharyngeal, and the palato-pharyngeus by the pneumogastric and accessory. The stylo-pharyngeus is supplied by the glosso-pharyngeal.

Œsophagus. Two new muscles connected with the œsophagus are described by Professor Hyrtl* of Prague, [and I can bear witness to at least the general truth of his description.] One serving, perhaps, to diminish the movement of the left bronchus when the food passes it, is connected by a broad base with the posterior wall of the bronchus, and thence extends to the left wall of the œsophagus, and mingles with its longitudinal fibres, continuing two or three inches with them. The other (for fixing the œsophagus below the point where it crosses the bronchus,) takes its origin from the left wall of the posterior mediastinum behind the aorta, over which it turns to reach the œsophagus. The first muscle is named by him broncho-œsophageal, the second pleuro-œsophageal.

STOMACH.

Gastric digestion. A very extended examination of the phenomena of gastric digestion has been made by M. Blondlot.† The chief subject of experiment was a dog, in which he maintained, without affecting the health, a fistulous opening into the stomach for more than two years. His examinations have furnished many new and important facts, and have confirmed those of Dr. Beaumont made on Alexis St. Martin in nearly every point.

Secretion of the gastric fluid. Like Dr. Beaumont, he has found that no mechanical irritation of the interior of the stomach will produce a secretion of nearly so much or so pure gastric fluid as the introduction of food. By mechanical irritation he could never obtain more than 180 grains of fluid, and this was mixed with mucus: when food was introduced, the gastric mucous membrane immediately became turgid, and yielded ten times as much digestive fluid pure. In this turgid state, also, both these observers agree that the mechanical irritation, which was ineffectual under other circumstances, greatly increased the secretion of the gastric fluid. In the turgid state, moreover, chemical irritation, such as was produced by putting pepper, salt, sugar, &c., on the food, produced a still greater effect than mechanical irritation did; and so did alkalies, but acids seemed to have a contrary influence. The act of digestion and of secreting gastric fluid was not, in either set of observations, found to be attended by an increase of the temperature of the stomach. The statement of Dr. Beaumont is also confirmed, that, cæteris paribus, the quantity of gastric fluid secreted is directly proportionate to the quantity of food taken, provided that quantity, however great, is not more than the organism requires; and the quantity secreted appears to bear a close relation to the degree in which the food taken is digestible or the contrary.

Its quantity is also apparently influenced by impressions made on the mouth: e. g. sugar introduced into the dog's stomach, either alone or mixed with human saliva, excited a very small secretion; but when the dog had himself masticated it and swallowed it, the secretion was abundant.

Some strange experiments on the secretion of the stomach are also related by

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* Wigner Zeitschr. i. ii, 1844; and Schmidt's Jahrbucher, Sept. 1844

† Traite Analetique de la digestion, 8vo; Paris, 1844. Of course so novel a mode of experimenting has been imitated. M. Payen and Dr. Basson (Froriep's N. Notizen, Feb. 1844.) have published the results of their experiments; but there is nothing in them which has not been long known.

M. Claude Bernard.* The results of the strangest are these:† 1. The mucous membrane of the stomach, during fasting, is not alkaline nor neutral: if the mucus be wiped off it, it gives an acid reaction. 2. Its acidity, even during digestion, is quite superficial; if a part of its surface be scraped, the acidity of that part disappears. 3. It is acid, even in the young fœtus.‡ 4. If arterial blood, just drawn from one dog, be injected into the coronary artery of the stomach of another, just killed, and having the stomach laid open, there will gradually ooze from the gastric mucous membrane a transparent dew-like fluid, with an acid reaction. If a small quantity of ferrocyanuret of potassium have been added to the blood, it will also be detected in the oozing fluid. 5. A dog was well fed; half an ounce of weak solution of ferrocyanuret of potassium was injected into its jugular vein, and half an hour after, (having fed again in the interval,) it was killed. The partially-digested food in the stomach, and the internal surface of the stomach, struck a blue colour on the contact of a solution of salt of iron; but, with the exception of the urine, no one other fluid, whether secreted or exhaled, and no tissue of the body afforded a similar indication of the presence of any of the injected salt. (?) 6. If the same salt be injected into the blood of a fasting animal, it is not effused into the stomach. 7. If ferrocyanuret of potassium be injected into one jugular vein of a dog, and protosulphuret of iron into the other, they do not appear to unite in the blood, nor in any tissue or organ [or fluid ?] of the body, except the gastric fluid: the partially-digested food in the stomach is the only thing that is coloured blue. (?) 8. If lactic, phosphoric, butyric, or acetic acid be injected into the blood, it is found in the stomach. 9. If alkaline solutions of magnesia and iron be so injected, those bases are never found in the gastric fluid. 10. If salts, such as the lactate of iron, or butyrate of iron or magnesia be injected, the acids are found in the gastric fluid and the bases pass into the urine. If cyanuret of mercury be injected, hydrocyanic acid is smelt in the stomach, but the mercury is never found in it. 11. If a mineral salt which cannot be decomposed in the blood be injected, it passes entire into the stomach: e. g. the ferrocyanuret of potassium and protosulphate of iron.

Analysis and properties of the gastric fluid. The most remarkable results of M. Blondlot's investigations relate to the composition of the gastric fluid, and different as his conclusions may be from those usually received, yet the large quantity of fluid he was enabled to collect in a purer state than any one hitherto has collected it, entitles his account to every consideration. He very carefully distilled on a sand-bath 3875 grains of pure gastric fluid obtained after feeding his dog on raw meat; he repeated the distillation, and repeated the whole experiment, several times, with the gastric fluid of other animals, as well as of the same dog, and the constant result was, that the product of the distillation did not once exhibit the slightest acid reaction; but the residue in the retort was always strongly acid. It was thus proved that the acid of the gastric fluid cannot be either the hydrochloric or the acetic, for both these are volatile at the boiling point of water, and would have distilled over.

A further proof that it is neither of these nor lactic acid, was furnished by the fact that no effervescence is produced when chalk, marble, or any other car-

* Gazette Medicale, Mars 16, 1844.

† Part only are quoted, and many of these need confirmation. None of the author's deduction's are mentioned; for these are adapted to the opinion that the gastric mucous membrane has no glands, except such crypts as are found in the intestines, and that it has villi identical in structure with those of the intestine. The experiments which are here omitted are explicable by physiological facts, familiarly known in most parts of Europe, but which the author, assistant to Mr. Magendie, appears not to have heard of.

‡ M. Bernard says he found it acid in a human fœtus of about seven weeks; but the separation of the stomach from the rest of the digestive canal does not begin till after two months. Perhaps he means seven months.

bonate of lime is added to the gastric fluid; and it was this fact which chiefly led M. Blondlot to his conclusion, that the true and almost only source of the acidity of healthy gastric fluid is the presence of super-phosphate and biphosphate of lime. The evidence which he gives in addition to the above is: 1st, there is no acid salt, except this super-phosphate of lime which could retain its acidity and remain in contact with carbonate of lime without exciting decomposition; 2d, sulphuric acid, added to gastric fluid, produces an abundant precipitate of sulphate of lime, and oxalic acid a similar one of oxalate of lime. 3. Potash, soda, ammonia, and lime-water, produce abundant precipitates of neutral phosphate of lime. 4. The calcined ash of gastric fluid was not deliquescent, was dissolved without effervescence by a few drops of hydrochloric acid, with which it formed chloride of calcium; it had, therefore, contained neutral phosphate of lime, the excess of the acid having been decomposed in the calcination.

The general conclusion of his analysis is, that the gastric fluid is composed of ninety-nine parts of water, with one part of super-phosphate of lime, super-phosphate of ammonia, chloride of sodium, mucus, an aromatic, and a peculiar, principle. Similar results were obtained from the analysis of the gastric fluid of several animals.

For further evidence that the acid reaction of the gastric fluid depends on these acid phosphate salts alone, M. Blondlot has completely examined the question, whether during healthy digestion, lactic acid is ever formed by transformation of the food in the stomach. His conclusion is that neither it, nor a transformation of sugar into starch, nor any kind of fermentation takes place. He has often analysed the fluid expressed from food which had remained for various lengths of time in the stomach, and never found the least trace of lactic acid; and the reason he assigns for its absence is, that the acid of the gastric fluid prevents it, just as other acids prevent the lactic fermentation from taking place in a solution of sugar, provided they are present in proportion sufficient to give the solution a degree of acidity equal to that which it would acquire if the lactic acid were formed in it. In confirmation of this he shows, by numerous experiments on ruminants and birds, that lactic acid is formed by the transformation of the sugar of their food in all those parts of the digestive canal in which the food is delayed without the presence of an acid; namely, in the first and second stomach of ruminants, the crops of birds, and the cœcum of man and other animals. He first proves that the acidity often observed in the food taken from these cavities is not due to any secretion from their walls. He fed, for four days each, sixteen sheep and goats, and several pigeons and chickens with different kinds of food containing no sugar; and in every instance the portions of food which were found after twelve hours' fasting, in the first stomach, or the crop, were not acid, but alkaline, proving that the walls of these cavities secrete an alkaline fluid. On the other hand, when, the other circumstances being the same, as many ruminants and fowls were fed on food containing sugar, the portions of food found in the same cavities were always acid, and, in the case in which they were analysed, the acid obtained was the lactic.

In regard to the cæcum he states that its contents are never more acid than those of the small intestines, except when the animals examined have had sugar in their food; from which, and the absence of any proof that the cœcum secretes an acid fluid, he believes that the acidity often found is due to a portion of the sugar of the food which has not been absorbed, and has undergone the lactic acid fermentation in the cæcum.*

These experiments are confirmed also by those of Mr. Ross,† who finds that rabbits fed on farinaceous food have lactic acid in their small intestines, though it is not found in their stomachs. They appear to be contradicted by those of Tedemann and Gmelin, who found acid in the crop of a pigeon which had fed

* Traite Analytique de la Digestion, pp. 91-104.
† Lancet, Jan. 20 and Feb. 10, 1844.

for several days on nothing but meat: but M. Blondlot shows that this acid had probably flowed from the stomach into the crop after death; an accident which happened in his experiments when means were not used to prevent it.

Besides these experiments concerning the chemical properties of the gastric fluid, M. Blondlot relates others, which add to the evidence already known, that the real digestive property of the fluid depends, not on its obvious chemical qualities, but on an organic principle. If exposed to a temperature between 104° and 122° F. or higher, it entirely and irrecoverably loses its digestive powers, although apparently, and as to analysis, unchanged. Kept from the air, the gastric fluid retains its active properties for at least two years; but, exposed to the air and a moderate temperature, it putrefies in five or six days, although the chyme which it forms from nitrogenous organic substances may be preserved for two or three months without apparent change. The precipitation of all the lime which it contains does not affect its activity; neither are its chlorides indispensable; but whatever much alters its organic constituents, (such as heat, strong alcohol, or strong acids,) or removes them, (such as animal charcoal, tannic acid, chlorine, or acetate of lead,) destroys all the digestive properties.

Digestive properties of the gastric fluid. Some singular evidence of these is furnished by MM. Bernard and Barreswill* who have found that nutritive substances injected in simple aqueous solution into the blood are not assimilated: but are assimilated if dissolved by the aid of the gastric fluid. Among other experiments are these: portions of cane-sugar, albumen, and gelatine, seven and a half grains of each, and severally dissolved in water, were injected into the jugular veins of three dogs. Three hours after, the urine of each was examined, and in each the injected substance was found. Under other similar conditions, the same quantities of the same substances dissolved in gastric fluid were injected, and three hours after, gelatine was detected in the urine of the dog into whom it had been injected, but not a trace of albumen or sugar in the urine of either of the others. Three dogs were then fed exclusively and respectively on gelatine, albumen, and sugar; and the first alone could ever be detected in the urine. The authors fed themselves in the same way and obtained the same result; and they conclude (as others presently to be mentioned do) that gelatine is not assimilable and therefore not nutritive.

Among the experiments which M. Blondlot† made to determine the mode in which the gastric fluid, in or out of the stomach, acts on different animal substances, some afforded novel and interesting results: A. He shows that coagulated albumen owes its long resistance to the digestive fluid only to its compact form. When coagulated in very fine particles (as by pouring white of egg beaten into a froth, into boiling water) it is digested as quickly as soft fibrine. B. He adds further evidence that the action of the stomach in coagulating milk is not due peculiarly to its digestive principle, but to its acid, which acts like the lactic acid developed from the sugar under the influence of rennet or any other decompozing azotized compound. C. The effect of the gastric fluid on bones, observed both on the bones in their entire state, and on their animal and inorganic constituents separately is, that first, it very slowly disintegrates the animal matter, attacking them from the surface, and then, also very slowly, disintegrates and reduces the earthy matter into a fine chalky powder, but without either dissolving or decomposing it. The earthy matter not being dissolved proves that no hydrochloric acid had acted upon it: and in its minutely divided state it all passes through the intestines and is discharged with the fæces.

The results of many more of his experiments of this kind are interesting. They confirm Mr. Beaumont's, and appeared to M. Blondlot to show that, of all the simple alimentary substances, those which are fluid at the ordinary temperature of the stomach, and those which are easily soluble (in the ordinary

* Gazette Medicale, 27 Avril, 1844; Report from the Acad. des Sciences, seance du 22 Avril, 1844.

† Loc. cit. pp 254 383. &c.

manner of solution) in its secretion, such as fluid albumen, sugar, gum pectine, &c., are at once absorbed by the veins; and that others, which are not liquid nor easily soluble, such as fibrine, coagulated vegetable and animal albumen, caseine, gelatine, &c., are, in only a very small proportion, if at all, dissolved, the action of the gastric fluid on them being limited to the softening of them, so that they are reduced into very minute particles which (out of the stomach) appear like a very fine precipitate. The same general rule is said to be observed in the digestion of the compound alimentary substances, both animal and vegetable; the fluid and easily soluble parts cannot be said to be digested, for they are at once absorbed by the stomach; the rest are softened and reduced into very minute particles, which are carried into the intestines, without any change in their chemical constitution, and are, in this state, absorbed by open mouths of the lacteals, visible with the naked eye, at the extremities of the villi.(!) This act of softening is, in some cases, due merely to the acid of the gastric fluid; e. g. in the case of the parenchymatous tissues, and succulent fruits and roots, which are similarly softened, at the same temperature, in acidulated water; in the cases of fibrine, coagulated albumen, &c., it is the effect of the *peculiar* mode of action of the gastric fluid.

In any case, chymification is, in M. Blondlot's opinion, no *solution* but a *division* of the aliment; it undergoes no kind of decomposition.

Influence of the pneumogastric nerves upon digestion, M. Bernard* has instituted fresh experiments to determine this still-debated question, making use of the artificial fistulous openings into the stomach, invented by M. Blondlot. A dog's digestion had been thus watched for eight days, and had always been well effected. On the ninth day, after a day's fast, M. Bernard sponged out the stomach, which contracted on the contact of the sponge, and at once secreted a large quantity of gastric fluid; he then divided the pneumogastric nerves in the middle of the neck, and immediately the mucous membrane, which had been turgid, became pale, as if exsanguine, its movements ceased, the secretion of gastric fluid was instantaneously put a stop to, and a quantity of ropy neutral mucus was soon produced in its place. After this, no digestion was duly performed, and milk was no longer coagulated; raw meat remained unchanged, and the food (meat, milk, bread, and sugar, which the dog had before thoroughly digested) remained for a long time neutral, and at last acquired acidity only from its own transformation into lactic acid. In the stomachs of other dogs after the division of the nerves he traced the transformation of cane-sugar into grape-sugar in three or four hours; and in ten or twelve hours the transformation into lactic acid was complete. In others, when the food was not capable of an acid transformation, it remained neutral to the last. In no case did any part of the food pass through the peculiar changes of chymification. In a last experiment, he gave to each of two dogs, in one of which he had cut the nerves, a dose of emulsine, and half an hour after, a dose of amygdaline (substances which are innocent alone, but when mixed produce hydrocyanic acid). The dog, whose nerves were cut, died in a quarter of an hour, the substances being absorbed unaltered and mixing in the blood: in the other, the emulsine was changed by the action of the gastric fluid before the amygdaline was administered, and it survived.

Act of vomiting. A case is related by M. Lepine† of Châlons sur Saône, proving the partial influence of the stomach in vomiting. The patient's abdomen was torn open by a horn, and the stomach wholly protruded. For half an hour it was seen repeatedly and forcibly contracting itself, till by its own efforts it expelled all its contents except the gases.

Mr. Anderson,‡ to prove that the diaphragm is not, as Dr. Marshall Hall supposes, inactive in vomiting, gave tartar emetic to two dogs, and, when sickness commenced, he opened the trachea, and at the same time introduced his finger

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* Gazette Medicale, Juin 1, 1844; from the Report of the Acad. des Sciences, seance du 27 Mai, 1844. † Bulletin de l'Academie de Medecine

‡ London and Edinb. Monthly Journal of Medical Science, Jan., Feb., Mar., &c., 1844.

into the abdomen so as to feel the state of the diaphragm. During each effort of vomiting, the diaphragm became tense and rigid, and descended towards the abdomen.

[But neither these nor any other experiments prove that the diaphragm actively compresses the stomach in vomiting, by descending towards the abdominal cavity. Indeed no experiments in which the trachea is opened, can illustrate the action of the diaphragm when, as in vomiting, the glottis is closed; for, in the former case, the diaphragm is free to move either way; in the latter, it cannot move at all without either expanding or compressing the air in the lungs. The true explanation of the act of vomiting must, I think, be intermediate between that which supposes the diaphragm to be one of the muscles actively compressing the stomach, and that which supposes it to be inert. The inspiration which usually precedes the act of vomiting is terminated by the closure of the glottis: after this the diaphragm cannot descend further, except by expanding the air in the lungs, but it remains fixed in its position, and serves as an unyielding surface against which the stomach may be pressed by the contracting abdominal muscles. This position of the diaphragm might be nearly maintained, though it were relaxed, for, if the glottis remained shut, the diaphragm could not be raised except by compressing the air in the lungs. But Mr. Anderson's experiments, as well as several other facts, make it more probable that the diaphragm continues in the act of contraction; or rather, since in his experiments it descended when the trachea is open, we may conclude that when the trachea and glottis are shut, it would not descend, but would remain, as other muscles often do, in the rigid and resisting state of contraction, so as to afford a completely fixed and firm surface for the stomach to be compressed against. And this continuance of the contracted state is proved by rupture of the diaphragm in vomiting, for this rupture is not from over extension of the whole muscle, but from some of the fibres or parts of fibres contracting so vigorously as to tear others which are contracting less or not at all.

A condition essential to vomiting, but not sufficiently considered, is the relaxation of those oblique fibres of the stomach, which like a sphincter embrace the cardia. Unless it be relaxed, no vomiting can take place; for when contracted, they can as well resist all the force of the contracting expiratory muscles as the muscles of the glottis can resist it in the act of straining. The activity of the stomach in M. Lepine's case shows that its movements may be associated with those of the abdominal muscles; and, probably just as coughing, sneezing, &c., are perfect when the relaxation of the muscles which closed the glottis is exactly coincident with the contractions of the expiratory muscles, so is vomiting perfect when in exact coincidence with the same contractions the oblique fibres of the cardia are relaxed.]

LIVER.

Structure. Some valuable papers on this subject have appeared from Muller,* E. H. Weber,* and Dr. Kronenberg,* the general effect of which, (though those of the last two are written with an opposite intention,) is to confirm in a remarkable degree the description by Mr. Kiernan. The amounts given by Weber and Kronenberg, though written independently, are in almost everything alike. Especially, they agree with Mr. Kiernan in describing the hepatic ducts as commencing in very fine networks, which, they add, are interlaced with the capillary networks between the portal and hepatic veins. They demonstrate these networks in both the uninjected and the injected state. E. H. Weber's account is that the blood capillaries are from 1-1463th to 1-1959th of an inch in diameter, forming a solid uniform network, with meshes not wider than the vessels themselves; and that the distance through which blood has to pass from the smallest portal to the smallest hepatic veins is from about 1-70th to 1-80th of an inch. The meshes of the blood-capillary network are occupied by the interlac-

* Muller's Archiv., 1844, Heft iii.

ing network of hepatic ducts. These are smaller than any other gland-ducts yet known, being from 1-900th to 1-1340th of an inch in diameter, and have no capillaries on their walls. Their network extends uniformly, and without any division according to lobules, through the whole substance of the liver, and its meshes are very small. There is no anastomosis between the blood-vessels and the ducts; but they are in contact on every side, each filling up the meshes of the network formed by the others, and both together filling every space, and forming the whole snbstance of the liver, except when large vessels, nerves, &c. run into it.

Full accounts are given of the modes in which the demonstration of these things are obtained. Among them are injections (of necessity only very partial) of the bile-ducts; and these demonstrate, according to Weber, another form of bile-ducts which are found imperfectly developed on the surface of the transverse and longitudinal fissures, the edges of the gall-bladder, and especially (as M Kiernan also showed) at the connexion of the left lobe and left lateral ligament. In these parts are net-works of comparatively large branches of ducts, beset by cells, and having many branched appendages, which terminate in closed ends filled by cells, and which Weber named *vasa aberrantia* of the liver.

[It does not appear that Weber and Kronenberg have made more complete injections of the hepatic-duct plexuses than Mr. Kiernan did, whose demonstration of this arrangement, so far as injections are concerned in it, is as satisfactory as theirs; for all confess the injections to have been very partial. The chief new evidence for this mode of arrangement is afforded by the microscopic examination of the uninjected ducts. I had a fortunate opportunity for confirming, to some extent, the account already given, in examining parts of a liver last summer, from a case of intense jaundice. The case was of a kind not very unfrequent, in which jaundiced persons die with coma or delirium, and other rapidly supervening signs of cerebral disturbance, and in which, after death, the liver is found pale, or orange-coloured, small, soft, but tough, generally or in most parts nearly bloodless, and with the minutest bile-ducts, in some parts, gorged with bile, although the large ones are not closed nor apparently obstructed; so that sometimes parts of the liver stand out from the rest, of a deep orange or olive colour. In this case, the distended ducts were easily traceable in thin sections of the liver, with a single lens of 1-10th inch focus; and they appeared tortuous, and freely anastomosing, so as to form an irregular network with very small meshes. They appeared filled, not with fluid bile but with bile-cells; and these, as seen with a higher power, were all pale yellow, and spotted here and there with brilliant yellow points and granules; in some also the nuclei appeared peculiarly bright yellow.]

The chief point in which these accounts differ from Mr. Kiernan's is in denying that the component parts of the liver are arranged in lobules. This has also been denied by Henle and Mr. Bowman, who agree with Weber and Kronenberg in describing the capillary networks as *solid*,(i. e. extending uniformly through the liver.) They also all deny the existence of any fibro-cellular partitions dividing the liver into lobules, and even the existence of more fibro-cellular tissue than serves to invest the larger vessels, &c. of the liver, They deny also that there are any such *interlobular* veins and fissures as Mr. Kiernan descrlbed, and state that the smaller branches of these veins communicate by branches only just larger, if at all larger, than capillaries.

Muller's paper is written chiefly for the purpose of maintaining the old view of the lobular arrangement of the liver, and contains many facts which had long appeared to me to afford satisfactory evidence of its truth. He justly observes that the complete injections of the blood-capillaries, on which the objections to the lobular arrangement of the larger vessels are founded, are not the best preparations for demonstrating the distribution of the larger vessels, since these are sure to be concealed by the full capillaries. In less complete injections, they may be traced, as Mr. Kiernan describes them, though not usually with that stiff uniformity in which, for clearness sake, they are represented in his diagrams. But without injections, the lobular divisions of the liver may be seen, especially in the pig's liver, in which, as Muller exactly describes it the whole natural surface, as well as the surface of every secretion, is marked by white lines inclosing angular spaces, which lines are no arteries (as they are supposed by Kronenberg), but the ends of membranous septa of cellular tissue, which form distinct capsules round each lobule, and, altogether, divide the whole liver into minute spaces, so that when the glandular substance inclosed within these capsules is scraped away, they remain like a fine honeycomb, composed of oval cells, about a line in length, and half a line wide. [The general truth of this description can be easily seen in the pig's liver, and traces of the of the same arrangement in the human liver. The only point in which I think Muller is wrong is in describing the partitions as formed of fibro-cellular tissue. If one be cut from the interior of the liver, it will be found covered on both sides with hepatic cells and granules, which adhere to it much more firmly than those in the interior of the lobule do to one another. When these are scraped off, there remains a very thin and tough membrane, in which there are only a few filaments of fibro-cellular tissue, and which appears to be composed of a very dense network or networks of vessels, with gland-cells still adhering among them. The appearances presented in the pig's liver are such as to indicate that its lobules are by no means generally or uniformly traversed by plexuses of ducts; in their interior they appear to contain only large nucleated biliary cells, with various granules loosely arranged: the ducts appear only in the walls of the lobules.]

Muller adds to these evidences, that, if portions of liver be macerated for eight days in vinegar, the lobules may be easily separated from each other, and all will present smooth surfaces; and that, though the lobular structure seen in most vertebrata is absent in some fish, yet in several of the plagiostomatous fish it is shown by the arrangement of black pigment cells, which everywhere follow the arrangement of the interlobular substance, so that the surface and sections of the liver exhibit islands of yellow substances, inclosed by dark lines.

Secretion and properties of bile. A series of experiments by Schwann* has led to the distinct conclusion of the bile being indispensable to life. They consisted in removing a portion of the common bile-duct, and establishing an external fistulous opening into the gall bladder, so that the bile might be naturally secreted, but be discharged externally, and not be permitted to enter the intestine. Their general result was, that, of eighteen dogs thus operated on, ten died of the immediate consequences of the operation (by peritonitis and other affections aggravated, probably, by the want of bile): and, of the remaining eight, two recovered, and six died. In the six which died, death was the result of nothing but the removal of the bile; after the third day, they daily lost weight, and had all the signs of inanition, e. g. emaciation, muscular debility, uncertain gait, falling of the hair. They lived from seven to sixty-four days† after the operation; and the inanition was the greater the longer they survived. Young dogs appeared to die rather sooner than old ones. Licking the bile as it flowed from the fistula and swallowing it, had no influence on the consequences of the operation. In the two dogs that recovered, the importance of the bile was equally well shown; for in these it was found, when they were killed, that the passage for the bile into the intestine had been restored; and the period of its restoration was distinctly marked by their weight (which had previously been regularly decreasing) being augmented and continuing to increase till it amounted to what it was before the operation; and also by the fistulous opening into the gall-bladder healing and the discharge of bile ceasing.

Schwann says he is engaged in further and minute examinations to prove in what way the bile serves its important purpose; and these will probably prove how far several theories respecting it (of which not a few have appeared this year,) are true or false.

The chemical composition of the bile has been the subject of careful examination

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* Muller's Archiv, Heft ii, 1844.

† One lived two months and a half; but it is not impossible that the bile-duct was for a time restored.

in the Giessen laboratory by Drs. Theyer and Schlosser.* They obtained the bile in what they regard as the perfectly pure state, by evaporating that of the ox, immediately after death, to the thickness of an extract; dissolving it in common alcohol and adding alcohol till all the mucus was separated; treating the clear solution with animal charcoal till all the coloring matter was removed; then distilling off the alcohol and washing the residue repeatedly with ether, till no more fatty matter could be separated from it: and, lastly, evaporating to dryness. Elementary analyses were made of this pure bile, and of its combination with oxyde of lead. The latter was formed by mixing an aqueous solution of pure bile with a diluted solution of acetate of lead; it formed a white ropy plaster-like substance, in which it was proved that the organic substance of the bile remained complete and undecomposed, by reproducing it in its combination with soda. The compound thus formed by separating the organic principles from the combination with lead and uniting it with soda, was in no material respect different from the pure natural bile; so that it was quite evident that the substance which is united with the soda in the bile is (as Demarçay and Dr. Kemp already maintained) a peculiar organic acid. To separate this, in a pure state, various means were used, but the only satisfactory plan was by decomposing the salt which it forms (as already said) with oxyde of lead, by passing (with several necessary precautions) sulphuretted hydrogen through an alcoholic solution of it, and filtering and evaporating the remaining solution. The elementary analysis of the acid then obtained in the separate state agreed with those made of it in its combinations (both natural and artificial) with soda, and with oxyde of lead.

The bilic acid (Gallensaure) thus separated, agrees completely with the *bilifellic acid* of Berzelius; (his *bilin*, the authors regard as pure bile, or bile with an excess of bilic acid); it corresponds also to Demarçay's *choleic acid;* and Kemp's *bilic acid*† is the same, not completely separated from its combination with soda. Thenard's *picromel* and Gmelin's *sugar of bile*, and Berzelius' *bilifellinic* acid, are also this same *bilic acid*, more or less imperfectly separated, and the authors adduce the identity of composition in all the samples of bile that are examined as a proof that it is not, like the urine, a fluid by which a variety of morbid and accidental substances are separated from the blood—a compound of various and uncertain materials,—but a fluid separated by a true process of secretion under the determinate and regular influence of its secernent gland.‡

Pancreas. M. Blondlot§ says that having obtained three or four grammes of pancreatic fluid from the duct of a large dog, and examined it by means of an electric current, he could find no trace of albumen in it. He considers it to be of the same nature as the saliva, which he holds to be only a common mucous fluid, a kind of detritus or *caput mortium* serving no active part in digestion, but merely protecting the organs on which it lies (!)

Fæces. The analysis of the ashes of firm human fæces by Enderlin‖ yielded, in 100 parts:

Chloride of sodium and alkaline sulphate	1·367
Tribasic phosphate of soda.	2·633
Phosphate of lime and phosphate of magnesia	80·372
Phosphate of iron	2·091
Sulphate of lime	4·53
Kieselerde	7·94

* Annalen der Chemie und Pharmacie Oct, 1843. Translated in the Medical Times, Feb. 3, 10, 1844. Additions in the Annalen of May, 1844.

† See last Report, p. 13.

‡ For an account of the mode in which Dr. Platner has prepared crystallized bilic acid and neutral bilate of soda from ox bile, see Muller's Archiv. Heft ii, and Poggendorf's Annalen. Juli 1844. On the Analysis of the Bile of the Astacus fluviatilis, and some other Crustacea, see J. F. G. Schlemm 'De hepate ac bili crustaceorum,' &c.; Berolini, 4to. The bile of the Astacus is acid, and contains no bilin. In the same dissertation there are general confirmations of the received doctrine of the development of secernent cells, and a minute account of the nerves of the liver in the astacus and helix pomatia. See also some account of Mr. Ross's observations at p. 272.

§ Loc. cit. p. 124, &c. ‖ Annalen der Chemie und Pharmacie, Mars 1844.

From the absence of carbonate of lime he deduces that the fæces could contain no choleic acid.

ABSORPTION.

Lymphatic and lacteal absorption. A systematic work on the lymphatics has been published by Dr. Herbst.* He considers (as M. Bouisson also does, and as Tiedemann and others may be said to have considered) that a process of secretion is combined with that of absorption in the extremities of the vessels. [But the opinion is maintained on very imperfect evidence; for neither of its authors is acquainted with the best accounts of the structure of the villi, or with the physiology of secretion, as an act performed by cells. Some other singular opinions are maintained in this work; but I give only those new results in matters of fact stated to have been obtained from experiments; and even of these, it is necessary to say, that the evidence, especially that derived from the microscope, is not altogether satisfactory.†]

1. The coagulability of the lymph is directly proportionate to that of the blood; and is probably due to coagulable matter passing from the latter into the former. 2. Blood-corpuscles are a common constituent of lymph; and their number is greatly and proportionately increased in all cases of unusually active circulation, congestion or inflammation, whether local or general. In the former case, they pass in abundance into the lymphatics of the congested part. 3. When fluids are injected into the blood vessels in quantity sufficient to distend them, the injected substance, (whether blood, milk, water, gelatine, starch, or whatever it may be) may be almost directly afterwards found in the lymphatics. And this same result is obtained, whether the injection be made during life or soon after death; nor is it only the fluid part of that which is injected which passes into the lymphatics; the solid parts also, such as the blood- and milk-corpuscles and the starch-granules, pass unchanged (though in less proportion) into both the lacteals and the lymphatics, Nineteen experiments are related in proof of these statements: the author ascribes the result to a transudation different only in degree from that which is normal. 4. More than twenty experiments are detailed at great length to prove (chiefly by miscroscopic evidence) that the lymph-corpuscles found of various sizes (from 1-10 to $1\frac{1}{2}$ of the size of a blood-corpuscle) in the thoracic duct, are not essentially different from those in the true lymphatics and the mesenteric lacteals, nor from those of milk (!) and of chyme formed from fatty substances: (!) and that therefore the various corpuscles of chyme and milk may be considered to be absorbed entire and unaltered by the lacteals of the villi, and to be thence transmitted to the blood, in which they may also be found unaltered. 5. Another large series of experiments is related to prove that colouring matters (chiefly indigo), salts of potash, lead, &c. and starch in imperfect granules, are rapidly absorbed by the lacteals and by the lymphatics of the stomach. (But there was nothing in the mode of performing them by which it can be explained why their result was different from that obtained by others, who, in similar experiments, have found no absorption of the same substances: they therefore need only be referred to.] 6. Many of the experiments in which the preceding conclusions are founded, and some others purposely made, give evidence (as the very first observations by Asellius‡ do,) that, for some time after apparent death, the lymph and chyle continue to be moved onwards by the peristaltic movement of the digestive canal, and by the contraction of the walls of their own vessels; and that also for some time after death, absorption itself is carried on, for considerable quantities of fluids injected into the stomachs of recently-killed animals which had fasted a long time, were carried into the thoracic duct.

* Das Lymphgefassystem und seine Verrichtung; Gottingen, 1844. 8vo.

† For example, it is often stated that after lymphatic vessels had lain in water till all the colour was taken out of their coats, unaltered blood-corpuscles were found in their contents; these are also so described in lymph diluted with water.

‡ De Lactibus seu Lacteis' Venis.

A few facts not generally, if at all, hitherto known, are also recorded in the Studies on the Chyle, by M. Bouisson.* 1. When a few drops of sulphuric acid are added to the chyle of any animal, the same kind of odour is emitted as when the blood of the same animal is similarly treated: an odour, which as M. Barruel showed of the blood, and M. Couerbe of many secretions, is peculiar in each of many animals. [The fact, as M. Bouisson states, had already been observed by Vauquelin.] 2. Chyle, like blood, will often remain for a long time in its vessels without coagulating, but will coagulate rapidly on being removed from them. In one case it was fluid in a man twenty-four hours after death, but soon coagulated after its escape from the vessels. 3. The chyle-globules in the thoracic duct are, as Wagner has described them, lenticular. Some experiments, apparently not very carefully performed, showed that milk injected into a dog's rectum (after purging and abstinence) was coagulated, acquired an acid reaction, and was nearly all absorbed by the lymphatics. 5. In rabbits fed for a short time with madder mixed in their food, no tinge of red is commmunicated to the chyle, even though the serum may be red; but if the same diet be continued till the colouring matter has thoroughly impregnated the blood, and is mixed with the urine and other secretions, it is imparted to the lymph and, thence, indirectly to the chyle.

Calculating from the analyses of Tiedemann and Gmelin, which showed a far larger proportion of fatty matter in the chyle of the recently fed, than in that of the fasting, horse, and a proportionally smaller quantity of albumen, Mr. Ross† has adduced further evidence for the view (assigned in the last Report to MM. Sandras and Bouchardat,) that the lacteals absorb none of the usual solid matters of the chyle, except the fatty matters; and that the proportion of solid matter in the chyle of the thoracic duct being less than that in the lacteal vessels is due to the chyle of the latter being diluted by mixture with the contents of the lymphathics. The other constituents of the chyle he considers to be absorbed by the roots of the portal vein, by which they are carried to the liver, and he believes that the observation of Tiedemann, respecting the apparent absence of fatty matter in the chyle when the bile-duct is tied, proves that the lacteals obtain only matter, and from the chyle alone, but also and chiefly from the substances secreted by the liver. He calculates from formulæ, that the bilic acid may be decomposed into an oily matter and an azotized substance which may assist to form protiene-compounds.

Lymphatic hearts. By Professor Stannius‡ the full discovery has been made of the existence of lymphatic hearts in birds, analagous to those in reptiles. He has found them already in the stork, ostrich, cassowary, goose, swan, diver, and hawk, and in all, with the exception of the last two, has found the walls of the heart formed by transversely striated muscular fibres. In the ostrich and cassowary these fibres form a layer from half a line to a line in thickness: in the *natatores* it cannot be discerned with the naked eye, but can (though, in some, still very sparingly) with the aid of the microscope.

It is the existence of these fibres which gives to these organs (already described as lymph-vesicles by Panizza) the right to be considered hearts. Their positions and connexions vary much in different birds. In all, several lympathic vessels open in to the cavity of the heart, and a vein proceeds from it which passes under the os illi and joins the vena cava inferior. Lymph only has been seen in them, and they always have valves which prevent the passage of the lymph backwards into its vessels, and that of the blood from the vein into the lymphatic heart. In the swan and goose, in which alone these hearts have been observed during life, no active independent motion of their walls has yet been clearly seen, though there has been an appearance of a slow approximation of their walls, expelling their contents.

Absorption by blood-vessels.§ The experiments of MM. Flandin and Dan-

* Gazette Medicale 1844, 29 Juin, 6 Juillet, 3 et 17 Aout, &c.
† Lancet, Feb. 10 and 17, 1844. ‡ Muller's Archiv, 1843, Heft v,
§ I use this term rather than absorption by veins, because there is no doubt that this

ger* confirm the general rule of the absorption of poisons from the digestive canal, by the branches of the vena portæ. Hence they are all found in large quantities, and some exclusively, in the liver. Their latest examinations were made on the absorption of the salts of lead, which they detected in the digestive canal, the liver, spleen, kidneys, and lungs: but not in the blood, heart, brain, muscles, or bones. Lead differs from copper in that its salts after absorption may pass off with the urine.

Experiments, by Oesterlen,† have also proved that mercury in its crude state is capable of being freely absorbed and circulated with the blood. It may be absorbed by the skin with the aid of friction, or from the intestinal canal. After absorption from the walls of the abdomen or the digestive canal, minute particles of it are found, for the most part, in the spleen, liver, and kidneys,: and it is especially through the last two organs (at least in cats) that that which is absorbed is subsequently discharged. Globules of it have also been found in the saliva of a woman in whom it had been long applied in friction: and they existed in still greater number, (mixed, as in the saliva, with the epithelium) in the urine. In one case mercury, absorbed in its metallic state, produced pneumonia with depots of pus, apparently like that which ensues when mercury has been injected into the blood-vessels of dogs.

Mr. G. Robinson,‡ continuing the experiments alluded to in the last Report, and varying them, has shown that when a stream of water is made to flow through a flaccid membranous tube perforated by numerous small apertures, it will exercise a force like that of absorption upon a fluid external to the tube. Under favorable circumstances some of the fluid outside the tube is drawn into it and carried on with the current that is flowing through it. [The results of such experiments, ingenious as they are, cannot be safely admitted to prove more than that the circulation is necessary to absorption by blood-vessels, and that flaccid vessels and a rapid stream are favorable to it, by permitting imbibition and by carrying off the imbibed fluid as fast as it can enter. In the small veins and the capillaries near them the current of blood is not rapid; but, as already said, its rate is only about an inch per minute in the latter, and 1-8th faster in the former. The *drawing* power such a current must be incalculably small; and it is, visibly, so minute that a part of the blood in every small vessel adheres to the internal surface of the wall, the adhesion between them being greater than the force of the current can overcome. It is, therefore, not imaginable that the same force should be capable of overcoming the powerful capillary attraction of the fluids held in the pores of the walls of the vessels. It is only when these fluids, by mixing with the still layer, have passed into the central current, that they are carried on with the blood.]

kind of absorption takes place through the coats of all blood-vessels that are not too thick, although, of course, the absorbed fluids are usually detected only in the veins.

* Report from the Academie des Sciences, Jan. 29, 1844, in the Gazette Med. Fevr. 3, 1844. But in an extract from some journal in the Oesterreichische medic. Wochens. Mai 25, 1844, a Prof. Cozzi is said to have detected "a salt and oxyde of lead combined with the albumen of the blood of a patient suffering from lead colic."

† Oest. Medic. Wochenschrift, Fevr. 24, 1844; from Roser und Wunderlind's Archiv, Heft iv, 1843. ‡ Medical Gazette, June and July, 1844.

TO BE CONCLUDED IN OUR NEXT NUMBER.

Part Fourth.

MEDICAL INTELLIGENCE.

FOREIGN.

1.—*Antagonism of Disease.*—Several writers have given facts, confirmative of the theory advanced by M. Boudin, to the effect that a real antagonism exists between phthisis and ague, so that in any district where one is a frequent disease, the other is rare.

But the evidence adduced by other observers is directly opposed to the theory of Boudin; and the results of Dr. Genest's examination of the reports of Major Tulloch and Mr. Wilson on the sickness and mortality of the British army at home and abroad seem decisive against the theory, as will appear from the following table, showing the relative number of cases of phthisis and intermittent fevers admitted into hospital:

	Intermittent Fever.	Phthisis.
United Kingdom	2 per 1000	6.5 per 1000
Gibraltar	5 "	5.6 "
Malta	7·5 "	6· "
Ionian Isles	132 "	5· "
Canada	78 "	5·3 "
Nova Scotia	0.8 "	7· "
Bermudas	2·5 "	8·8 "
Western America	250 "	9·5 "
Jamaica	85 "	13· "

Brit. and For. Med. Review.

2.—*Parasites.*—The zeal with which microscopical investigations have latterly been pursued has led to the detection of numerous parasites, both animal and vegetable, in various parts of the body. It will, however, be impossible to do more than point out the references to some of the discoveries and observations that have been made in this department of pathology. An interesting paper, giving an account of the various entozoa that have been detected in different parts of the eye, is furnished by Messieurs Nordman and P. Rayer. Filaria oculi humani has been seen in the liquor morgagni, and in the capsule of the crystalline lens; monostomata in the crystalline lens, affected with cataract; distoma in the capsule of the crystalline lens of a child born with lenticular cataract; ecchinococcus between the crystalline lens and choroid; others in the subconjunctival tissue, chiefly cysticerci; confervoid growths in the posterior chamber, removed by paracentesis oculi; filaria papillosa in the anterior chamber of the eye of a horse. Dr. Livois' researches on ecchinococci led him to examine 800 acephalocysts, and in no one instance did he fail to detect within them ecchinococci. He, therefore, concludes that the true hydatid is nothing more than the containing cyst of these parasites; which, instead of being rare, are among the most common. They occupy different situations in the cyst, according to their degree of development, appearing as granulations or gemmules, adherent to the interior of the cyst as long as their head is not protruded; but when fully developed floating loose in the fluid of the sac. They are generally found in both situations, but sometimes only loose, escaping when the sac is opened. The containing cyst increases as the ecchinococci multiply by reproduction. Prof. Klencke's experiments lead him to believe that hydatids may be, and probably are, in every case, developed by contagion as he calls it, or by actual introduction within the body. When he injected them within the veins, or introduced them, into the cellular tissue or digestive tubes of dogs, cats, birds, &c., he found that they excited the disease. He also detected them in the milk of the cow. Mr. Drewry Ottley has detailed the history of a case in which the ecchinococcus was found in the brain, A now species of intestinal worm has been described by M. Dumeril, under the designation of opiostoma pontieri. Two examples are recorded of intestinal worms escaping by the umbilicus. One by Dr. Hecking, in which several lumbrici (spulwurmer) escaped from an abcess over the umbilicus, which closed shortly after. A second by Dr. Siebold, in which a tænia solium escaped from the same situation, and in the same way, without the discharge of fecal matter or gas, or any indication of perforation of the intestine. The fasciola hepatica, so seldom found in the human subject, has been detected in the vena porta by M. Duval. The species of demodea (acarus folliculorum) described by MM. Simon and Wilson, has been discovered by Mr. Topping in the pustules of a mangy dog. Vegetable fungi found in the air-sacks of birds, in the mouths of newly-born children, and in the crusta of porrigo favosa, are considered by M. Rayer as invariably secondary productions. Dr. Remak has succeeded in transmitting porrigo favosa by inoculation with the fungi. M. Mandl states that the tartar of the teeth consists of the calcareous carapaces of defunct vibriones, which, abound in the buccal mucus. Dr. J. Hughes Bennett found, in recently expectorated sputa of a man in the last stage of phthisis, with pneumothorax, cryptogamic vegetation, consisting of jointed, transparent tubes, giving off several branches, mingled with numerous round or oval globules.

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3. *On Valeric Acid, and the mode of obtaining from the valerian root.*—Since the valerianates have acquired claims to the attention of the profession, it has been desirable to improve the method employed for obtaining this acid, so as to diminish the price of these salts. The following method recommends itself, on account of its simplicity, and the large amount of the product:

Professor Giovanni Righini submitted to the section of chemistry of the sixth scientific Congress of Milan, an essay showing that valerianic acid does not exist in a free state in the root of valerian; but requires a temperature of 230° Fahrenheit, to produce it by a transformation of the volatile oil of the root. He shows too, that the high temperature which is necessary to the development of the oil, together with some other immediate products of the root, acting on this oil, determines its composition, and the acid is thus produced.

He operated with twenty-seven pounds of the root cut up in small pieces, and bruised in a mortar, gradually adding as much water as was, when submitted to pressure, sufficient to extract the natural juices. The liquid obtained by pressing was put into a basin and heated to ebullition: carbonate of lime or lime-water was then added and valerianate of lime thus precipitated; and after allowing it to stand two or three hours, water acidulated with hydrochloric acid was poured on the precipitate. This combined with the lime, and the valerianic acid was rendered free. The whole was then submitted to a moderate temperature, and the evaporation continued until a pellicle formed on the surface; then the liquid introduced into a retort and distilled by the sand bath, yield the valerianic acid; the chloride of calcine remaining as a residue in the retort. The distillation should be continued to dryness.

By this method the above quantity of the root yielded five and one-third ounces of pure valerianic acid.—*Journal de Chemie Medicale.*

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4. *Physiological Anomaly, of a he-goat giving milk.*—M. J. Geoffroi-Saint-Hillaire made a verbal communication to the Academy of Sciences, respecting the curious anomaly offered by a he-goat, which was recently presented to the museum. This animal possessed all the attributes of the male sex; and, in addition, has a pair of teats likewise, as well developed as in the she-goat. The milk which they yield in abundance has all the physical characters of common goats-milk, and differs in taste, only in being a little more saline. The analysis which M. Chevreul is to make, will show if it differs in chemical composition. This animal is not, as might be supposed, hermaphrodite, for the male organs are completely and fully developed.—*Gaz. des Hopitaux*

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5. M. Piorry strongly recommends a nourishing diet in typhoid fevers, as preferable to the severe diet which many French physicians enforce for a fortnight, or even a month.—*Gaz. des Hopitaux.*

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6. M. Blandin, in reference to the treatment of *fissura ani*, concludes, that, when there is fissure of the anus, with spasmodic contraction of the sphincter, whether this be primary or consecutive, it is necessary to operate. This may be formed in two ways: by Boyer's method, which consists in making a free cut through the muscle; and by the sub-cutaneous method. He recommends the sub-cutaneous method, as it has among other advantages, that of healing more promptly, and is not so apt to give rise to serious accidents, such as phlebitis, for example. In a word, it promises all the advantages of Boyer's method, without many of its inconveniences.

He prefers making the incision laterally, as by cutting anteriorly. there is risk of wounding the bladder, and posteriorly, in order to divide the muscle, it makes too deep a wound.

The muscle once divided, it is necessary to keep the anus dilated, for some time; to excise any little excrescences, and to treat the fissure by topical applications.—*Gaz. des Hopitaux.*

7.—*Spermorrhœa cured by pressure applied to the Perineum.*—In an article contained in the June No. of the Annales de Chirurgie, J. L. Brachet of Lyons, says that a respectable citizen of that town had used pressure applied to the perineum, as a means of curing spermorrhœa, and learning from him what had been the result of the treatment, he had employed it in a number of cases with complete success. He does not propose it as a substitute, in all cases, for Lallemand's treatment, but thinks it applicable to many, even in which the other treatment had failed. The cases treated by him had originated from the usual varieties of causes, such as gonorrhœa, masturbation, and other venereal excesses; in some, the emissions were nocturnal, in others, both nocturnal and diurnal, and others continued and unperceived. The debility, emaciation, and other deplorable consequences of this affection existed in the different cases, and some were brought to the brink of the grave, when the pressure was applied, and in all the reported cases, a cure was effected.

In some of the cases, all the ordinary remedies had been employed, such as ferruginous tonics, baths; and even cauterization, without any apparent advantage.

M. Brachet does not describe the particular apparatus used; but states that, in some of the cases, the pressure produced a considerable degree of irritation in the parts over which it was applied, which caused its use to be suspended, until the inflammation induced could be combatted by appropriate treatment. This symptom, which sometimes existed to an extent which led to the apprehension of prostatic, urethral, or vesical inflammation, was regarded as a favorable indication, as the artificial irritation replaced, or modified, the existing pathological condition. In some cases, the pressure was continued until a cure was effected, and in others, its use was suspended whenever the irritation became considerable, and reapplied when this was subdued. The time required for relief was from one to three months.

M. Brachet says, that the few cases treated by him, are insufficient to enable him to establish general rules, or any positive precepts: but merely desires to call the attention of the profession to the experiment, that they may by repetition prove either useful or valueless.

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8. —*Functions of the Pancreas.*—MM. Bouchardat and Sandras, following out their researches on the chemical phenomena of digestion, have recently ascertained that the pancreatic juice possesses the same properties as the saliva. This liquid, taken from the pancreas of strong barnyard fowls, was transparent and viscous, presenting a slightly alkaline reaction. Mixed with amidon jelly, it liquified it, and transformed it into dextrine and glucose. By adding alcohol, it formed a white deposit, which also acted on the jelly of fecula in the same manner as diastasis. A few fragments of the gland, mixed with starch, tepid, and very consistent, convert it, after a few minutes, into a liquid free from viscosity. Pounded and mixed with water, they give a fluid, which with alcohol, yields a flaky precipitate endowed with the power of dissolving fecula. We may therefore conclude, from these facts, that the principal function of the pancreas is to secrete a liquid able to dissolve feculaceous substances in the digestive process.

Diseases of the Pancreas. Dr. Claessen's treatise on diseases of the pancreas contains a vast amount of materials for the elucidations of diseases of that organ. In reference to diagnosis, he states that though in thirty cases there was a watery discharge from the mouth, he objects to the inference that this intimates either increased pancreatic secretion or vicarious action of the salivary glands. He rather refers it to the stomach, more particularly as the pyrosis was frequently associated with vomiting and other evidence of gastric disturbance. He therefore, places no confidence in the diagnostic value of pyrosis. Pain and costiveness are frequent symptoms of pancreatic inflammation. Dr. Batersby has also collected a great deal of valuable information on the obscure subject of pancreatic disease. In one of the cases detailed by himself the diseased pancreas was at first mistaken for aneurism of the aorta; and in a second case, disease of the pancreas was diagnosticated by some German physicians attending Dr. Grave's clinique, from the extreme moisture, cleanness, and macerated appearance of the tongue and mouth generally. In the former cases the same state of tongue existed, and there was also salivation. Dr. B. alludes particularly to the diagnostic importance of both salivation and pyrosis, and of the sympathy existing between the buccal and abdominal salivary glands.

9,—*Albuminuria, Causes of.*—M. Fourcault's valuable investigations into the effects of suppression of the cutaneous secretion, have shown that albuminuria can be thus readily induced. He supposes that this effect is produced by the excess of lactic acid which is then found in the blood, and which reacts on the albumen. The introduction of lactate of soda into the veins also produces albuminuria, by favouring the excess of lactic acid in the blood. When the acid secretion of the skin is suddenly checked, it produces a marked change in the organic elements of the blood; and when gradually suppressed, a number of chronic diseases are produced, among which are albuminuria, scrofula, lepra, &c. &c. He admits, however, that albuminuria may also, though rarely, originate in a primary affection of the kidneys. Dr. Meyer of Tubingen, concludes from his researches, that albuminuria may be produced by an accumulation of blood in the kidneys, (without any organic alteration of their structure) either from augmented arterial supply, or stagnation in the veins. In this way he accounts for albuminous urine in diseases of the heart and lungs; and his conclusions are deduced from five experiments on animals, in some interrupting the flow of venous blood, and in others tying the aorta below the origin of the arteries; in all of which cases the urine became albuminous.

—*Pathology and Treatment of.* Dr. G. O. Rees, assuming that most observers are now agreed that the blood-corpuscle consists of a membranous sac inclosing colouring matter, has directed attention to the extreme tenuity of the blood in certain stages of the morbus Brightii; and shown how this condition constitutes the true cause of the deficient proportion of hematosine observed in the later periods, inasmuch as it must interrupt those endosmotic changes occurring between the contents of the corpuscle and the chyle, when each fluid possesses its ordinary specific gravity. The increased quantity of water circulating in the early stages, he considers to be caused by the discharge of albumen by the kidneys. The iron which colours the contents of the corpuscles he believes to be communicated by the *aqueous extractive* of the chyle which passes into the corpuscle by endosmosis; and this process being interrupted by the abnormal tenuity of the blood, the red corpuscles are diminished. In a subsequent paper, after pointing out the analogy in the pathology of various forms of anemia, to morbus Brightii, he recommends in the early stages the same plan of treatment that is found beneficial in chlorosis and anemia from loss of blood, viz: chalybeate tonics, saline purgatives, and nutritious diet, which though not immediately calculated to remove the condition of kidney known to exist, he has has found efficacious in preserving the normal state of the blood, and thus assisting in recovery. He condemns any attempt to relieve the nephritic congestion by depletion, but recommends counter-irritation and dry cupping. Numerous instances have been recorded of granular degeneration of the kidney, even in an advanced stage, unattended with albuminous urine; and of persistent albuminous urine, independent of any structural disease of the kidney.

An instructive series of cases illustrative of albuminuria, arranged so as to exhibit the influence of particular remedies or plans of treatment, has been published by Drs. Bright and Barlow. Dr. Alken has found hydriodate of potash and iodine ointment useful; and Dr. Gutbrod having observed in the Vienna hospital great benefit from iodine, tried the ioduret of iron in two well-marked cases in the advanced stage, and with the best results. M. Monneret obtained great improvement in one case from tinct. cantharidis, in doses increased up to 60 drops; and in another case, from the use of vapour baths.

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10. *Condition of the Spleen in Intermittent Fever,.*—M. Piorry, in a memoir presented to the Academy of Sciences, has given the conclusions to which he has been led by the consideration of 165 recorded cases, and upwards of 1000 others of which he has no written record. The condition of the spleen was ascertained by means of the plessimeter and percussion, and the results are, therefore, in the author's estimation, of the utmost certainty. From the analysis of 163 cases, he considers it certain that ague occurs, independent of miasmatic causes; and that in many instances it arises from falls, blows, and inflammation of the spleen. Enlargement of this organ is so frequent in ague that, in 154 of 161 cases, it exceeded the normal size, and in four of the remaining seven it was painful, which was also the case in eighty-two of the whole number. Splenic pains sometimes precede the fever. Organic affections of this viscus may either pro-

duce or keep up intermittents. He thinks there is no evidence that any persistent alteration of the blood can directly produce ague. Miasmatic causes act through the nervous system of the spleen. Sulphate of quinine quickly dissipates a large majority of the cases of enlarged spleen, and even in its healthy state its volume may be reduced by the introduction of quinine through the stomach or bowels. In two cases he thinks fatal hæmorrhage might by attributed to the too rapid diminution of the spleen under the influence of quinine; hence the dose of this medicine should be proportioned to the enlargement. The quinine, he believes, is absorbed by the veins, and cures ague by its direct action on the spleen. A case of intermittent fever observing a septan period, is recorded by Dr. Laroche (père) of Angers.

M. Piorry has, in addition, recently reported cases. (*Gazette des Hopitaux*) in which very obstinate intermittent was produced by mere displacement of the spleen. In one case of a woman thirty-four years old, the spleen was found in the superior part of the left iliac fossa; very moveable, so as to be easily returned to its normal position under the ribs. It was but little, if at all, enlarged.

Whenever she walked, sharp pains were experienced in the part occupied by the spleen, which immediately became larger, and at the same time a paroxysm of fever came on; which might lead to the false notion that the enlargement of the spleen was due to fever. The following is the order of the phenomena: displacement of the spleen; dragging of the splenic plexus; congestion of the spleen, and finally the febrile paroxysm. By a long and careful examination, it was ascertained that in each of her pregnancies, she was free from the febrile attacks; which was, no doubt, due to the development of the uterus, which pushing the spleen upwards, restored it to its normal position.

The sulphate of quinine had been frequently tried during the twenty years she had been suffering; but it was again resorted to in larger doses. The consequence was, that the spleen diminished sensibly in size; which proves that the remedy acts upon that organ. The patient even had days in which she was clear of the attacks; but this was explicable rather by her lying down, thus relieving parts otherwise dragged by the spleen; and whenever she attempted to walk, she was again attacked.

When a sufficient trial of the quinine had been made to show that it would not cure this case, an apparatus was applied which served to retain the spleen in its proper place; when she was entirely relieved, and she wished to leave the Hospital. She was, however, retained some time, and induced to walk a great deal every day. She had no more paroxysms, and though frequently seen since, has remained entirely free from them.

11.—*Spleen, removal of.* M. Berthet de Grey relates the following case: A middle aged man received a wound in the side, through which the spleen eventually protruded, and becoming gangrenous, was removed. The man recovered and lived thirteen years, enjoying sound health, his digestion being usually good. After death, produced by pneumonia, all that remained of the spleen was found to be a small portion of the size of a filbert, adhering to the stomach. Mr. Eagle asserts that fattening and cicatrization of the tubercles were the results of the removal of the spleen in his experiments on rabbits affected with tubercle (?) of the liver and marasmus; he therefore proposes to tie the splenic artery in patients moribund from inanition, arising from disease of the nutrient circulation, rather than from structural disorganization, as in some cases of phthisis and marasmus! !—*Brit & For. Med. Rev.*

Tympanitis. In a case in which intestinal disorder was attended by great tympanitic distension of the bowels, M. Levrat (ainé de Lyon) had recourse to paracentesis of the small intestine over the most salient point. The operation was performed with a trochar of the size of a stocking-needle [apparently similar to Dr. Babingston's,] and gave immediate relief. Fifteen days afterwards the patient was about his business. In the case already referred to of so-called gangrene of the lung, terminating by perforation of the diaphragm and peritonitis, which was attended by great tympanitic distension of the abdomen, the peritoneum was four times punctured with considerable temporary relief.

DEATH OF DR. JAMES JOHNSON.

We find in the December number of the Bulletin of Medical Science, (Philadelphia,) the following announcement, which fills us with deep regret. The name of James Johnson is known wherever medicine has an enlightened votary, and is associated with profound attainments, ardent professional zeal, and indefatigable industry. He has truly been an ornament to his profession, and a benefactor to the human race. *Requiescat in pace!*

"The profession has to regret the loss of one of its worthiest and most accomplished members, in the death of Dr. James Johnson, long known in medical literature, as editor of the Medico-Chirurgical Review. He expired at Brighton, after a short illness, on Thursday, the 9th instant, (October,) in the 69th year of his age.—(*Medical Gazette, October* 17.)

AMERICAN MEDICAL INTELLIGENCE,

Four cases of Trismus, or Tetanus Nascentium.—By Augustus Eberle, M. D., Jefferson County, Kentucky. First Case.—October 10th, 1844, was called in haste to see the infant daughter of Mr. James J. F——, æt. seven days, and learned that its bowels had been deranged for two or three days—it appeared to be restless, laughing incessantly; slight rigidity of the jaw, and refusing the breast.

This being the first case that I had ever seen, and having paid little or no attention to this disease, we only ordered such remedies as we thought proper to correct its bowels; also, warm baths. As yet no spasm had seized it, but, soon after our departure, they came on, and quickly terminated the life of the little sufferer.

Second Case.—October 29th, 1844, about midnight, was called to see the infant son of Mr. R. T. R——, æt. seven days, and found it labouring under the following symptoms:—tetanic convulsions of the whole body; jaws closed; and upon any attempt to give anything *per orem*, the symptoms were aggravated; face livid and distorted; also, general rigidity of the muscles; deranged bowels.

This child, the mother informed me, was, at bed time, to all appearances, well, and that she was awakened by the child having a hard fit whilst sleeping in her arms.

We ordered a warm bath, as hot water was ready, and to repeat in fifteen minutes, until a large mush poultice could be prepared, and have sub-mur. hydrar. grs. 2, pulv. rad. rhei, grs. 1, every two hours, also, tinct. opii, gutt. 1-6, every hour. The poultice to be kept continually on, extending from the stomach to the feet. The symptoms increased in violence, and it appeared impossible to produce the least narcotic effect by the opium, and within twenty-four hours the patient died in a violent convulsion, attended with hæmorrhage from the mouth and nostrils.

Third Case.—May 6th, 1845, was called see a negro infant, æt. nine days, and found it labouring under the following symptoms:—body stiffened; continual crying; tetanic convulsions; face contorted; thumb thrown across the palm of the hand, and there firmly held by the other fingers; bowels costive; jaws closed; had refused the breast two days ago, but takes food greedily; the symptoms aggravated upon handling. The umbilical cord had fallen off in all the cases, upon the third day, leaving a perfectly healed surface,

This being the third case that had fallen under our care in a short time, and having fully made up our opinion as to the nature of the disease, we considered it to be Traumatic Tetanus, arising in our mind, from a division of the cord, together with the falling off of the same, leaving a perfectly healed surface, and we are now more fully convinced, seeing the effects of blisters in two cases.

Under our view of the case, we applied a blister two inches square to the umblical region, and one to the inferior portion of the occiput, extending two inches down the spine, gave sub-mur. hydrar. grs. 3, to be followed by oleum ricini, also, tinct. opii. gut. 1-6, tinct. assafœtidæ, gutt. 6, every hour, and to take every thing from the head.

May 7th.—The symptoms moderated; blister on the naval drew well; the one on the back of the head did not; still unable to suck. Treatment same, with the exception sub-mur. hydrar.

May 8th.—Condition improved; no spasm since last night; blister on the naval running; continue tinct. opium and assafœtidæ.

May 9th.—Still improving; unable to take the nipple; blister running; gave sub-mur. hydrar. grs. 2, pulv. rad. rhei, grs. 3, and in six hours, oleum ricini. drs. 2; continue tinct, opium and assafœtidæ.

May 10th.—Still improving; blister dry; still unable to take the nipple; gave oleum ricini, and omit other medicines.

May 11th.—Still improving; using the sucking bottle; omit medicines.

May 12th.—Found the patient in the same condition that it was on my first visit; ordered a blister to be applied to the umbilical region; gave tinct. opii, gutt. 1-6, tinct. assafœtidæ, gutt. 6, every hour, also, sub-mur. hydrar. grs. 2; pulv. rad. rhei, grs. 3, to be followed in four hours, by oleum ricini, drs. 2; discontinued sucking bottle, and fed with spoon.

May 13th.—Blister drew well, symptoms moderated; continue drops, and give oleum ricini at night.

May 14th.—Blister running; no improvement; same treatment.

May 15th.—A slight improvement; blister running; medicines the same.

May 16th.—Improved; same treatment.

May 17th.—Blister still running; improving; discontinue tinct. opium and assafœtidæ.

May 18th.—Blister running; improved; sleeps well.

May 19th.—Blister still running. This day, for the first time since its sickness, it has taken the nipple, and sucks well. Give oil at night.

May 20th.—Blister running; ceased attendance—hardly able to say whether the child will be deformed or not, on account of the rigidity of the muscles which still remain, and the face having the appearance of one worn out by pain and fatigue.

August 20th.—The child since my last visit, has improved a great deal, and presents the appearance of a fine healthy child.

Fourth Case.—July 14th, 1845, my own infant, æt. eight days, was attacked with the following symptoms:—extreme restlessness, continual twisting of the whole body, appears to be in great misery, smiling and laughing incessantly, and occasionally utters a peculiar cry—hands closed to some extent, and the thumb thrown across the palm of the hand, puckering of the mouth, a continual changing of the countenance; bowels, costive. Tho cord had fallen off on the third day, leaving a perfectly healed surface. No spasm had made its appearance.

We considered this case one of this formidable disease, and therefore commenced immedtate treatment—put blister two inches square to the umbilical region, aud kept in on for six hours, and then dressed it with savin ointment until the blister had filled, and afterwards with mutton suet. Immediately upon the filling of the blister, an evident improvement took place, and the child rested well that night. At the same time gave tinct. opii. gutt. 1-6, tinct. assafœtidæ gutt. 4, tinct. castor. gutt. 1, every two hours, and oleum ricini, drs. 2, with sub-mur. hydrar. grs. 3, which produced copious operations. We continued the use of the anti-spasmodics for two days, and gave castor oil for three, and the blister having drawn well, it was left to itself, only dressing it night and morning, (never washing it), which ran for seven days, and after five days the child was considered safe.

From the symptoms in this case, we believe spasms would have made their appearance but for the application of the blister.

Maunsel, in his work on children, says, "the child appearing to smile during its sleep, is also considered as an indication of the approach of fits," which was a very plain symptom in this case.—*Mo. Med- and Surg. Jour.*

Remarks.—This is a most unmanageable disease, which destroys nearly one hundred and fifty children annually in the city of New Orleans. Our mortality report for October shows 11 deaths from it, and for November, 12. This is about the monthly average. The suggestions of Dr. Eberlie in regard to the use of counter-irritation about the umbilicus, and correcting the intestinal secretions, are valuable. We should be pleased to have a communication from some of our city practitioners who have seen much of the disease.—*N. O. Eds.*

2.—*Extraordinary case of Protean Malady.*—In a communication to the New York Journal of Medicine, (Nov. 1845.) by Dr. James M. Gardner, of Newburg, N. Y., under the title of "*Brief Notes of Cases in Practical Medicine, Surgery and Obstetrics*," we find the following remarkable case, which is certainly worthy of being filed amongst the *curiosities* of medical experience.

The subject of the very singular case which I now propose to condense from my notes is a very beautiful young girl of seventeen years; her complaint commenced at the age of fifteen, and she may now be said to have entirely recovered. At the present writing, January 3, 1837, she has called, with her mother, on foot, a short distance, to wish me a happy new year. Many physicians in the county have seen this patient, and can bear witness to some of the singular phenomena that the case has presented. She has been visited by many intelligent persons, not physicians; by several ministers of the gospel; and her case has been one highly interesting to the profession; inexplicably wondrous to the curious, and indissolubly connected with witchcraft by the ignorant. As to a name for this very singular congregation of symptoms, I think the best to be the *Protean malady*; for, I must say, that, during my attendance on this case, which has been constant, I do not recollect scarcely a disease of which I have read in Cullen or Good, the symptoms of which she has not at different times exhibited. To detail the symptoms of this case *seriatim* would occupy too much time and space. I cannot do better, therefore, than to follow Dr. Torbet's plan, and quote his words in the description of a case similar, which has lately occurred in his practice in Paisley, Scotland.

Symptoms in the Head.—They have headache, of which no words can convey an idea; ending, perhaps, in insensibility—or there is a heavy weight pressing down their eyelids—a tight bandage compresses, or hammers beat within their heads, bells ring in their ears, vertigo and sickness overpower them, noise and light disturb them.

In the chest.—A sepulchral cough, each fit of which seems to threaten existence, over which medical aid seems to possess no control, harasses, from morning till night and night till morning. There is excruciating pain in the chest, especially during the cough, which is perfectly devoid of expectoration. There is a feeling of swelling and choking at the under part of the neck, tremulous palpitation of the heart, the pulse is quick and agitated, but varying greatly with the severity of the cough, and the heart's pulsations seem to occupy the whole left side of the chest.

Digestive Apparatus.—Vomiting without apparent cause, incessant retching for hours, occurring, perhaps, every day for weeks, without anything being vomited; no appetite, unquenchable thirst, occasional severe pain in the belly, or muscular cramps, and costiveness, alternating with diarrhœa; the stools unaccountably copious.

Organs of Voluntary Motion.—Cramps, prickling or numbness, sometimes severe spasms, more like tetanus or hydrophobia than anything else, ending possibly in catalepsy; or in paralysis of one or more limbs. Such, says Dr. Torbet, is an epitome of the symptoms of the more violent cases; but almost endless variety must of necessity occur, according as the several nervous centres are affected, and their affections modified by individual circumstances. All of the above symptoms, and many more, were seen in my patient. To detail the endless variety of symptoms in this case would be a tedious task, but I will merely add, in concluding the symptomatology of the case, that, together with a deprivation of voice and speech, every affection that a medical man can conceive of to attach to the muscular system was here present, even *Chorea* and *Hydrophobia.* In extending the same remark to other systems, I will but have given a true picture of this singular case. But the most curious part of this disease iremains to be told. After the patient has so far recovered as to be sensible of ampression on the organs of sense, she was found to have lost all her memory, And all recollection of any transaction previous to and during her sickness. the did not know her relatives or attendant—she had forgotten the name of any article presented to her, together with the power of pronouncing it had she even recollected it. By degrees she slowly learnt, by the industry of her attendants, to articulate. She was then, in all respects, treated as an infant about to learn to talk—she was pleased with every little toy presented to her, and her bed for a long time presented the appearance of a toy shop. By degrees she relearned her spelling, and was progressing in writing, still ignorant, however, of the relationship of her father or mother, or sisters or brother, and scarcely recognizing them from strangers. When at about the end of the third month from the time when her organs of sense regained their functions, she was suddenly seized whilst sitting up in bed with violent acute pain in the site of the sagittal suture, causing her to cry out. The pain lasted a few minutes and went off with a rumbling cracking noise at both ears—her countenance, for a moment or two, wore the look of wild surprise—tears unbidden gushed forth, and a full and perfect consciousness of existence was instantly established. She now called her mother, father, and other relatives by name—she took the book presented and read with ease; the pen and ink were produced and she wrote. She recollected the scenes of her childhood and youth; in short, her memory of all transactions previous to her sickness was restored, but the story of her wonderful sickness repeated over and over to her, with all the particulars, was to her a tale of wonder, of which she, the subject, knew nothing. It ever will remain a blank in her existence.

In reflecting on the probable cause of an assemblage of symptoms so strange and opposite, I am inclined to think that Dr. Torbet has truly divined the cause; at least it appears to me that no other can reasonably be offered, viz: a neuralgic affection of the ganglionic system of nerves commencing at their different centres. In the case I have detailed, I think the phenomena warrant the conclusion that such was the case primarily, and that the neuralgic affection of the spinal and cerebral nerves was a secondary effort, occurring sympathetically, or in consequence of that intimate connexion subsisting throughout the nervous system.

Could the formation of false membrane on the surface of the brain during this patient's sickness (itself an effect) yet become a cause for the loss of memory, and might not the gradual thinning by absorption of this membrane, as the recovery progressed, occasion a sudden disruption of it, and would such a supposition be entitled to any weight in accounting for the immediate restoration of memory? These are questions perfectly conjectural, and which I offer with great diffidence for consideration.

MEDICAL CONVENTION IN MISSISSIPPI.

We are pleased to learn that the Physicians of Mississippi have resolved to hold a Convention in the city of Jackson, on the second Monday of January, for the purpose of appointing delegates to the National Convention in New York in May next. We are indebted to our esteemed correspondent Dr. W. G. Williams, of Rodney, Miss., for the proceedings of the physicians of Jefferson county, at a meeting, over which Dr. John A. Duncan sr. presided, and Dr. Williams, acted as secretary. The following gentlemen were appointed delegates to the State Convention, viz: James Andrews, M. D., F. B. Coleman, M. D., Thos. H. Young, M. D., and W. G. Williams, M. D.

We learn that the physicians of Natchez and Vicksburg have also appointed delegates, and hope there will be a general attendance. We are pleased to perceive that the proposition for a National Convention meets with universal favour, and doubt not it will be one of the most interesting assemblages, ever held in the country. We are sorry we have not more space to devote to the subject.

NEW ORLEANS. JANUARY 1, 1846.

HEALTH OF THE CITY.

We have nothing of peculiar interest to offer in regard to the prevalent diseases of the day. The general health of the city may be said to be very good. Scarlatina still prevails to a moderate extent, and in the opinion of some of our physicians, in a very questionable form. They doubt whether the disease now prevailing, and generally termed scarlet fever, is really that disease—they think it may be some other eruptive fever—and generally of so mild a character as hardly to be entitled to any distinct appellation.

Notwithstanding the questionable form of many of the cases that have occurred, we ourselves cannot doubt that scarlet fever has prevailed here throughout the year. And the result our inquiries is that those who use the least medicines are the most successful in its management.

As far as we have ascertained, the mortality from it is very trivial. A few cases of pneumonia, continued fever, sore throat, catarrh, &c., are to be met with. There have been but two cases of small pox admitted into the Charity Hospital. The disease was introduced here by arrivals on ship. We cannot impress upon our citizens too strongly the importance of strict attention to vaccination, and even re-vaccination, where a period of ten years has elapsed since it was first performod. The neglect of this salutary precaution is extremely culpable. We have small pox in this city every winter, and it is frequently carried to the interior of the country. If vaccination were properly attended to, small pox would be robbed of its terrors.

We have had two or three spells of exceedingly cold weather, during which the ground was frozen, and plenty of ice to be seen. By reference to our Meteorological Table, it will be seen that the thermometer has been as low as 26°. Our cold spells seldom continue longer than four or five days, but the changes are often so sudden and great that even this moderate degree of cold is felt as keenly, and complained of as bitterly, as any experienced at the North; this is remarked by our citizens from the North. And it may be readily accounted for by the unguarded state in which our systems are placed by the general prevalence of warm, and frequently wet weather at this season. We have had a great deal of rain during the month of December, and if there has been a corresponding amount of snow in the region above us, we may expect a great rise in the Mississippi at a future period. The river is lower at this time than it has been for fifteen or twenty years. We learn that navigation even to Memphis is extremely difficult. Bald and bleak sand-bars, and myriads of firmly set snags and sawyers have emerged from their abyss into the light of day, and remind the steamboat traveller of the thousand dangers over which he has so often been borne in safety.

—As our journal is issued on the first day of the month it is impossible for us to furnish in this number, annual statistics in regard to the mortality of our city, and the admissions, discharges, and deaths of our hospitals. In our next number we hope to furnish a comparative statement of these, with other cities. During this year a strict account has been kept of the mortality here, and the comparison may develope some curious and unexpected results. It is much to be regretted that a registry of births is not enforced in this city, as it would afford valuable and interesting statistics. The time will surely come when a more rigid police will exact attendance to this matter.

HEALTH OF THE COUNTRY.

We must entreat our correspondents to be more punctual in their communications. Whether they have any thing of importance to communicate or not, it will always be deemed interesting, and we should like to furnish in each number of our Journal, a glance at the existing state of health throughout our region. The correspondents whose letters we give below, will please accept our sincere thanks for their kind attention. Our esteemed friends at Huntsville, Ala., and Nashville Tenn., will perceive that their dates are rather ancient; will they please write so that their letters may reach us by the 20th of the month preceding publication. The facts related by our Nashville correspondent are very interesting; an accummulation of such from every section of the country, may lead to important results in the investigation of the remote cause of our summer and autumnal epidemics. We would respectfully refer our correspondent to an interesting summary of what has been said about *malaria* in Watson's Lectures.

We regret that we have not more letters to give in this Number on the *health of the Country*; and we sincerely hope that the appeal which we now make will not be disregarded.

So far as we have been able to learn, the country is uniformly healthy at present.

Huntsville, Ala., Oct. 20, 1845.—Our correspondent writes as follows: "In Huntsville and the immediate neighborhood, the health of the inhabitants has continued to be unusually good. In some portions of the county, the northern part particularly, a continued fever of a congestive character prevailed during the months of August and September. Although tedious, the disease did not, in many cases, prove fatal. The physician who had the management of most of these cases, informs me, that the remedy most relied upon by him, and most successful, was the cold bath, freely and frequently applied, conjoined with mild aperients, &c.

"We have not been visited by any epidemic, this season, and not with our usual quantum of bilious, intermittent and remittent fevers."

Nashville, Tenn., Nov. 6, 1845.—OurNashville correspondent writes:—"Since I wrote you last, we have been dreadfully scourged with disease. Although our town had been healthful as usual, the surrounding counties of Middle Tennessee, and the adjacent counties of Kentucky, have suffered more from sickness, than the "oldest citizen" can remember at any former period. The disease has been principally fever, and of the ordinary remittent and intermittent types, requiring, as far as I have observed and inquired, no important modification in the treatment.

"The disease commenced about the middle of September, and continued with unabated violence until about the 25th of October, when it ceased with the approach of cool weather, and very suddenly. I have not been able to ascertain that one locality has been more liable to its ravages than another; the high, hilly, and even mountainous districts; as well as the flat rich country, have been alike sufferers. And during its severest form, the country was perfectly dry, not having had rain for many months; the beds of the creeks and ponds, were too dry even to afford *moisture* enough to justify Dr. Cooke himself in tracing the disease to *malaria.* And those regions of country at great distances from marshes, ponds, and rivers, havs presented the same disease, identical with the cases which were seen on the sides of dried up ponds and creeks.

"It has been said that *a quart* of water is sufficient to produce malaria what will affect the health of a whole neighborhood. To such a medical *philosopher* it would be useless to present the facts I have stated above. I think these facts deserve due consideration, not as isolated facts, but as *probably* connected with many parallel cases. I acknowledge that I have not been at all satisfied with the malarious origin of this wide spread epidemic. If parallel cases are brought to our knowledge, it would be well to notice their similarity, and the circumstances connected with them. One fact has struck me, this year, as having some agency with the disease. Tennessee, you know, is decidedly a *bacon* country; it is the staple article of food with almost every family; and this year, for the first time in a long series of years, the supply has given out, and four-fifths of the families in the country have been compelled to live on fresh meats. Such changes in diet are always calculated to produce disturbed action in the animal economy. Another fact: the towns are daily supplied with fresh meats, and use them as well as bacon; and the change this year has not been so sudden or as great ot them as to the country; and it is a fact that the towns have been comparatively healthful.

"I throw out these suggestions merely by way of hints."

Montgomery, Ala., Dec. 12, 1844,—Our correspondent from this place writes: "Our place is extremely healthy, and the very few cases of disease among us are sporadic, no one predominating sufficiently to engross attention; nothing of an epidemic character. Slight febrile affections, catarrhs, and an occasional case of pneumonia and bronchitis are presented to us; but the cases of this character are much less numerous than is usual at this season of the year."

Woodville, Miss. Dec. 12, 1845.—Our Woodville correspondent writes; "I have nothing to say, this month, of sickness, as there has been, and is now, a complete dearth.

"The weather has been remarkably cold and severe for the last three weeks, attended with rain, sleet and ice, such as has not been known at the same period, for a number of years in this lattitude. The mercury has been as low as 24°. Sunday, 30th Nov., was the coldest day we had.

"Since the 11th Oct. up to date, there have been rains on thirteen days. During the latter part of that month, and nearly all of November, the weather was clear and beautiful. The winds have been generally from the N. and NE., sometimes SE."

MEDICAL COLLEGE OF LOUISIANA.

The annual course of lectures commenced on the third Monday in November, to the largest class that has ever been in attendance upon this institution. Medical students are beginning to appreciate the superior advantages presented by this city for the study of their profession. We observe among the class several students who have attended the lectures of other colleges—attracted here, doubtless, by the fine opportunities of seeing disease, and of studying anatomy. We are pleased also to see several practitioners from the interior, who have come to spend as long a time as they can stay from home, for the purpose of refreshing and improving themselves, Our hospitals are always open to visitors, and the professors invite all graduates to attend their lectures, *gratis*. We are fully aware that the life of a dependant medical practitioner is one of almost abject servitude. He never knows when he may be wanted, nor can he leave home without sacrifice. Yet the occasional sacrifice of a couple of months from his practice spent in visiting the congregations of the sick, in attendance upon lectures, and in dissecting, will be amply repaid by the increase of professional knowledge acquired. Besides, nothing is better calculated to give an active impulse to study and close observation; without which no proficiency in medical knowledge can ever be attained. Our science is rapidly progressing, and he that does not avail himself of the general diffusion of knowledge by the press, or attend a course of lectures occasionally, will very soon find himself behind the age. Upon our profession devolve the weighty responsibilities of the health, happiness, and even the lives of our fellow beings; and *his* must be a stony heart, that can trifle with matters of such serious importance. We are generally poor, and for the most part miserably compensated for the toil and corroding anxiety we undergo, it is true; yet it is a God-like task to minister to the various afflictions of our race, and he that dovotes himself to it, should not be idle, for he must reap the reward either of honor, or shame and remorse. We must look *beyond the dollar*, or we will not be able to appease the silent but ceaseless monitor within our own breasts.

Our city is so convenient and accessible to a large number of surrounding practitioners, that we sincerely hope we shall have the pleasure of seeing many of them here every winter.

HOSPITAL REPORTS.

NEW ORLEANS CHARITY HOSPITAL.

The term of service of the late visiting physicians and Surgeons of this Institution having expired on the 1st of November, the following gentlemen were chosen to serve until May next; viz:

Surgeons.—Warren Stone, M. D. and A. J. Wedderburn, M. D.

Physicians.—James Jones, M. D., W. M. Carpenter, M. D., A. H. Cenas, M. D., Y. R. Le Monnier, M. D., W. M. Rushton, M. D., E. Martin, M. D., C. Turpin, M. D., and C. Faget, M. D.*

Of these, the six first named are Professors in La. Medical College. Professor Stone delivers Surgical cliniques and Prof. Jones Medical cliniques on Wednesdays and Saturdays. The Medical class appear to be very attentive to Hospital visits, and have fine opportunities for seeing disease.

Monthly Report for October and November.

MAIN BUILDING.

October—Admitted: Males, 575; Females, 84; Total, 659.
Discharged: Males, 537; Females, 72; Total, 609.
Died: Males, 48; Females, 5; Total, 53.
Remaining on 1st. November, 343.

INSANE DEPARTMENT.

Admitted: Males, 26; Females, 7; Total, 33.
Discharged; Males, 18; Females, 6; Total, 24.
Died: Males, 5; Females, 2; Total, 7.
Remaining 1st November, 66.

MAIN BUILDING.

November—Admitted: Males, 535; Females, 74; Total, 609.
Discharged: Males, 463; Females, 79; Total, 542.
Died: Males, 38; Females, 9; Total, 47.
Remaining on 1st December, 347.

INSANE DEPARTMENT.

Admitted: Males, 23; Females, 13; Total, 36.
Discharged, Males, 17; Females, 8; Total, 25.
Died: Males, 1; Females, 0; Total, 1.
Remaining 1st December, 73.

SURGICAL WARDS.

There have been no capital operations in these Wards since our last Number. The general character of the admissions has been such as are common in large hospitals, viz., fractures, ulcers, syphilis, gonorrhœa, &c. Dr. Wedderburn has kindly furnished us the following observations, in which is presented *a new remedy for indolent ulcers*, which promises to be very beneficial, viz.: the local application of the sulphate of quinine. But let the Doctor speak for himself.

SERVICE OF DR. A. J. WEDDERBURN.

On taking charge of three surgical wards in the Charity Hospital about the 1st of November last, I found a number of cases of ulcer of an

*Dr. Faget has since resigned, and Dr. Wederstrandt, the House Surgeon, attends to the service of the Insane Department at present.

indolent character, some of which had been in the Hospital for many months without improvement, although the treatment pursued had been varied from time to time, according to the conditions presented by the ulcers.

From the powerful remedial effects produced by the sulphate of quinine in the various diseases in which it has been applied so largely of late by many physicians of the South, and from the recollection of its action, in arresting the ulcerative process in a very remarkable manner in an ulcer of the leg from a compound-comminuted fracture of the same in a case occurring in my practice several years since, I was induced to believe that good effects might be obtained from its topical application to the above mentioned ulcers, from a conjecture that the quinine in substance would act as a local stimulant, and by its absorption as a general tonic. I therefore had it applied in 12 or 15 cases, and found that on the next day all with one or two exceptions, had undergone a change much for the better, and that in a few days the surfaces of indolent ulcers had assumed a healthy appearance, with the property of secrating a healthy pus. I have been using this remedy in several cases of extensive ulcer of the leg since I first commenced the treatment, and in every case the improvement has been rapid, nor have I had any reason to substitute any other remedy for this. In connextion with the above remarks I will state a few of the most striking cases.

Case 1. A. K. aged twenty-eight years, entered the Hospital on the 31st of August, with a phagedænic ulcer of two and a half months' standing, with caries of the tibia. Before I took charge of the ward, a large portion of the bone had exfoliated. When I first saw the case, the ulcer extended from the internal malleolus to within four inches of the patella, with an extent in breadth of about four inches, and at the upper and middle part, one inch in depth, and half an inch at the lower. The lower part of the ulcer presented several dark spots with sinuous openings, discharging a sanious fluid; whilst the upper portion presented the general characters of an indolent ulcer. I ordered the entire surface to be covered with equal parts of the sulphate of quinine and flour, and in a few days, the character of the ulcer was entirely changed, presenting a healthy granulating surface, throughout its whole extent, secreting healthy pus. At the present time it is reduced in length to about five inches, in breadth two inches, and the greatest depth about half of an inch. Since the commencement of the treatment, nothing has been used but the sulphate of quinine, except on two occasions there appeared to be a too rapid growth of granulations, and the quinine, in substance, was withheld, and a mixture of tannin, grs. ij., and the sulph. quinine, grs. v., to the ounce of water, was applied for one or two days.

Case 2. D. D., aged thirty-five years, entered the Hospital, on the 23d of November, with an indolent ulcer just above the external malleolus, circular in form, and near three inches in diameter, more than half an inch in depth, and at the posterior part a deep sinus penetrating beneath the tendo-Achilles. After using as a topical application to the ulcer, Labarraques chloride of soda; five or six days, without the slightest improvement, the sulphate of quinine was resorted to, and the character of the ulcer entirely changed in twenty-four hours. The same treatment has been continued up to the present time; the diameter of the ulcer is much diminished, the granulations are on a level with the surface, and the new skin is forming rapidly.

CASE 3. B. S., aged 38; entered the Hospital, on the 8th December, with a sloughing phagedænic ulcer of the penis, from primary syphilis. About two-thirds of the skin had separated from the dorsum of the penis, so as to expose the corpus cavernosum. The separation at the under part had not taken place, at this time, between the living and the dead parts. The discharge was sanious, and very offensive. Ordered the parts to be washed with the chloride of soda, and apply an anodyne poultice. Second day—no improvement—ulcerative process progressing—a large eschar formed in the corpus cavernosum immediately behind the glans penis—ulceration progressing rapidly upon the corona glandis—discharge continues to be very offensive—pain excessive, with great constitutional disturbance. Ordered the whole penis to be enveloped in the sulphate of quinine and flour, in equal parts. Third day—ulcerative process entirely arrested; the whole surface, except the sphacelus on the dorsum, covered with granulations secreting healthy pus—little or no pain, and no constitutional derangement. This ulcer is now healing rapidly.

CASE 4. The operation for phimosis by circumcision was performed on one of the inmates of the Hospital, about ten days since. The day after the operation, the penis was in an œdematous and inflamed condition—the inflammation of the erysipelatous kind: a solution of five grains of the acetate of lead to the ounce, was continually applied during the day. The next day solutions of tannin and the sulphate of quinine were applied with good results: on the third day, the sulphate of quinine and flour were applied to the cut surfaces, and continued for several days, until the inflammation had almost entirely subsided, when an ointment of the acetat. plumb. was ordered, with a view to favor the healing process. On dressing the ulcer twenty-four hours afterwards, the penis was found again very much enlarged, and highly inflamed, with very great pain. The sulphate of quinine was again applied, in substance, to the part, and over this a poultice containing a solution of the sulphate of quinine and tannin. The improvement was so great the next day, that the tannin was withheld and the quinine applied to the surface with a light elm poultice. This treatment has now been continued for three days, and there is every prospect of a rapid recovery.

I could mention a number of other cases, showing the effects of the quinine, but I must defer it until the next number of the Journal. I will only add that several small ulcers have been cured under this treatment.

The operations during the last two months have been, one for fistula in ano, one for club-foot, (talipes varus,) one for phimosis, and one for the removal of a melicerous tumour, situated beneath the aponeurosis of the biceps muscle in the angle formed by the supinator radii longus and the pronator radii teres.

There are now in the wards five cases of fracture: one of the thigh, two cases of fracture of the radius, one an oblique fracture near the corpus. One case of both bones of the fore arm, and one case of both bones of the leg.

A large number of cases of gonorrhœa, and syphilis have been under treatment. Of the latter we have had cases of primary, secondary, and tertiary syphilis : of the last mentioned variety, there have been three cases ; one cured and discharged in between three and four weeks, the two remaining are improving rapidly. The treatment, in the three cases, has been the administration of about thirty grains of the iodide of potassium daily. This remedy has been administered largely in other forms of the disease, but entirely without effect.

There have been several cases of phlegmonous erysipelas ; two of the leg, one of the thigh, and two of the fore arm. In one of these cases, the disease was so formidable as to have produced the almost entire destruction of the sub-cutaneous cellular tissue, from the ankle to within a short distance of the knee-joint. An opening was made over the spine of the tibia, about two inches in length, from which there was a large discharge of pus. The discharge continuing for several days to be very profuse, a counter opening was made at the back of the leg, and in a few days the injury had been sufficiently repaired to enable him to leave the Hospital. At times the constitutional symptoms were very alarming, but yielded readily to large doses of the sulphate of quinine combined with opium.

MEDICAL WARDS.

The admissions into the Medical Wards have presented a great variety of diseases, acute and chronic. We can only make room for the follow-case of hepatic abscess.

Ward 21—Service of Dr. W. M. Carpenter.

Case of Hepatic Abscess.—J K. laborer, born in department Morelle, France, aged twenty-nine years—residence in New Orleans, four years—nervo-bilious temperament—was admitted into ward 21, on the 1st of December.

The following is the condition in which he was first seen by Dr. Carpenter. His aspect denoted the existence of extreme emaciation. The skin, which was extremely dark, had a cadaverous hue, and was tightly drawn around his mouth and over the face. The cheeks were sunken, the eyes hollow and glaring, and having a wild expression—the countenance expressed great anxiety.

Upon examination of the thorax and abdomen, other evidences of extreme debility, and wasting of the vital powers were visible. The ribs were prominent, the intercostal spaces depressed, the abdominal parieties drawn inwards except at the epigastric and umbilical regions, where they swelled and formed the appearance of an extensive tumour. He had a slight cough and some expectoration ; his bowels were extremely loose, having as many as twelve evacuations per day ; complained of great pain in the abdomen. The tumour being the most remarkable symptom present, excited the greatest attention. Its situation, as remarked before, was in the epigastric and umbilical regions. The centre was somewhat to the left of the median line ; the superior border was about an inch and a half below the extremity of the ensiform cartilage : the inferior border extended to a little below the umbilicus, and its diameter laterally was about four a half inches. The surface was not regularly rounded off, but irregular and lobulated in appearance. It gave a sensation of tension to the touch, and a decided one of fluctuation. "Hepatic Abscess," was the diagnosis.

The treament was partly surgical, and directed towards the indications. It was commenced by puncturing the tumour an inch above the umbilicus, and about a quarter of an inch to the left of the median line. Through the orifice about a quart of rusk, or rather chocolate colored pus was discharged; without, however, emptying it completely when, it was closed, with a tent of lint, confined to its place with adhesive plaster, in order not to shock the system of the patient too greatly by the discharge of a larger quantity. The tent was withdrawn and replaced twice daily, for fifteen days; during which time he cannot have discharged less than four gallons—the colour of the discharge became changed in its character towards the close of his life, becoming nearly white. Mistura cretæ, morphia, liq. morph., sulphat. in mucilage, and enemata of starch and laudanum, were given at various times with a view of arresting the excessive discharge from his bowels, with but temporary benefit. Quinine alone, and in combination with morphia, in the proportion of ℨ ss to gr. ss, iii pills, to be taken, one three times daily, was administered occasionally, together with wine, ale and such nutritious diet as his stomach would bear, in order to stimulate his enfeebled condition. The local application of anodyne poultices to abdomen was made in order to relieve the pain. The patient died on the 16th.

Post mortem, twenty-four hours after death. An incision was commenced at the clavicle, carried over the costo-cartillaginous articulations around the abdomen, &c., in the usual manner: and the cavities of the thorax and abdomen cut into. Adhesions almost cartillaginous in density, between the anterior surfaces of the right and left lobes of the liver and the peritoneal covering of the abdominal parieties, were required to be dissected apart, before the flap could be detached from the diaphragmatic attachments and thrown back. The internal surface of the flap presented these characters: there was a deposit of firm, nearly cartillaginous, organized lymph in a circle, marking its points of adhesion to the liver. The intervening space was excavated slightly and filled with thick, tenacious, yellowish-white pus. The ring of lymph did not constitute the boundaries of the abscess, for the pus had burrowed inferiorly into the abdominal muscles to the extent of an inch and three quarters, and superiorly and laterally, of half an inch; thus enlarging the extent of the abscess and the size of the tumour, as well as giving to it its irregular appearance. The organs in the cavity of the abdomen were then examined. The peritoneum was very much reddened and injected, extensive adhesions were observed among the intestines, &c.; the most remarkable was one of the pyloric junction of the stomach and duodenum to the right lobe of the liver, just on the right side of those (adhesions) forming the walls of the abscess. The liver presented the circular orifice, to an abscess within its structure, of about two inches in diameter, each way. It (the orifice) encroached equally upon its right and left lobes; the remains of the falciform ligament, which had partially suppurated away, dividing it into two nearly equal proportions. On each side of the ligament, the depth of the abscess was equal, and amounted to about an inch and a quarter. Upon the right side there was a communication between it and the stomach and duodenum, which were perforated. On the left side of the ligament, the abscess communicated with one longer, and occupying nearly the whole of the left lobe. The stomach was now divided at the cardiac orifice—the duodenum cut in two, and the liver removed from the body, which was done with difficulty, owing to adhesions between it and the diaphragm ; upon its removal a number of incisions were made into its substance, which cut through innumerable abscesses of various dimensions. The pus found in them was peculiar in colour, differing from that most generally observed in abscesses of the liver, which is a chocolate —this being almost white. One of the abscesses on the inferior surface of the right lobe, it was discovered had discharged its contents into the cavity of the abdomen, and thus, perhaps, produced the peritoneal inflammation observed. The mucous membrane of the stomach was reddened with innumerable red dots over its surface, and greatly thickened at its pyloric extremity: that of the duodenum was also thickened at the same points: many ulcerations were found in it, as well as those of the cœcum, the only portion of the intestines examined. The presumption is, that they existed along the whole course of the small intestines, and perhaps also of the large. No tubercles were found in the lungs—heart was normal.

This case is interesting, from the number of its complications; from the peritonitis supervening; the immense discharge from the tumour; the excessive alvine discharges from the bowels, and the clear manner in which all was accounted for by the post mortem appearances, and the immense number of the abscesses.—*Reported by F. Barnes, attending student.*

MORTALITY OF NEW ORLEANS.

List of DEATHS and DISEASES in the City of New Orleans, during the months of October and November, 1845.

October.—Fevers: Bilious Remittent, 2; Pernicious Intermittent, 1; Congestive, 3; Typhoid, 6; Putrid, 1: Fracture of Cranium, 2; Congestion of the Brain, 2; Ramollissement of Brain, 1; Meningitis, 4; Stomatitis, 1; Pharyngitis, 1; Angina Maligna, 2; Laryngitis, 2; Croup, 4; Bronchitis, 2; Pertussis, 1; Hæmoptysis, 1; Pleuritis, 2; Pleuro Pneumonia, 1; Pneumonia, 4; Phthisis Pulmonalis, 22; Disease of the Heart, 1; Gastritis, 1; Gastro Enteritis, 6; Enteritis, 8; Diarrhœa, 6; Cholera, 2; Intestinal Hæmorrhage, 1; Dysentery, 6; Hepatitis, 3; Nephritis, 1; Ascites, 1; Metro Peritonitis, 2; Parturition, 1; Dislocation of Cervical Vertebra, 1; Tetanus, 1; Trismus Nascentium, 11; Delirium Tremens, 2; Epilepsy, 1; Convulsions, 3; Eclampsia, 1; Poisoning by Opium, 1; Hydrophobia, 1; Scarlatina, 6; General Dropsy, 1; Marasmus, 2; Dentition, 2; Cancer, 1; Gunshot Wound, 1; Steamboat Explosion, 1; Burn, 3; Gangrene of Leg, 1; Laceration, 1; Intemperance, 2; Drowned, 2; Old Age, 3; Still Born, 15; Unknown, 21. Classified as follows, viz.:

White Male Adults	White Female Adults	Colored Male Adults	Colored Female Adults	White Male children	White Female children	Colored Male children	Colored Female children
76.	14.	9.	11.	30.	25.	13.	10.

Total - - - - - - - - - - - - - 188

November.—Bilious Remittent Fever, 2; Pernicious Intermittent, 2; Typhus, 1; Typhoid, 5; Congestive, 1; Concussion of the Brain, 1; Congestion of the Brain, 1; Apoplexy, 3; Meningitis, 6; Cerebritis, 3; Angina Maligna, 1; Croup, 3; Laryngitis, 1; Bronchitis, 5; Pertussis, 1; Pneumonia, 3; Pleuro Pneumonia, 1: Phthisis Pulmonalis, 31; Hydrothorax, 1; Hypertrophy of the heart, 2, Cancer of Breast, 1; Gastro Enteritis, 3; Enteritis, 7; Entero Colitis, 1; Diarrhœa, 3; Dysentery, 6; Hepatitis, 2; Disease of the Liver, 1; Metro Peritonitis, 1; Cancer Uteri, 1; Chronic Cystitis, 1; Injury of Spine, 1; Myelitis, 1; Paralysis, 2; Trismus Nascentium, 12; Tetanus, 1; Convulsions, 7; Delirium Tremens, 1; Scarlatina, 21; Erysipelas, 1; Dropsy, 5; Anemia, 2; Dentition, 3; Congenital Debility, 1; Marasmus, 2; Syphilis, 1; Scrofula, 1; Spina Ventosa, 1; Carics of Hip, 1; Gangrene, 1; Gunshot Wound, 1; Poison by Arsenic, 1; Intemperance, 5; Old Age, 4; Still Born, 14; Drowned, 8; Unknown, 16. Classified as follows, viz.:

White Male Adults	White Female Adults	Colored Male Adults	Colored Female Adults	White Male children	White Female children	Colored Male children	Colored Female children
62.	25.	14.	11.	36.	36.	12.	14.

Total - - - - - - - - - - - - 217.

We are indebted to Dr. Lewis, the obliging Secretary of the Board of Health, for the above report.

ABSTRACT OF A METEOROLOGICAL JOURNAL FOR 1846.

By D. T. Lillie, at the City of New-Orleans

Lat. 29° 57′ Lon. 90° 7′ west of Greenwich.

weekly.	Thermometer. Max.	Min.	Range.	Barometer. Max.	Min.	Range.	Course of Winds.	Force of wind in ratio 1 to 10.	Rainy Days.	Quant. of Rain. Inches.	Thousandths.
1845.											
Oct. 25	72.5	55.0	17.5	30.28	30.07	.21	N. E.	$2\frac{1}{2}$	1	1	390
Nov. 1	77.0	59.0	18.0	30.31	29.98	.33	E.	2	3	1	840
8	75.0	47.0	28.0	30.12	29.95	.17	N.	$2\frac{1}{2}$	1	0	750
15	64.0	44.2	19.8	30.33	30.16	.17	N. E.	$1\frac{3}{4}$	0	0	000
22	71.5	54.0	17.5	30.25	30.00	.25	E.	$2\frac{1}{4}$	1	1	420
29	67.0	32.5	34.5	30.30	30.08	.22	N.	$3\frac{1}{4}$	1	0	750
Dec. 6	55.2	25.0	30.2	30.32	30.00	.32	N. E.	$2\frac{3}{4}$	3	0	582
13	69.	40.0	29.0	30.29	30.06	.23	N.	$2\frac{1}{2}$	6	4	340
20	55.	26.0	29.0	30.37	29.99	.38	N.W.	3	2	0	955

Remarks.—The Thermometer used for these Observations is not attached to the Barometer, but is a self-registering one, and is placed in a fair exposure. Regular hours of observation, 8 A. M., 2 P. M., and 8 P. M.

The Barometer is located at an elevation of 19 feet above the level of the ocean, and is suspended clear of the wall of the building.

The Rain Guage is graduated to the thousandth part of an inch, and the receiver is elevated 40 feet from the ground.

On Saturday 20th December, the Thermometer stood, at 6 A. M., 26°, at 8 A. M., 29°, at 2 P. M., $35\frac{1}{2}$°, and at 8 P. M., 32°, being the coldest day experienced in this city for several years. It will be observed that the Thermometer was on one day as low as 25°, but the cold was not so sensibly felt as on the 26th, when the atmosphere was very damp.

THE NEW ORLEANS

MEDICAL AND SURGICAL JOURNAL.

MARCH, 1846.

Part First.

ORIGINAL COMMUNICATIONS.

ART. I.—*Remarks on the Periodical Maturation and Discharge of Ova in Man, and other Mammiferæ; and the Practical bearings of this Theory.* By W. M. CARPENTER, A. M., M. D., Professor in the Medical College of Louisiana.

Since the earliest times, distinguished men have labored to solve the wonderful and mysterious problems of generation. Theory after theory has arisen, to be destroyed in succession, as the hypotheses on which they were founded gave way. In all these theories it was attempted, in the absence of positive knowledge, in reference to the ovum, to substitute hypotheses in its stead. This only led the investigators further astray; and it may be asserted that no subject can illustrate more strikingly than this, the absolute dependence of human knowledge on direct observation.

It has long been known that, among organized beings, there are two ways in which individuals commence their existence. The first, which is only observed to take place among the less complex forms of animals, and in many plants, is by the production and separation from a single parent, of an element containing all the essentials for independent existence. Such are what are termed fissi-parous and gemmi-parous generation. The second mode of reproduction depends upon the secretion of certain peculiar substances, from two different kinds of organs; and consists essentially in the reciprocal action of these two kinds of matter on each other, which results in the formation of an embryo capable of development, and fitted to pass through various transformations to arrive at the degree of complexity characteristic of the type of its parents. This may be called the bi-sexual mode of reproduction, and almost all animals and plants propagate by this mode; and even those which reproduce generally by the uni-sexual or fissi-parous mode, are likewise provided with organs enabling them to propagate by the other mode, which are called into activity at particular times.

Throughout the organized world, we find an endless variety in the specialities of the form and distribution of the organs which elaborate the two generative elements, but everywhere we find a similitude in the elements secreted, and in the result of their reciprocal action. Everywhere, in one set of organs called the female organs, we observe cells, peculiar to the tissue of those organs, which, undergoing certain changes of size and structure, arrive at the state in which they are known by the name of *ova*, and are then separated from the tissues in which they have been formed, and are discharged. The other set of organs called the male organs, furnish in like manner, an organized element, which taking its origin in the peculiar cells of the tissues of those organs, is developed into forms which are peculiar and constant in each species, and which are in this state discharged from the organs. In these organs the respective functions are performed entirely independently of each other; the female organs form and mature the ovum, and among animals, it is discharged from the organ independently of any influence of the male element; the development and discharge of the male element is equally independent of the other.

Great diversity exists in the organized kingdom in the relation, distribution and arrangement of the two kinds of organs. In a majority of plants, and many of the inferior animals, the male and female organs are found in the same individual; either approximated so as to form a bi-sexual group, or placed separately in more or less distant parts of the individual. In some other plants, and in all the higher animals, the two sets of organs are placed in distinct individuals. Now, whether the organs exist in the same or in different individuals, they perform their respective functions independently of any mutual relation or influence, and the ovum and the male element are equally discharged, whether they are to come into contact or not. This being the case their contact and reciprocal action, which is indispensable to the perpetuation of species, would be abandoned to chance, were it not that both kinds of organs perform their functions under the direction of general influences and circumstances, which control and harmonize their respective operations, and establish such a degree of relation between their vital manifestations, as fully to subserve the great end which these arrangements were intended to fulfil.

The influences which thus control and harmonize the functions of the male and female organs, do so only indirectly, by modifying general nutrition simultaneously in the individuals to which the respective organs belong. Among the general influences capable of producing such effects, are those of climate, seasons, temperature, degree of moisture, quantity and quality of aliments, mode of life, &c.; all of which are extraneous to the economy of the individuals, and often altogether fortuitous.

Under the influence of these agencies the ovum is formed, matured and discharged from the female formative organ, at epochs separated by fixed and determinate intervals, which are constant in species. The male element, likewise, is formed, matured and discharged periodically, in the great majority of species; and, indeed, those species in which this element is continually discharged, are rare exceptions to a general law. In most species living in a state of nature, the reproductive impulse occurs annually, once a-year; that is to say, the organs influenced by favoring seasons take on simultaneously the function of developing and discharging their respective elements. In some species, however, the phenomena occur at shorter intervals; and in some the development and discharge of the ovum occur periodically, while the male element is continually being produced. These diversities are sometimes observed even among species of the same genera. Thus in some wild species, the organs are in a state of reproductive excitement but once a-year; while in domestic species of the same genera, the periods of excitement are less under the control of seasons, and consequently less regular in their occurrence. In all cases the meeting of these elements is provided for; sometimes by the mere physical operation of purely mechanical contrivances, and sometimes by mechanical means controlled by coördinate instincts.

The foregoing remarks apply to all species, both of the animal and vegetable kingdoms, though this explanation has not generally been regarded as applicable to the reproductive process in man and the mammiferæ, in whom it was supposed that the ova were developed and discharged from the ovaries only as a consequence of fecundation. In many classes, as birds, reptiles, fishes, many of the lower animals, and in plants, it was easy to ascertain the existence of the ova in the ovaria and their efferent ducts, prior to fecundation; so that, in these, there was no room for attributing this development in, and discharge from, these organs, to any influence foreign to a peculiar modification of the nutritive process *in the ovaria themselves.* Thus in many insects, females will lay eggs, though never approached by the male, but the eggs, being unfecundated, never give issue to young insects.* Even among the inferior animals, which undergo metamorphoses, the ova may be detected in the ovaria while the animal is yet in its nymph state, and long before its final development into its perfect form. It is well known, and has been mentioned by numerous writers,† that birds of different kinds lay sterile eggs, when they are deprived of the society of the male. It has likewise been long known that in fishes and amphibious reptiles, the eggs were formed and emitted by the female, and their fecundation took place only after their emission from her body;‡ in numerous species coition never takes place.§

In man and the mammiferæ, however, the extreme minuteness of the ovum at the term of its full development and discharge from the ovarium, caused its existence in this class to be overlooked, even by close observers; and the inference was, that it did not exist in these animals until developed by fecundation. The generally received opinion was, that the male semen ascended to the ovaries, and produced an impression on those organs, causing the development of an ovum which after a certain time was discharged, and descended to the uterus. Some writers, among whom were Buffon and Blumenbach, observing the formation, in the ovarium, of little vesicles which contained and discharged a drop of sero-albuminous liquid, concluded that this liquid was the element which the female contributes in the process of conception. In the controversies on this matter, numerous hypotheses, assumptions and metaphysical subtleties,

* Burmeister, quoted by Burdach, Traité de Physiol. T. 1, p. 76—also Lacordaire, Introd. à l'Entomologie, T. 2.

† Buffon, Hist. Nat.; Blumenbach, Nat. Hist.; Home; Geoffroy Saint Hillaire, Philos. Anat., &c.

‡ Lacépède, Hist. Nat. des Poissons, &c.; Cuvier, Règne Animal; Valenciennes, Hist. Nat. des Poissons, &c.

§ Art. Reproduction des Poissons, in Dic. des Sc. Nat. T. 45.

were tried as substitutes for the want of knowledge of the fundamental fact of the pre-existence of the ovum. Cuvier* and Duvernoy† alone arrived, by inductive reasoning, at the conclusion that the ovum in all animals was developed and discharged by the ovary, independently of fecundation.

Such was the state of the question, when in 1827, Baër discovered the ovum, as developed in the ovarium, prior to fecundation, and his discovery was followed by the researches of Valentin, Carus, Costé, Purkinge, Wagner, and especially by Bischoff and Raciborski, who have arrived at conclusions, most important in their theoretical and practical consequences. These investigations show that a number of ova are always in progress of development in the ovaries of all females capable of breeding ; but that they arrive at maturity only at stated intervals, when they are discharged, independently of any action of the male. When discharged from the ovary, the ovum is received by the fallopian duct, and passes down to the uterus, and may be thrown off with the discharge from that organ. This transit of the ovum through the duct and uterus requires, in most species, several days, and fecundation may take place at any period from the time of its discharge from the ovary, until it passes out of the uterus. The male semen, too, is not carried up immediately to the ovary in the act of copulation, but requires some time to reach them. The semen and ovum may meet in any part of the passage from the ovary to the uterus, and fecundation be thus effected.

The opinion now entertained by Bischoff and Raciborski, is, that fecundation takes place generally in the fallopian tubes ; but no doubt can be entertained, in view of authentic facts, that it sometimes occurs when the ovum is first discharged from the Graafian vesicle, and before it enters the oviduct. Those which are fecundated and fall into the abdominal cavity, and constitute that variety of extra-uterine pregnancy, must be regarded in this light. Ovarian pregnancy, as it is improperly termed, goes to prove the same thing. While the ovum is contained in the unruptured Graafian vesicle, the parietes are solid barriers, which protect it from the influence of any semen that may have ascended through the oviducts ; but, when this vesicle is ruptured, the ovum may not immediately escape with the fluid, and the semen then reaching it may fecundate it, and it then forms attachments, and commences its development as an embryo, still occupying the cavity of the vesicle, which will enlarge with the growth of the embryo, and causing the absorption of the parenchyma, may thus occupy the whole space contained with the proper membrane of the ovary. Such cases are stated to have occurred ; but Velpeau is sceptical in regard to their nature, and this explanation is certainly in harmony with what is known respecting fecundation.

Those who have held the opinion that fecundation takes place while the ovum is still contained in the ovary, and before the rupture of the Graafian vesicle, make use of the occurrence of ovarian and other extra-uterine pregnances to sustain their opinion. They likewise lay great stress on the experiment of Nuck‡ who ligatured the fallopian tubes in a

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* Traité Elim. d'Hist. Nat.; also Règne Animal.

† Comptes Rendus Hebdom. des Séances de l'Acad. des Sciences, Tom. 17.

‡ Bichat, Anat. Descrip., v. 5, p. 337.

bitch three days after copulation, and afterwards, on examination, discovered two fœtusses in the tube above the ligature. Now this shows nothing; as the semen had time to ascend in the fallopian tubes before the ligature was applied, and would be likely to fecundate any ova that might enter from the ovaries. The researches of Raciborski on the arrangement of those parts in the bitch, throw farther doubt on the experiment, by rendering it probable that Nuck ligatured the slender *cornua* of the uterus instead of the fallopian tubes. Many other experiments have been reported by Haighton, Cruikshank, Grassmeyer, Blundell, Haussmann and others, and repeated by Bischoff*; all of which when properly interpreted only confirm the opinion that fecundation cannot be effected until the Graafian vesicle is ruptured, and that it may take place at any time after this happens, till the ovum is discharged through the uterus or destroyed. It is probable that fecundation takes place in the great majority of cases while the ovum is in the fallopian tubes.

Woman presents an apparent exception to the rule that fecundation takes place only after the discharge of the ovum from the ovary. The researches of Raciborski† and Ritchie‡ show that coition, a short time prior to the appearance of the menses, sometimes results in conception. This fact, taken in connexion with the general notion, that while animals generally copulate only at the periods of heat, man is repelled by woman only at the periods of menstruation, would be an obstacle in the way of the supposition of a strict analogy between this function in woman and the females of other species; were it not that close observation has thrown light on the true relations. The researches of Raciborski, while they show that coition a day or two prior to menstruation may give rise to fecundation, are equally conclusive in showing that it cannot take place indifferently, at all periods intervening between two menstrual epochs; but that, in harmony with ancient and universal experience, it almost invariably takes place immediately after; and it appears that in some rare, exceptional cases, coition just before menstruation may be followed by fecundation. Fecundation in these latter cases may be accounted for by two hypotheses; either, that coition, at the period when the ova are mature and ready for discharge, may hasten the rupture of the Graafian vesicles; or, that the semen may ascend into the fallopian tubes, and there retain its fecundating properties till menstruation is over and the ovum is discharged.§

In regard to the alleged difference between women and other mammiferous females in regard to sexual desire at the periods of the discharge of the ova, the observation of Bischoff (op. cit.) and other attentive observers are perfectly conclusive. According to them all females just before the discharge of the ova, give indications of uneasy sensations and a degree of indisposition; and while this state continues they always decline the advances of the males. This state is analagous to the menstrual state, and in all, equally, a state of comfort and good health succeed,

* Ann. des Sc. Nat., 3d series, v. 2, Zoologie, p. 109, et seq.

† De la Puberté et de l'age Critique chez la femme; p. 459, et seq.

‡ London Med. Gazette, May 1844.

§ Donné has shown that the spermatic animalcules will live a long time in blood, or the luecorrhœal discharge; and they have recently been detected in the fluid of tumours, &c., in the neighborhood of the genitals in man.

during which the ova are discharged; and at this time the venereal desire is especially manifested. There appears, then, to be a perfect analogy between the conditions and phenomena presented by women with those presented by the females of other species of mammiferæ.

The interval that separates the periodical maturation and discharge of the ova, is modified by climate, temperature, food and other agencies. There is likewise much diversity in the subordinate phenomena, but periodicity is an invariable concomitant of reproduction among the mammiferæ. The accomplishment of the end in view is provided for by the existence of this capacity for conception with corresponding instinctive impulses in the individuals. "So rigorous is nature," says Raciborski, "in the observation of this law that most females among the mammiferæ. particularly those in which the epochs of reproduction are separated by long intervals, do not permit the approach of the males except at these epochs. But at those periods their instinct is a hundred-fold more energetic than those which have short intervals, and while the latter merely allow the embraces of the male, the others burn to satisfy their venereal desire, and will pursue the male with this view. So decided is this character that all languages have names for this epoch, signifying '*times of heat.*' " It is only during these epochs that fecundation can take place; and as fecundation is an absolute requisite to the ulterior development of the ovum, it follows that the fertility of females is limited to these periods.

In the males of most species of animals, analogous series of phenomena are observed. The testes, which are the precise analogues of the ovaries, elaborate and discharge an organized element analogous in some respects to the ovum. This product has a rounded or oval body and a filamentous tail-like appendage, and seems to be endowed with vitality, and possesses the power of performing certain motions of the tail, which have been compared to the ciliary motions observed on epithelial surfaces.* The terms *spermatic animalcules* and *zoospermes* have been bestowed on these productions under the belief that they were real animalcules, but it is now maintained by some that they are only particles or cells derived from the parenchyma of the testes.† They are found distributed through the semen of prolific male animals. In some animals, particularly in man and some domestic animals, they are found in the semen of healthy individuals at all times; but in the larger number of animals the semen contains them only at particular epochs; and at those times the animals exhibit their sexual instinct, and are *in heat.* The period of heat in males in each species corresponds with that of the females; being dependent upon the same general influences of season, &c. Males are incapable of procreating, except in their time of heat; as, excepting at those periods, their semen is devoid of the essential elements of fecundation.‡

At each of these epochs, certain other phenomena are generally observed in females, which are referrable to a state of excitement in the

* Todd and Bowman, Physiological Anatomy, v. i, p. 66.

† Todd and Bowman, op. cit. v. i, p. 66.

‡ Prevost and Dumas; Zoospermes; Ann. des. Sc. Nat. Also, Lallemand, sur les Zoospermes, Ann. des Sc. Nat., &c.

organs; among these the discharge of a sanguineous fluid in some species, and an engorged state of the parts in others, are the most striking. These, however, are mere consequences resulting from, and entirely subordinate to, the actswhich are transpiring in the ovarium, and are by no means necessary to the healthy and complete fulfilment of the reproductive function. But of this we will speak farther after having studied the phenomena which are observed to occur in the ovarium.

The ovary of an adult female mammifer, exhibits, dissemminated through its substance, a number of little cells or vesicles, called after the one who first described them well, the *vesicles of Graaf.* These vesicles increase gradually in size, and approach the surface of the ovary. After the lapse of a certain time they arrive at the surface, and even form little projections beyond it, which give it a granular feel and appearance. These vesicles contain a transparent, viscid, and albuminous liquid, coagulable by alcohol, heat, or nitric acid. When they have arrived at a certain state of development, a degree of congestion takes place; the contained liquid becomes sanguinolent, and in this state the ovum may sometimes be seen in the liquid, about as large as a poppy seed.* After remaining in this condition a few days, the vesicles are spontaneously ruptured, and the contents evacuated. The ovum enters the oviduct, and is either fecundated or destroyed in its passage. The number of ova discharged at each epoch varies in different species; in some species, as in man, only one is habitually discharged, though sometimes two or three; while in others twenty or thirty will be discharged in succession during the period of heat.† In some animals, generally reptiles and birds, which discharge many ova at each period of sexual excitement, we frequently find the phenomena governed by a double periodicity; thus, the general epoch is periodical, and the discharge of each ovum not unfrequently occurs periodically, with diurnal intervals.

The cavity of the emptied vesicle now becomes smaller, by reason of the contractility of the tissues; its lining membrane being now too large to fit evenly, is folded on itself so as to form plaits, which partly fills the cavity, and the remainder is filled with a little clot of blood. The little mass thus formed in the cavity of the vesicle has the colour and consistence of liver, and when cut horizontally, presents a radiated appearance.† It requires but little force at this time to extract this fleshy mass from its cavity, but after a short time they become more firm; the colour changes, and they become yellowish, still offering a radiated appearance. In this state they are called *corpora lutea.* The *corpora lutea* are mere indications that ova have been discharged, and are always formed when this takes place, whether fecundation is effected or not.‡

Valisneri long ago found corpora lutea in the ovaries of a bitch which had been long secluded from the male. Malpighi, Bertrandi, Brugnoni, Santorini, Buffon and others made the same observation on other animals; and the three first named writers, as well as Rœderer, Haighton, Meckel, Home, Cruikshank, Brechet, Velpeau, Blundell and Pouchet, have stated the discovery of corpora lutea in the ovaries of virgins. The corpora lutea, too, seem to be identically of the same character, whether

* Raciborski.

† In some fishes, many thousands are discharged at each epoch.

‡ Ibid. § Negrier, Bischoff, Raciborski.

the ovum discharged be afterwards fecundated or not. Bischoff,* in one case, cut away the right uterus of a rabbit, thus rendering it impossible for the semen to reach that ovary—she afterwards went into heat, and when he examined her afterwards, he found in the left uterus a fecundated ovum, and in the left ovary a corresponding corpus luteum; in the right ovary, where fecundation was impossible he found four corpora lutea in the same state as the one in the left ovary. Blundell‡ reports an exactly similar result. Dr. Ritchie§ says, that, in some women pregnant with a single fœtus, two corpora lutea of "equal organization, were sometimes found, either in the same ovary, or in the two; and in these cases it was impossible to tell the productive from the non-productive vesicle; and their rupture was, therefore, believed to have been coetaneous."

A few days, previous to the discharge of the ova from the ovarium, the uterus and other genitals become the seat of a sanguineous engorgement, and a bloody liquid exudes from the lining membrane of the uterus. This discharge varies very much in quantity in different species; in some it consists of a mere increase of the natural mucus not colored by blood, while in woman it generally amounts to many ounces in a few days, and resembles blood in its colour. Most writers have followed the learned Pliny in the opinion, that "the only menstruous animal in nature is woman," but this is now known to be a partial view of the subject. Raciborski,‖ dissecting a bitch which was in heat, found the internal surface of the womb gorged with blood, and covered with a bloody exudution; and a sanguineous discharge has been often remarked to take place from the external parts of the same animals. Buffon, F. Cuvier, Geoffroy Saint Hillaire and Raciborski have observed in apes, in the Jardin des Plantes, a genuine menstrual hæmorrhage, sometimes so abundant that their cage was sprinkled with the blood. The fact that the females of our own species have a greater hæmorrhagic discharge than other species, is by no means wonderful, but in harmony with the general fact, that in our species congestions are much more liable to terminate in hæmorrhage than is the case in other animals, which may be regarded as exempt from all hæmorrhages excepting those of a traumatic origin.

In woman, and the females of the mammiferæ generally, the discharge from the uterus commences some time before the discharge of the ovum from the Graafian vesicle, and ceases about the time of its discharge.* With the discharge of the ovum from the ovarium, begins the capacity of fecundation, which lasts until the ovum, in its transit through the oviduct and uterus, is either discharged from the uterus, or is destroyed. This time is very different in different animals, depending upon the rapidity or slowness of the passage of the ovum through the oviduct, and likewise upon the length of time which the ovum in different species, retains its susceptibility of being fecundated. The researches of Bischoff led him to the belief that in rabbits the ovum remains 3 days in the oviduct, in the ruminantia 4 or 5 days, in the bitch 8 or 10 days, and in woman 8 to 12 days; while it appears by the observations and experiments

* Annales des Sc. Nat., new series, v. 2, Zoologie. p. 114.
† Principles and Practice of Obstetrics, p. 60.
‡ London Med. Gazette, May 1844.
§ De la Puberté et de l'age Critique chez la Femme, p. 377.
‖ Bischoff.

of Zeigler, that the ovum of the deer requires months to traverse the oviduct.*

The menstrual hæmorrhage is entirely dependent on, and subordinate to, the functions of the ovaries, as is shown by many circumstances. As soon as the ovaries have attained their full development, menstruation commences, and with it the sexual desire; and when they are atrophied, removed, or destroyed by disease, menstruation and desire cease together. In many animals, the womb has been removed and the ovaries allowed to remain, and the sexual desire continued as energetic as ever; but whenever the ovaries are removed, the animal at once loses the sexual instinct entirely. This experiment is easily made; and it is common to remove the ovaries of some animals to prevent their breeding, and render them better adapted for the purposes of food; this is often done with the sow, in this country, and it is well known that they lose all the characteristic instincts of their sex. In women the same thing is observed. Cases are mentioned by Pott, and other surgeons, in which the ovaries were removed by operation; and a change of form, constitution and character resulted, and an entire cessation of menstruation. Some of the Asiatic sovereigns, too, have had the ovaries extirpated from the females who were destined for eunuchs. Mr. Roberts, in his travels, gives an account of some of these female eunuchs, called *Hedjeras*, seen by him in the vicinity of Bombay. They were large, muscular people, with small nippleless mammæ, like those of men; and they had no hair about their genitals. They, of course, did not menstruate; they never felt any venereal inclination, and the entrance of the vagina was obliterated. These facts and others go to establish the fact that the menstrual hæmorrhage is entirely dependent on, and accessory to, the function of the ovaries.

The development and discharge of ova must therefore be regarded as the essential feature of the epoch of reproductive excitement in woman, and the essential condition to conception. The absence or presence of the menstrual hæmorrhage implies nothing positive respecting the activity of the ovaries, or the condition of fertility or sterility of the individual at the time. Dr. Ritchie† has shown that the Graafian vesicles in the human female do not require the presence of menstruation for their development or rupture. Indeed, cases are by no means rare, of women who were prolific without ever having any menstrual discharge. Dr. Ritchie has likewise shown that even in pregnant, puerperal and nursing women, the development and discharge of ova takes place, in much the same way as at other periods, and it is well known that at these epochs the menstrual hæmorrhage does not accompany the process. The importance of the menstrual discharge has, therefore, been much over-rated as a function, and is still over-rated, in view of its relations to hygiene and therapeutics. Respecting this more will be said on a future occasion.

It is stated‡ that in mules the ovaries are very imperfectly developed, having not the slightest trace of the Graafian vesicles, and this is given as an explanation of their barrenness. This is an interesting fact, and if further investigations show the same thing to be general among hybrid

* Annales des Sc. Nat. 1844. 3d series, vol. 2, Zoologie, p. 121.

† Ritchie, London Med. Gazette, May, 1844.

‡ Raciborski, Op. Cit. p. 384.

animals, it will fully account for their general sterility. Raciborski says that his confidence in this opinion was shaken by reading an account, given by Brugnoni, of genuine corpora lutea, found by him in the ovaries of mules. This is exactly in accordance with what we would expect. Mules are not universally barren; for several instances have occurred under my own observation, of their breeding; and in one instance, near Opelousas in this state, a mule bore twins. The probability, therefore, is, that in hybrid animals the ovaries are in general defectively developed, and, consequently as a general rule, they are barren; but there are many exceptions to the rule and its consequences. The hybrids of some species are known to be generally fertile, and to these the general rule respecting the defective development of the ovaries cannot apply.

This summary of some of the principal points of the theory of the periodical discharge of ova has been given merely with the view of making it the basis of some observations on a few practical deductions, which will be presented on a future occasion.

[*To be continued.*]

ART. II.—*Observations on Tetanus.* By W. A. McDowell, M. D., of Louisville, Ky.

My observations on tetanus have induced the impression that this disease generally arises from the presence of a fixed irritant in contact with nervous fibrillæ; and that the most feasible treatment consists in means adapted to dislodge and remove the irritant. As my practice, predicated on this hypothesis, has been somewhat successful, the following detail of the cases may be of some interest to the profession. Besides, having detected the existence of such irritants in several cases of tetanus, and having also, in experiments on animals produced the disorder by inserting particles of foreign matter into their nerves—the history of a very large proportion of the cases that have been handed down to us I conceive to be somewhat corroborative of my conclusions. These cases we find in the proportion of more than 20 to 1 to have arisen from wounds inflicted by instruments well adapted to leave detached particles of their substances in the wound; such as rusty nails, splinters, friable stones, &c. The smallest portion of any such foreign matter fixed in the substance of, or lodged in contact with, these sensitive structures, may, as well as the largest, suffice to produce the malady. Particles of metal or of metallic oxides, such as a film of the turned edge of a knife, a particle of oxide of iron or of lead, a briar, a splinter of wood, or a particle of glass or sand, in contused, lacerated or gunshot wounds; or in burns, a hard or deadened portion of animal matter, even a deadened fibrilla in a filament, may become sufficiently irritating to produce the disorder; such dead matter being as unnatural a stimulant, in the situation, as though it had been introduced from without. Such irritants are generally detached and ejected from wounds by process of suppuration; but excessive irritation, either of an organ or a tissue, is found to suspend the secretion, which peculiarity is almost always observable in tetanic wounds. Larrey, in his extensive observations, informs us, he always observed that wounds productive of tetanus either did not suppurate at all, or, if having suppurated, that the suppuration ceased sometime prior to the outset of the tetanus, whereby this, the ordinary process of nature for relief, is cut off.

In cases that have come under my own observation there has occurred not only interruption of suppuration, but the interruption of function has also been manifested in the biliary, the perspiratory, and sometimes in the urinary secretions. In cases which have suddenly succeeded the infliction of wounds, I have met with such manifestations of deficiency in the digestive secretions as to have induced the belief that this must have preceded and predisposed to the disease. Irritation from deficient alimentary secretions or from the presence of vitiated acrid matter, or from wounds in the alimentary canal, is believed alone frequently to have caused tetanus. Under these views the leading objects of treatment in traumatic tetanus would seem to be:

1st, To dislodge the local irritant.

2d, To restore the functional secretions.

The first indication may be accomplished by several methods; the solution should be made in adaptation to the situation and character of the wound, Firstly, by excision of the wound or cicatrix; Secondly, by free incision into it, and the application of medicinal articles adapted to produce speedy and free suppuration; Thirdly, extirpation of the part by cautery; Fourthly, amputation.

I conceive the first to be the best method, when the situation of the wound will admit of it. The two last are objectionable on account of the fact that these operations of themselves often produce the disease. The hard eschar left for a period of three or four days by the one, in contact with nerves already in a state of tetanic irritation, and in the other the augmentation of this by dividing so many nerves while in this condition, are unpromising circumstances; yet they must often be our only resources. I have practised the three last only. It has been recommended by some writers to divide the injured nerve between its origin and the wound. In practice, I believe, this has not succeeded. Our present knowledge of the reflex motions in the nervous system, through the inosculations and interlacings of the nervous extremities, readily accounts to us for such failures.

The mode of accomplishing the second object, the restoration of functional secretions, is so familiar that although deemed very essential, it need not here be treated of.

Case I.—The subject of the first case of this disease that came under my observation was a healthy, athletic, middle aged man, who was stung on the nose by a bee. Spasms ensued immediately, and in a few minutes after the reception of the injury he was perfectly dead. The sting was found remaining fixed in the point of the nose.

Case II.—In June 1820, a negro boy, aged 15, the property of W. Guliford, of Bottetourt county, Virginia, complained in the morning of pain in the forehead, but went about his work as usual. He was in the afternoon taken with spasms and died in a few hours; he was in *articulo mortis* before any medical aid arrived. Enquiries, made by the physicians who had been brought together, led to making an incision into a scar on the forehead, to which he had referred as the seat of his pain in the morning. Here, a splinter of wood, nearly an inch in length, and a fourth of an inch in breadth, was found, imbounded between the occipito-frontalis and the pericranium. The family then recollected that several months before,—the precise time they were unable to determine—he had fallen, and wounded his forehead against the end of a fence-rail; that the wound had healed readily, and without any trouble, and that he had afterwards occasionally complained of pain in the forehead. His health was believed, otherwise, to have been uninterruptedly good.

Case III.—May 14, 1822. Visited Mrs. S——, aged 54, (a midwife, of Fencastle, Virginia, of good constitution,) in consultation with Dr. J. C. Madison, who had been in attendance the two preceding days, within which period the spasmodic rigidity of the lady had become complete. I found her speechless, and apparently in a state of insensibility. Dr. M. had commenced treatment by the administration of a powerful cathartic, eliciting free purgation, and had subsequently administered tinct. opii at the rate of from 1 to 2 oz. per day, together with wine and brandy without stint. I advised an increase of the laudanum, and ℥iv. were administered within the next 24 hours, without any perceptible effect. On my visits through the day, I repeatedly enquired of her family and acquaintances whether she had latterly received any wound; there was no recollection of any thing of the kind. On visiting her the following day, it had come to their recollection that in February, whilst trimming a tree in her garden, she had cut her finger. The cut readily healed, but she had afterwards sometimes complained of numbness and unpleasant sensations in the finger. On examination, we found a distinct scar, within a half an inch of the extreme joint of the ring finger. Upon this scar an incision was made to the bone. The spasms instantaneously relaxed. The patient darted up to a sitting posture, and with open mouth screamed aloud. The cut was filled with pulv. canthar.; laudanum was discontinued. Small doses of calomel and opium every three hours were prescribed, to regulate the biliary secretion, which the appearance of the tongue, &c., indicated to be much deranged. Within the succeeding 15 hours she suffered a few slight spasms in different parts of the body. Free suppuration of the cut ensued on the third day. In about a fortnight she was restored to her usual health and customary occupation. Notwithstanding the apparent insensibility and unconsciousness which in this case continued nearly three days, the patient was conscious of all that was said and done.

Case IV.—November 7, 1828. Visited Elizabeth C——, of Botetourt county, Virginia, aged 22; in consultation with Dr. J. H. Griffin. She had, six days before, received a wound of the foot between the first and second toes by striking the part when running, against the sharp, splintered end of a cedar limb. Dr. G. had visited her the preceding day, when the jaws first became locked; the spasms had since become general, but were still intermittent. His treatment had consisted in free resort to bloodletting, stimulants and anti-spasmodics. Neither inflammation nor suppuration having occurred in the wound, I advised, in addition to his treatment, a free incision and exploration of the wound. The snag was found to have penetrated about two inches. No splinter nor any other foreign matter was detected in the wound. With the view of exciting suppuration we filled it with pulv. canthar., and kept the foot covered with poultices. No benefit whatever was derived. From intermittent, the spasms proceeded to remittent, and finally became continuous. She died on the 11th; neither suppuration nor any appearance of inflammation having in the whole time occurred in the wound. On post-mortem examination of the foot, a small splinter was found, lodged under the second metatarsal bone, having separated and diverged laterally from the main snag. Post-mortem examination of the body was not permitted.

Case V.—L. M——, of Botetourt county, Virginia, a vigorous youth, aged 13, apprentice to a waggon maker, when chopping in the woods, in company with his master, cut the inside of his right foot to the bone, and then walked through the leaves and sand eighty or a hundred yards, to his master to get it dressed. He, to use his own expression, "bound it up in the blood," and it healed by the first intention. Twenty-seven days afterwards the boy was taken with lock-jaw, and was transferred to the county poor-house to be treated. I visited him on the 28th of April. His jaws had remained immovably clinched since the 26th, within which period the previous intermittent spasms had become general. I at once made an incision upon the cicatrix, embracing the whole of it both in breadth and depth, to the bone. He instantly, with expanded jaws, screamed aloud from pain, which he represented to be incomparably greater than he had experienced from the original cut. The whole of the sides and bottom of the wound were lightly sprinkled with a powder composed of equal parts of corrosive sublimate and red precipitate—a compound, the efficacy of which to promote suppuration I had witnessed in the hands of farriers; and the foot was covered with a poultice. Subsequent to the incision, there was not the slightest recurrence of spasm. On the following day I found the whole wound in process of free suppuration; the entire cicatrix completely separated from the tissues; it had united, partly by sloughing, partly by purulent interposition. My subsequent prescriptions were directed to the restoration of the digestive secretions, in which great derangement was manifested. Dismissed, cured, the 6th of May.

Case VI.—July 22, 1839. Visited a negro woman aged about 30, the property of Capt- Wm. Jouit, U. S. A., at the plantation of Mrs. T. of Jefferson county, Kentucky. Found her affected with general spasms without intermission; speechless, and apparently unconscious, presenting a boardlike rigidity of body. This was the second day of the disorder. Dr. L. Powell had preceded me in attendance, by whom the patient had been freely bled and purged, and subsequently stimulated with tinct. opii. On my arrival I inquired whether a wound of any description had been received, and was informed she had stumped and wounded her foot some two weeks before, of which she had made very little complaint. On examination, a lacerated wound of moderate extent was found near the articulation of the second toe with its metatarsus. It exhibited not the slightest appearance either of suppuration or inflammation, presenting more the appearance of a laceration in the integuments of a dead than of a living subject. The character and situation of the wound was such, that I conceived its extirpation could be accomplished with greater facility by cautery than by the knife.

Accordingly, a suitable piece of iron was procured, and heated to a whitish heat, and the limb being firmly held by several stout men, I applied the cautery with a firm pressure. The limb was so suddenly and violently exerted, as to upset some of the men who held it; her jaws expanded, and with a yell she bounded upon her feet. She had no return of spasm. The foot was covered with a ley and bran poultice, until suppuration was induced, which occurred in about sixty hours. The bowels were kept in an open condition with mild mercurial purgatives, under which she was restored to her ordinary health in about a week.

Case VII.—J. S. I., a merchant of Louisville, Ky., an uncommonly stout and athletic gentleman, on the 18th of September, 1840, at about 5 o'clock P. M., trod upon a nail that projected through a plank, which penetrating the double sole of his boot, entered deeply into the ball of the great toe. The nail was so rusty and rough, that in extricating the foot, the sole was torn from the boot. At about seven o'clock he was attacked with spasms; I visited him between seven and eight, where I met Drs. Leach, Knight, Sprout, and several other physicians who had been called together by the different runners. His spasms greatly surpassed anything of the kind I had ever witnessed; the jaws had already become immoveably clenched; the spasms of the body, the anterior muscles of which were chiefly affected, were intermittent, the paroxysms recurring at very short intervals, jerking the extremities of the body together with irresistible force. As many of the stoutest men in the room as could get on or about him, were endeavoring, by holding and making pressure, to prevent the contractions. These, on the spasmodic recurrence, were thrown as if they had been pigmies—some of them hurled to a considerable distance. The development or contraction of the sterno-mastoid muscles was particularly striking, exhibiting muscular bellies of greater magnitude than a common sized arm.

Several ounces of tinct. opii. had been administered before my arrival, and ejected by vomiting. Free blood-letting, and the application of cautery to the wound, was now determined upon. After bleeding, the point of a common poker, heated to a high degree, was inserted, and firmly pressed into the wound. The spasms instantly relaxed; the patient, who was unaware of the character of the application, manifested the greatest pleasure and delight, exclaiming "what are you doing? delightful! you make me feel as if I were in heaven."

An ounce of laudanum was now administered and retained. The foot was covered with a ley mush poultice. Three slight spasms, affecting the muscles of the neck, occurred after this, but every thing of the kind finally subsided before 11 o'clock, after which he slept well until morning, 19th—prescribed calomel grs. x, opii. grs ii.; to be followed by small doses of calomel and opii. every three hours until biliary secretion was reinstated, the derangement in which was such as to indicate that this condition must have preceded the wound, and was probably the predisposing cause of the tetanus. Suppuration occurred on the 3d day, the cauterized eschar separated on the 5th, exposing the bone to view. The wound was after this dressed with ung. basil. until healed.

Case VIII.—Visited Jane, a middle-aged free negro woman, on Green-street, January 25, 1842. This was the 7th day of the disease; the spasms had become general and without intermission; pulse pliable, and too frequent to be counted. Two respectable physicians had preceded me, who had on this morning abandoned the case. Their treatment I ascertained to have been the ordinary stimulant and anti-spasmodic; among others large draughts of tinct. opii had been administered. My first inquiry was whether any wound was known to have been received within a few months or years past, from snags, nails, or the like. After some time it was recollected that "a long time ago" she had wounded the top of her foot against a billet of wood, "but it healed up in no time, and never troubled her." A cicatrix was found on the top of the foot, between the first and second metatarsal bones. An incision upon this through the skin brought into view a small hole through the fascia; on enlarging this and pursuing the sinus, which inclining outward and downward passed entirely under the tendon of the second toe at the distance of nearly an inch from the outer scar, I detected a piece of wood about the size of a grain of wheat, lodged between the second and third metatarsal bones. This was removed, a light sprinkling of corrosive sublimate and red precipitate was thrown in, and a poultice applied to the foot at 11 A. M. In about a half hour the spasms had subsided, and she desired to have her neck and legs rubbed, and drank some water. Before 12 she fell into a profound sleep, the first since the commencement of the disease; pulse 100, soft, and moderately full; I directed that she should not be disturbed.

Visited at 4 P, M. Still sleeps profoundly, and as if from the influence of opium; has had three slight spasms about the neck, since my morning visit. Directed that patient should remain undisturbed and in quiet. Visited again at 7; patient pulseless and covered with cold clammy perspiration; stimulants and frictions unavailable; died a little before eight.

I think it possible that this patient might have lived some time, had I not been deterred by the extreme exhaustion and debility manifested in this case after the spasms subsided, from the use of the purgatives and alterants that the condition so manifestly demanded; marked by general deficiency of secretions, tongue covered with dry ash colored fur, bowels immovably bound since the commencement of the attack. She had no spasms after 4 o'clock.

Case IX.—A negro man aged 20, of Jefferson county, Kentucky, was attacked with tetanus on the 4th of October, 1845, in consequence of a cut into the ancle joint four days before. I visited him on the 5th, with Dr. Gatt, his attending physician. The wound being gangrenous, and the synovia flowing from the joint, nothing short of amputation was considered adequate to the emergency. Dr. Bayless, subsequently called in, concurring in this opinion, the operation was performed forthwith. It produced no change whatever in the spasms. He died the day following.

Dr. E. F. Wilson, one of the surgeons of the Louisville Marine Hospital, has recorded an interesting case of tetanus in the third volume of the Western Medical Journal, which occurred in July, 1841, in an athletic young woman, 19 years of age, in consequence of wounding the great toe with glass 4 months before. He visited her on the third day of the disease, administered a large dose of tartrate of antimony, followed by a large dose of calomel and opium. On the following day he made an incision into the toe, and extracted several small pieces of glass, and prescribed poultices and mild aperients, under which she perfectly recovered. Dr. Wilson inclines to ascribe the restoration to the medicine.

Tetanic irritation is not peculiar to the motory nerves, but is in many cases perceived to arise from irritating matters in the intestinal canal. I once saw a case which seemed to owe its origin to pulmonary irritation. I have met with several of these cases which are commonly distinguished as idiopathic, and conceived to be referrable to irritation of the functional nerves, chiefly in infants, but in two instances occurring in grown subjects. All these cases I observed to be marked with the peculiarity of an open instead of a closed mouth—"the jaw fall," as trismus nascentium is vulgarly called. The uniformity of this peculiarity occurring in adult as well as in infantile subjects, has induced the impression that this may be diagnostic of tetanus from irritation of the functional nerves.

From this form of the disease I have never known a recovery; and post-mortem examinations have exhibited a singularly excessive vitiation of the digestive secretions, the contents of the alimentary canal being inconceivably offensive in odor, and presenting to the eye nothing whatever that is natural to the situation. Mr. Abernethy informs us that these secretions are of somewhat similar character in traumatic tetanus. The idiopathic cases, I have attended, have all died before I had time to effect the dislodgement of such matter from the bowels.

With the hope of devising some method whereby the disease in such cases might be temporarily suspended, and time gained for medicinal operations, and also, the better to satisfy myself with regard to the exciting causes of traumatic tetanus, I instituted the following experiments.

EXPERIMENTS ON DOGS.

Nov. 22, 1840.—Introduced minute tack points into the muscular spinal nerve of dog Gumbo, at the point where the nerve passes over the lower end of the radius; and, on the same day, into the ulnar nerve of dog Watch, at the point at which this nerve passes over the upper head of the os ulna; passing the tacks completely into the nerves, burying both extremities within the theca. The wounds healed kindly; the licking of the dogs preventing the ability to determine, whether with or without suppuration. They fed well, and continued healthy for six weeks.

On the morning of the 5th of January, 1841, Gumbo was found convulsed, with his jaws firmly clinched. Nothing was done in the case until 3 P. M., when the whole muscular system had become rigid, respiration difficult, the abdomen retracted, and a bloody looking sanies flowed from the bowels.

In this condition with the assistance of Dr. Fenley, he was trepanned without evincing sensibility to the operation. On compressing the brain with the ball of the thumb, complete relaxation of all the muscles was effected in the course of a few seconds. In about an hour and a half the spasms recurred, when they were again relaxed by the compression; the animal recovering some degree of sensibility during each relaxation. On the next recurrence of the spasm, at nearly six o'clock, an incision was made at the wrist, and the portion of the nerve containing the tack was removed, when the spasms relaxed and returned no more. This circumstance, by the by, may be attributable to exhaustion. The animal died half after seven o'clock.

Autopsy 18 *hours after death.*—Lungs, heart and pleura natural; liver congested and preternaturally dark; gall bladder distended with dark, viscid bile; mesentery healthy, presenting rather a deficiency of blood: not one turgid vessel discovered in its substance. Stomach: its mucous coat unnaturally pale, containing a large quantity of liquid resembling turbid coffee, and depositing a coffee-ground sediment; duodenum, jejunum and ilium of dark red color externally; mucous coat livid, but no where sphacelated, containing much viscid, greenish-brown matter; cœcum, colon and rectum empty, and of natural appearance.

This mode of relaxing spasms by pressure upon the brain might readily be practiced in trismus nascentium through the fontanelle, and might, perchance, facilitate, as well as afford more time to purge off the vitiated irritating contents of the alimentary canal, I have not met with a case since experimenting, on which to make the trial. The removal of spasm by this operation is manifestly the consequence of the production of paralysis. Might not the like temporary relief be afforded by compressing the main trunk of a nerve in case of a wounded extremity? Or by impeding the venous circulation by means of a bandage about the limb, and producing general pressure by sanguineous congestion?

The second dog, Watch, (mentioned above) became affected, three days after Gumbo's decease, with irregular spasms; these increased as the day advanced, and in the evening he appeared unusually uneasy and restless; the following morning he was missing, and has never since been heard of.

In the commencement of these experiments I had intended prosecuting them much farther, with the hope of arriving at some satisfactory conclusion as to the ordinary cause of the disease, and of deriving some improved mode of treatment. But, at about the period to which I have narrated, my attention became so engrossed in investigations in another class of diseases, that I was but too easily induced to suffer the torture inflicted upon the animals, and the trouble of the operations to deter me from any farther experimental attention to the subject. But the few facts and cases herewith submitted, inconclusive as they are, may nevertheless, by adding to the facts already possessed, possibly become instrumental in enabling some master mind in the profession to develope a more successful treatment of this obscure and terrible malady, than has been hitherto attained.

Art. III.—*Pneumonia—"Winter Fever"—its Symptoms and Treatment.* By Richard H. Day, M. D., of Batesville, Arkansas.

Messrs. Editors:—This essay was written several years ago for publication in the Maryland Medical and Surgical Journal, but was never sent on, in consequence of the demise of that Journal. Having revised and made such additions and corrections as recent observation and experience have supplied, I solicit its publication in your most excellent and highly promising periodical.

Pneumonia, commonly designated by the people and physicians where it prevails, by the term "Winter Fever," is a disease of frequent occurrence in the low-lands of Indiana, Illinois and Arkansas, and from what I can learn, in all of the Western states ; and from its extreme and often sudden fatality is more dreaded than all other diseases to which we are liable in these regions.

It is a disease which I regard as endemical, differing in several points from the disease bearing the same name, which I used to see while practicing medicine in Calvert and Prince George's counties, of Maryland. During a five years practice in that state, I do not recollect of seeing a single case bearing any close resemblance to the affection now under consideration. There we frequently had, in the low counties adjoining the Potomac and Patuxent rivers, in their miasmatic districts during the winter and vernal seasons, cases of acute bronchitis, pleurisy and pneumonia biliosa ; all of which are faithfully and correctly described, and their proper treatment suggested, in that excellent and practical work of Dr. Eberle on the "*Practice of Physic.*" We also had an occasional attack of typhoid pneumonia, occurring in the West and South-West.

In the spring of 1837, I emigrated to Illinois, and settled at Mount Carmel, Wabash county. I had no case of it until the succeeding winter, at which time it prevailed.

It will be necessary to premise here that wherever these peculiar cases of pneumonia are common in the cold and variable seasons of the year, we find that in autumn bilious remittent and intermittent fevers, and their kindred, abound. I make these remarks at this time as it may serve in my further investigations to throw some light upon the nature of this unique and complex malady.

Mount Carmel is situate on the west bank of Wabash river, upon a high eminence about one fourth to half a mile from the river. Between this eminence and the river the ground is on a level with the other bottom lands of this river, and is only inundated once in a few years. This ground has all been cleared off, and is now entirely dry, although a few years ago it was almost impassable. Above and below Mount Carmel, on the Illinois side, the lands are only partially cleared, are of a rich sandy character, and rather wet, with ponds and ravines frequently interspersed. These bottoms are from one to two miles wide. On the Indiana side, immediately opposite Mount Carmel, Patoka, a small sluggish stream, empties into the Wabash. Above about half a mile or three-fourths comes in White river ; and for miles above and below, varying from two to eight and ten miles wide, the land is low, frequent sluices and ponds, and a good deal of it wet and swampy. The land however is very rich and fertile, and on both sides of the Wabash river many families have settled. It is needless to say that in the latter part of summer, and during autumn, miasmatic diseases are extremely prevalent in this district of country.

I have digressed to make these remarks upon the topography of this district, believing them of some importance in the right understanding of the main subject of this essay.

From all that I could learn of "winter fever" through the representations of neighboring physicians, I was led with themselves, to regard it as typhoid pneumonia ; and this cognomen may probably be as near correct as any which might be applied; and yet in very many cases it will utterly fail to convey to the mind the true character of the disease. If such an humble individual as myself might be allowed a choice in the application of nomenclature, I would prefer pneumonia without any prefix at all; or, if any, that of malignant. First, Because I do not regard the type of the disease essentially as typhoid, but as it is rendered partially so by fortuitous circumstances; and, Second, It is at all times a most alarmingly rapid disease, often terminating fatally in twenty-four or forty-eight hours, and requires the most prompt and judicious treatment for its removal.

This disease generally begins with a slight chill, succeeded in a few hours by reaction, attended with dull heavy pain in one side, oppression or weight of the chest; dyspnœa; hurried respiration; pulse accelerated, small and rather tight than hard; cough; expectoration of a glairy mucus, with a small quantity of blood occasionally intermixed; tongue slightly coated brown or white, disposed to get dry, and a great thirst for cold drinks. There is a marked remission every morning of several hours, with increased evening exacerbations; the remission becoming less distinct, the expectoration more bloody with less mucus, the dyspnœa more aggravated; until finally, about the ninth or eleventh day, death closes the scene of suffering by laying its chilling and fatal touch upon the diseased and struggling powers of life. This is not, however, the invariable course of inception, progress and termination of this dreaded affection. Sometimes, for a few days, or week or a two, the individual will have apparently a slight catarrh; a little hoarseness and the expectoration of thin mucus, mixed with bubbles of air; an occasional uneasy tightness or sense of stricture in some part of the chest during inspiration. These may be followed after a while with a severe chill as first described; or slight creeping sensations of cold may be experienced for twenty-four or forty-eight hours, inclining the patient to remain about the fire, or wrapped up in bed; and then in a gradual and insiduous manner the other symptoms supervene.

At other times there is from the first a copious expectoration of mucus and blood, so intimately intermixed as to be of an uniform color, resembling strongly current jelly, and this even before the individual complains of much indisposition. There is frequently an inability to lie with the head and shoulders low; the decubitus is not, however, always the same—sometimes the affected, sometimes the sound side; and at other times the back, with the head and shoulders moderately elevated, is preferred. The pain is never very acute, but rather of an aching, burdensome and oppressive character. It is sometimes the case that reaction is never established; after the occurrence of the chill the extremities remain cool; the patient restless, and most distressingly oppressed; pale or suffused countenance; head and face bathed in a cold perspiration; pulse tremulous and almost imperceptible; a wheezing or rattling low in the chest, as though the bronchia were filled with effusions and secretions; breathing excessively difficult; breath offensive, and the expression anxious and wild. These cases end in death in a few hours, unless relief can be obtained by suitable measures; this, however, is rarely the case, as physicians are seldom called in sufficiently early to be of any service in such violent attacks.

Percussion affords a sound, over the region of the diseased organ, more or less dull, according to the severity or lightness of the attack. Of the sounds elicited by auscultation with the stethoscope, I can say nothing, as I am entirely unacquainted with its use, practically. One of the most common—indeed, I might say invariable—symptoms in this malignant disease, in all of its complications, is the expectoration of mucus and blood in the beginning, which gradually changes to pus and blood, and finally, in cases that result favorably, into pus and mucus alone.

I have before expressed a preference for the term Malignant Pneumonia for this affection, believing it to convey more nearly a true notion of its true character than either bilious or typhoid. It seat is essentially in the proper substance of the lungs, (though most always complicated) or of the membrane lining the air vesicles, and might, as Dr. Stokes properly remarks, be called "bronchitis, of the terminal extremities of the bronchia." As congestion and engorgement of the capillaries and minute venous and arterial trunks, is an essential condition of inflammation in any tissue, I can readily account for the transposition of blood and serum into the air vesicles, giving rise to dyspnœa and bloody expectoration.

Should the stases of blood be so complete in the minute vessels as to offer any great resistance to the propelling force of the heart and larger arterial trunks, rupture of some of these vessels might take place, or a general engorgement of the pulmonary tissue ensue, constituting a true case of pulmonary apoplexy. And this I apprehend to be the cause of those sudden deaths, which sometimes take place in this disease. In what way the cause operates to induce the result, though obscure, will be a matter of some inquiry, when I come to treat of that part of my subject. As I remarked at the outset, it is very rare to meet with this disease in its clear and uncomplicated character. It is most generally accompanied by bronchitis, but sometimes with inflammation of the pleura, and accordingly as it is associated with one or other of these affections, are the symptoms differently modified. Both lungs are frequently implicated: very often, however, only one is affected; and, when such is the case, I have been struck with the wonderful prevalency of its location upon the right lung instead of the left. And even when both are coincidently diseased, the right one is most always more largely involved.

An inquiry into the determining cause of the preference for the right lung, is not within the legitimate domain of this essay.

The writer exceedingly regrets, that owing to his practice being very scattered, rides long and laborious, he has not been able to avail himself of the practical application of the stethoscope, nor of immediate auscultation, by which the true nature, intensity and extent of this disease, in its different stages and complications, may be so accurately known. He has relied upon the signs educed by percussion, and the symptoms previously given, and he is under the impression that, from an extensive experience, and close and careful observation for the last eight years, it is rarely he can be mistaken in this affection. The dyspnœa, obtuse pain in the region affected, the hurried respiration, and the peculiar, tenacious, viscid, bloody expectoration, are a combination of symptoms, which he regards as pathognomic of this malady. He is, however, free to admit, that sometimes these symptoms have been measurably obscured by the existence of some other morbid condition; or that pneumonia has insidiously supervened, during the existence of some other disease, and progressed to an alarming extent before it could be distinguished; which the stethoscope would have detected in its incipiency, and rendered much more amenable to remedial treatment.

This disease is not confined to age or sex; although it is true, as most authors affirm, it occurs more frequently in the male than female. So far as my observations have extended, I am authorized to state that its existence in the early years of infancy is an exceedingly rare incidence. During a period of nearly nine years I have been its careful observer, in districts of country where, for four or five months in each year, it has been abundantly rife; and I do not recollect of more than four or five cases occurring in individuals under the of ten or twelve years; and indeed, it is uncommon under the age of opening puberty.

From the prevalence of peripneumonia in cold and temperate latitudes, during the cold and changeable seasons of the year, and its infrequency in warm and uniform climates, we are led to regard cold with humidity (and I think correctly,) as its common and almost invariably exciting cause. There must, however, be a predisponent; something which, prior to contact with the exciting cause, has brought about that peculiar liability of the lungs to this malignant inflammation. There is something more than accidental circumstances meeting together, at the peculiar juncture of exposure to cold, determining disease upon the lungs in preference to the other viscera; at least there is something, which imparts to it its peculiar and dreaded malignancy. This, so far as my reading extends, has never been attempted to be accounted for. And why this singular silence upon such an interesting topic? Surely it has not failed to thrust itself upon the attention and consideration of medical men. Next to its true and successful treatment, did this subject concern my mind, and mingle with my reflections. This predisposing cause I believe to be malaria. I am aware that the agency of this poison in the causation of even our autumnal bilious remittent and intermittent fevers has been learnedly and ably disputed by Drs. Dunglison and Bell, of this country, and by others, equally eminent, of Europe. Notwithstanding the ability and ingenuity of these authors, I have not been able to yield them my assent. My faith in the entity of miasmata, and their influence in the production of disease, rests upon other facts and observations, not easily explained in any other way. But this is not the place to argue that fruitful and interesting subject of dispute. That must be committed to older and more experienced writers than myself. A few words upon this point, however, may not be amiss in this place.

I have already spoken of the peculiar topography of the country, on both sides of the Wabash river, near Mount Carmel, Illinois, and of the rifeness of intermittent and remittent fevers. Indeed, in the Summer and Fall, they exist alone, or in combination with almost every disease that may afflict that people. These fevers, although serious and fatal if neglected, readily yield to prompt and judicious treatment; which consists in aperients, mild purgatives, and the almost prodigal useof quinine. The very aspect of this people, living near the foci of that malarious district, their relaxed and inelastic muscles, their sluggish circulation; their sickly colour, and their slow unsteady steps, evince that they are subject to the action of a *malaria morbi;* while the ruddy complexion, quick step, firm muscle and bounding pulse, of those who live in high and airy situations, in the same county, point to those low marshy situations as the sources of that morbid material. I was led to regard this poison whatever may be its name, nature, or source, as the cause which induced that condition of the system, favoring the condition of malignant pneumonia, from the fact that this disease prevailed universally in the fields of miasmatic fevers; and generally attacked such as to appearance were most depressed by this malarious poison—while the more rugged, who were exposed to the same exciting causes, most generally escaped, or were lightly affected. And what is worthy of notice, is, that during a residence of six years in Mount Carmel, a place of remarkable salubrity for the Wabash country, I do not recollect of but two cases in the town, and they were individuals who lived on the river bank under the hill upon which the town is built, and spent the most of their time in Indiana, in the infected districts before spoken of.

Since I first noticed this coincidence I have carefully observed and noted the progress of this disease, and its sphere of action; and all my experience and observations go to confirm the opinion before expressed, that *malaria is a predisposing cause of malignant pneumonia.* But if malaria be a predisposing cause of malignant pneumonia, why does not the same form of this disease as takes place in the miasmatic districts of the West, also occur in the low marshy regions bordering upon the Atlantic? This question will be of difficult solution to the satisfaction of the inquiring mind; nor have I been able fully to satisfy myself in regard to it. But observing the fact, I have endeavored to account for it, upon the supposition that the aqueous vapour containing salt, floating in the atmosphere upon the Atlantic coast, might have some effect in arresting or counteracting the deleterious agency of malaria.

That there is a material difference between the same diseases as they occur in the Western and Atlantic States, both in regard to their character and treatment, is a fact to which I believe all observing physicians who have practiced in the two localities will testify. What agent, then, exists, to whose influence we can so rationally attribute the difference, as the saline aqueous vapour which evidently abounds to a greater or less extent in the atmosphere contiguous to the ocean? But in what manner malaria operates in predisposing to the induction of this disease is probably more than can be explained. Whether it primarily operates upon the nervous systems, producing a change in the arrangement of their molecules; or induces some particular alteration in their centres; Whether it operates as a sedative, or a morbid or narcotic poison; Whether it is absorbed into the circulation, operating upon the different organs and tissues through that medium, or whether it produces any chemical and physical changes in the blood itself, are questions yet to be settled, and which I am not prepared to answer.

While, however, I would not attempt an explanation of the rationale of its operations, or of the particular morbid change effected in the human system by its operation, I may be permitted to state it as my belief, that whatever change is wrought takes place while the system is exposed to its deranging impressions; (and not that malaria itself continues in the system for months or years, dormant and unproductive of harm, as was strangely supposed by some of the oldest writers upon medicine ;) which derangement, whatever may be its nature, continues, and gradually more deeply implicating the system, determines to the production of this species of pneumonia, upon an application of the proper exciting causes.

I would also hazard an opinion based upon careful observation, that this morbific agent acts as a narcotico-sedative, upon both the cerebral and great sympathetic system of nerves.

I diffidently advance this hypotheses, hoping that some of my professional brethren better qualified than myself may be induced to investigate this highly interesting subject.

The judicious treatment of this disease, as already intimated, requires a deep and thorough knowledge of the principles of medicine, combined with close and critical observation, and a strong discriminating judgment. No constant and specific treatment can here be instituted with any hope of success. The mere routinist must have the mortification to see his plans baffled, and his suffering patients sink in death in despite of his best directed efforts.

Much has been written for and against the use of general blood-letting; while some make it the great sheet-anchor of safety, others, of equal eminence, affirm that if not really injurious, it possesses no salutary control over the violence, progress, or termination of this disease. If, as Dr. C. J. B. Williams and others of like reputation assert, there is no corresponding increase of the red globules with the fibrine of the blood, during the progress of inflammation, we may readily conceive of its occurrence in a constitution already so enfeebled, and the blood so impoverished, as to render the use of the lancet not only of dubious utility, but of actual and positive injury. But inflammation may also supervene upon quite an opposite condition of the system, rendering the bold and judicious use of the lancet one of the best and most potent remedies in the hands of the medical practitioner. Hence, when we become the exclusive partizans of its use or disuse, we assume an attitude in medicine directly opposed to the advancement and ostensible object of this science.

This disease as it occurred in the districts previously specified, very rarely admitted of general blood-letting; indeed, it was a measure which had been attended so generally with fatal consequences, that it was dreaded almost by every citizen, and it was with difficulty that the physician could meet with a patient who would, without deep reluctance, submit to its performance ; and the physician who was so unfortunate as to lose his patient after using the lancet, was most sure of being charged by the neighbors as accelerating the death of his patient. I have no doubt, from the exceeding unpopularity of the measure, many lives have been compromitted which otherwise might have been prolonged. During my residence in those infected districts, I occasionally met with cases in which general bloodletting was practiced with success ; it could never be carried, however, to a very great extent ; one or two bleedings at the commencement of the attack were about as much as I ever deemed advisable. So extensive was the prostration of the general system, and so early did it manifest itself, that the utmost caution was always necessary in the practice of depletion. In almost every case, whether general bleeding was practised or not, I found cupping a most invaluable remedy. Under the use of cups freely applied, I witnessed pain removed, difficulty of breathing vanquished, expectoration improved, in a manner more prompt and effectual than by the use of any other agent. The posterior part of the chest, opposite the region affected, is the situation I prefer the cups being applied; for this very good reason, of none others, that the anterior and lateral portions are ready for the application of vesications, or emollient poultices whenever they may be needed. I regard, then, general or local bleeding, or both, according to the particular state of the constitution, as of primary importance after reaction is established in the treatment of this formidable disease.

In what manner blood-letting controls inflammation has been too accurately and philosophically investigated by Andral, Laennec, Williams, Stokes and others, for me to undertake it in this short essay.

In regard to internal remedies, I have generally confined myself to a few, varying them in quantity and combination as the condition of the patient, and the stage, violence and complication of the disease seemed to require. After general and local bleeding I have frequently used the following prescription with decided benefit:

℞ Sub. mur. hydrarg. grs. v.
Pul. ipecac. gr. $\frac{1}{2}$.
Pul. camphor. grs. ii.
Acet. morphia. gr. $\frac{1}{8}$.

Repeated once in four hours till some four or six doses had been taken; the bowels then to be gently opened by some mild purgative, such as ol. ricini, sulph. magnes. cum. senna, or pulv. rhei, cum. sup. carb. sodæ. This course should be repeated daily until the disease yields, or some condition arises forbidding its further continuance.

Sometimes it was necessary to omit the calomel and continue the others. Sometimes the addition of pulv. nitre, and at others tart. ant. et potassæ, would be important. Sometimes I found ten or fifteen grs. of blue mass. at night, following by a mild purge in the morning, succeeded by the use of ipecac, morphia and camphor, a most excellent course. During the use of these medicines I had the chest well rubbed several times a day, with some hot stimulating liquid, generally cayenne or mustard steeped in whiskey or vinegar, and a large emollient poultice applied over the part affected. Under this treatment in the course of a few days there was generally an evident amendment, the pulse becoming soft and regular, the skin warm and moist, the breathing more free and the expectoration changed from a mixture of blood and mucus to a yellow consistent pus. Moderate stimulation, suitable diet, and good care on the part of the patient and his attendants, soon restore him to the enjoyment of his usual health.

But it is not always the case that the termination is so promptly favorable. Sometimes the symptoms become more aggravated while the powers of life are fast giving way. Here it becomes necessary to use camphor, opium, carb. ammonia, quinine and epispastics. This course will sometimes rescue our fellows from the very verge of death, and add laurels to the noble powers of the healing art.

There is always in this disease a great liability to a relapse, and much care will be necessary in guarding the patient against exposure by throwing the cover from his arms and shoulders, or talking with his family or friends, lest after you have about gained a victory, you have the mortification of being at last defeated.

There is a peculiarity in the subjects of this disease in the Wabash country, that I have seen nowhere else, which is an almost universal propensity to diarrhœa. Not only in the treatment of the disease now under consideration, but of every other, the greatest caution has to be observed in the use of purgatives. They must be of a mild unirritating character; and an opiate should be left, to be given if the stools are large and watery, or small, frequent and griping. Physicians in that region are almost of necessity driven to be Broussaisists.

When reaction is slow in being developed at the beginning of this affection, the use of camphor and opium internally, and hot frictions and sinapisms to the surface will generally suffice to establish it.

Much has been written of late by practitioners of the highest eminence, upon the superior utility of the large use of tartar emetic in pneumonia. Of this practice I am not able to say much from experience. The almost constant gastric derangement accompanying the disease in the region where I practiced for six years, forbade its use, except occasionally in small doses, as a diaphoretic and contra-stimulant.

I have no doubt in cases where there is no tendency to gastritis or gastro-enteritis it will prove an invaluable remedy. I have had a few cases that admitted of its free use, and so far I have been highly gratified with its effects; but the cases allowing such use have been limited.

This disease as it occurs in the broken country of Arkansas, presents the same general aspects as I have noticed in Illinois and Indiana; but in Arkansas, so far as my observations extend, the constitution being less enfeebled, and the powers of life less worn down, cases here admit of freer bleedings, and are more easily brought under the salutary control of medicine.

While this holds good in the broken country and uplands of Arkansas, in the large bottoms formed by the deltas of large rivers, it is even more fatal,than in Illinois and Indiana. This fact brings me to advert to the opinion before expressed, that malaria has something to do in the causation or control of pneumonia. What else can account for the great difference in the treatment and curability of the same disease in districts so near each other? And what else can account for its greater frequency in miasmatic districts?

In discussing the treatment of this peculiar form of pneumonia, I have designedly been concise, and have not thought it compatible with the plan of this dissertation to enter upon a discussion of the *modus operandi* of the several medicines used in its cure. I have simply stated the remedies generally successful in my hands, wishing particularly to call the attention of medical men to its frequency and peculiarity in marshy districts of elevated temperature.

ART. IV.—*A Case of Aneurism cured by Compression.* By WILLIAM DESPREZ, M. R. C. S., London; of Franklin county, Alabama.

MESSRS. EDITORS,—That injuries of the brachial artery are not of more frequent occurrence is rather a subject of wonder than otherwise, particularly when we come to consider how often phlebotomy is performed by persons ignorant of its proximity to the median basilic vein, and of its anomalous distributions. The following case, if you deem it worthy of a place in your valuable journal, is strongly illustrative of the above remarks.

Miss J. A., æt. 17, of spare habit, nervous temperament, was thrown from her horse in the early part of July, after which she complained of general soreness and some slight fever, for which she was bled by a neighboring practitioner. Next day a small tumour was perceptible at the bend of the arm, attended with pain extending up to the axilla, and very considerable ecchymosis. The ecchymosis subsided under the use of cold applications, but the tumour and pain remaining, induced her to apply to my friend, Dr. Huston, an eminent physician in Tuscumbia, who at once pronounced it false aneurism, and recommended an operation forthwith. As she naturally had a dread of so serious an operation, she waited some ten or fifteen days, when she was advised to consult me.

I saw her on the 24th July, about three weeks after the accident, and found a pulsating tumour, about the size of a hickory nut, at the bend of the arm; pulsation, synchronous with that of the radial artery, considerably diminished by applying pressure on the brachial artery, but enlarging again as soon as it was removed. On examining the cicatrix, which was very small, I found that the operator had missed the vein and cut into the artery. The stethoscope gave the "*bruit de soufflet*" very distinctly. She had occasional shooting pains along the course of the ulnar nerve, particularly attending the least motion, which was consequently very limited. As she and her friends dreaded the operation so much, I was induced to try what pressure would do. I commenced by applying a bandage on each finger; after which I put on a roller bandage, and applied graduated compresses over the tumour and along the course of the artery, gave her tinct. digitalis gutt. x quarta q. q. hora; recommended rest and low diet, and to keep the arm in the horizontal position.

I saw her occasionally during the month of August and beginning of September. The tumour seemed but little reduced in size; the only well marked improvement was, ease from the pain. Your September number just then came to hand, in which I saw Mr. Bellingham's mode of treating popliteal aneurism. It instantly struck me it ought to answer in this case. I got two *presses-artéres*, made much on the principle recommended by him, and applied them on the 17th September; but considered that as the compress on the tumour and the bandages did not give any annoyance, I would leave them on, and thereby make assurances doubly sure. On the 25th I had the satisfaction of seeing the tumour somewhat smaller. She continued to improve slowly, but perceptibly, and about the 15th inst. she found her arm so well that she took of all my "fixings," as she called them. Since then the arm, which in consequence of the long pressure kept up, was smaller than the other, has nearly recovered its normal size and strength. On my last visit, the 18th inst., I could perceive no remains of the tumour; the artery seems pervious along its course;

the cicatrix is nearly ⅛th inch to the inner side of the vein, obviously showing the sad mistake the operator made in bleeding.

Buzzard-Roost, Franklin Co. Ala. December 22d, 1845.

ART. V.—*Case of difficult Labour; breech presenting; Hydrocephalus; delivery effected by the forceps, after the head had been lessened.* Reported by R. J. Farquharson, M. D., of New-Orleans.

Mrs. W. æt. 35, born in Indiana; for a few years past a resident of New Orleans; of lymphatic temperament, but robust constitution; the mother of seven children, all of whom were born at the full term, in an easy manner. According to the account of her attendant, she was taken in labour on the morning of the nineteenth of December, about daylight; the presentation was of the breech, and by 7 oclock A. M., all but the head was delivered. At this time the pains ceased, and the woman was seized with a violent rigor, which lasted about an hour. Between the time that this chill left her, and half past 11 A. M., when I first saw her, in connection with Dr. Bernadon, the midwife in attendance had administered two or three doses of ergot of rye, of size unknown. Her condition was as follows: viz.. pulse, hard and frequent; countenance flushed and anxious; skin hot and dry; slight wandering of her mind. She had not passed her urine since the preceding evening; the bladder was greatly distended, and the seat of great pain; violent and almost uninterrupted contractions of the uterus, and bearing down efforts, had now been brought on by the ergot; an attempt made to pass a catheter, had almost failed, owing to the pressure of the head in the superior strait; when the instrument entered the bladder fairly, no urine flowed, and it was only by a patient elevation of the head during the intervals of pain, (these having happily now become longer,) that relief was at length afforded by very small emissions of urine. An examination per vaginam discovered the nape of the neck at the point of the strait, towards the left groin, and the forehead at the right sacro-iliac symphisis. Thinking it one of those cases where the chin had departed from the breast, perhaps by unnecessary traction upon the lower extremities by the midwife, an attempt was made to restore flexion by means of two fingers into the mouth of the child; this failing, and the child being dead without doubt, I introduced the blunt hook into the orbit of the eye, and continued the attempt to bring down the head. During this traction, which was necessarily violent, the instrument cut its way through the superior maxillary bone, from the orbit of the eye into the mouth. A slight descent of the head had taken place, and a distinct and preternatural mobility of the bones of the vault of the cranium was detected just above the right ear. The nature of the difficulty being now apparent, the blunt hook was introduced through the roof of the same orbit into the cavity of the cranium, and the encephalic mass broken up. The forceps were now introduced, the head grasped, compression as firm as possible made, and the head extracted, after the escape of its contents and consequent collapse. Considerable difficulty was met with in the proper adjustment of the blades of the forceps, owing to the size of the head, * and the neck and shoulders of the child occupying the soft parts of the mother. After the delivery of the child, a severe rigor came on, which lasted two hours, and during its continuance the woman was affected with violent clonic spasms of the muscles of the right arm, and of those of the back and neck. The woman was ordered a laxative enema, and ℞ liq. morph. sulph. f ʒ i, q. h. until the irritation be quieted. Since her delivery no untoward symptoms have supervened, and, according to her account, she feels as well as after any of her preceding confinements. A partial paralysis, (very slight, however.) of her lower extremities, under which she has been laboring since August last, has been gradually disappearing since her delivery.

At the date of this publication, the patient is enjoying her usual robust health.

Art. VI.—*Case of Abscess of the Liver, probably occasioned by forty-two Lumbricoid Worms found in its substance.* By F. A. Bates, M. D., of Marion, Alabama.

On the 18th of February, 1844, I was desired to visit a negro boy, æt. 3 years, owned by Mrs. W. On entering the room where he lay, I found him apparently quiet and free from disease. His tongue was furred, a little white, but otherwise natural; pulse 80; and respiration good; heat of surface natural, and no symptom of disease, with the exception of a considerable fullness of the abdomen, with some little eructation of flatus. At the time of entering the room he complained of no pain in any part of the body, and seemed to notice every circumstance that occurred. Upon making enquiries I learned that he had, for the last few hours, suffered from severe paroxysms of pain, occurring about every two or three hours, but that during the intermission he suffered none at all.

The conclusion arrived at from the description of his attendant was, that he was suffering from colic, and this opinion was confirmed in a few minutes by a return of the paroxysm. The pain was most excruciating, and the sufferer screamed violently, flexed his thighs upon his body, clasped his hands on the seat of pain, the umbilical and epigastric regions, and rolled from side to side. The extremities became cool, and the surface clammy with cold sweat. For a moment the paroxysm subsided, but when it returned, it was with increased violence. A large dose of tinct. opii. was administered immediately, sinapisms applied to the seat of pain, and friction was resorted to, and as soon as practicable he was immersed in a warm, full bath, and kept in twenty minutes. The relief afforded by these means served for awhile to promise permanent ease; he was perfectly quiet and felt disposed to sleep. In an hour, however, another paroxysm came on, but was less violent. Ten drops more of laudanum were administered, with directions to give a large dose of castor oil and spirits of turpentine if the patient did not complain of

* The following measurements were made when the head was in a state of moderate distention, by means of water poured into the cavity of the cranium through the orbit of the eye. Length 25 inches; from umbilicus to vertex, 15 inches; occipito-bregmatic circumference, 23; occipito-frontal, 21 inches.

pain in an hour and a half. The next morning I learned that the patient had no return of pain until near day-light. The oil had been given and had procured copious evacuation of the natural character, mixed with a great deal of mucus.

As the day advanced the paroxysms returned, at intervals of about two hours, and from their frequency he had become quite weak. New symptoms supervened—he frequently picked and rubbed his nose, was constantly rubbing his throat above the larynx, and working the muscles of the pharynx, as if endeavoring to swallow. The white of the eyes appeared of a blueish tinge, and the cornea was uncommonly lustrous. The abdomen was still largely distended, but not sensible to pressure. It struck me that I had seen such appearances before in children suffering from vermicular irritation, and that possibly these symptoms might be referred to worms in the stomach and bowels.

I immediately gave an emetic of ipecac., and tart. antim. et potass., which produced copious emesis, and at the same time two lumbricoid worms, 4 inches in length, were ejected. After the operation of the emetic strong infusion of spigelia and senna was given and kept up for the rest of the day, at suitable intervals, alternated with such remedies as would tend to relieve the pain. The action of the spigelia and senna not being satisfactory, an infusion of the bark of the azedarach was used, and two more worms brought away. Besides the above, calomel, ol. chenopodi anthelmintici, with spts. turp. and castor oil were used, and one or two more were evacuated. These remedies were persisted in until their further continuance was deemed useless. Still the paroxysms returned, but not so frequently. The administration of powerful anthelmintics for three or four days had disappointed our expectations, and the patient had by this time, as might have been anticipated, some fever, with considerable tenderness in the stomach and bowels. There was loss of appetite, emaciation, thirst, small and frequent pulse, so that our remedies were obliged to be directed toward the secondary lesions. The application of remedies failed to produce the desired results; the paroxysms continued till the last, and the patient died in nine days from the time he was attacked. After the first three days opium and its preparatives seemed to fail in relieving pain; he frequently took from 160 to 230 drops of laudanum in the 24 hours, combined with other remedies, without any effect.

Autopsy six hours after death.—The stomach and small intestines indicated a great deal of irritation from their redness. In the latter were found five lumbricoid worms. The liver was considerably enlarged, and the left lobe covered the stomach almost entirely. Upon raising its anterior part, an abscess was discovered on the outer edge of the *lobulus quadratus*, and another small one near the *sulcus* that divides the right from the left lobe. When opened, they discharged about an ounce and a half of pus. An incision made from one abscess to the other, and extended into the left lobe some two inches, revealed a large quantity of worms similar to the others. They were closely packed together in a figure similar to the letter S, and numbered 42. Some of them were six inches long. The other portions of the liver were of a firmer consistence than natural, and appeared somewhat granular. The gall bladder contained a little glairy fluid, the ducts were extremely small, and upon opening them no trace of bile could be discovered. The manner in which these worms found their way into the liver, I presume, must have been through the hepatic duct, while very small. This child had never before presented any symptom that would indicate he was laboring under vermicular irritation. The other organs were apparently healthy.

I have preserved this specimen, with the worms, for the museum of the Medical College.

ART. VII.—*The probable Constitution of Matter, and Laws of Motion, as deducible from, and explanatory of, the Physical Phenomena of Nature.* By. J. L. RIDDELL, M. D., Professor of Chemistry in the Medical College of Louisiana; Melter and Refiner in the Branch Mint, New Orleans.

1. In all enlightened ages of the world, the studious and reflecting mind of man has sought out and entertained expanded views in regard to the physical constitution of Nature. Such views generally obtain credence, provided they harmonize with all the known natural phenomena. But it is frequently the fate of philosophical theories to fall before the progress of experimental science; and, where such progress is great and rapid—where multitudes of new physical facts are brought to light, as they have been within a century past—it becomes a matter of deep philosophical interest, to collate and compare them with the prevalent philosophical opinions, in order to determlne whether some modification of such opinions be not required.

2. Within little more than half a century past, human knowledge has been enriched and enlarged by the entire science of electro-dynamics, by the doctrine of combining proportions in chemistry, and by great discoveries and advances in all departments of physical science: The parallax of many fixed stars has been determined, and the existence of a subtile inter-planetary resisting medium has been established. In the work of deducing, from known and special phenomena, more general laws and conditions, we have advantages, therefore, that were not possessed by Newton, Descartes, or the ancients. In the following pages I shall set forth such conclusions as I have arrived at, in calmly and carefully attempting to contribute something towards so great a work.

3. I am fully aware of the responsibility of suggesting any general views at variance with received opinions. The whole history of science shows that men are always loth to abandon the venerable philosophic opinions in which they have been educated; nothing short of the most clear and rigid demonstration brought home to the conviction, being able to shake their faith in the favorite doctrines and systems of philosophy; such is the affectionate tenacity of the human mind.

4. In giving a preliminary sketch of the views I have been induced to entertain, without preceding them with the chain of inductive reasoning and demonstration by which they have been arrived at, my object is at once to present the chief subjects clearly. That I do not build upon mere assumption, I shall take pains to make sufficiently apparent, before I finally conclude.

GENERAL PROPOSITIONS.

5. PROPOSITION I.—*Matter is any thing real, which occupies by itself, length, breadth, and thickness in Space.*

6. PROPOSITION II.—*Matter exists aggregated into spheroids or atoms, forming in respect to the dimensions of the different terms of atoms an indefinite series, probably geometrical, in which each atom is composed of an aggregation of an indefinitely great number of atoms subordinate in the series in respect to size.* Fixing the attention upon one atom of each term, they present in their relative dimensions, a decreasing or increasing series, whose ratio is indefinitely great or small, and whose number of terms above and below any assumed point, is perhaps infinite.

7. It is convenient to express the assumed material series algebraically; and if

$M =$ a visible material sphere like the sun or earth,
$\ominus =$ any ratio infinitely small,
$\not\infty =$ any ratio or quantity indefinitely great,
$\infty =$ a ratio or quantity infinitely great;

then the material series may be written in geometrical proportion thus:
$* \ * \ *: M\not\infty : M : M\ominus : M\ominus^2 : M\ominus^3 : M\ominus^4 : M\ominus^5 : * \ * \ * : M\ominus^{\infty-2} : M\ominus^{\infty-1} : M\ominus^{\infty}$

8. Here $M\ominus =$ a molecular atom, such as oxygen, or any other of the so called chemical elements,
$M\ominus^2 =$ a more attenuated matter, probably such as an atom of the luminiferous medium,
$M\ominus^3$ &c. $=$ matter probably instrumental in producing the phenomena of attraction,
$M\ominus^{\infty} =$ the unassignable, trancendental last term of matter.

9. That the sun, earth and planets, are spheroidal masses of matter, we are in possession of sufficient proof. And that other terms of matter, though indefinitely removed in point of size, are probably spheroidal in shape, may be inferred from the nature of the forces to which matter is subject.

10. To assign a last term of matter on the scale of minuteness, would be to set limits to a subject which wears every aspect of infinity. Whatever we call great or small, cannot be absolutely great or small; only relatively so. And if there be an assignable ultimate atom of matter, no reason can be given why it should have any special limit of dimensions. Admit its existence, and then comes the natural inquiry as to its theoretical divisibility; which affirmatively forcing itself upon our conviction, we are again compelled to assent that there cannot be assigned an ultimate term of matter.

11. Should it be alleged that it is unphilosophical to invoke the agency of matter so attenuated as to elude our direct observation, I would reply that I think the existence of several material media, differing from each other almost immeasurably in respect to degrees of attenuation, or the relative sizes of their component atoms, may be logically deduced from known phenomena of nature. The impulses causing sound, travel in the ponderable and comparatively gross medium air, at the rate of 1142 feet per second. The impulses causing light, travel in the imponderable and refined medium existing between us and the sun, at the rate of 192,500

miles per second. We infer the luminiferous medium is different from, and far more refined than air, from its apparent want of weight, and the greator velocity of the impulses it transmits. For it may be conclusively shown, that the less dense and the more refined the material medium, in respect to the smallness of its component atoms, the greater the velocity with which it transmits impulsively any given momentum.

Now, as the influence causing gravity, has a velocity according, to LA PLACE, at least 100 million times greater than light, it is philosophical to infer that a still far more refined medium than the luminiferous, is the instrument of its transmission.

12. PROPOSITION III.—*Around each material atom or aggregated sphere, there lies a sort of atmosphere or medium, consisting of diffused atoms belonging to the subordinate terms in the material series.* Probably in the immediate constitution of visible bodies, as iron, gold, water, &c., the actual molecular nuclei, in reference to their intervening spaces, bear some such proportion as that borne by planetary and stellar bodies, to the immense intervening wilds of ethereal space.

13. I think the phenomena of nature warrant us to infer, that probably the intervening spaces are jointly occupied by diffused atoms belonging to the subordinate terms of matter. I do not regard such a space as others have, as being occupied simply by a homogeneous medium of attenuated matter; but as presenting at the same time an indefinite number of media, each perhaps indefinitely more, or indefinitely less attenuated than the next medium above or below in point of grossness or rarity. We are well assured that such is the actual condition of the sun, earth and planets; each being enveloped immediately by a molecular atmosphere, around which lies the luminiferous medium; and from facts innumerable, in chemistry, pneumatics and electricity, we may clearly infer such a constitution in respect to molecular atoms themselves. We cannot rationally comprehend the action of matter on matter at a distance, which often seems to be presented to us in nature, without admitting the existence of material intervening media, as the agents of such action.

14. PROPOSITION IV.—*Matter is inherently inert, or possesses what has been called vis inertiæ: by which is meant that matter can neither of itself begin to move, nor cease to move when set in motion.*

15. PROPOSITION V.—*Matter is indestructable.* Since mankind have never been able to create or annihilate matter, and since mankind have never observed these occurrences in nature; so far as human science is concerned, we may regard matter as uncreatable and indestructable.

16. PROPOSITION VI.—*Matter is inherently and necessarily possessed of no qualities, unless its extension, mobility and inertness be called qualities.*

17. To admit that inherent qualities necessarily pertain to matter, would be equivalent to denying that matter is inherently inert.

18. PROPOSITION VII.—*Motion, existing in time, (of which it is the cause and measure,) is the translation of matter through space.*

19. Though we may conceive of matter without motion, it is obvious motion can have no existence without matter. Momentum is the measure of motion. If $w=$ the mass of a moving body, $v=$ its velocity, and $f=$ its momentum; $vw=f$, The terms force and power, though often vaguely used, mean generally momentum.

20. PROPOSITION VIII.—*Motion is the source of all qualities, and the proximate cause of all phenomena which matter exhibits.* Upon this proposition I shall comment fully in the sequel.

21. PROPOSITION IX.—*Momentum is physically indestructible and uncreatable.*

22. Motion may be transferred from matter to matter, but can never, as nature is constituted, be lost or destroyed, as I shall hereafter attempt to make apparent. So where force seems to be generated it must be derived from prëexisting momentum.

23. PROPOSITION X.—*Momentum is transferable from matter to matter solely by impact or collision.*

24. The transference of motion by impact from matter to matter, is a rational effect from a rational cause; yet it often appears that this impact is mediate, not immediate, between the bodies concerned in its occurrence. This subject, the right understanding of which is so essential to further progress, I will proceed to remark upon, under the head of

IMPULSE.

25. To avoid ambiguity and circumlocution, in speaking of these subjects, where adequate terms of expression are wanting, I find it necessary to express certain ideas of frequent recurrence by algebraic symbols, which I will here for convenience again insert:

Let M=a globe of matter, as the sun or earth,

$\ominus$=any finite quantity or ratio, indefinitely small.

$\not\infty$=any finite quantity or ratio, indefinitely great.

∞=infinitely great quantity or ratio.

0=infinitely small quantity or ratio.

26. The material series of aggregated atoms, as before set forth, may be written down as a geometrical infinite series, thus: * *: M: $M\ominus$: $M\ominus^2$: $M\ominus^3$: * * * *: $M\ominus^{\infty-2}$: $M\ominus^{\infty-1}$: $M\ominus^{\infty}$. Here $M\ominus$ represents molecular matter as the chemical elements; $M\ominus^2$ the luminiferous medium; $M\ominus^{\infty}$ the transcendental first or last term of matter, or ultimate matter.

$M\ominus$ being a molecular atom, let also

[$M\ominus$ =molecular atoms;

[$\dot{M}\ominus$ =molecular atoms aggregated, or under the sensible influence of mutual attraction;

[$\overline{M}\ominus$ =molecular atoms in the gaseous or medial condition, not aggregated, because at too great a mean distance from each to feel very sensibly their mutual attractions.

27. *Impulse*, in physics, is nearly synonymous with the communication of force, or momentum, or motion, from one atom or body to another atom or body. It can best be illustrated by having recourse to an imaginary last term of matter, which to our comprehension has no assignable existence, the series being probably infinite. But if the term $M\ominus^{\infty}$ be imagined, and the individual particles considered as perfectly hard and unyielding; and if we then suppose a rod made of such particles lying in perfect contact, to extend say from New Orleans to New York, it is evident from the premises, that if a blow be given to this end of the rod with a hammer, as its substance cannot yield, the whole rod would at the same instant of receiving the blow be impelled more or less forward in space; and consequently the effect or impulse of the blow would be felt

simultaneously in New York. Thus, though the rod might not progress through space, in consequence of the blow, more than perhaps the ten-millionth part of a line, yet the force of the blow would be conveyed, and might produce an equivalent effect at the other extremity. If, instead of supposing the $M \supset^{\infty}$ particles in contact, intervening spaces be allowed, then the progress of the impulse would require time. A medium of $[M \supset^{\infty}$ might be supposed thus to transmit impulses of motion or momentum, without any assignable translation of the $[M \supset^{\infty}$ medium.

28. Let us next suppose that the $[M \supset^{\infty}$ particles are in part aggregated, so as to make a larger grade of atoms, $[M \supset^{\infty - 1}$ of far greater density than the same volume of the $[M \supset^{\infty}$ medium. If $M \supset^{\infty - 1}$ receive from any one direction impulses from the medium $[M \supset^{\infty}$, a length of time proportionate to its greater density must elapse before it can attain the velocity of the impinging $[M \supset^{\infty}$ particles. Long before it would acquire this velocity, $M \supset^{\infty - 1}$ may mediately impinge upon $M' \supset^{\infty - 1}$, when its motion is gradually arrested by being gradually communicated to $M' \supset^{\infty - 1}$; momentum being successively communicated to every $M \supset^{\infty}$ particle component of $M' \supset^{\infty - 1}$. Thus, to be in equilibrio, the velocity of impulses suffer a slower transmission, the more gross or more highly aggregated the particles composing the medium of transmission.

29. Motion, or momentum, its measure, consists in the progression of matter through space. It may be transferred impulsively from one portion of matter to another, always by the supposed actual contact of the $[M \supset^{\infty}$ atoms. Let $w=$ a given mass of matter, $v=$ its velocity, and $f=$its momentum. By the principles of dynamics $v\,w=f$. It is theorically possible, w remaining constant, that v and f may vary from 0 to ∞. Through any assumed or given medium $[\overline{M} \supset^{n}$, it is also theorically possible that any amount of momentum from 0 to ∞ may be transmitted impulsively. For if $w=$ the material mass of the given medium, and $vw=f$, when v becomes v', f becomes $\frac{v'f}{v}$. So when v becomes 0 or ∞, f becomes 0 or ∞.

30. The circumstances or conditions determining or making definite the velocity of impulses transmitted through any given medium, are: First, the grossness or rarity of the medium as to which term of the geometrical material series it belongs; in other words, the actual amount of matter embraced in the atoms of the medium occupying a given space: Second, the amount of momentum which the medium has to transmit in a given time. For if f be supposed constant, the impulses must travel ∞ times faster in the medium $[\overline{M} \supset^{n}$, than in the next grosser medium $[\overline{M} \supset^{n-1}$. Because if the tenuity of the medium be in any degree proportionate to the masses of the respective atoms, and if $w=$ the mass of $[\overline{M} \supset^{n}$ in a given space, $\infty w=$the mass of $[\overline{M} \supset^{n-1}$ in an equal space; and since $vw=f$, if w become ∞w, the equation becomes $v'\infty w=f$. Now since $vw=f$, and $v'\infty w=f$, $vw=v'\infty w$, and $v=v'\infty$. Whereas if in any medium as $[\overline{M} \supset^{n}$, f be supposed to increase or diminish until it become af, the equation becomes $v'w=af$, and $v'=\frac{af}{w}$; whereas $v=\frac{f}{w}$. Hence $f : v :: af : v', =av$.

31. In our atmosphere we see this exemplified in the impulsive transmission of sound; it being a well known fact, that the warmer the

air—that is, the greater the amount of molecular momentum, which is essentially heat—the more rapid will be the transmission of sound. In reference to this matter, we may estimate the actual amount of molecular momentum in the air at any temperature t, expressed in degrees as units. Put $t = 60°$ Fahr., $t' =$ some higher temperature $v = 1145$, v'=velocity of sound at temperature t', and $x =$ the molecular momentum sought. Now $v : x :: v' : x + t' - t$. Hence $x = \frac{vt' - vt}{v' - v}$.

ON THE INDESTRUCTIBILITY OF MOTION.

32. When we carefully scan those phenomena of the physical world which come within our cognizance, it is forced upon our conviction, that motion or momentum like matter itself, is indestructible. When, to ordinary observation, momentum seems to be destroyed or lost, it is merely diffused so as to become, to us, insensible. Friction is the transference of motion from a moving body in part to molecular atoms, and doubtless in part to the subordinate material media.

33 The assumption that motion is destructible, which may be regarded as the basis of the present received philosophical theories, is necessarily connected with the doctrine of occult inherent qualities. For if motion be constantly destroyed in nature, the universe, like a clock, would run itself down. Hence it is found necessary to derive fresh supplies of momentum, from the supposed inherent qualities of matter. The admirable consistency and harmony, which nature every where presents, when her ways are sufficiently inquired into, force upon us the conviction that every natural effect must have an adequate and rational cause. The most eminent philosopers from the days of PLATO to the present time, assent to the self-evident proposition, that mere lifeless matter is of itself inert. Yet with this truth before them, they inconsistently attempt to explain the phenomena of nature by reference to the supposed specific, occult, inherent qualities of this same matter.

34. The only really serious argument which can be adduced in favor of the destructibility of motion, may be drawn from the mechanics of collision, as set down by Wallis and others, viz: "If two perfectly hard bodies *A B* impinge against one another directly, they will either be at rest after impact, or will move on together as if they were one mass," (Ed. Encyc. xiii, 616.)

35. We see no perfectly hard bodies in nature but on the contrary, all bodies possessed of nearly the same elasticity within greater or less limits of compression. Yet the unassignable $[M \Subset^{\infty}$ atoms, may for illustration be supposed to be perfectly hard, and as they alone can be concerned in the actual, material contact of collision, it will be well for argument's sake, to admit the truth of the before mentioned proposition, and determine its bearing in the premises.

36. Collision is the only rational and comprehensible means, by which motion can be communicated from one portion of matter to another. And those of the last term $[M \Subset^{\infty}$ excepted if we suppose, as seems probable, that every atom of matter has around it an envelope of the next subordinate medium, the only real material contact can take place between $[M \Subset^{\infty}$ atoms of matter. Hence, in the $[M \Subset^{\infty}$ material terms alone, could there occur a loss of momentum. Now there are necessarily ∞ chances to 1, that the loss of momentum would not be

complete; inasmuch as there are ∞ chances to 1 that the impact would not be direct. Admitting the loss of momentum by the aforesaid impact among the $[M\ominus^{\infty}$ atoms, let us inquire what ratio of this loss would necessarily be felt by the molecular, and visible bodies about us.

37. Suppose that $M\ominus^{\infty-1}$ impinges upon $M'\ominus^{\infty-1}$ causing $M'\ominus^{\infty-1}$ to move through a finite distance, and then in like manner to transfer its momentum by impact to $M''\ominus^{\infty-1}$. At the time of impact of $M\ominus^{\infty-1}$ against $M'\ominus^{\infty-1}$, collision would occur among the $[M\ominus^{\infty}$ particles composing and surrounding $M'\ominus^{\infty-1}$: then a translation of $M'\ominus^{\infty-1}$ through space would ensue, in obedience to the impulse, and as $M\ominus^{\infty-1} = \not\infty M\ominus^{\infty}$, it would follow that $\not\infty$ times greater length of time would be occupied in this translation through the said finite space, (during which time the repetition of impulse is not necessary to maintain the motion,) than what was previously occupied by collision of $[M\ominus^{\infty}$ particles. Hence, in reference to a given momentum, f, impulsively transmitted through media. on the rational supposition that the distances between the atoms of media, differing as to the term of matter to which they belong, bear some proportionate ratio to the magnitude of the atoms themselves, there would necessarily be $\not\infty$ times as many collisions of $[M\ominus^{\infty}$, when the momentum f was traversing a pure medium of $[M\ominus^{\infty}$ matter, as when f was being transmitted through the $[M\ominus^{\infty-1}$ medium.

38. And so, if the momentum f were transversing impulsively the $[M\ominus^{\infty-n}$ medium, the number of collisions of $[M\ominus^{\infty}$ atoms (the only collisions of actual contact which could occur, upon the supposition before stated,) would necessarily be $\not\infty^{n}$ times less than in the case where f was transmitted wholly in the $[M\ominus^{\infty}$ medium. Hence, as an atom of molecular matter, (one of the chemical elements, as oxygen,) $= M\ominus = \not\infty^{\infty}$ times $M\ominus^{\infty}$, in the transmission of f momentum through air, the hypothetical loss of motion in consequence of the number of $[M\ominus^{\infty}$ collisions becomes $\frac{1}{\not\infty^{\infty}}$ of what it would be, were the $[M\ominus$ particles of air themselves the ultimate term of matter.

39. Hence, admitting the proposition, that the collision of perfectly hard bodies results in a loss of momentum, and that the transcendental ultimate $[M\ominus^{\infty}$ atoms of matter are perfectly hard; yet, if visible bodies embrace in their inherent structure a series of never-ending subordinate terms or atoms; the collision and friction of visible bodies are attended with a loss of motion$=0$, or infinitely small.

EXPERIMENTS ON CONTINUOUS AND OPPOSING IMPULSES.

40. As illustrative of this subject, I will here cite some experiments which I have often repeated, demonstrating in a striking manner, the impulsive transfer of momentum through rods of iron, without a sensible movement of the mass of iron. 1. Bend a rod of iron ten or fifteen feet in length into the form of the letter U; fix each limb firmly in a vice, or confine them otherwise; a slight blow on one end of the rod, will project with considerable force a marble in contact with the other end, the direction in which the marble is impelled being opposite to that of the blow given. 2. Arrange continuous rods so that the impulse from a single blow may be divided and traverse the same rod in opposite directions at the same instant;—and it may in like manner be shown, that opposing impulses in the same substance do not annihilate each other.

MOTION OF THE FIXED STARS.

41. Motion being indestructible, and intercommunicable among all grades of atoms and masses, we should not expect to find any matter in the universe, in a state of absolute rest. That the component molecules of visible bodies possess incessant movements of rotation and oscillation, will be rendered apparent under the heads polarity and heat. But the mass of mankind, judging from inadequate observation, find an apparent exception to this alleged universal prevalence of motion, in the so called fixed stars.

42. In contravention of this idea I would remark, that it has been demonstrated by Dr. Halley and others, that many of the so called fixed stars "have a motion of their own, which cannot arise from parallax, precession or aberration." Lalande is of opinion "that there is a kind of equilibrium among all the systems of the universe, and that they have a periodical circulation round their common centre of gravity." (Edin. Encyc. ii. 638.) Among the stars of the first magnitude which are known to move, are Aldebran, Capella, Rigel, Alpha Orion, Sirius, Regulus, Spica Virginis, Arcturus, Antares, and Alpha Lyræ. Enough is known in this department of astronomy, to leave no reasonable doubt, but that motion pertains to every visible star, which might be made apparent by accurate observations, at periods of time sufficiently remote.

43. Our sun may very properly be regarded as one of the fixed stars; and yet the observations of Argelander, published in 1837, and the still later observations and computations of Otho Struve at the Pulkova Observatory, leave no room to doubt, that the sun with the attendant planets, is at present progressing through space towards a point in the constellation Hercules, at the annual rate of near 147 millions of miles. (Vide Silliman's Jour. xlvii. 93.) It is worthy to be borne in mind, that two known circumstances conspire to veil the movements of the fixed stars from common observation: their immense distances from us, so great, that light itself sojourns for many years in its passage from the nearest of them to us; and the grandeur of their periodic movements, occupying periods of time beyond our conceptions, if commensurate with the vast and immeasurable distances in space that intervene between them. Were our sun and solar system to continue moving at the present rate (147,000,000 miles annually), and in the same direction, as before mentioned, near one million of years would elapse before we should attain the neighborhood of, or arrive opposite to, the nearest fixed star in the constellation which lies in our path.

IMPULSIVE ATTRACTION.

44. Attraction is that tendency which bodies or atoms apparently manifest to approach other bodies or atoms. Since matter is inherently inert, the tendency must proceed from an external cause. For reasons which will be subsequently given, we are warranted to infer that gravitation, as well as the attraction among the particles of liquids, etc. has its origin in the transference of momentum from media of the more refined terms of matter.

45. As with others, I cannot but regard mere matter as absolutely inert, and as any material finite body is confined within definite space and dimensions; the very idea of its inertness would preclude the notion that it could itself act; while the idea, that inert, and confined, it could act upon another

body at a remote distance from itself, is in my opinion doubly absurd. But it is perfectly rational and easy to comprehend and explain how two bodies might be impelled to approach each other in obedience to right lined impulses of motion received from a circumambient medium, whence it would necessarily follow, as bodies in respect to each other must intercept an amount of these impulses inversely proportionate to the squares of their mutual distances, the *laws* of gravitation as developed by NEWTON must be true.

46. That impulsive motion should travel in all possible directions through all possible points in space, may be reasonably deduced, from the assumptions that no assignable portion of space is devoid of matter indefinitely divided, and that motion is communicable from matter to matter without loss; for if all the sources of reflected and transmitted motion be taken into the account, nothing short of impulsive motion universally radiant will present a result commensurate with the cause. If it should be averred in opposition to these views, that this is complexity instead of simplicity, it is what nature every where presents, a complexity of harmonious results from the operations of laws of the greatest possible simplicity.

47. If, for illustration, we suppose our earth to be the only planetary or stellar body in the universe—and, further, that impulses of equal force come from all points of space in a subtile surrounding medium, it is obvious that as the effects of these impulses felt by the earth, would oppose and neutralize each other, the earth therefrom could derive no power or tendency to move. If, now, we suppose the moon to coëxist with the earth, it is clear that as they would mutually intercept from each other impulses that each would otherwise receive, they would be impelled to approach each other in obedience to those impulses which, in consequence of the interception, would not be counterbalanced.

48. Similar reasoning is obviously applicable in explanation of the molecular attraction manifested between particles in liquids, etc.; though the mainly efficient material agents bearing the coherefacient impulses may be proportionately more attenuated. But whatever may be the field or sphere of the attraction, it can observe but one law in respect to varying distances between the attracting atoms or bodies, provided it be caused by right lined intercepted impulses, as above set forth. The force of such attraction must vary inversely as the square of the distance, as I shall soon show.

GRAVITATION.

49. In respect to the comparative merits of the hypothesis of inherent gravitation, as contrasted with the doctrine of impulsive gravitation, much might be said. The former stops short and assumes an inherent inexplicable and occult quality, as residing in ponderable matter; the latter refers the phenomena to antecedent causes, rigidly in accordance with the known *vis inertiæ* of matter and the established laws of motion. If gravity be an inherent quality, what is its nature, and how may it be defined? Admitting its existence, it cannot be material; else it would necessarily be inert, like other matter: it would require to be forcibly radiated to the objects or bodies proposed to be affected by it; in which case it would necessarily produce repulsion rather than attraction. Matter may be defined, properly, to be something, anything or everything,

occupying, *per se*, length, breadth and thickness in space. And, conversely, whatever independent entity occupies space, must be matter. If gravity be not matter, then it cannot be conceived to exist in space; and if it does not exist in space, it exists nowhere, and must be nothing.

50. Gravity cannot be abstract motion or momentum, for motion or momentum obviously can have no existence separate from matter moving. Thus, if $b=$ the mass of a moving body, $v=$ its velocity, and $m=$ its momentum, by the laws of motion $bv=m$. If, then, we remove the idea of the body or call $b=0$, then in the equation, $ov=m=0$. Hence, velocity and momentum$=0$, when there be no moving body.

51. Gravity cannot be momentum necessarily inherent in matter, because we can clearly conceive of matter at rest; and, further, it cannot arise from momentum resident in the attracting body, for this momentum in being propagated to a distance, and then transferred to another body, would tend to make the bodies recede, not approach.

52. If gravity be an inherent quality, pent up and quiet in matter, how can it produce action at a distance? If it be an incessant emanation from matter in all directions, why does not matter become exhausted of it? If it emanate only towards attracting bodies, how it can it know in what direction to travel?

53. Thus it may be seen, that the admission of attraction as an inherent quality precludes all rational inquiry. Yet so far as man has studied and comprehended nature, her ways are in accordance with reason, and with the equivalent relation of cause and effect. And infinitely beyond what human researches and inquiries can ever attain to, we are, by analogy, warranted in the inference that this equivalent relation holds good.

54. Cause and effect stand in the relation of antecedent and consequent. When by collision the body A in motion, puts the body B in motion, the momentum av of A, is the cause, the momentum bv' of B, the effect. When any particular species of adequate cause, is uniformly observed to precede any particular correlative species of effect, it would be unphilosophical to refer such effect to any other species of cause. Newton says, (Principia ii. 160.) "To the same natural effects, we must, as far as possible, assign the same causes." Now as to the cause of motion, millions of clear instances are presented to every human being in the course of his life, in which motion is caused by precedent motion; and momentum produced from preëxisting momentum. Such is, and ever has been the experience of mankind; while we may challenge the production of a single instance, wherein it can be demonstrated, that motion or momentum has originated from any other cause. Is it not, then, irresistibly conclusive, that any specified motion must have been caused by an equivalent antecedent motion? And, remembering these facts, what reason have we to assume, unsupported by a single known instance, that inherent, quiescent qualities can be a cause of motion?

55. If inherent qualities have the power of originating motion out of nothing, then the mechanical problem of the *perpetual motion* is no chimera, but a promising object of pursuit. Ten thousand active minds have vainly sought in mechanical and other contrivances, for a perpetual, and inexhaustible source of momentum or power. They have not found, and cannot find it, for not in matter is the inherent power to produce motion.

56. From universal observation, then, we may set down as incontrovertible these deductions : I. *Every present material motion has resulted from exactly equivalent antecedent motions.* And, the course of nature continuing as heretofore, II. *Every present material motion must be followed by exactly an equivalent of consequent motions.*

57. The force of momentum giving origin to the phenomena of gravity, must therefore have had an equivalent congeneric antecedent existence, essentially in the direction of the body moving in obedience to it. When we refer the origin of this force to impulses of momentum traversing a subtile medium, we assign a proximate cause, and a cause that can be comprehended and explained. Determining the relations of the assigned cause, by the most rigid mathematical analysis, we find a perfect and every way satisfactory coincidence and equivalency with the phenomena of the motions of the heavenly bodies.

58. Let it suffice for the present to determine according to what law, the force of right lined impulsive attraction, must vary in reference to variable distances between attracting bodies.

A *B*

Let A be a material spherical body in space, whose sectional area as seen from B shall $=a$. Let B represent a point upon a second body, whose distance from A shall $=d$. Now as seen from B, a may be regarded as the base of a cone, whose height $=d$, whose vertex is at B, and the diameter of whose base may here be taken as $\overline{a}|^{\frac{1}{2}}$. Since ponderefacient impulses, if unobstructed must needs come to the point B, in an equal degree from every direction, a represents that portion of the concave spherical presentation of space, from which these impulses are more or less interrupted, which in the form of square aliquot parts of the circle, can be expressed in reference to the whole. $\overline{a}|^{\frac{1}{2}}$ may be regarded as the chord of the arc subtended by A as seen from B, at distance d. If d be changed d', then

$$\left(d' : d :: \overline{a}|^{\frac{1}{2}} : \frac{d\overline{a}|^{\frac{1}{2}}}{d'}\right),\ \overline{a}|^{\frac{1}{2}} \text{ becomes } \frac{\overline{da}|^{\frac{1}{2}}}{d'}$$

Hence the apparent diameters of A, as seen from B, at distances d and d', have the proportion

$$\overline{a}|^{\frac{1}{2}} : \frac{d\overline{a}|^{\frac{1}{2}}}{d} :: d'\overline{a}|^{\frac{1}{2}} : d\overline{a}|^{\frac{1}{2}} :$$

while the apparent areas of A as seen from B at distances d and d', have the proportion $ad'^2 : ad^2 :: d'^2 : d^2$. That is, the apparent area of A as seen from B varies inversely as the square of the distance of A from B. Since the proportion of impulses which A can intercept from B, as before shown, must vary with the apparent area of A, (a representing a constant mass), it is perfectly clear that the force of impulsive attraction which the body A must indirectly exercise on B, varies inversely as the square of the distance between A and B; which result perfectly accords with nature. While if gravity be an inherent power or quality, no reason can be assigned why its force should vary according to any given law.

59. An objection to the impulsive origin of gravity might occur to some, based upon the hasty conclusion, that the impulses intercepted from

any given direction would necessarily vary as the bulk or surface of the intercepting body, and not as the mass; it being well known that bodies have weight in proportion to their mass.

60. This I admit, upon the first view, is a specious and even rational objection. But when we reflect that the nuclei of molecular bodies, probably occupy incomparably less space than what intervenes between them, this objection vanishes. Far more of the impulses may pass unobstructed through a body, than what are intercepted by the atoms immediately composing the body; so that each atom might nearly alike feel these impulses. If for sake of illustration, we assume as the body to be acted on by impulse, the immeasurable yet finite region occupied by the visible fixed stars, the stars themselves being considered as the atoms immediately composing that body, we can readily comprehend how impulses in an attenuated medium, from any assumed direction, could be felt by each individual star, without varying the assumed direction more than the smallest observable fraction of a degree.

61. As connected with gravitation, it is well to inquire whether the views advanced throw any light upon the probable cause of the wonderful coincidence generally observable among the members of the solar systems in the direction of their movements of rotation and revolution. It can be shown, that as the ponderefacient impulses must require time for their transmission through space, they would necessarily tend to give to the planets, movements of rotation and revolution, homologous with the rotation of the sun, such as we see; though it would not necessarily follow that these movements originated in this cause.

62. In respect to the homologous rotation and revolution of the members of the solar system, I will barely rehearse at present, the application of these views to show probably, why the planets revolving about the sun do not sensibly manifest the presense of the resisting medium in which they move, because their projectile movements may be sustained against this resistance by the rotary momentum in the same direction of the sun. That an interplanetary resisting material medium does exist, seems fairly deducible from observations upon the periodic revolutions of ENCKE's comet.

63. The sun rolls on his axis from west to east. The planets revolve about the sun in the same direction. Were two bodies to approach each other with the velocity of the ponderefacient impulses, they could not, while approaching, attract each other, for they could not mutually intercept these impulses in reference to each other. So, it may be shown that acceding bodies attract less, receding bodies attract more. That half of the sun's disc which is by rotation receding from the planet, exerts greater attraction, while the other half of the sun's disc, approaching the planet, exerts less attraction. The centre of the sun's attractive influence upon the planet, is therefore always maintained a little to the east of the sun's centre; a circumstance necessarily contributary to the projectile force of the planet, which is revolving around the sun from west to east. Small as this continual accession of projectile force may be, it is probably adequate to make up for the slight resistance offered by the attenuated material medium through which the planet moves. And the sun is so vast a mass that in countless ages it would not sensibly manifest, in its

own retarded movements, the vicarious influence of obstruction thus reacting upon it.

64. Touching the inapplicability of these, or any other attainable views, in explaining the origin of motion, it is proper to say that I do not attempt to inquire physically into the origin of either motion or matter. Such inquiries infinitely transcend the limits of human philosophy. Observing the matter and motion concerned in the production of particular phenomena, we may sometimes trace them both to anterior conditions, and shew whence the matter and the motion are immediately derived. We may thus, in a few instances, rationally assign a limited series of antecedent proximate causes. But the further we attempt to follow the chain, the more obscure do its apparently never-ending links appear.

65. In respect to the projectile motion in her orbit possessed by the earth, it is questionable whether we can assign a rational antecedent cause; although, as I have before explained, it is apparent how the rotation on his axis of the sun, can contribute to keep up the projectile force of the earth against a subtile resisting medium. It is sufficient for me to know that the earth and planets possess a projectile motion.

66. This motion alone would carry the earth off in a straight line, a tangent to her orbit. Were any constant force in another direction impressed upon the earth, she would compound the forces and move in at straight line, the resultant of the two; but were the second force accumulative, as a force derived from incessant impulses must be, the resultant would be a curved line, bending in obedience to the accumulative force. Now as the centripetal force operating upon the earth, tends towards the sun, by which it is indirectly caused, it is easy to see how the earth, possessed of projectile force, would revolve in an orbit about the sun. For if. r=radius of the earth's orbit, and s=sine of a small arc thereof; and if while the projectile velocity would carry the earth in a tangent parallel and equal to s, the centripetal impulses would carry the earth towards the sun the distance $r-(r^2-s^2)^{\frac{1}{2}}$, it would necessarily follow that the earth would describe a perfectly circular orbit about the sun. But if the centripetal impulses fell short of, or exceeded that effect, the path of the earth would be an ellipsis. Hence we may perceive that the theory of impulsive gravitation, perfectly accords with the mechanical phenomena presented by the solar system in the revolutions of the planets.

MOLECULAR REPULSION.

67. Molecular repulsion is that manifestation of molecular force which in effect gives the integrant particles of a mass or medium a tendency to recede from each other. Its measure is the amount of molecular momentum; its usual proximate cause, the mutual mediate collision of molecular atoms. Repulsion in respect to any medium, is the mutual reaction by impulse, of the integrant particles of that medium upon themselves. All the molecular momentum they possess, must directly contribute to repulsion. Unlike impulsive attraction, where momentum is transferred from the more attenuated to the incomparably less attenuated atoms, as from impulses of $[\overline{M}\infty^{n}$ communicated to atoms of $[M\infty^{n-m}$. Assuming a given surface to be overlaid with atoms of $[\overline{M}\infty^{n-m}$ the proportion of the impulses which would be transmitted through the surface unobstructed, to those which would be communicated to the $[\overline{M}\infty^{n-m}$ particles, would be as ∞^{m} to 1. That is, incomparably more would pass, than would be retained. Hence, as in nature, impulses are necessarily

directed towards all points, the impulsive attraction which would be induced between $M \circ^{n-m}$ and $M' \circ^{n-m}$ would vary in force inversely as the square of their mutual distance, as I have before demonstrated. (¶ 58.) But not so with the force of repulsion.

LAW OF MOLECULAR REPULSION.

68. I will first demonstrate what the law of molecular repulsion for varying distances must be theoretically, and then adduce facts and experiments in confirmation of the law so deduced. We will suppose the molecular momentum of a given cubic mass or medium m^3, to remain constant; in other words that in number the oscillating atoms remain the same, and continue to possess individually the same mean velocity of oscillation; the normal force of repulsion may be measured by the molecular momentum offering itself outward from a side of the cube, m^2; where n particles present themselves at t intervals of time, with v mean velocity. Let m become m', m^2 becomes m'^2, and m^3 becomes m'^3. Now, since each particle has a different space through which to oscillate, the intervals of return to any particular point become $\left(m : m' : : t : \frac{m't}{m}\right)$, $\frac{m't}{m}$; and the number of particles presenting themselves upon a surface $=m^2$ becomes $\left(m'^2 : n : : m^2 : \frac{m^2n}{m'^2}\right)$, $\frac{m^2n}{m'^2}$.

It is obvious, v remaining constant, that the momentum (or repulsion) offering in a given time, upon a given surface, must depend directly upon the number of particles presenting, and inversely upon their intervals of return. Hence, as $\frac{n}{t}=$ the normal force of repulsion on m^2 surface,

$$\frac{nm^2}{m'^2} \div \frac{m't}{m} = \frac{nm^3}{tm'^3} =$$

the force of repulsion upon an equal surface, when m, the linear measure of the supposed cube, becomes m'. It is also clear that the mutual distance d, of the integrant particles, must vary directly with m. $m : m' : : d : d'$. Hence the force of repulsion at distance d : to force of repulsion at distance $d' : : \frac{n}{t} : \frac{nm^3}{tm'^3} : : 1 ; \frac{m^3}{m'^3} : : m'^3 \; m^3 : ;$ or substituting d and $d' : : d'^3 : d^3$. Therefore, *the effective force of molecular repulsion among atoms, as exercised in a given time, through a given plane varies inversely as the cubes of their mutual distances.*

69. I will now proceed to compare the foregoing theories with the actual results of experiment. Elastic fluids, as air, occupy an amount of cubic space, exactly proportionate to the degree of pressure applied to them. Under a pressure of ten atmospheres ten cubic inches of air become one. Here the pressure is increased from 15 to 150 lbs. to the square inch. And so, proportionately, for other degrees of pressure.

Here let d=mean molecular distance at pressure p=repulsion r;
d'=mean molecular distance at pressure p'=repulsion r';
wherefore d^3=volume of elastic fluid at pressure p,
and d'^3=volume of elastic fluid at pressure p'.

Now since the volume varies inversely as the pressure, $p : p' : : \frac{1}{d^3} : \frac{1}{d'^3}$;

and $r=p$, and $r'=p'$, $r : r' : : \frac{1}{d^3} : \frac{1}{d'^3}$.

That is, from the most rigid experiments on the influence of pressure upon the volume of elastic fluids, it is mathematically deducible that *the force of repulsion varies inversely as the cubes of the molecular distances,* as before set forth.

CAUSES WHICH GIVE DEFINITE VOLUMES TO LIQUIDS.

70. Since it is demonstrable as I have shown, that the force of right lined impulsive attractions varies inversely as the square of the distance; (¶ 58, 136); and the force of molecular repulsion varies inversely as the cube of the distance; (¶ 68); the causes which give definite volume of liquids, are clearly apparent. For if great pressure be applied to force the integrant molecules nearer each other, the pressure will be met and equalled by the consequent excess of the repulsive over the attractive force; while any temporary cause operating to produce expansion or recession of the molecules from each other, would have to encounter a comparatively increasing force of attraction; which, when the expansive force be removed, would bring the molecules back to their former equilibrium.

FORCE OF MOLECULAR ATTRACTION BETWEEN ADJACENT SURFACES OF WATER.

71. In fluids where the molecules are mutually so remote as not to be sensibly influenced by polaric attraction and repulsion, and where consequently, they are free to move past each other and take new positions, right lined impulsive attraction must be the only bond of union among them. This force, in respect to varying molecular distances, necessarily varies inversely as the squares of those distances, as shown in ¶ 136. With the view of demonstrating the degree of this force, perhaps no better data can be made use of than the rate at which water is compressible. For one atmosphere of pressure (15 pounds to the square inch), water suffers a compression of 513 ten millionths of its volume. It is proper to observe, that if in effect the intensity of the molecular forces vary from an increase or diminution of the $[\overline{M}\circ^2$ medium (¶ 107); or if the forces of polaric attraction and repulsion (¶ 127,) be sensibly operative in liquids, conjointly with those which I have in this memoir designated as molecular, then the following formulæ and consequent numerical results, (¶ 71, 74, 76, 98,) will require corresponding modifications.

Let p=15 lbs. pressure per square inch,
a=.0000513,
x=normal force of molecular attraction =normal force of repulsion.

If the normal volume of water be 1, the volume will be, during pressure, $1-a$.

$$1 : x : : \frac{1}{1-a}\Big|^{\frac{2}{3}} : \frac{x}{1-a}\Big|^{\frac{2}{3}} = \text{abnormal force of attraction. } (\P\ 136.)$$

$$1 : x : : \frac{1}{1-a} : \frac{x}{1-a} = \text{abnormal force of repulsion. } (\P\ 68.)$$

Now, from the conditions of abnormal equilibrium, we have the equation

$$\frac{x}{1-a} = \frac{x}{1-a}\Big|^{\frac{2}{3}} + p.$$

Putting $c = \overline{1-a}|^{\frac{2}{3}} = 0.999966$, and reducing the equation,

$$x = \frac{cp-cpa}{c+a-1} = 866,978 \text{ lbs.}$$

Hence the usual force of molecular attraction among the particles of water, as exercised perpendicularly to any imaginary plane, cannot exceed 866,978 lbs. per square inch, as a maximum.

CAPILLARY ATTRACTION.

72. When one end of a tube of small bore with both ends open, is dipped into a liquid capable of wetting it, the liquid ascends in the tube to a height inversely proportionate to the periphery of the bore. From very many experiments made, I could observe no variation of the height of water and aqueous solutions referable to the chemical or mechanical nature of the tube, provided it be completely wetted by the liquid; from which I am inclined to infer that the limit of ascent in a tube of given diameter is solely determined by the molecular forces in the liquid itself. The weight of liquid thus raised is always directly proportionate to the periphery of the bore.

73. The molecules of liquids, wetting solids by contact, must come within the influence of the polaric attractions of the atoms or molecules of such solids. These forces, as exercised between the highest and last row of liquid particles in the capillary tube, and those particles of the solid composing the tube which lie obliquely a little higher still, must alone be essentially effective in sustaining the capillary column of liquid, in opposition to the weight of the same. The upper extremity of the liquid column presents a concave surface; and the very highest portion of the liquid, sustaining as it does the column below by attraction, must necessarily possess less than a normal density. Fortunately we may approximately determine the limits of this decrease of density, from the laws of molecular forces; and thus will we posssess data for estimating the minimum amount of space occupied by a single particle of water; a most curious physical problem, the definite solution of which has never that I am aware, been heretofore attempted.

DEGREE OF MOLECULAR ATTRACTION OPERATIVE IN PRODUCING AND LIMITING THE CAPILLARRY ASCENT OF WATER.

74. If $d=$ the normal distance $=1$, between adjacent atoms of water, when the forces of molecular attraction and repulsion are supposed to be equal, then (¶ 70) $\frac{1}{d^2}=\frac{1}{d^3}$. If d increase and become d', then $\frac{1}{d'^2}$ is greater than $\frac{1}{d'^3}$; and the expression $\frac{1}{d'^2}-\frac{1}{d'^3}$, will have a maximum value, as may be shown by the differential calculus, when the value of d', is taken at 1.50. At this distance no other forces intervening, there would exist a maximum surplus of attraction over repulsion. If the normal attraction and repulsion at distance d, be each$=1$; the force of attraction at distance $1.5=0.444$, the force of repulsion$=0.296$, and the difference or surplus of attraction$=0.148$. Now it necessarily follows from the laws of equilibrium, that this surplus of attractive force, when developed at the upper extremity of a liquid column sustained as before described in a capillary tube, must be exactly counterbalanced by its cause, the weight of the liquid column. And as I have shown (¶ 71) that the normal forces of cohesion among particles of waters cannot exceed 866 78 lbs. to the square inch, so here by proportion we infer (1 : 866 978 : : 0.148: 128 289.77) that the operative force in the capillary attraction of water cannot possibly exceed 128 289.77 lbs. per

square inch. It probably falls much short of this, and should be determined experimentally. With present data no other maximum can be certainly assigned. (Vide ¶ 136.)

EXPERIMENTAL DETERMINATION OF THE CAPILLARY ASCENT OF WATER.

75. Finding the experimental results given by writers on this subject, discordant and irreconcilable, I determined to make experiments with extraordinary care. By means of a tinsel wedge 5.18 inches long, and 0.15 at bore, tapering to a point, I determined the mean diameter of a very even bore flint-glass quill tube, to 0.432 inches; in this, distilled water rises to the height of 0.91 inches, the thermometer standing at 62° Fahr. From these data it can be inferred, that one linear inch of the periphery of a glass tube, (or in the same ratio, if the tube be smaller), will sustain by capillary attraction .00035441 lbs. avoirdupois of water.

APPROXIMATIVE DETERMINATION OF THE MINIMUM DIMENSIONS OF SPACE OCCUPIED BY A SINGLE PARTICLE OF WATER.

76. Put e=.00035441,=the force of capillary attraction in water per linear inch (¶ 75) in lbs. av.

b=128289.77=the force of capillary attraction in water per square inch (¶ 74) in lbs. av.

x=the thickness of space in parts of an inch, occupied by the extreme highest portion of water in a capillary tube, necessarily consisting of a single row of aqueous molecules.

Now, since it is obvious that $bx=e$, $x=\frac{e}{b}$ =.000000 002762, of a linear inch, while x^3=.000000 000000 000000 000000 02105, which is certainly as small as, if not smaller than, the cubic space in decimals of a cubic inch, occupied by a single molecule of water. That is, the nucleus and envelope of a single molecule of water, occupy as an assignable minimum so little space that nearly 200 quatrillions of them are contained in a cubic inch of water; and the weight of a single one not far from six quatrillionth parts of a grain. To assist the mind in attempting to conceive these numbers, let us compare the molecules of water to grains of common black writing sand, of which a single grain is barely visible to the naked eye at the distance of two or three feet; and of which about 1,000,000 in number are contained within the space of a cubic inch. Did each molecule of water of the limits of size assigned, occupy as much space as a grain of this sand, the 200 quatrillions before mentioned as being contained in a cubic inch of water, would necessarily fill a space so large, that if in the form of a cube, this cube would be near 95½ miles across.

LAWS OF MOTION, CONSEQUENT UPON THE DIRECT COLLISION OF ELASTIC BODIES.

77. To explain the mechanism of many natural phenomena, it is essential that the results of the collision of elastic bodies, should be thoroughly understood. In works accessible to me, on mechanics, this subject is treated on the general assumption that motion is destructible, and therefore in a manner which seems complicated, inconvenient, and objectionable. I shall deal briefly with the subject, my principal object being to give certain dynamical formulæ, of which I may make subsequent use.

78. Proposition I.—If two perfectly elastic spheres of equal mass,

approach with equal velocity and impinge directly upon each other, they will mutually interchange momenta and directions of movement, and recede with the same velocity.

79. PROPOSITION II.—If two perfectly elastic spheres of unequal mass, approach each other with equal momenta, their velocities being inversely as their masses, after direct impact they will recede with equal momenta, each with its first velocity, as before impact. In the two foregoing propositions the centre of gravity between the bodies is supposed to be at rest.

80. PROPOSITION III.—In reference to two perfectly elastic bodies before and after collision, their centre of gravity is either at rest, or moves uniformly in the same direction, both before and after collision.

81. PROPOSITION IV.—In all cases of collision, the two bodies approach their centre of gravity with equal momenta.

82. PROPOSITION V.—Two bodies separating elastically after collision always recede from their centre of gravity with equal momenta.

83. In accordance with the preceding propositions,

A C B

Let $A=$ an elastic body moving uniformly towards C.

$B=$ an elastic body, also moving uniformly towards C, where it is to meet A in direct collision.

$C=$ centre of gravity of A and B, moving uniformly, both before and after collision towards A.

Put $a=$ a mass of A,

$b=$ a mass of B,

$v=$ velocity of C,

$h=$ velocity of A before impact,

$h'=$ velocity of A after impact,

$n=$ velocity of B before impact,

$n'=$ velocity of B after impact.

It follows that before impact

$h+v=$ velocity of A towards C,

$n-v=$ velocity of B towards C.

By PROP. IV. $ah+av=bn-bv$ - - - - - - (1)

$$v=\frac{bn-ah}{a+b} \quad \text{. - - - - .} \quad (2)$$

So likewise, after impact,

$h'-v=$velocity of A from C,

$n'+v=$velocity of B from C,

and by PROP. V. $ah'-av=bn'+bv$. - - . - (3)

Forming an equation from the first members of (1) and (3), $a h'-av=ah+av$

$$h'=h+2v \quad \text{- - - - -} \quad (4)$$

Forming an equation from the second members of (1) and (3), $bn'+bv=bn-bv$

$$n'=n-2v \quad \text{- - - - - -} \quad (5)$$

84. The formulæ, (4) and (5,) are rendered far more simple than those usually given, in consequence of the introduction of v, (2,) as the velocity of the centre of gravity; yet they will be found rigidly correct, and universally applicable to all possible cases coming within the conditions proposed.

85. By these formulæ it may be shown, as I have demonstrated by

actual experiment, that when a large elastic body in motion, impinges upon a very small one at rest, the large one communicates almost twice as great an absolute velocity to the small one. as the velocity possessed by itself.

ON THE DENSITY OF GASES, AND EQUIVALENT VOLUMES

86. A gas or aerial body is made up of molecules, at so great a mean distance from each other, as not very sensibly to manifest, in the phenomena they present, their mutual influence of attraction. The force of molecular repulsion is equilibrated by external pressure, which is ordinarily a consequence of the weight of the atmosphere. By the word molecule, I here mean the $M\circ$ nucleus, whether simple or compound, which is central to a comparitively extensive envelope of $\lceil\overline{M}\circ^2$ matter.

87. We will first determine the necessary laws of distribution in space, pertaining to gaseous nuclei of different masses, under like circumstances of pressure and temperature. Let us suppose the gas *O*, a single molecule of which possesses the mass *o*, to be confined in a thin flexible impermeable bag; and that this bag is surrounded by the gas *H*, a molecule of which has the mass *h*, It is required to determine the relative mean molecular distances at which the molecules of *O* and *H* will be in equilibrio.

Put x=molecular distance in *H*,
y=molecular distance in *O*,
v=mean molecular velocity of oscillation in *H*,
v'=mean molecular velocity in *O*,
S=a given amount of the surface of the bag, separating *O* from *H*.

It will follow that

$\frac{1}{x^2}$=relative number of *H* molecules presenting themselves on *S*.

$\frac{1}{y^2}$=relative number of *O* molecules presenting themselves on *S*.

$\frac{h}{x^2}$=mass of *H* presenting on *S*.

$\frac{o}{y^2}$=mass of *O* presenting on *S*.

Now in order that the pressure may be equal within and without the bag, the molecular momentum reciprocally transmitted through *S*, in a given time, must be equal from both directions. Therefore (vide ¶ 90, 91.)

$$\frac{v^2h}{x^2}=\frac{v'^2o}{y^2} \quad - - - - - - - - - - - - - - - - - (1.)$$

But when h separates impulsively from o, the impact having occurred mediately through S, h and o must possess equal momentum. (Collision, *Prop*. V, ¶ 82.) Therefore $v^2h=v'^2o$ - - - - - - - - - - - (2)

Hence, dividing (1) by (2), $\frac{1}{x^2}=\frac{1}{y^2}$ and $x=y$ - - - - - -(3)

88. It is, therefore, clear that different gases in separate containers, under like circumstances of pressure, temperature, etc., are necessarily so constituted, dynamically, in respect to space, that an equal amount of space will contain an equal number of molecules of each. Hence the density of a gas is a direct measure of its relative molecular weight.

MECHANISM OF GASEOUS DIFFUSION.

89. It has been experimentally established that gaseous bodies differing in density, such as oxygen, nitrogen, hydrogen, watery vapour, ammonia, carbonic acid, etc., possess the power of spontaneously commingling with each other so as to become equally diffused throughout. The only explanation ever offered, deserving a present notice originated with Mr. Dalton, of Manchester, who regarded any one kind of gas as a vacuum in respect to any other kind of gas; which implies that repulsion is not exercised between heterogeneous gaseous molecules. (*Turner's Chemistry, London edition*, 1842, *p.* 213.) This assumption in its full extent, is not only inexplicable, but is untenable. For as repulsion must be the mere physical effect of collision, it must ensue when mass impinges upon mass, or molecule on molecule, regardless of any other consideration.

90. I have shown (¶ 88) that under like circumstances, the same amount of space must contain the same number of any kind of gaseous molecules. As before (¶ 87),

Let o = mass of a gaseous molecule, O,
h = mass of a gaseous molecule, H,
v = mean velocity of oscillation of h,
v' = mean velocity of oscillation of o.

Now, since the molecular distances in O and H are equal, we may safely assume that o and h oscillate through distances nearly equal. It would hence follow that if we assume

av = number of oscillation of h in time t,
then av' = number of oscillation ot o in time t.

The momentum estimated for the time t, will necessarily be directly as the mass, the velocity and the number of oscillations. Therefore o will have a momentum in the time $t = o \times v' \times av' = oav'^2$, - - - - - - - (1)
And so for h, the momentum for the time t, will be $= h \times v \times av = hav^2$. (2)
As before explained these momenta must be equal. Therefore, making (1)=(2), we have $ov'^2 = hv^2$. - - - - - - - - - - - - - - - (3)
Converting (3) into proportion, $v^2 : v'^2 :: o : h$, wherefore $v : v' ::$
$\frac{1}{\sqrt{h}} : \frac{1}{\sqrt{o}}$ - (4)

91. Hence it appears that under like circumstances, the mean velocity of molecular oscillation, varies inversely as the square root of the molecular weight or mass; or what amounts to the same thing, inversely as the square root of the density of the gas. That such is the fact, Mr. Graham has placed beyond a doubt, by experiments upon the velocity of gaseous diffusion through porous septa.

92. I have prepared the following table as relevant to the subject.

I. NAMES OF GASES.	II. COMBINING PROPORTIONS.	III. RELATIVE DENSITIES.	IV. SQUARE ROOT of density. = an approximation towards the comparative length of time occupied in a single molecular oscillation.	V. RECIPROCAL of the square root of density = relative mean molecular velocity.
OXYGEN	$8 \times 2 = 16.00$	16.00	4.000	0.250
NITROGEN	14.15	14.12	3.757	0.266
HYDROGEN	1.00	1.00	1.000	1.000
VAPOUR OF WATER	9.00	9.00	3.000	0.333
AMMONIA	$17.16 \div 2 = 8.57$	8.56	2.925	0.342
CARBONIC ACID	22.12	22.12	4.708	0.212
COMMON AIR	MEAN 14.50	14.50	3.808	0.262

As to column IV, approximating the comparative length of time occupied in a single molecular oscillation, since the centres of the molecules would at each impact approach more or less near each other, dependant in some ratio upon their velocities, a small correction should be made, derived from some direct function of column V.

93. The dynamical condition of a gas, when composed of homogeneous molecules, must necessarily be such that the molecular oscillations are isochronous, meeting each other in collision at equal intervals of time, and with equal momentum. But this harmony and regularity would be disturbed, by offering in immediate contact, heterogeneous gases, differing in molecular mass or density. It can be easily seen that the dynamical equilibrium would be disturbed, in consequence of the different periods of molecular oscillation, as set forth in column IV of the table (¶ 92.)

94. In exemplification, suppose a quantity of hydrogen gas transferred into a jar containing carbonic acid gas, standing over water. Most of the hydrogen would immediately ascend and temporarily occupy the upper part of the jar, by virtue of its relative levity; its density being 1, that of carbonic acid 22.12, which is also the relation of their molecu-

lar masses (columns II, III,). These numbers may be designated by h and c. Let o=the velocity of $h=1$, and v'=the velocity of $c=0.212$, as per column V. Their periods of oscillation are nearly for $h=1$, for $c=4.708$. Before mixture h and c possess in equal time equal momentum (¶ 87). But immediately after being presented to each other, a little reflection will render it apparent, that such cannot be the case. For though $hv^2 = cv'^2$ (¶ 90,) yet when h and c come in collision, they react on each other at that instant with the momenta hv, and cv', the momentum of c being $\frac{v}{v'}$ = about five times greater than the momentum of h. Hence the momentary collision of h on c, does not prevent c from pushing itself forward into the hydrogen. So on the other hand, if c' is oscillating towards the carbonic acid, the superior velocity of h' enables h' to follow c' into the carbonic acid, beyond the normal point of collision for homogeneous gases. In a similar way it can be shown, that a stable dynamical equilibrium cannot be attained until the molecules of carbonic acid and hydrogen are equably diffused throughout each other.

95. Hence we may deduce, that the cause which produces the equal commingling of different fluids, is to be found in the mechanical laws of collision; and that it depends essentially upon the different masses, or amounts of matter, naturally embraced in the different molecules. It must be apparent to any one, after a little reflection, that the commingling of different kindred liquids is produced in a closely similar manner.

96. That carbonic acid, the density of whose molecule is greater than that of nitrogen, in the ratio of 22.12 to 14.12, should diffuse itself into the atmosphere in opposition to gravity, may at the first view seem strange. But when we consider the velocity of its molecular motion, 923 feet per second (¶ 97), and take into the account the transcendentally small absolute weight of a single molecule, 2.45 times that of the molecule of water, (¶ 76) and therefore near 15 quatrillionth parts of a grain; we can readily appreciate the inefficiency of gravity in the way of confining it to the lowest stratum of the atmosphere.

VELOCITY OF SOUND THROUGH GASEOUS MEDIA.

97. It has been determined that air transmits sound ordinarily at the rate of 1142 feet per second, which must be the mean molecular velocity in air, say at 60° Fahr., and 30 in. barometer. From the last column (V) given in the table above, it is easy to determine approximately the velocity with which sound would travel through the several gases named, under like circumstances of temperature and pressure, expressed in feet per second, with the following results:

	RESULTS OF THE ABOVE CALCULATION.	From Edin. Encyc. I. 115. Results set down in Campbell's Table.
In Air, as quoted,	1142	1130
Oxygen,	1089	1064
Nitrogen,	1159	1149
Hydrogen,	4358	3899
Watery Vapour,	1451	
Ammonia,	1490	
Carbonic Acid,	923	922.

TIME OCCUPIED BY A SINGLE MOLECULAR OSCILLATION IN AIR.

98. We have data, I think, which enables us to determine the minimum period of time in which a molecule of a given medium, as air, say at 60° Fahr., and 30 in. barometer, performs one of its orbital oscillations; the interval elapsing between its departure from, and near return to, any assumed point. Thus;

Let v=1142 feet=13704 inches=the velocity of sound in air, as above, per second of time.

m=14.5÷9=1.611=the molecular mass of air, that of water being assumed as unity. (¶ 92, II.)

s=815.=the density of water, that of air being assumed as unity.

x^3=.000000 000000 000000 000000 021=the cubic space occupied by a molecule of water in parts of a cubic inch. (¶ 76.)

It can be seen that hence $\overline{msx^3}|^{\frac{1}{3}}$=.0000002925 of a linear inch = the molecular distance in air, or the central mean distance between the molecules of air in parts of an inch, which is 10.44 times greater distance than that between the molecules of water. Now by proportion,

$$\frac{v}{2} : 1 : : \overline{msx^3}|^{\frac{1}{3}} : \frac{\overline{2msx^3}|^{\frac{1}{3}}}{v} = .000000\ 0000042686 \text{ decimals of a second}$$

of time,= the period of oscillation sought. That is, a molecule of air cannot perform, in one second of time, more than four billions of complete oscillations, as a maximum.

NATURE OF HEAT.

99. By induction from known facts, and established principles, it may be inferred, as I hope hereafter to show:

100. I. That *heat* or *temperature* is that appreciable condition of ponderable bodies, immediately and mainly dependant upon the greater or less intensity of molecular oscillation; and subordinately upon the luminiferous medium, $(\overline{M}\circ^{2})$.

101. II. *Sensible heat* or temperature is referable to that portion of the molecular momentum, mutually interchangeable, and incessantly interchanged among adjacent bodies, such as solids, liquids and gases. Or more specifically, in adjoining media of the same temperature, sensible heat is the measure of intensity of the molecular momentum reciprocally antagonized by each others pressure.

102. III. *Absolute caloric* may be properly defined to imply all the molecular momentum possessed by a body.

103. IV. *Specific heat* is the quantitative relation which the absolute caloric has to the sensible heat.

104. V. *The absolute zero of cold*, is that imaginary condition of temperature in a body, whose molecules are supposed to be wholly divested of oscillatory movements among themselves; and though by calculation the absolute zero may in certain bodies be definitely assigned, as I shall hereafter show; yet as nature is constituted, such a condition is practically unattainable.

105. VI. Contributary impulses from the luminiferous medium $(\overline{M}\circ^{2})$ may increase temperature, by increasing molecular momentum; and conversely, temperature may be lessened by the transfer of momentum from molecular $(\overline{M}\circ)$, to luminiferous atoms $(\overline{M}\circ^{2})$. This proposition involves the doctrine of radiant caloric.

106. VII. When the volume of a body becomes enlarged, in consequence of an increase in the amount of its molecular momentum, the $\overline{M}\supset^{2}$ envelope of each $M\supset$ molecule, must tend to acquire concurrently, an additional quantity of attenuated matter from the surrounding $\overline{M}\supset^{2}$ medium, in obedience to laws analogous to those of pneumatic equilibrium. And conversely, when the volume of a body becomes less from diminished molecular momentum, it would tend to contain within its limits a smaller amount of said medium.

107. Any considerable accession or diminution of $\overline{M}\supset^{2}$ matter, probably in effect sensibly modifies the laws of molecular attraction and repulson.

PROBABLE CAUSES OF THE LUMINOSITY OF THE SUN.

108. The continuous supply of light and heat furnished by the sun, for indefinite ages past, with untiring energy, and without symptoms of abatement, has ever excited the wonder of mankind. Terrestrial fires, which we are apt to consider analogous, require a continual and bountiful supply of fuel, or they go out; while the sun, more brilliant and efficient, beyond the power of imagination to compare, shines on from century to century with undiminished mass, and without any apparent contributions of matter from abroad.

109. Hence the sun cannot, in the proximate causes of its luminosity, be analogous to terrestrial fires, nor can its surface or mass be undergoing the process which we ordinarily call combustion. Yet as heat and light are but sensible manifestations of material motion, the immediate causes of the heat and light emanating from the sun, can I think be demonstrated in a rational and tolerably satisfactory manner.

110. In attempting this demonstration, I shall make use of the following proportionate data. (Vide Hassler's Logarithms, page 11, and Bowdwitch's Laplace, III. 108.)

Mass of the sun - - - - - -	329630.	$=s$.
Mass of the earth - - - - -	1.	$=e$.
Mass of the moon - - - - -	0.0146	$=m$.
Surface of the sun - - - - -	12486.	$=S$.
Surface of the earth - - - -	1.	$=E$.
Surface of the moon - - - -	0.0742	$=M$.

Ratio of the surface to the mass, in the sun $=\frac{s}{S}=26.4$; - (1)

In the earth $=\frac{e}{E}=1$; - - - - - - - - - - - - - (2)

In the moon $=\frac{m}{M}=0.196$ - - - - - - - - - - - - (3)

111. Our only measure of the mass of a heavenly body, is its efficiency in intercepting the ponderefacient impulses, as determined by its consequent power of gravitation. The amounts of ponderefacient momentum retained by the sun, earth, and moon, are respectively in the proportions s, e, m. If we assume a definite equal amount of surface, a, on each of these bodies, it is obvious that through this surface there would be transmitted equal amounts of ponderefacient momentum, much of which might be transmitted through the body without being transferred to its mass. But of the amount intercepted by the body, and received by its mass, there would enter through a, proportionately,

For the sun, (1), ... 26.4 ,
For the earth, (2), ... 1. ,
For the moon, (3), ... 0.196.

112. If the bodies were of equal density, and similar shape, these numbers would vary directly as the diameter. For if d, d', d''= their diameters, nd^2, nd'^2, nd''^2 will express their surfaces, and cd^3, cd'^3, cd''^3 their masses. The expressions (1), (2), (3), would then become $\frac{cd^3}{nd^2}=\frac{cd}{n}$, $\frac{cd'^3}{nd'^2}=\frac{cd'}{n}$, $\frac{cd''^3}{nd''^2}=\frac{cd''}{n}$; and their ratios d, d' and d''.

113. Since momentum is indestructible, it is clear, that to maintain equilibrium, there must emanate from the aforesaid bodies through the space a, on the surface of each, as much momentum in a given time as that received, as expressed in (1), (2), (3). And it is obvious from the induced gravitation and other phenomena, that the emanating momentum does not wholly attach itself to the ponderefacient medium in which it came; but finds exit in part in the luminiferous medium more gross, $\overline{M}\odot^2$; and doubtless partly in media more attenuated than the ponderefacient.

114. Thus it is apparent, that in equal time, through a given and equal amount of surface, there is sent forth into open space, near $26\frac{1}{2}$ times more momentum from the sun than from the earth.

115. We may now institute a collateral inquiry, as to the probable temperature of the sun's surface, as compared with that of the earth, which may for this purpose be assumed at say 60° Fahr. In prosecuting this enquiry, we must necessarily admit certain pure assumptions, since we have no certain knowledge of the nature of the molecular constitution of the sun, nor of the ratio of the momentum alluded to, possessed by the sun's molecular atoms. We will assume that the nucleus of the sun, is immediately invested by an aerial atmosphere as dense as ours at the level of the sea; and that the molecular atoms at the surface of the sun's nucleus, possess two-thirds as much as the molecular atoms at the earth's surface, of the whole ponderefacient momentum to be disposed of, respectively through equal surfaces on the sun and earth.

116. Let 1=the volume occupied by air near the level of the sea, at 60° Fahr.;

$r=\frac{1}{480}$=the increase of volume acquired by the same air when heated to 61° nearly;

x=amount of latent and free heat, possessed by said volume of air at 60°=the amount of molecular momentum, expressed in reference to 1° (or the momentum acquired in being heated from 60° to 61°) as unity.

Ratio of molecular distances=1, (at 60°) : $\overline{1+r}|^{\frac{1}{3}}$, (at 61°).

117. Now since the pressure of the atmosphere which antagonizes the force of repulsion, may be considered constant: we may derive an equation involving x, in the following manner:

$1 : x :: \frac{1}{1+r} : \frac{x}{1+r}$=the repulsive efficiency of x at 61° (¶ 68), and $\frac{x}{1+r}+1=x$.

Hence, by reducing the equation, $x=\frac{1+r}{r}=481$. Hence, air near the level of the sea, at 60°, entertains molecular momentum equivalent to 481° Fahr. of absolute heat. Hence the absolute zero of cold for such air is near —421° Fahr. Though I do not consider this result as rigidly true, yet it is certainly a sufficiently close approximation for our present purpose. It would consequently follow, from the preceding assumptions, and from (1) and (2), that $\frac{481° \times 26.4 \times 2}{3} = 8465°$ nearly=the molecular momentum of air at the surface of the sun, expressed in degrees Fahr., and its temperature would be near (8465°—421°=8044°) 8044° Fahr.; a far higher degree of heat than we have any instrumental means of measuring, and at which all known substances would doubtless exist in a condition of intense incandescence or luminosity.

118. The two assumptions admitted into these calculations may be varied widely within the bounds of probability, without essentially changing the result.

119. In like manner it might be rendered probable that the surface of the moon is intensely cold. For even admitting it has as dense an atmosphere as the earth, while in truth it is questionable whether it has any, it would follow, as before shown in reference to the sun, that its surface temperature, due to necessary inherent warmth alone, would be probably near (481°×0.196 (3)—421°=—327° nearly)—327° Fahr. It is presumable, however, that the influence of the sun maintains the moon's surface at a somewhat higher temperature than this.

120. In respect to the luminosity of the sun, having demonstrated that an exceedingly high temperature necessarily pertains to that body, as a consequence of its enormous mass, it only remains to inquire into the probable condition of the electric or luminiferous medium, $\overline{M}\circ^2$, pervading its structure, and immediately surrounding its surface. It is first proper to determine, whether that medium probably possesses a greater degree of density in and about the sun, than in and about the earth. That its atoms are more gross than those of the ponderefacient media, I have already rendered apparent. They must, therefore, tend to amass themselves around the heavenly bodies, in obedience to impulsive gravitation, and about the larger centres of gravitation this accumulation must be proportionately more considerable. Hence the sun would be imbued with, and surrounded by, a more dense medium of the luminiferous and electric matter than the earth. Still, from its probable constitution, $[\overline{M}\circ^2$, it would be less under the influence of gravity than molecular matter, such as our atmosphere $[\overline{M}\circ$, in the ratio of 1/[illegible] to 1; which taken in connection with the known velocity of its its impulses, 192.500 miles per second, enables us to comprehend how it can fill the immense measure of space occupied by the fixed stars.

121. Since, therefore, the luminiferous medium must be more dense at the surface of the sun, than at the surface of the earth; it would necessarily follow, that a proportionately greater amount of the momentum emanating from the sun, would travel off impulsively in that medium. This circumstance taken in connexion with the very elevated temperature that, I have shown, must pertain to the sun; or with the fact, that though an equal space on the surface, 26.4 times more momentum must in the same time escape from the sun than from the earth, clearly ena-

bles us to comprehend the probable causes of the sun's luminosity. When we behold the multitude of fixed stars, that send their impulses of light to us, from immeasurably distant points in space, we are by analogy impressed with, the conviction, that each is some enormous solar sphere, vieing with or perhaps exceeding in magnitude our sun. For if very much smaller, they could not be thus luminous.

122. Innumerable as may be the modifications of impulse in the $\overline{M}\supset^2$ medium, our sense of vision can take cognizance of only a limited range of these modifications. From all surfaces of all molecular and visible bodies, must at all times emanate impulses in the $\overline{M}\supset^2$ medium. Comparatively little of this momentum do we recognize as light; yet oftentimes we may perceive the effect of such invisible rays, in sundry electric, magnetic and chemical phenomena; and especially in the increase or diminution of momentum thereby induced in molecular bodies, as evinced by a change of temperature.

123. We will now inquire if the $\overline{M}\supset^2$ impulses, originate solely at the surface of the sun, or whether they are essentially derived directly from the sun's mass, being transmitted through the surface. And here without dwelling long, considering the density of the $\overline{M}\supset^2$ medium which must pervade the sun, it is highly probable that to the total amount of $\overline{M}\supset^2$ impulses emanating from the sun, every molecular particle in the sun's mass, directly contributes. Hence the radiation from the sun, must proceed, not merely from the surface, but also from the interior mass. Hence the intensity of the light and heat of the sun, as emanating from any particular part of the disc, might be expected to be greater or less, somewhat in proportion to the amount of the sun's mass lying in the special direction of the ray. Whereas if the solar radiation originated at the surface, the centre of the disc should be least, and the margin the most productive in light and heat.

124. Bouguer determined (Bowditch's Laplace, IV. 556) that the centre of the sun's disc is most, and the margin least luminous. The light contributed from a point, one-eighth the sun's diameter from the margin, bears in respect to intensity, to the light from the centre, the ratio of 35 to 48. Now if the line of vision be carried continuously through the sun's mass, in the direction of these two points, it will by calculation be seen, that the chord of an arc which the first will describe, will bear to the sun's diameter which the second will describe, the ratio nearly of 35 to 52, which is too close an approximation to the ratio of intensity before stated to be accidental.

CENTRAL HEAT OF THE EARTH.

125. For the present, I merely advert to the high temperature, probably existing in the interior of the earth, for the purpose of saying, that I think it can be shown to depend on causes now in operation. That a greater proportionate amount of molecular momentum should be possessed by the interior molecules of the earth, would seem fairly to result from the laws of dynamical equilibrium;—from the relation of such molecules to others more nearly superficial; to the $\overline{M}\supset^2$ medium investing the earth, and to the ponderefacient impulses.

POLARITY.

126. When we enquire into the habitudes of those enormous spheres revolving in space, properly denominated stellar atoms, the sun, planets,

&c., we find they rotate in a regular manner upon axes. So when we study the phenomena, presented by the integrant molecules of bodies within our reach, we find abundant evidence to warrant the belief, that they too, though incomprehensibly minute, are incessantly performing movements of rotation.

127. Thus in their rotary motion, the atoms of matter may possess untold and incalculable stores of momentum, respecting which we can become indirectly informed, by interpreting aright such natural phenomena as are more or less dependent thereon. The explanation of many important physical facts, is, in my opinion, to be found by studying the peculiar relation of forces due to molecular rotation or polarity.

128. I have lately made the following experiments:—Cause two spheres or circular discs to rotate in the air rapidly on their axes, leaving their centres of gravity free to move, and it may be seen that, mediately, though the passive agency of the air, each sphere or disc acquires a ring of repulsion and two poles of attraction; and the discs or spheres, according to the positions in which they are presented to each other, will either mutually attract or repel each other. Here induced currents of air tend to coalesce in obedience to pressure indirectly caused, and on the principle of least action. So, likewise, one of these whirling discs, brought near the other at rest, will induce rotation and consequent attraction or repulsion in the latter.

129. Although the medium air, passively and reciprocally producing the aforesaid attractions and repulsions, is incomparably more gross than the medium of light and electricity. yet in the latter, analogous facts are observable. Neighboring currents of electricity moving in the same direction, on conductors free to move laterally, will tend to coalesce into one, for they will then encounter less resistance or friction in their movement. If $d=$ the diameter, $pd=$ the periphery, and $ad^2=$ the sectional area of each separate current; then $2\,ad^2=$ the sectional area, and $\frac{p}{a}\times\overline{2ad^2}|^{\frac{1}{2}}=$the periphery of the combined current. Now, $2pd:\frac{p}{a}\times\overline{2ad^2}|^{\frac{1}{2}}::2:\overline{2}|^{\frac{1}{2}}$, so is thefriction offered the two currents, to the friction offered the combined current. That is, more than one fourth the friction is thus avoided.

130. In accordance with these views, electro-dynamics may be considered as a branch of physics, embracing phenomena not dependent upon the occult for explanation. And although much remains to be developed by future research, yet I conceive so much is already known that we may begin to entertain rational notions. Mr. Wheatstone found that the electric fluid travels along good conductors nearly 200,000 miles per second. Such a current being sent through a coiled conductor, repeatedly around a rod of iron, obviously induces the electric medium which pervades the iron to run in corresponding circular currents. This revolving influence is probably felt by the integrant molecules of the iron itself, and must be propagated by induction from particle to particle, to the extremity, of the iron rod. If another piece of iron be presented near said extremity a similar dynamical condition is induced therein in like manner. And as, by the experiments with the whirling discs, we know that bodies rotating on axes that nearly correspond in place and direction, mutually

attract by the agency of the intervening air; so here molecules by the like means may mutually attract, through the agency of the refined and attenuated electric fluid, which, like air, constitutes a material medium, though differing widely in tenuity.

CONDITIONS OF AGGREGATION ASSUMED BY PONDERABLE MATTER.

131. The *solid*, *liquid* and *gaseous* conditions are observed to pertain to ponderable matter. The forces concerned in producing and maintaining these three states of aggregation, are what I call molecular attraction and repulsion, and polaric attraction and repulsion.

SOLIDS.

132. In solids, all these forces are manifestly operative. *Crystalization* is essentially due to the polaric forces. Molecules operating on each other by poles of attraction and rings of repulsion, would necessarily arrange themselves, if free to move, in obedience to those forces, and hence would primarily tend to approximate and string themselves along in the direction of the poles; producing a crystalline fibre, and at the same time divesting themselves of that freedom to move from place to place among themselves, which constitutes fluidity.

133. A sphere may simply rotate on a fixed axis, when there would ensue, the sphere being enveloped in an appropriate medium, an equatorial ring of repulsion, and two poles of attraction. A sphere may rotate around a moveable axis, the axis itself rotating; and thus it is possible that an infinite variety of regular movements may be given to the axis. It is at least probable, that in the different chemical molecules, different movements of the axis of rotation obtain, thus conferring diverse qualities of attraction and repulsion, and giving origin to the numerous varieties of crystalline form.

134. Specific chemical qualities are probably thus mainly attributable to modifications of the polaric forces. In a compound molecule, the component atoms probably retain essentially their peculiar polaric forces, while as a result of their union, the compound molecule may possess a special polarity, though of a less energetic character.

LIQUIDS.

135. In liquids, like water, oil, etc., the molecules seem free to take any mutual position. If the polaric forces are in any degree operative, which is, perhaps, not impossible, they are evidently disguised by the prevailing forces of molecular attraction and repulsion.

136. To determine the amount of molecular attraction exercised transversely to a plane of given dimensions, in liquids,

Let $d =$ the distance between two adjacent molecules.

Between any two molecules the force of right lined impulsive attraction varies as $\frac{1}{d^2}$. (¶ 58.) The number of molecules presenting themselves to the plane must also vary as $\frac{1}{d^2}$. Hence the effective amount of attraction arising from these circumstances alone must be as $\frac{1}{d^2} \times \frac{1}{d^2} = \frac{1}{d^4}$. But as d varies, so the extent and amount of $\overline{M} \circ^2$ matter enveloping the molecules must vary (¶ 106) in some dependant ratio. Now it seems probable that molecular attraction may arise mainly from impulses in the [$\overline{M} \circ^2$ medium. Each molecule must hold around itself, by virtue of attraction induced by subordinate media, ($\overline{M}\circ^3$, $\overline{M}\circ^4$, &c.,) a special atmosphere of $\overline{M}\circ^2$ matter, which being affected by the $M\circ^2$ right lined impulses, would necessarily affect in like manner the $M\circ$ nucleus. The variation of the force of molecular attraction from this cause alone between two adjacent molecules, as a maximum, would be directly as d^2. And multiplying $\frac{1}{d^4}$ above, by d^2, the expression for the law of molecular attraction in liquids considered in reference to a constant plane, becomes $\frac{1}{d^4}\times d^2=\frac{1}{d^2}$.

In paragraphs 71. 74, 76 and 98, where I have determined certain maximum and minimum molecular values, I have therefore used the expression $\frac{1}{d^2}$ for the law of attraction. Yet it can be made apparent that where molecules recede very considerably, as in the conversion of liquids to gases, the amount of attraction through a given plane varies according to a constantly and regularly varying law. Commencing with a minimum variation of $\frac{1}{d^2}$, the law of variation must finally become $\frac{1}{d^4}$, as a maximum or extreme, as above set forth.

GASSES—AERIAL MEDIA.

137. Molecular repulsion, as before explained, (¶ 68), is generally regarded as the only force operative among the molecules of aerial fluids. It seems a very fair inference, however, that molecular attraction does act, though in a very feeble and inappreciable degree, which accords with the law of its variation, $\frac{1}{d^4}$ before expressed. (¶ 136). By calculation in accordance with this law, the force of molecular attraction, as exercised through a plane of given dimensions in water, is 11880 times greater than that exercised through a plane of equal dimensions in air. The force of repulsion in gases, therefore, appears to be equilibrated by external pressure, or by the weight of the atmosphere itself.

FLAME APPLIED TO AN EXPLOSIVE GASEOUS MIXTURE.

138. Why should the application of flame to an explosive gaseous mixture, determine sudden chemical union ; and why should this union be accompanied with a great and sudden increase of temperature or molecular momentum?

139. To offer rational answers, it will be requisite to suppose that each individual molecule of the gases concerned in the chemical union, consists of two or more smaller molecules, held near each other by polaric attraction. Let us suppose we have a mechanical mixture of two gases, capable of entering into chemical combination, say O and H; and further that each gaseous molecule of O and H consists of two equal atoms o, o' and h, h' bound together by a feeble polaric attraction. It must be admitted, that under equal circumstances the polaric attraction which an atom of O and H would exert on each other, is stronger than the polaric attraction subsisting between the two atoms of O, or the two atoms of H; the possibility of which can be shown. In their ordinary oscillations, the nuclei O and H do not approximate each other sufficiently close, for the efficient reciprocal exercise of polarity. But should any ex-

traordinary force be given to the mutual collision of O and H, in any part of the gaseous mixture, then o, and h, and o' and h' enter into molecular union. That is $o+o'$ and $h+h'$, becomes $o+h$ and $o'+h'$. It is easy to demonstrate how after this union, polaric repulsion might operate between the new molecules $o+h$ and $o'+h'$; and how consequently their velocities on receding would be greatly increased above the normal rate; whereby they could in turn impel other molecules of O and H into close collision, with like consequence of chemical union. Thus the action once begun, would propagate itself with more than the velocity of sound through the whole gaseous mixture. Here the increase of molecular momentum, developed during the act of union, must be derived from that incalculable store of momentum, possessed by the primary $\overline{M}\circ$ atoms in their movements of rotation. Now a tip of flame, or any analogous cause, serves merely to commence the process of union by giving to comparitively few of the molecules, the requisite velocity, much in the manner above set forth.

CATALYTIC ACTION.

140. By catalytic action, chemists generally mean to express the influence which a body sometimes exerts in facilitating or causing chemical action, without itself suffering any chemical change. Though this is apparently a very recondite subject, yet I am of opinion, that in every known case of catalysis, so called, a more or less satisfactory explanation may be given.

141. It is well known that the clean surface of platina, and other metals in a less degree, will determine the union of oxygen and hydrogen in a gaseous mixture. I have not space to enlarge upon these matters, and will therefore only remark that primarily the gases may be condensed upon the surface of the metal by the polaric attraction of the metallic molecules, and thus brought to combine. The polaric repulsion developed as before described (¶ 139.), as a consequence of the chemical union, would, as the process went on, tend to increase the molecular momentum thereabouts; and should this in the platinum, attain sufficient intensity; in other words, should the platinum became hot enough, then conjointly with the said polaric repulsion, it would impel some of the gaseous molecules with sufficient force against neighboring molecules, to induce the commencement of chemical union in the uncondensed gaseous mixture itself; and hence might ensue an explosion, as if primarily caused by flame.

CONCLUSION.

142. In concluding this memoir, I would observe that there are many topics, embracing much relevant matter, not herein even hinted at. I regret that a want of the requisite time and space should exclude them. Yet having drawn up this sketch in the midst of diverse and imperative duties, I fear that as it is, I shall hereafter find in it more than enough to modify and amend. I trust, however, I am wedded to no opinions which I will not promptly relinquish, when they be clearly shown to be untenable.

143. Deeply impressed with the conviction, that all the current physical phenomena of the universe are but rational and regular sequences from rational and regular causes, I have discarded the idea of occult and inherent qualities. Where the relation of things seem occult and incompre-

hensible, I have uniformly ventured to infer that they merely seem so, because the true and essential relations have not yet been developed. When we look abroad into the workings of nature, few, comparatively, are the physical objects and relations which we truly comprehend; and countless, those which to us are dim and distant forms; admonishing us of our weakness, and of our infinitely small importance in the grand system of the universe. But let us not despair. For since we see that neither matter nor motion admit of natural annihilation, but that both possess the passive attributes of endless perpetuity; let us exult in the firm conviction, which by parity of reason forces itself upon us, that the gift of the Divine Being also,—that more subtile portion of ourselves which thinks, remembers and reasons, is destined to immortality.

Part Second.

REVIEWS AND NOTICES OF NEW WORKS.

I.—*Animal Chemistry with reference to the Physiology and Pathology of Man.* By Dr. Franz Simon, Fellow of the Society for the Advancement of Physiological Chemistry at Berlin, &c., &c. Translated and edited by George E. Day, M. A., L. M., Cantab, Licentiate of the Royal College of Physicians. Philadelphia: Lea & Blanchard; 1845. pp. 292. Part First.

2. *Urinary Deposits, their Diagnosis, Pathology and Therapeutical Indications.* By Golding Bird, A. M., M. D., Physician to, and Lecturer on Materia Medica at Guy's Hospital; Licentiate of the Royal College, and member of numerous learned societies, &c. &c. Philadelphia; Lea & Blanchard. 1845. pp. 227.

The light which the science of organic chemistry has shed upon physiology and pathology, will establish a new epoch in the history of our science, hardly less brilliant than the discovery of the circulation of the blood itself. We are now enabled to demonstrate many of the morbid changes which the fluids of the body experience from deranged action; we can establish definitively the fact that the elements of the blood undergo an absolute or relative change; in either case constituting an abnormal state, and more or less subversive of healthful action; and we know that this fluid carries to all the tissues of the body the nutritive materials necessary to supply the wear and tear of human life. In this, the First Part of the "Chemistry of Man," various subjects are brought to view, some of which we shall enumerate in the language of the author.

"The first part comprises physiological and pathological chemistry. It treats of the physical and chemical relations of the fluid and solid portions of the human body in a state of health, and of the modifications they experience in different deseases. Moreover in every instance, the chemical examination of the fluids and solids of the lower animals is appended to each chapter."

Our author apologises for the order in which he discusses his subject, asking the reader to regard them as rather natural than physiological. After the introduction, comprehending over ninety pages of the work, written by Mr. Day, the editor and translator, Dr. Simon opens the subject, commencing with the circulating fluids ; the blood, lymph and, chyle. In his Second Part, which is not yet published in an English dress, Dr. Simon tells us he will examine the secretions and excretions of the chylopoietic viscera of the female breast, of the mucous membranes and skin of the kidneys; then the fæces, and matters vomited. He then studies the various tissues composing the animal body; as bone, muscle, skin and glands, concluding the whole with a description of the various solid and fluid morbid products, as calculi, tubercular matter, &c., &c.

The progress of organic chemistry has taught us that the blood is constantly undergoing a kind of metamorphosis, that it is subject every moment, to a series of changes, which is necessary to the complicated funtions of the organism ; that, through the intervention of the digestive organs, including those of assimilation and nutrition,, certain elements are poured into the torrent of the circulation, some of which are permanently incorporated with our tissues, others eliminated from the body, as *debris*, through some one or more of the various emunctories. By a species of elective affinity, as inexplicable as it is universal, each tissue appropriates to itself just those elements of the blood which are adapted to its nourishment and reparations, rejecting those which are not requisite or deleterious. A derangement or perversion of this *vital* affinity is another name for "lesion of nutrition." Thus the liquor sanguinis is destined to nourish the peripheral system, and affords nutriment to the cells and organs which are endowed with the faculty of selecting suitable material for the products of decomposition. Albumen, fibrin, and fatty matter constitute the nutritive elements in the liquor sanguinis. Extractive matters and lactic acid are the products of this metamorphosis.

"Urea, bilin, and carbonic acid are either not products of the metamorphosis of the blood during the act of nutrition in the peripheral system, or at most they are only in part formed by it."

Dr. Simon thinks animal heat due to chemical action, to the combination of oxygen with the carbon of the globulin ; the product of this chemical union are carbonic acid and urea or uric acid ; hence he regards the amount of urea excreted as the measure of the animal heat developed. In a state of health, the consumption and reproduction of blood-corpuscles go on *pari passu*—in other words, the two must be *in equilibrio*. Facts are cited by the writer to establish the above theory in relation to the production of animal heat. He has found that the amount of carbonic acid and of urea is lessened by a torpid, and augmented by an accelerated circulation ;—the multiplication of the corpuscles and the rapidity of the circulation invariably increasing the animal temperature. Hence the high temperature in birds, and the reverse in the amphibia. In the aged and the chlorotic, whose temperatures are reduced, we find much less urea ; whereas, in the phlegmasiae, with an excited state of the system and an accelerated pulse, the urea experiences either an absolute or a relative increase. In pursuing the "chemistry of man," an organised entity, in whom it may be said without a figure of speech, an atom lives and an atom dies, at every pulsation of the heart, we are embarrassed at every step of our progress, not only from the complex nature of the products submitted to analysis, but from the ever-changing, the ever-varying composition of the solids as well as fluids which constitute him a perfectly formed being. Not thus in the investigation of inorganic bodies; they remain without material change; their elements are fixed, and our analysis has but to establish their composition once, and the question, *quoad* their chemical constituents, is known for the future. What we call life is but the expression of a series of phenomena, some of which are physical, others physiological, others chemico-physiological, &c. But we do not mean to digress from the text before us; we shall now examine, after these few general observations, the "chemistry of man," beginning with the introduction by Mr. Day. He divides the proximate constituents of animal body into two great classes, the "*mineral* and the *organic*."

"The *mineral* constituents are again subdivided into those which are of service to the animal, because of their physical properties; secondly into those which effect important objects in the system by their chemical actions; and thirdly, into those which, being incidentally present, may be eliminated without much effect upon the organism."

Included in the first class, of those constituents useful by their physical properties, we have *water*, *phosphate of lime*, *carbonate of lime*, *phosphate of magnesia*, *ammoniaco-magnesian phosphate*, and the fluoride of calcium, which exists, however, in small quantities, in the animal organism. In the second class, or those *constituents useful by their chemical properties*, we have *hydrochloric* (chlorohydric) acid in the digestive fluid of man, of the mammalia and also in birds. Lehman detected it in morbid saliva; *hydrofluoric acid* has been found in the gastric fluid of birds; *chloride of sodium* exists in the blood, gastric juice, urine, bone, &c; *carbonate of soda* in many of the secretions of the graminivera, and in the ashes of animal substances; *phosphate of soda* abounds in the blood, lymph, chyle, bile, milk and urine; *chloride of calcium* has been detected in the saliva and gastric juice, as also the *chloride of iron*. In hæmatin, the chief coloring matter of the blood, in the lymph, chyle, hair, &c, *iron* is detected in considerable quantities. It has been found likewise in the various secretions but in small quantities. In the third class, or the "incidental constituent," is included the *chloride of potassium*, which abounds in nearly all the animal fluids. *Alkaline sulphates* have been detected in the blood, milk, urine and sweat; but Mitscherlich could not find any of the alkaline sulphates in the saliva, and Lehman positively denies that they exist in the bile. *Carbonate of magnesia*, it is well known, constitutes the principal part in the alvine excretions, urinary calculi, &c. *Manganese* has been detected in the hair, in gall stones in the form of protoxide, as also in the "urinary calculi of the graminivera."

"In the enamel of the teeth, in bone, urine, saliva and in almost every variety of calculi, *silica* has been found to exist. It also abounds in the excrements of the sheep in the proportion of 6 per cent."

Vauquelin detected traces of *albumina* in the human bones, in fossil teeth, &c. MM. Orfila, Devergie, and Lefortier regard *copper* as a normal constituent of "all the soft parts," and likewise of the blood. This metal is probably introduced into the system through the ingesta, as Cattanei was enabled to detect its presence in new-born infants. This supposition is strengthened by the researches of Rossignon, who detected copper in gelatin, chocolate, bread, coffee and sugar, doubtless derived from our "culinary apparatus," most of which contains a portion of that metal in a state of combination. The same chemists have found *lead* under precisely similar circumstances as copper. These facts should be borne in mind when we are called upon to investigate the subjects of poisons, in reference to medico-legal science. Having enumerated the mineral constituents of the animal body, and the particular fluids and solids in which they most abound, as stated above, we shall next in order proceed to treat of the second great class, or of the "*organic constituents.*"

He arranges the:

"*Organic constituents* into two groups, the one embracing the 'nitrogenous, the other the non-nitrogenous matters.' The nitrogenous group includes protein and its various modifications,—gelatin, bilin and the products of its metamorphosis—hæmatin, urea, uric acid, &c.; in the non-nitrogenous he places the animal sugars, fats, lactic and acetic acids &c. &c."

In the first class, or "*nitrogenous constituents*," Mr. Day places *protein*, which Mulder has discovered to be nothing more than the modification of albumen, fibrin and casein. Protein, or its modifications, albumen, fibrin, and casein, may be regarded as "the commencement and starting point of all the tissues." This singular substance, *protein*, in every respect "identical with that which forms the basis of the three aforesaid animal principles, may be obtained from similar elements in the vegetable kingdom.

Thus we find albumen, fibrin and casein in the vegetable as well as in the animal tissues; in each kingdom possessing similar habitudes, and in both instances but modifications of *protein*. Chemical analysis has revealed the very interesting fact that these "three substances contain the same organic elements, united in the same proportion by weight," and are identical in composition with the chief constituents of the blood, animal fibrin and albumen. Dissolved in muriatic acid, animal fibrin and albumen exhibit the same deep purple colour, and possess the same physical characters as vegetable albumen and fibrin. The following process is described for the extraction of protein:

"When animal or vegetable albumen, fibrin, or casein is used for this purpose, the substance chosen is to be washed with water, alcohol and ether, in order to remove the extractive matter, fats and soluble salts. It must then be treated with dilute chorohydric acid; then dissolved in a solution of caustic potash, maintaining the temperature for some time at 20°. The protein is then to be thrown down from the solution, after filtration, by acetic acid; collected on a filter, and washed till every trace of the acetate of potash disappears. Thus prepared, protein is in the form of grayish white gelatinous flocks, becoming, when dried, hard and yellow, and finally assumes the appearance of an amber-coloured powder."

The composition of an atom of protein may be stated in the following formulæ: C_{40}, H_{31}, N_5, O_{12}. Protein is precipitated from any acid solution, by ferro-cyanide and the ferridcyanide of potassium, by the metallic salts, by tannin, anhydrous alcohol, and the alkalies. In inflammatory-diseases, the oxides of proteine exist in a state of excess in the blood;

when the binoxide and tritoxide of protein accumulate in any organ of the body, we have inflammation of that organ, as usually indicated by the buffy coat, of the blood oxydized fibrin. The true indications, therefore in the treatment of the acute phlegmasia, is to *deoxydize* the protein, to diminish the amount of fibrin, which absorbs oxygen from the atmosphere and thus becomes converted into the binoxide and tritoxide of protein,

We hence find that the same element, oxygen, which is so indispensable to life, does, in certain conditions of the system, become the indirect agent of disease and death. Our space will not permit us to notice many of the organic constituents described; but we can refer the reader to the work itself, where he will find a condensed summary of all the important facts which the researches of chemists have made known touching the organic elements of the human body. So much for the Introduction.

To that portion of the work which bears more immediately upon the practical part of medicine, we now propose to direct the attention of the profession. We shall commence with the second chapter by Dr. Simon, which treats of that important fluid, the blood. In the study of this fluid he speaks first of the

"General physiological chemistry of the blood; the development of the blood-corpuscles; the phenomena of the circulation and respiration; the metamorphosis of the blood, and animal heat."

Under this section, "special chemistry of the blood," he explains the steps necessary in the analysis of the blood; first, of *healthy* blood, and second, of *diseased* blood.

With the general physical relations of this fluid the reader is already familiar; he knows that the blood in a fluid state consists chiefly of a nearly colorless fluid, in which the blood-corpuscles are suspended; he also knows that the red color of the blood-corpuscles is derived from the globulin and hæmatin with which the fluid contained in these corpuscles is impregnated; he is already familiar with the fact that the average specific gravity of this fluid in the mammalia, is set down at 1042, as established by Simon and Berzelius. There is some difference in the specific gravity of this fluid in the two sexes; in a state of health it reaches as high as 1053 in man, whereas in woman it frequently falls below 1050. The temperature of the blood is higher in birds than in the mammalia; that of the arterial, being in man, and some other animals, $1^\circ.8$ higher than that of venous blood. We shall not stop to dwell on the microscopic examination of the blood: nor give the admeasurement, in lines, of the size of the blood-corpuscles; suffice it to state that all this is executed in *decimals* with as much assurance as an architect would set down the dimensions of the "New Exchange."

"The blood freezes below 32°, and when thawed, the fibrin coagulates. The blood of animals in a state of hybernation, although the temperture be reduced to that of cold blooded animals, remains fluid; a fact which proves that its vitality is not destroyed"

It has been ascertained that the contact of the blood with certain membranes retards its coagulation; tie an artery, and the contained blood remains fluid for several hours. We know that blood when effused in certain parts of the body, as from the rupture of small vessels by a blow, remains fluid for days. If an electric current is made to traverse a mass of blood continuously, it will prevent coagulation; hence, in those killed by the electric fluid, the blood rarely coagulates. This fact was long ago noticed, and recent experiments have demonstrated its truth. The coagulation of the fibrin is retarded or entirely prevented by the sulphate of soda, nitrate of potash, chloride of sodium and potassium, the alkaline acetates, and their carbonates. Among the metallic salts which exhibit the same effect, may be enumerated the sulphates of copper and iron, chloride of this metal, ferrocyanide of potassium, sugar of lead and tartar emetic. The dilute mineral and vegetable acids, also retard the coagulation of the blood, converting it into an "oily or syrupy appearance." The coagulation of the blood may be accelerated by the presence of alcohol, æther, the decoctions of digitalis and tobacco, and some of the narcotics. In his chapter on the chemical physiology of the blood, Dr. Simon advances some speculations as to the formation of this compound fluid. It is maintained by some, that the spleen is the principal organ for the production of the blood-corpuscles, which are formed from the lymph-granules. This is the view advocated by Hewson. But it may be replied that the spleen has been extirpated and yet the blood-corpuscles continued to be reproduced, apparently as fast as they were required for the purposes of life. Schultz, on the contrary, maintains that the corpuscles are formed in the lymphatic glands, and reach, through the thoracic duct, the circulating mass.

This supposition derives some support from the fact that blood corpuscles and lymph-corpuscles have been detected in the thoracic duct of the rabbit and horse, although the former are much less perfect than when found in the blood proper. To convert the lymph-globules into blood-corpuscles, it is only requisite to surround the former with a coloured capsule. It has been recently maintained that this process is consummated in the lungs. Henle regards the lymphatic glands as the chief, though not the exclusive seat, of the formation of the blood-corpuscles. If we examine the lymph just on the point of traversing a lymphatic gland, as the mesenteric, and again soon after its elaboration through these organs, we shall find it has acquired a deeper colour, and approximates more nearly, both in physical appearance and chemical properties, the blood-proper. Hence, we have reason to suppose that the *genesis* of the blood-corpuscles, or of the coloured capsule, has its seat in the lymphatic glands. Every physician is conversant with the influence of the diseases of the lymphatic system upon the blood. When this system is deranged, nutrition is impaired; the subject becomes anæmic, because the lymph, no longer elaborated as in health, enters the venous system before it has undergone that degree of *sanguification* so requisite for the proper nutrition of the various tissues and organs of the body.

"Reichert believes the liver to be the 'blood-preparing organ in the adult, and the preparation of the blood as the principal function of that gland; the secretion of bile must then be regarded as a consequence of the metamorphosis that occurs during the above process."

Chemical physiology teaches that for the proper performance of the functions of organic life, the presence of blood is not only required, but this fluid must likewise experience a degree of metamorphosis as necessary to the vitality of the tissues as it is difficult of explanation.

As the different organs of the body are endowed, each with peculiar and *sui generis* powers, it is evident that they must separate from the blood, different principles and elements in order to preserve that harmony among the complicated acts of life which we designate health. Thus, the kidnies separate from the blood, *urea* ; the liver, bilin, &c. Should these organs, in consequence of disease, fail to elaborate these principles, they are retained in the blood and may be detected by the chemist. The investigations of Reichert and Schwan have shown that all the tissues of the body are made up of cells, and that the nutrition and growth of the various organs and tissues are carried on by the production of new cells, (or a multiplication of the old ones!) developing themselves from the blood, wherever they are required by the organism.

" That these cells, by their organic formations effect a change in the nutritious plasma, by appropriating from it matters homologous to themselves, and that the cells are finally consumed or dissolved, as is obvious from the general phenomena of the circulation. "

Dr. Simon regards this cellur action, which is demonstrated in the metamophosis of the animal organism, as purely chemical ; hence, he considers the heat thus engendered as a chemical process, and therefore those functions of the body which must be carried on for the preservation of life, contribute either directly or indirectly, to develope animal heat, which must necessarily take place wherever the metamorphosis occurs, and hence not only in the lungs, but likewise in the periphery of the body. Our author believes that animal heat is generated by the absorption of oxygen and its combination with the carbon of the system, not exclusively in the lungs but over the whole peripheral surface of the body. But does this chemical combination satisfactorily account for all the phenomena of animal heat, even admitting that it may take place on the surface of the body as well as in the lungs. By the periphery of the body, Dr. Simon doubtless means the capillary system of vessels, otherwise his theory would seem to be inadequate for the explanation of the phenomenon. Besides this union of oxygen with carbon for the formation of carbonic acid, he also believes that a certain amount of oxygen is absorbed, which probably combines with hydrogen, or with binary or ternary radicals of carbon and hydrogen, of carbon and nitrogen, or carbon, hydrogen and nitrogen. and thus doubtless contributes in some respects to the general production of heat. Simon believes that the blood corpuscles have some influence in the production of animal heat ; when their number is diminished, the necessity for the absorption of oxygen is likewise lessened, and vice versa ; all this is illustrated by reference to a table containing the number of blood-corpuscles, pr. cent. in different animals, with their temperature, pulse, and respiration.

" Another secondary source of animal heat, is the fact, developed by Poullet, "that all solid bodies, organic and inorganic, undergo an elevation of temperature when moistened with different fluids. In organic substances, it may amount to from 11° to 18°. *Since the act of metamorphosis is always effected through humid membranes, this source of heat must be regarded as of great importance, even if it be not actually identical with the catalytic metamorphosis of the cells themselves.*"

MM. Bebquerel and Breschet, as far back as 1835, demonstrated by means of a thermo-electric multiplier that each muscle, at the moment of its contraction, rises in temperature from one to two and a half degrees. It was recently advanced in some of the American Journals, that the development of animal heat was due (in part) to the alternate contraction and elongation of the arterial tubes, effected at each systole of the heart. Hence, it was alleged, in favor of such a supposition that violent muscular exercise, by increasing the action of the heart, and consequently, the contraction of the arterial system, augmented the temperature of the body. We shall not pursue this subject, but refer the reader to the work for a full development of M. Simon's views and those of some other distinguished chemico-physiologists.

To devote a few remarks to the "proximate constituents of the blood," will be our next effort.

It has been already observed that the blood is a complicated fluid; but chemical analysis has enabled us to detect the following as its constituents "in man and in certain mammalia:"

Among the *protein-compounds* we have water, fibrin, albumen and globulin; the *coloring matters* are composed of hæmatin and hæmaphin; in the *extractive matters* we find alcohol-extract, spirit-extract, and water-extract; the *fats* are constituted of cholesterin, serolin, red and white solid fats, containing phosphorus, margaric and oleic acids, peroxide of iron; of the *salts* albuminate of soda, phosphate of lime, magnesia and soda, sulphate of potash, carbonate of lime, magnesia and soda, chloride of sodium and potassium, lactate of soda, and the oleate and margarate of soda; and lastly the *gasses*, consisting of oxygen, nitrogen, and carbonic acid, sulphur and phosphorus, the latter particularly, entering into the composition of the cerebral structure.

The above constituents enter into the composition of the blood in a normal state, but they may be modified by disease. Traces of the following substances have been detected in the blood in certain pathological states of the system:

"Sugar, urea, bilin and its acids, biliphæin, gluten hæmacyanin, erythrogen, hydrochlorate of ammonia, acetate of soda, benzoate of soda, margarin, olein, copper, manganese, and silica.

All these substances, "the morbid products of the organism," must be derived, either directly or indirectly, from the blood; nor are they eliminated indifferently from any organ of the body; thus sugar and urea are separated by the kidnies; bilin and biliphaein, by the liver, and so on of the other secreting organs. Dr. Simon has likewise given us an analysis of the arterial and venous blood, and compared the results obtained; in the former he found more water and more globulin; in the latter, the solid residues, fibrin, fat, albumen, hæmatin and the extractive matters and salts preponderated.

In the next place M. Simon gives us a comparative statement of the properties of the blood of the hepatic vein and that of the vena portæ; of the blood of the renal veins and of that of the aorta ; he compares the venous blood with that of the capillaries ; he examines the influence of age, sex, temperament and constitution upon the constituents of the venous blood. From these investigations, it appears that the blood of lymphatic subjects is poorer in solid constituents, and in blood-corpuscles, than in those of a sanguineous temperament ; the quantity of albumen remaining about the same in both.

Andral and Gavarrett have arrived at the conclusion, in their late work on hæmatology, that the blood becomes impoverished and more watery in proportion to the number of venesections : this explains the fall in the density of the defibrinated blood. In consequence of the slight diminution of the albumen, the density of the serum is but little affected. On the contrary, the fibrin is uninfluenced by venesection ; it is, however, increased in certain inflammatory affections, most generally other things being equal, in the direct ratio to the violence and intensity of the phlogosis. The buffy coat which forms on the surface of the blood, abstracted from the system when a prey to severe inflammation, does not consist of "true fibrin, but of the binoxide and tritoxide of protein." It is now well known that the buffy coat may occur in other affections than those of an inflammatory character ; in chlorosis, for example, a disease in which the blood-corpuscles and the hæmato-globulin are deficient, we find the blood buffed ; in the horse, this phenomenon is said to be of constant occurrence, although that animal may be exempt from disease.

Venesection does not materially alter the amount of extractive matters and the various salts ; it, however, directly diminishes the blood-corpuscles—the true carries of oxygen, according to the theory of Leibig. In hyperinosis, the first form of diseased blood according to Simon, we have an excess of fibrin over that of the blood-corpuscles, the latter decreasing as the former augments.

"This form of diseased blood is characterized by a tardy coagulation ; the clot is quite firm and does not readily break up ; is invariably covered with a buffy coat, which is exceedingly tough, and cuped ; the serum is of a lemon-colour and not tinged red."

The blood is of a higher temperature than in the normal state, and always possess an alkaline reaction. The remarks of M. Simon upon the modifications which the blood undergoes in several inflammatory diseases, as *metrophlebitis puerperalis*, *carditis*, *bronchitis*, *pneumonitis*, *pleuritis*, *tonsilitis*, *hepatitis*, *peritonitis*, *nephritis*, and *acute rheumatism*, all go to establish the correctness of the views recently advanced by M. M. Andral, and Gavarett, in their work on the blood, from which our author draws largely when he enters the domain of pathological hæmatology. It is therefore deemed superfluous to follow up this subject in detail, as the profession must have become conversant with the beautiful work on the blood, written by the two gentlemen above named.

Hypinosis indicates a condition of the blood the reverse of hyperinosis : it is most generally witnessed in typhus, in some of the eruptive fevers complicated with adynamia ; and in most of the diseases attended with anæma or spanæmia. Simon, Schonlin, Chomel and Ancell. describe the blood in *yellow fever* as watery, very poor in fibrin, and in the latter stages assuming a dark colour. The clot, when formed, is diffluent and soft: the serum is of a deep yellow colour, partly, say they, from the colouring matter of the bile, and partly from dissolved hæmato-globulin. They describe it as possessing a peculiar smell, which is attributed to a volatile salt of ammonia. The latter writer, Ancell, who witnessed the yellow fever in the West Indies, and is therefore entitled to some respect on this account, describes the blood in the early stage of this disease as of a brighter red, containing more salts and hotter than in a normal state. At a more advanced stage of the disease, it loses its saline and animal principles, and becomes black and thin. It is in this state that the effusions of blood take place. In the onset of yellow fever, during the stage of full reaction, the blood when escaping from a vein, is so light and bears such a resemblance to arterial blood, that we have sometimes thought that an artery instead of a vein, had been punctured: this fact has been observed by others. It is not true, according to our observaion, that the blood does not clot so readily in this disease as the other pyrexiæ: we are not prepared to assert that the clot is as firm as in the class phlegmasiæ. Dr. Stevens, who speculated upon the changes effected in the blood by yellow fever, attributes the dark colour which it assumes in the latter stages of its course, to a loss of its saline principles; hence he recommends the free use of the *chloride of sodium*, the *chlorate of potassa*, and the like, with a view to supply to the blood the necessary soluble salts.

In some blood, contained in two separate vessels taken from the vena cava of a yellow fever subject, we poured a saturated solution of the chlorate of potass, and the chloride of sodium: and from a very dark venous hue, it gradually assumed a bright arterial aspect, which remained permanent for some time.

We now close this imperfect sketch of the "*First Part*" of M. Simon's "Chemistry of Man;" It evinces much labour, and a thorough knowledge of all that has been written and published on vital Chemistry. To those who are fond of the beauties and *mysteries* of organic chemistry, this work will prove highly acceptable; but for *us* it is too chemical; the *vis vitæ* plays a subordinate part in the production of animal heat, in the functions of nutrition, secretion, &c., according to the views here developed. We are ready to admit the great value of chemical researches in unfolding the phenomena of life; in explaining the proximate cause and nature of many diseases; in pointing out the changes effected in the blood and other circulating fluids by certain morbid affections; but we believe there is superadded to these chemical changes, a *dynamic force* —a *vital power* which presides over, and governs many of these chemical operations; this force, or influence resides in the *nervous system*. The practical application of many of the discoveries of organic chemistry is yet to be made; many of these lie as "*disjecta membra*," to be hereafter collected and made available in elucidating the diagnosis and treatment of disease.

The "Chemistry of man" has occupied so much of our attention, that we are forced to pass over the greater part of Dr. Golding Bird's valuable work on "Urinary Deposits," restricting our remarks to an analysis of his observations upon a certain principle found in the urine of pregnant women, called KIESTIN. This is the less to be regretted, since we published from this work, in a former number of this Journal, the mode of examining the urine, its morbid products, the re-agents necessary for their detection, and diagnosis.

As a diagnostic sign of pregnancy KIESTIN is regarded as more certain than any other taken separately. The urine, when received in a cylindrical vessel and allowed to stand for three or four days, is first covered with a cotton-like cloud, which afterwards "becomes resolved into a number of minute opaque bodies, that rise to the surface, forming a fat-like scum, remaining permanent for three or four days." After this, the urine assumes a turbid appearance, flocculi are detached from the scum and are precipitated to the bottom of the vessel. In this manner, the entire pellicle disappears. *Kiestein*, thus developed, is distinguished from similar pellicles which sometimes form in the urine, by its never becoming mouldy. Dr. Bird deemed this subject of sufficient importance to give it a more thorough examination; accordingly he tested the urine of about thirty pregnant women, from the third to the last month of utero-gestation, and in every instance, "copious fat-like pellicles were observed in the urine after two or three days' exposure."

Whilst collecting, says Dr. Bird, these specimens of the urine of pregnant women, I directed several young women, who presented themselves at the Finebury Dispensary to be treated for a amenorrhœa, to bring specimens of their urine, which were exposed simultaneously with those furnished by the pregnant women; and in two instances only, says Dr. B., was any evidence of the presence of the peculiar matter manifested.

In the two instances, forming the exceptions, the one, a girl aged 18, the other, 33 years, it was subsequently ascertained that pregnancy existed, although stoutly and obstinately denied by both parties.

In urine containing *kiestin*, the odor of putrescent cheese is not unfrequently witnessed. From some microscopic investigations made by Dr. Bird in the scum above described, it was evident that the greasy aspect of the pellicle of the so called *kiestein* arises not so much from the presence of fat, but from a number of the crystals of the triple phosphate, giving origin to this glistening appearance." The presence of *kiestein* in the urine of pregnant women may be attributed to the absorption of milk from the breast, and its introduction into the general circulation; from which it is eliminated by the kidnies, with the urine, in the form already described. Dr. Bird has given us indubitable evidence of this fact by two or three conclusive experiments which it is not necessary to reproduce.

In concluding this subject, he remarks:

As a test for the existence of pregnancy, the formation of the caseous pellice, especially if accompanied by a cheese-like odour, will be an extremely valuable *corroborative* indication, but it would be unsafe to found in it alone any positive opinion, because, as a sufficient number of observations have not yet been made on this subject, we have no right to assume, however probable it may be, that the caseous pellicle can appear *only* when pregnancy exists.

We shall now conclude with a few "*remarks on the therapeutical employment of remedies influencing the functions of the kidnies.*" After alluding to the proverbial uncertainty in the action of diuretics upon the kidnies, Dr. Bird seems to admit that, aside from absorption by the lymphatics, fluids may enter the capillary system of vessels by direct imbibition; and that as to imbibition and exudation, *id est*, *endosmose* and *exosmose*, the membranes, whether living or dead, are obedient to the same physical laws. From the ascertained experience of the profession in relation to the mode of action of the class of medicines called diuretics, Dr. Bird has deduced the following laws: *Law* 1:

All therapeutical agents intended to reach the kidneys must either be in solution when administered, or capable of being dissolved in the fluids contained in the stomach or small intestines after being swallowed.

This law is too obvious to claim any explanation; since no substance, except in a state of extremely minute division, can enter the circulation through the absorbents, without a positive lesion of the capillary system, or a direct solution of continuity. *Law* 2:

Bodies intended to reach the kidneys must, to ensure their absorption, have their solutions so diluted as to be of considerably lower density than the liquor sanguinis or serum of the blood (i. e. below 1.028.)

This law, whilst it points out the necessity of a free use of diluents in order to determine copious diuresis, is likewise founded upon the beautiful phenomena of exosmosis and endosmosis long since discovered by Dutrochet, and subsequently verified by the experiments of Dr. J. K. Mitchell of Philadelphia.

If, therefore, the density of a solution intended to act on the kidnies be higher than that contained in the capillaries, no diuresis will take place; but, on the contrary, we shall have a purgative effect—watery alvine evacuations—in obedience to the laws of exosmosis; that is, water will leak out of the capillaries of the gastro-enteric surfaces, by reason of the greater density of the diuretic solution passing over the primæ viæ.

These facts are highly practical, and should on that account, be borne in mind by those who desire to cure their patients.

As diuresis and diaphoresis are antagonistical to each other, the careful practitioner will avoid every means known to determine the latter, when he is desirous to establish the former, action in the system. He will not forget to keep the surface of the body cool; as otherwise he is sure to be disappointed with the reputed diuretics. Dr. Barlow has recently announced the very interesting fact, that

"Whenever a stricture or other obstruction exists in the course of the small intestines, sufficient to prevent fluids readily passing along them, the urine will be diminished in bulk in the direct ratio of the proximity of the obstruction to the pylorus; nearly absolute suppression of urine occurring when the stricture is so high up as to allow but a small quantity of the fluid contents of the intestines to be exposed to the absorbing influence of the portal capillaries."

We cannot say whether this fact has received confirmation by the profession; we have never observed any thing of the kind in such cases. Certainly, in a disease of such serious import we find the kidneys, in common with the other excreting organs of the body, materially deranged. In the latter stages of yellow fever the suppression of urine is of frequent occurrence; and yet we have no stricture or other obstruction of the bowels. To those of the profession who are ambitious to keep pace with the progress of our science we commend the above works.

A. H.

II.—*Manual of the Diseases of the Skin;* from the French of MM. Cazenave and Schedel, with notes and additions. By Thomas H. Burgess, M. D., Surgeon to the Blenheim Street Infirmary, for Diseases of the Skin, &c. Revised and corrected with additional notes, by H. D. Bulkly, M. D., Lecturer on Diseases of the Skin, &c., &c. New York: J. & H. G. Langley, 1846, 12mo., pp. 338.

The high reputation of Cazenave and Schedel, and the known popularity of their works and practice in Europe, will lead medical men in this country to regard the appearance of this edition as a most desirable event. Our medical literature is by no means rich in works on diseases of the skin, and in fact even if it were so, we would look for the appearance of anything from such men with interest, as conveying the results of their more mature and recent experience. This work, under the modest title of "Manual of Diseases of the Skin," is really one of the most interesting and even complete that has appeared; and may be regarded either as a complete and distinct work, or as a supplement to Cazenave's former work. That part of this volume relating to syphilitic eruptions will be interesting to American readers, from the fact that it contains a compendious form, the facts and principles advanced in Cazenave's valuable "Traite des Syphilides," a work which is but little known in this country, except by reputation.

We recommend this work to the profession, with full confidence, as one of the best practical treatises on the subject; and of great value to the student. In the medical schools of the United States, cutaneous diseases are too much neglected, and the young physician goes out totally ignorant of the means of diagnosis, prognosis, treatment, and in fact of every thing relating to this important branch of the profession. Our text books, too, say but little upon the subject, as their authors incline to view this branch as one requiring special consideration in separate works. The consequence is, that most young physicians commence their career ignorant of the principles which should guide them in the treatment of these affections; and having generally no work in their library, on the subject, to which they can refer for information, are driven to a system of empiricism which is totally inexcusable, and which may destroy the reputation of an otherwise promising young practitioner. Such a work as the present will enable them to obtain, in a comparatively few days, an amount of accurate knowledge that a long life of active practice would not furnish; enabling the physician to proceed with vigour, confidence, and satisfaction in the discharge of his duties; and a correct knowledge in these matters will often establish a most enviable reputation for a young practitioner.

We are much obliged to the publishers for a copy of this excellent little work; and it affords us much pleasure to state, that it is one of the neatest little volumes we have seen; and what is somewhat unusual with American republications, the typography of the work is excellent.

W. M. C.

III.—*A Practical Treatise on Medical Inhalation. With numerous cases demonstrating the curative powers of the local application of various remedies in Bronchitis, Consumption, and other diseases of the Respiratory Organs, embracing the opinions and experience of Rush, Sir Charles Scudamore, Eberle, Mudge, Crichton, Thomas, Corrigan, Ramadge, and others.* By EDWARD JENNER COXE, M. D. Second Edition, enlarged and improved. Philadelphia: John Pennington, 1845: pp. 117.

The little volume that bears the above ponderous and comprehensive title, is from the pen of our worthy fellow citizen Dr. Coxe, of the firm of Coxe & Macpherson, Druggists, Camp Street. Dr. C. was regularly educated for the Medical Profession, and, we believe, pursued it for some years, but on account of ill health was compelled to abandon it in a great degree, for the doubtless more lucrative business of selling drugs. It would seem that he was led to investigate the remedial powers of medical inhalation from having been himself subject to breast-complaint, and having derived great benefit from this method of treatment. Dr. C. gives a brief and concise account of the progress of medical inhalation from the time of Rush down to the present day. The opinions of the authors enumerated in the title page, with copious extracts from their writings, compose the bulk of this work. These are neatly compiled, and are well calculated to make a favourable impression in behalf of the virtues of medical inhalation. Quite a number of powerful medicines may be advantageously used by inhalation, to suit different cases according as the chief irritation may be seated in the larynx, trachæa, bronchia, &c., amongst which may be mentioned the following: viz, iodine and the iodides, the balsams, creosote, opium, hyosciamus, conium, turpentine, sulphuric æther, &c., &c. Dr. C. gives minute instructions for the administration of these medicines by means of an improved inhaler which he has for sale. We have examined this neat and convenient instrument, and think it admirably adapted to the purpose. Although we cannot speak from an extended experience on the subject, we have no doubt that medical inhalation is a valuable curative means, and one that it is too much neglected by practitioners. Although this little work is dedicated "to the members of the medical Profession," it is evidently intended as much for the general reader; and the only thing to be apprehended, is that invalids may incautiously venture to use, in this way, powerful drugs without a knowledge of the attendant danger, or a proper discrimination as to the nature of the case, or the fitness of the remedy.

E. D. F.

IV.—*Commentaries on the History and Cure of Diseases.* By William Heberden, M. D. From the last London Edition (In Bell's Medical Library.) Philadelphia: 1845. pp. 214.

It is superfluous to analyse a work which has been consecrated by the lapse of fifty years, and is now regarded as one of the classic productions of the profession. The author of the Commentaries spent the early part of his life in learning, the middle in practising, and the latter in teaching his profession to others. His was a life honorable to himself, and beneficial to mankind. He commenced the study of disease, and noted down the symptoms and the effects of the remedies employed at the time, as if no other record had been kept, as if no one had written a line upon the same subject; not that he had not read and studied the best masters, both ancient and modern—not that he despised learning and repudiated the truths which others had discovered; but to learn to rely upon his own powers of observation, to leave his mind free to receive the truth—to study the process which nature adopts in her efforts to cure disease; such is the correct method when we attempt to advance a science founded upon experience and observation. Mere reasoning alone will never purify medicine of its present incongruities and absurd speculations; this Liebig attempted, and failed; Broussais after travelling at one while along the confines of truth, at another neck and heels in the dark, suddenly stumbled upon a *gastrite* and exclaimed ευρηκα "I have discovered the *fons et origo* of all the ills of life." Flushed with his supposed success, and anxious to convert the world to his new doctrines, he assailed all the facts and observations that conflicted with his new views in a series of publications which were characterized by powers of reasoning rarely equalled, and a quickness of perception but little inferior to inspiration. What was the consequence? time overthrew the illusions of medical philosophers, and the same diseases ravaged the earth as before! Alas! the vanity and delusion of the human intellect! We can only reason from the known to the unknown; isolated facts may serve as the foundation—as the nuclei around which analogous facts may be clustered; finally, if these can be made to harmonize—if they bear a philosophic relation towards each other—we may then venture to erect them into a *system*. To understand the true nature of disease, we must first learn the habitude of our organs—view each as a sort of independent being, yet sympathising, both in health and disease, with each other. Starting at this point, we shall soon perceive, that as each organ, or tissue, is endowed with special functions,—special sensation, in a word, a sort of individuality, all however, amenable to certain general laws, both in a normal and abnormal state, we shall then be better prepared to interpret the symptoms of disease—to trace each morbid phenomenon to its proper source—in fine, to discover the actual seat of the affliction. This is the diagnosis. In all this we must avoid confounding symptoms, The pain from the passage of a renal calculus along the ureters has been mistaken for the throes of parturition; rheumatism of the *recti* muscles of the abdomen, for a peritonitis, and so on. To avoid similar blunders, care should be taken to collect all the evidence to be obtained from an intimate knowledge of the special habitudes of the organ or tissue supposed to be affected, bearing in mind at the same time, that by means of nervous filaments, every organ is dynamically speaking, in juxtaposition to each other, and must therefore, when deranged, affect the entire system. The character of the pain frequently directs us to the seat of the disease ; if a *serous* tissue be inflamed, the pain is acute and lancinating; if a *mucous* structure, it is burning, gnawing, and attended with a sense of heat in the part ;—if a parenchymatous structure—dull, dragging, and frequently gravative, &c. Thus pain, although regarded in a popular sense, as a physical evil, still the man of art must view it differently ;—as announcing the invasion of disease, and this is the language of the organism to warn us of the fact.

But we cannot dilate on this subject; both time and space forbid it. Heberden was one of Nature's great physicians ; he did not pretend to see more than the case presented; he barely noted the facts and finished with comments explanatory of the morbid phenomena. The hippocratic aphorism ;—*Quo natura vergit, eo ducendum,* guided his practice,—he was ardent in the pursuit of truth, and bold to anounce it to the world ; he was awed by no authority, nor intimidated by the weight of great names. To show the nature of his views, and the manly independence with which he expressed them, we call attention to his remarks upon" *opening a vein in hæmorrhages*—read before the College of Physicians, Dec. 11, 1771. "It has been the practice," says Heberden, "of physicians to take away blood from the arm or foot,in order to stop violent hamorrhages from some other parts, which do not admit of a topical application.

"If it be intended by this practice to weaken the power of the heat, and to give the lips or the ends of the broken blood-vessel a chance of collapsing, or of being plugged up by means of a more languid circulation, would not all this be as likely to happen after the patient had been weakened by loosing the same quantity of blood from the original rupture.

"It seems probable, from all the experience which I have had of such cases, that when the hæmorrhages proceeds from the breach of some very large vein or artery, then the opening of a vein will not stop the efflux of blood ; and it will stop without the help of the lancet, when it proceeds from a small one ; in the former case, bleeding does no good ; and in the latter by an unnecessary waste of the patient's strength, it will do harm." We might be less learned, it is true, but better physicians, if we studied more carefully the writings of our predecessors, although to the neglect of the productions of many of our contemporaries. The sort of bibliomania with which half the profession, particularly in some of our sister-cities, is infected, is as great a misfortune to the medical public, as it is fatal to the reputation of its unhappy victims.

Is there no remedy for this disease ? no Hellebore to purge the "*guts of these brains*" of such "*perilous stuff!*" A. H.

V.—*An Elementary Treatise on Midwifery, of the principles of Tokology and Embryology.* By ALFR. A. L. M. VELPEAU, M. D., &c. &c. Translated from the French by CHAS. D. MEIGS, M. D., 3d Am. Ed. with notes and additions, by WM. HARRIS, M. D. &c. &c.—Philadelphia, Lindlay & Blakiston, 1845. 8vo. p. 800.

We welcome with pleasure this new and improved addition of Meigs & Velpeau's Midwifery, presented to the medical public by Dr. Wm. Harris, Philad., with notes, and appendix on puerperal fever that are not only highly interesting, but of no inconsiderable practical value.

It would be a work of supererogation to analyze its merits, for they have been already long and extensively appreciated by the profession. Suffice it to say that we consider it one of the most valuable systems that has as yet appeared upon the subject; and as such strongly recommend it not only to practitioners, but also to those medical students who may not as yet have provided themselves with a work upon this important branch of medicine. A. H. C.

VI.—*Outlines of the Arteries; with short descriptions, designed for the use of medical students.* By JOHN NEILL, A. M., M. D., Prosector in the University of Pennsylvania, Physician to Wills' Hospital, Lecturer on Anatomy, &c. &c. &c. — Philad., Ed. Barrington & Geo. D. Haswell, 1845.

If this little work should fall into the hands of a medical student before being acquainted with the importance and extent of anatomical science, he would most certainly flatter himself, that if there was no "royal," he had at last stumbled upon a "short road to the mysteries of anatomy." These "outlines" of the arteries, Dr. Neill tells us, were prepared at the request of, and for the benefit of medical students. They pretend to nothing more than "*outlines*," yet they are not mere diagrams or fancy sketches, since many of them were drawn from preparations."

Dr. Neill's work reminds us much of a similar production by Paul B. Goddard, M. D., once Prosector to the University. Why, therefore, a new edition of the same work, under the auspices of a new name. Perhaps Dr. Neill was ambitious, and P. B. Goddard complaisant.

To speak truth, we do not admire such a work; students are too apt to prefer fumbling and thumbing a neatly bound volume, to the use of the scalpel in the dissecting room. A careful examination of a single important arterial branch in the subject, is of far more value to the student, than the most finished "outlines," or best executed diagrams.

The medical student should never be encouraged to study "plates and outlines" to the neglect of actual dissections; they can never make him a good anatomist; although he may master the names of the principal arterial trunks, and describe, to some extent, their relative course; yet when he comes to practice surgery, or indeed any branch of medicine, demanding a knowledge of practical anatomy, his arm is feeble, because he feels a want of that acquaintance with the parts which nothing less than repeated dissections can confer.

To conclude: this work may be a suitable companion for a student, on his way from the dissecting room to the dinner table; but if a candidate comes forward for his degree, and it be ascertained that he has relied upon these "outlines" for his knowledge of the arteries, he should be consigned over to the mercies of the "*Prosector*" for at least six months longer. A. H.

VII.—*Lectures on Puerperal Fevers.* By William Harris, M. D. Philadelphia. Septemher, 1845.

This is a pamphlet of fifty pages, containing three Lectures delivered to a Medical Class, and published at their request. The Lectures are alike creditable to the author, and to the class that had the sagacity to appreciate their merit. We have read them with peculiar pleasure, and do not know where we could readily refer to so much valuable information on these important fevers. Dr. Harris delineates all the various forms of puerperal fever in a masterly manner; such as could only be attained by long experience, and a familiarity with all that has been written on the subject. The matter of these Lectures is altogether too valuable to be entrusted to the pages of an ephemeral pamphlet; we do hope they will be given to the Profession in some more durable form. F.

VIII—. *A Dictionary of Terms used in Medicine and the Collateral Scienes.* By Rich. D. Hoblyn, A. M. Oxon. First American, from the second London Edition. Revised, with numerous Additions by Isaac Hays, M. D. Philadelphia: Lea & Blanchard. 1845. p. p. 402.

Here is a cheap little volume that should be in the hand of every medical student, and will be found exceedingly convenient and useful to the reading physician. It presents a complete glossary of medical nomenclature, with plain definitions and derivations. The work has received the most flattering notice of the British medical press, and Dr. Hays deserves the thanks of the Profession for introducing it to the American reader. F.

IX.—*The Domestic Management of the Sick Room.* By Anthony Todd Thompson, M. D., F. L. S., &c. Revised, with additions, By R. E. Griffith, M. D. Philadelphia: Lea and Blanchard, 1845.

We can do little more than announce the reception of the above work on "Popular Medicine," and specify the subjects brought to view. In the "Introduction," the author treats of health and discase; causes of the latter in reference to *air*, food, exercise, clothing, mental affections, and sleep; recognition and prevention of disease. Chapter I. enters largely into the following, viz: choice and furnishing of the sick-room when the disease is likely to be protracted, or is of a peculiar description; ventilation, temperature, cleanliness, and darkening of the apartments, qualifications to be looked for in the choice of a nurse. Chapter II. is devoted to the necessity of intelligence, self control, and judgment in the unhired attendants of the sick room; the management of the invalid illustrated in different diseases. Chapter III. alludes to the domestic treatment of certain nervous affections. Chapter IV. enters into medicine proper, the administration of medicines, blood letting, application of leeches, cupping, &c. In Chapter V. he describes the application of lotions, sponging, cold affusion, shower baths, partial baths, douching, general baths, fomentations, steeping, poulticing, frictions, &c. In Chapter VI. Dr. Thompson dwells on the management of counter-irritants, rubefacients, sinapisms, suppurative ointments, vesicating oils, blisters, issues, setons; management of dry heat, by stomach-plates, salt-bags, hot bricks, moxas, cauterants; domestic management of accidents; bandaging. Chapters VII, VIII, and IX direct the management of the convalescent; including food and clothing, cookery for the sick; useful forms of domestic medicine; mental influences upon the body in disease and in convalescence; religious consolation in disease; the necessity of discretion and judgment in its application.

This work, though unpretending in its title, contains a large amount of valuable intelligence. It may do good; but we question very much, the necessity, not to say propriety, of any attempt to instruct the *public* in any branch of the healing-art.—Mankind much rather retain and adhere to old prejudices and popular errors than to learn new truths, more especially when they relate to medical matters. If Dr. Thompson had omitted his domestic formulæ, the book would have been more popular with the profession, and equally useful to the public. It is in matters of this sort that a little learning becomes a dangerous thing! However, Dr. Thompson's book will be likely to disabuse the public mind of many of its present errors in reference to the management of the sick. The table of contents, as above set forth, will give the reader some idea of the valuable matter which the volume contains.—It may be had of Mr. Woodall.
A. H.

X.—*The Southern Journal of Medicine and Pharmacy.* Edited by J. Lawrence Smith, M. D., and S. D. Sinkler, M. D. Vol. I, Jan. 1846, No. 1. Bi-monthly, Charleston, S. C. Subscription four dollars per annum, in advance.

A period has arrived when southern physicians can no longer be justly reproached with supineness and inactivity in the cultivation of medical science. Talent is beginning to show her strength, and enterprize to put forth her arm in the cause of science and education. Now that we have all the comforts and many of the luxuries of life spread around us, we begin to look forward to better things for the profession, and a brighter epoch for Southern literature. But a short time since we were called upon to announce the appearance of the *Southern Medical and Surgical Journal*, so creditable to the talents that conduct it, and now we find upon our table another champion, the *Southern Journal of Medicine and Pharmacy*, in the good cause, which we receive with more pride than envy, and cheerfully extend to the editors, Drs. Smith and Sinkler, the right hand of good fellowship. The first number contains, besides a neatly written "introduction," six original articles, two of which deserve particular mention; one on the "*Use of the Sulph. Quinine*" by Professor Dickson, the other on "*Rheumatic Inflammation of the Dura Mater*" by Dr. Cain.

Besides, the original papers, the journal contains some well written reviews, various extracts, and translations from foreign journals, all of which evince much industry and discrimination. Each number of the journal contains 120 pages; the type is clear, the paper excellent; in a word, every thing connected with the work bespeaks liberal and enlightened views,

A. H.

INTRODUCTORY LECTURES.

The following introductory lectures, published by the respective medical classes before whom they were delivered, have been duly received, and we beg the authors to accept our thanks for their kind attention.

1.—By Prof. John P. Harrison, of the Medical College of Ohio, on the *Sources, Evils, and Corrections of Professional Discontent.*

Delivered on the 4th November, 1845. An interesting lecture on a very appropriate theme. Nothing is more obvious, or more to be lamented than the general prevalence of discontent among the members of our profession; especially the young, or rather those who having obtained diplomas and ventured forth upon the theatre of life, are still in a state of probation, and have not yet succeeded in establishing themselves in business: they have not yet realised the flattering promises of youthful hope, and many are lingering on the brink of despondency. The tardy and precarious progress of medical reputation is but illy adapted to the impetuous and aspiring ambition of young men. The young physician must patiently abide the results of "*time and chance*," which Soloman says "*happen to all men.*" The young lawyer or divine may figure in youth, and shine before the gaze of the world, but the physician seldom attains substantial success or enviable renown before the meridian, and often not until the evening of life. Yet having "put his hand to the plough," it will never do to "look back."—he must move onward firmly and steadily, combat all difficulties, avail himself of all advantages, and never cease his exertions until success crowns his efforts, or he perishes in the struggle. Let all who are afflicted with *professional discontent* read the lecture of Professor Harrison on its *Sources, Evils and Correctives*, and we venture to assert they will rise from its perusal exhilarated and encouraged.

2. *An Introductory Lecture delivered by* Gunning S. Bedford, *A. M. M. D. Prof. of Midwifery &c. in the New York University. Nov.* 1845.

This is a very able Lecture, in which the author gives a graphic sketch obstetrical science and illustrates forcibly some of the most important practical points. We should judge from Dr. Bedford's style that he must be an eloquent Leturer—just the man for a professional chair. It is not enough that a teacher of medicine be capable and profound—he should possess the power of communicating what he knows, clearly, forcibly and pleasingly—in short he should be able to throw a *charm* around the branch he professes to teach. He should woo and win the attention of his pupils. We extract the following remarks on the *Practice of Midwifery by old women*, (too much the custom in this region,) which we commend to the especial attention of our readers.

Many whom I am now addressing are aware that prejudice, and a mistaken feeling of delicacy have, in certain sections of the country, induced intelligent women to commit themselves, during the perils of child-birth, to the entire government of ignorant *midwives.* In the South, more particularly, this custom has prevailed to an alarming extent. And, gentlemen, I now speak as a Southern man—as one who was born and educated under her genial sun, and whose love for her soil and her institutions has in no respect abated. I, therefore, feel that I have a right to speak on this subject, and would that my admonitions might tend to do away with a custom, which is not only full of peril, but which daily leads to the most distressing results. What are the claims of an ignorant *midwife*, who cannot distinguish the *os sacrum* from the jaw bone of an ass—who cannot tell you whether the womb is in the stomach or in the pelvis—who is so ignorant of the very first principles of the science, that she does not know whether she is pulling on the after-birth or a prolapsed uterus—what, I ask you, are the claims of such a creature, that the fond and devoted object of your heart should be committed to her charge dnring the most anxious and trying moment of her existence? After you shall have completed your medical studies, and received the diploma of this University, and when yon return to your homes to enter upon the details of practice, let me intreat you to exert your influence in your respective spheres to point out the dangers of the practice to which I have alluded, and protect from harm those who not only have a natural claim on our sympathies, but to serve whom is always a pleasure to every right-thinking man.

We cannot too highly command Prof. Bedford's caution against the improper use of *instruments* in delivery. In short the lecture abounds in useful and instruction hints, and does the auther much credit.

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3. *The Reciprocal obligation of Professors and Pupils* : *An Introductory Lecture delivered by* Thomas D. Mitchell, *M. D. Prof. of Materia Medica &c. Transylvania University. November* 3*d*, 1845.

Prof. Mitchell has here pourtrayed with generous feeling and glowing language *the reciprocal obligations of Professors and Pupils*, and but for a *severe*, though perhaps not unmerited fiing at a rival Institution, it would be considered one of the happiest Introductories of the season. Nothing can be more unfortunate and deplorable than *strife* among kindred institutions, or members of the same profession. Yet it seems almost impossible to prevent it. If the high-toned principles of honor and etiquette here so eloquently laid down by Prof. Mitchell were universally adopted, we might hope to see an end of the evil. Professors as well as pupils might read this lecture with profit, and be faithfully and feelingly reminded of the weighty responsibility that rests upon them. As the space allotted us for comment is exhausted, we must here close our remarks, but we cannot do so without reiterating our admiration of the moral and religious sentiments proclaimed by Prof. Mitchell. F.

Part Third.

EXCERPTA.

1.—*Report on the Progress of Human Anatomy and Physiology in the years* 1843-4. By James Paget, Lecturer on General and Morbid Anatomy and Physiology, and Warden of the Collegiate Establishment, at St. Bartholomew's Hospital. Concluded from our last number.

NUTRITION.

In its chemical relations. Use of gelatine in food. The Amsterdam commission* for determining the nutritive properties of gelatine, as obtained from bones by steam, and used in large quantities in "economic soup" in Dutch public institutions, have confirmed the conclusion of the commission of the Paris Institute, that it has had hardly any nutritive properties when taken alone; and, in regard to the important point left unsettled by the French commission, namely, whether, when added to other kinds of food, gelatine contributes to the total amount of nutriment, they have also come to a negative conclusion. None of the three dogs to whom considerable quantities of it were, for several long periods, given, both alone and in connexion with bread and potato-parings, was found to have derived the least benefit, or to have gained any increase of weight from it.

[The experiments were accurately enough made, and warrant the conclusions, as far as "economic soup" and dogs are concerned. But they are so opposed to the results of common observation of the nutritive value of jellies and other like gelatinous food, that some fallacy must be suspected in both these and those of the French commission. Either dogs are improper subjects for such experiments, or, more probably, the mode of preparation decomposes the gelatine. The action of the hot steam to which the bones, already boiled, are subjected for about seven hours in each of several days, may effect a part of the change by which, when it is complete, gelatine loses its power of *setting*, and in which the arrangement of its elements may be so altered, that they cannot be reconstructed in a nutritive form.]

Transformation of the sugar in food. M. Chossat† found that of many birds fed on sugar alone, none lived more than sixteen days; and he thought he observed that in those which had copious bilious evacuations, no unusual quantity of fat was accumulated in the body; but in those in which these discharges did not occur, fat was abundantly formed. He assumed, therefore, that the sugar is, under varying circumstances, sometimes converted into the constituents of

* Het Instituut, No. 2. 1843, pp. 97-114; imperfectly reported in the account of the sitting of the Academie des Sciences, 11 Mars; in the Gazette Medicale, 16 Mars 1844.
† Gazette Medicale, 21 Oct. 1843, from the Acad. des Sciences, Seance du 16 Oct.

fat, and sometimes into those of bile. But the experiments of M. Letellier.* which were more carefully made, contradict these. Their results were, that among seven turtle-doves fed on cane-sugar and bread with water, (coagulated albumen having been added, in two cases, after the sixth day,) not one possessed, at the time of death, the average quantity of fat; their general average of fat was nearly 60 per cent. less than that found in healthy individuals, i. e. the average in health was found to be 15.8 per cent., and in those fed on sugar only 6·3. Yet the fæcal evacuations had been, in most cases, moderate. But the sugar, though it did not increase the fat, served towards maintaining the temperature of the body and the average production of carbonic acid. The quantity produced by these birds, on ordinary diet, was 1.32 grains per hour; during seven days' starvation, it was 6·65 grains per hour; and during three days' diet of sugar, 11·08 grains. In turtle-doves fed for siz days on butter, the quantity of fat found after death was scarcely more than in those who had died on the diet of sugar without albumen; and the quantity of carbonic acid produced by them was 9·08 grains per hour. [Still these experiments of M. Letellier. though they may prove that sugar alone will not keep pigeons alive nor increase their fat, do not disprove that, under favorable circumstances, fat is formed from sugar. Such a transformation is proved, by the experiments of Huber and Milne Edwards on bees, which formed wax while feeding on pure honey; and to their evidence may now be added that of Grundlach.† He fed bees on sugar-candy in water, and they formed wax; from twenty pounds of honey, also, they formed a pound of wax.]

In controversial note on this and other subjects,‡ Liebig quotes a letter from M. Demesmay, who states that the results of abstracting from the food of fifty-eight cows a certain quantity of oil-cake, which contains from 10 to 15 per cent of oil, and substituting for it an equal weight of beetroot-molasses, were a more rapid fattening and a more copious production of milk. And to these evidences of the transformation of saccharine into fatty substances, it may be added that butyric acid may be formed in the fermentation of sugar,§ and that M. Avequin,‖ has noticed that the quantity of that crystalline wax which forms on the exterior of the sugar-cane, (and which he has named Cerosia,) always bears an inverse proportion to the quantity of sugar within the cane.

Production of fat in the animal body. Besides the experiments above mentioned, others have been performed by M. Persoz and M. Boussingault. Those of the former,¶ on the fattening of nine geese with maize, appear to prove that sometimes the weight of fat formed during the fattening exceeds the whole increase of weight of the body. He supposes, therefore, that the geese not only assimilate all the oily matters of the maize, and transform some of its starch and saccharine matter, but also transform a certain protion of their own tissues into fat. During the fattening, the blood becomes highly charged with oily matter, and much of its albumen disappears or is transformed. The experiments of M. Boussingault** afford evidence that some nutritive substance analgous to fat must be contained in the food in order that the fatty substance of the secretions and tissues may be duly formed.

Relation between food and excretions. M. Boussingault†† considers that he

* Annales de Chimie et de Physique, Juin; and Ann. des Sciences Nat., Juillet, 1844.
† Geschichte der Beinen, 1842. ‡ Annalen der Chemie und Pharmacie, Oct. 1843.
§ See last Report, p, 15. ‖ In Mulder's Physiol Scheikunde, p. 271.
¶ Report of the Academie des Sciences, 12 Fevrier. 1844; in the Gazette Medicale, 17 Fevr. I suspect that in that fatty degeneration of the muscles which takes place when they are not exercised, the change is not a removal of the muscular fibre and deposition of fat in its place, but a transformation of the fibrine, of which one of the products is fat; that it is, therefore, a chemical, not a nutritive process by which the change is effected. The nature of such a change is illustrated by Wurtz's observation (p. 250); and its occurrence is made very probable by the linear arrangement of the particles of fat, in the place of the former muscular fibres, on a plan different from that existing in any other form of fat. ** Annales de Chimie et de Physique, Oct. 1844.
†† Annales des Sciences Naturelles, Avril, 1844. Extract from M. Bouissingault's work 'Economie rurale dans ses rapports avec la Chimie, &c.'

has proved that in three days a horse and a cow each discharged in excrement and urine (and the latter in milk also), from 356·5 to 418·5 grains less of nitrogen than they had taken in their food. This quantity, therefore, he supposes must have passed off in respiration and transpiration in the free state. [But it is only presumed that the weight of the animals remained the same; and both the food and excretions were analysed by samples, so that small errors would be greatly multiplied in the general result; and no account is taken of what may have been discharged from the skin in hair, epidermis, &c.—an omission which must in many similar cases have left errors unaccounted for.]

A case of voluntary total abstinence from food and drink for ten days, is related by Dr. Casper.* The patient was thirty-six years old. For the first five days he suffered little, and confessed neither hunger nor thirst; during this time also he passed no fœces and very little urine. After this he became thinner and paler, his sight was weak, he had occasionally ringing in the ears, his speech was indistinct, his breath smelt unpleasantly, he discharged only a little urine, his abdomen sank in, and he was very weak. These conditions were increased on the ninth day, and he could not resist taking a little sugared water. In the night of the tenth day hunger, which he had before hardly felt, returned irresistibly, and he took food, and recovered. Mitscherlich examined the urine which was passed at the middle of his abstinence, and found that it did not differ from that of a healthy person.

Nutrition in its relations to structure. Development of cells. In the last Report,† the observations of Mr. Macleod are quoted, from which it appeared that in the chick, the blood-corpuscles are not developed by the formation of a cell round a preformed nucleus, but that each corpuscle is produced by the enlargement of a single granule, from the contents of which the nucleus is subsequently formed. Some other observations—by Schwann, on the development of the chorda dorsalis, by Vogt, on the development of new cells in the entercellular substance of the cartilage, and by others—had also afforded examples of cells formed before those cells within them which are regarded as their nuclei; and now, in an elaborate essay on the development of cells in general, Dr. Karsten‡ maintains that this is the regular plan of cell-development; that, at the first, each cell is a punctiform vesicle, which, subsisting on the surrounding substance, grows into a simple cell, and forms within itself some peculiar substance, either a secretion, or the rudiments of a new cell (or nucleus), or a new organism, as an ovum or gemmule.

A large portion of the evidence for this view is drawn from observations on vegetable cells. In that portion which relates to animal physiology, the chief examples are drawn from the development of the ova, gland-cells, the cells of the chorda dorsalis, cartilage, epidermis, pigment, and feathers.

In the *development of the ovum*, R. Wagner believed, from observations on the ovaries of the Agrio Virgo. that the germinal spot is the part of the ovum which is first formed, and that the germinal vesicle, vitellus and vitellary membrane are formed in succession around it; and it was in great measure through these observations that Schwann was led to adopt for the development of animal tissues the theory which Schleiden was supposed to have demonstrated for those of vegetables. Karsten considers that Wagner was deceived by the fineness of the membranes of the ovaries of the Agrio, which prevented his seeing that each of the follicles consists of two membranous cylinders—an internal one inclosing the ova, and an external one surrounding it; and that these have between them cells, which, through the delicacy of the membranes, appear as if they were within the internal cylinder, and were ova, but which disappear when the real ova are formed. The error cannot be committed with the ovaries of Lepidoptera: and among the best for examination are those of the Bombyx Mori, in the chrysalis state. The blind end of the internal cylinder may be found full

* Casper's Wochenschrift, June 8, 1844.

† Page 5.

‡ De Cella Vitali; Berlin, 1843.

of minute *simples vesicle*; and some of these may be traced, as they are examined further from the end, enlarged and filled by a granular blastema, and having in them a new cell which lies in the blastema, This is the germinal vesicle imbedded in the vitellus, and having the originally simple cell for the vitelline membrane. The same grades of development may also be traced in the ova of mollusca, if they are examined before they are filled with yelk,

The *development of secernent gland-cells* affords another example of the same plan of development. Some of Mr. Goodsir's observations agree with this view; and Karsten says the best evidence of it may be found in the follicles of the liver of mollusca and crustacea. In these may be seen, between the tunica propria and the central canal of the follicle, a great quantity of cells, surrounded by a liquid blastema, and containing a variety of granules, vesicles, and lesser cells; and there may be traced a series of formations, from the minute granule to the perfect cell, which may contain one or more cells, such as would be called nuclei, with one or more minute vesicles or nucleoli, in them. But in no member of the series will there be found a preformed cytoblast or collection of granules.

Schwann's and Vogt's observations on the cells of the chorda dorsalis and cartilage appear to Karsten (who has confirmed them) to support the existence of the same plan of development in those tissues; and he shortly describes the same as obtaining in the epidermis, pigment, and the rudiments of the feathers.

He denies also that, in either vegetable or animal growths, cells are ever multiplied by either generation or partition. He says of the apparent germination of the sporules of the Saccharomyces Cerevisiæ, and others of the lowest algæ (in which alone this process has been supposed to occur), that the process is not one of mere sprouting out of a part of the wall of the previous cell;* but that it consists in the development of a granule, which at first lies obscurely between the two membranes of which the cell is composed. As this granule is developed into a vesicle or cell, it expands over itself a part of the outer membrane of the previous or parent cell, till there is produced the appearance of two double-membraned cells united by a narrow constriction of their common outer membrane, but having their own cavities distinct. And as this goes on, the secondary cell has various contents formed within it; and may, by a repetition of the same process, develope a tertiary cell in its wall. The process may be best traced in the growing sporules of Phragmotrichum; but it may also be examined in the Saccharomyces, in which an external membrane, enveloping each apparently simple cell, may be demonstrated by moistening it after it has been dried for some time.

Anatomy of cells, Numerous examples of molecular movements of the granules in cells are described by Mr. Addison.† They are seen especially in the pale corpuscles of the blood, pus-globules, and mucus-globules, which all appear full of minute molecules in energetic movement. Prof. Rathke,‡ also, has often observed similar movements of particles within the nuclei of the cells of the ova of the frog, river crayfish, &c. But he shows that these movements depend, in all probability, on the currents produced in the fluid contents of the nuclei, by the imbibition of some of the water in which they are examined; for in every case in which they happen, the nuclei and cells become gradually larger; and, by examining them in oil, they neither enlarge nor exhibit the molecular movement. In this way may be probably explained the molecular movement of the pigment-granules of the choroid as seen within the cells. But the facts will not explain the observation of Dr. Sharpey,§ who has seen pigment-granules coursing round and round within the spherical epithelium-cells of a tadpole, and making the complete circuit of its cavity.||

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* See also Sharpey, in Quain's Anatomy, p. lii.

† Provincial Med and Surg. Journal, March 9 and June 5, 1844.

‡ Muller's Archiv, 1843, Heft vi. § Quain's Anatomy, 1843, p. lvi.

|| On all that relates to the healing processes and inflammation, I refer to the excellent Reports on the subject by Mr. T. Wharton Jones. in the last April and July numbers of this Review. For many experiments on the healing of fractures, see Lebert, "De la formation du Cal," in the Ann, de la Chirurgie, Fevr. 1844.

ORGANS AND FUNCTIONS OF EXCRETION.

Skin and its appendages. A complete examination of the structure and some of the functions of the skin and its appendages, with many original observations, has been published by Professor Krause.* Of course, the sizes and weights of everything that can be so estimated are given. In explanation of the colour produced in the epidermis by nitrate of silver, and supposed to depend on the decomposition of the tissue, Krause says that if thin-cut layers of epidermis soaked in a solution of nitrate of silver be exposed to the light, and then made transparent by acetic acid, their texture may be seen to be unaltered, but there are very dark granules from 1-1000th to 1-1500th of a line in diameter, on the outside of the larger cells, which are, no doubt, chloride of silver and reduced silver, and to these, not to a decomposed tissue, the change of colour is due.

Krause says also, that the colour of the cuticle of the Negro does not depend (as Henle supposes,) on pigment-cells, like those of the pigmentum nigrum, lying between the cutis and rete, and mingled with the cells of the latter, but, chiefly, on the color of the proper nuclei and cells of the epidermis. There are indeed some few pigment-cells mingled with the proper cells of the middle and superficial layers of the epidermis; but they are distinguished from those of the pigmentum nigrum by containing far fewer pigment-granules, and by having always dark (not a cléar) nucleus. The color depends especially on the dark- or almost black-brown colour of the nuclei, whether free in the deep layers of epidermis, or surrounded by cells. They have dark nucleoli, sharp outlines, appear only very obscurely granular, and cannot be broken into smaller pigment-granules. The cells surrounding them may be seen; in the deeper layers, they also are uniformly dark, though less dark than the nuclei. In the middle and superficial layers, the nuclei, as long as they can be seen, are still dark; the cells are much paler, but brownish and darker than in the corresponding layers in uncoloured persons.

The so-called *Tyson's glands*, the little white elevationi which are usually found round the corona glandis of the human penis, and which, after many disputes, have been usually considered as the secernent follicles of the *smegma preputii*, have been carefully examined by Dr. G. Simon.† They are, he says, no more than small round elevations of cutis covered by papillæ and epithelium. They consist of fibro-cellular tissue like that of the rest of the cutis; and the papillæ on them have no peculiar characters. The only function that can be ascribed to them is that of increasing the sensibility of the glans. The only organs which Simon could find for the special secretion of the *smegma*, (and these are not constant,) are whitish corpuscles lying in or beneath the cutis, which, with the microscope, appear as small roundish sacculi; closed below, and opening by a narrow orifice on the surface, and containing a white substance. These are usually situated on or behind the corona glandis, in front of or near the frænum, and sometimes on the anterior surface of the glans itself. Two or three may be found, or, in a few cases, as many as six.

Cutaneous perspiration. Krause ‡ has managed to collect, with great care to avoid mixture, a small quantity of pure cutaneous perspiration from the palm, in which, as is well known, there are no sebaceous glands. The fluid collected, yielded with boiling ether some small globules of oil and crystals of màrgarine: it was acid, but after twenty-four hours became alkaline by the development of ammonia, In another experiment, he found that the tissue of the epidermis, independant af the fatty matter secreted on its surface, contains a fatty substance.

He has also endeavoured to number and measure the sweat-glands. As an average, he says, it may be estimated that in each superficial square inch of the body there are 1000 orifices of glands of 1-6th of a line in diameter; the greatest and least numbers in this space being in the palm 2736, in the sole 2685, in the cheek 548, in the neck, back, and nates 417. The whole number, therefore, ex-

* In the unfinished article Haut in Wagner's Handworterbuch der Physiologe, Lief. vii.
† Muller's Archiv, 1844, Heft i. ‡ L. c. p. 146.

cluding the axillæ, in which they are peculiarly large and thick-set, may be estimated at about 2.381.248. Accepting these numbers, and supposing each gland of be occupied by a column of fluid presenting at the orifice a hemispherical surface, 1-56th of a line in diameter, (the size which Krause found by measurement in some drops in a warm and moist but not *sweating* skin,) then the whole of the glands would present an evaporating surface of 6.896 square inches. Hence it is probable, (according to ascertained laws of evaporation, and from experiments purposely made,) that a portion only of the fluid discharged by cutaneous exhalation is produced by these glands; for there could not be more than 3365 grains evaporated in the twenty-four hours from such a surface, under favorable circumstances; whereas Seguin's experiments show that the daily cutaneous transpiration varies from 5·93 to 10·465 grains per minute: probably, therefore, much of what is discharged by the skin, passes by simple transudation through the epidermis, and evaporation from its free surface.

Experiments by Valentin,* made with great care on his own person, afford evidence concerning the total daily amount of transpiration, both cutaneous and pulmonary. Taking three days of ordinary life in September, weighing himself while naked, fifteen times a day, and weighing all his ingesta and sensible excretions,— the averages of the three days gave,—nutritive matter taken, 45325·5 grains; excrement, 2956·3 grs.; urine, 22439.3 grs.; perspiration, 19327·4 grs.; i.e. the ingesta being taken as 1; the excrement was ·065, the urine ·503, the perspiration ·422. But there were differences in the days; in the first, the relation between the ingesta and the excretions was as 1·097 to 1; in the second, as 1·028 to 1; on the third, as 1 : 1·090.

The hourly amount of transpiration was at some times 4½ times as great as at others: the greatest difference being caused by whatever excited sweating or a perceptible moisture of the skin, e.g., on the same day, the hourly amount was, after taking two cups of coffee, and during gentle perspiration, 1213·65 grains; in the forenoon, in pretty active excercise and sweating, 1402·75 grains; and in the evening, during copious sweating from excercise, 2056·85 grains; but, while writing quietly in the forenoon of the same day, it fell to 858·7 grains; and three or four hours after dinner, was only 509·95 grains. Of all things, none influenced the transpiration so much as rest and bodily exercise; even when the latter did not produce evident sweating, its effect was considerable. After eating also, transpiration was generally increased; and its minimum was observed during fasting and rest in a cool temperature. During the night and in sleep, the transpiration was diminished, but not more than in rest during the day. Mental exercise had no obvious influence.

Experiments by M. Magendie† confirm those by M. Fourcault and others, on the effects of covering the skins of animals with varnishes impermeable to air. The animals always died as if asphyxiated, with their hearts and lungs gorged with blood; and during life the temperature of their bodies fell gradually 18°, 24°, and as much as 36° below the ordinary standard. The same effects were produced by inclosing animals, (all but their heads,) in dresses of caoutchouc cloth.

Structure of the nails. Riechert‡ considers, as Kohlrausch§ also does, that the streaks described by Henle in sections of nails, as indicative of their laminated structure, are only cracks and seams produced by the knife; and says that their direction is always determined by that in which the section is made. To him the nails appears homogeneous; except for those dark spots which Kohlrausch and Krause suppose to be remains of nuclei, but which Reichert considers to be vacant spaces in the nails.

Reichert further considers, that the only matrix of the nail is that part of the

* Physiologie, Bd. i, p. 7161. The details are given in Valentin's Repertorium, Bd ix, and some in the Medical Gazette, July 19, 1844.

† Constantin James, 'Voyage a Naples avec M. Magendie;' Gazette Medicale, Dec. 6, 1843.

‡ Muller's Archiv, 1843; Jahresbericht, pp. 270-9.

§ Gottingische Gelehrte Anzeigen, Ht. xxiv, p. 229.

cutis which forms the posterior half of the upper wall of the proximal semilunar groove, and the angle and lower wall of the groove, and that which lies beneath the lunula. There is a layer of cells similar in kind, but ln different stages of development on the whole of the surface of the cutis above and below the nail; but the destination of the cells on different parts of this surface is very different, and it is only on the part indicated above that they are developed into nail-substance. Above the nail, they are developed into the thin layer of epidermis, which forms a fringe upon the borders of the nail. On all the surface of the cutis anterior to the lunula, (on what is called the *bed of the nail*,) the cells from a layer like epidermis, (i.e. according to Krause, like the deepest layer of the epidermis in other parts of the body,) between which and the under surface of the substance of the nail, there is a distinct line of boundary, the long diameter of those of its cells which are immediately adjacent to the nail being *perpendicular* to its axis. This arrangement exists as far forwards as the nail is closely adherent, i.e., to within a line of the part at which the nail and cutis separate; for this line's breadth, a thin layer of ordinary epidermis is prolonged backwards under the nail, as another layer is prolonged forwards on the inferior surface of its free part.

The cells formed on the surface of the proper matrix of the nail are, in their early conditions like those of epidermis; but as they go on to form nail, they become larger, more transparent, oval and flat; their muclei (which according to Krause and very evident, by their darkness, in the negro,) become smaller and at lest disappear, and gradually the traces of their own outlines are lost, as they unite into the compact, uniform, and nearly transparent, proper substance of the nail. It is only the cells formed at the angle of the groove which, lying parallel to the plane of the nail, have from the first a right direction forwards; those formed on the upper and lower walls, only gradually assume the same direction. Hence the surface of vertical sections of the root of the nail are slightly marked in a penniform manner. The whole nail therefore may be described as formed from the cells produced at the matrix; and as sets of these are successively produced and coalesce, the older ones are pushed forwards over the layer of epidermis-like cells which covers the whole surface of the bed of the nail.

Structure of hair. Krause's* description of the structure of the nails, tends to show their exact analogy with epidermis; and he thinks the hairs may also be regarded as epidermoid tissues, their cortical portion being analogous to the outer or horny layer, their medullary portion iu the inner layers of epidermis, and their shape being due only to that of their matrix. The root-sheath of the hair, he says, is only the epidermis of the follicle; its outer layer is the continuation of the deep and middle layers of epidermis, and consists of nuclei and roundish or polygonal cells, which are especially distinct in the negro, and are set vertically to the wall of the follicle; the inner layer, (or inner root-sheath,) is continuous with the outer horny layer of epidermis, and is composed of long flat cells with few or no visible muclei. It is very apt to tear in long fibres; the holes in it, which led Henle to call it a fenestrated membrane, are produced by the manipulation.†

KIDNEYS AND THEIR SECRETION.

Composition of the urine. In a highly interesting paper on the constitution of the urine, Leibig‡ maintains the following points:

A. That neither lactic acid nor any lactate exists in healthy urine; the evidence being, 1, that hitherto no example is known of lactic acid being produced

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* L. c. p. 125, &c.

† On the chemical composition of the hair, I can only refer to the elaborate analysis of J. F. J. van Laer, in the Scheikundige Onderzoekingen; Utrecht, 1842. Analyses of it are given in Schmidt's Jahrbucher, Oct. 1843; the Medical Times, Feb. 3, 1844; and, briefly, in Mulder's Phys. Scheikunde.

‡ Ann. der Chemie und Pharm., Mai; and Lancet, June 1-8, 1844.

by the decomposition of a nitrogenous substance ; 2, that the urine of the herbivora, in which lactic acid or its salts might expected, (if they existed in that of the carnivora,) does not contain either ; 3, that lactic acid has never yet been clearly detected in the urine of men or carnivora , 4, that the carnivora take no food from which lactic acid could by transformation, be produced ; 5, that fresh urine will not dissolve the smallest quantity of barytes, though lactate of barytes is easily soluble in water ; 6, that in various and the most careful experiments, it has been impossible to detect even a trace of lactic acid in large quantities of putrid urine, in which, if it had existed when fresh, it could not have been altered by putrefaction, and if it had not existed when fresh, it might perhaps have been produced by putrefaction. An organic acid was produced in putrefaction, but it was acetic acid combined with a resinous highly azotised substance.

B. Hippuric acid is a constant constituent of healthy human urine ; for 1, benzoic acid is obtained, (as Proust observed,) with acetic acid, by distilling urine with sulphuric or hydrochloric acid ; but, 2, this benzoic acid cannot exist as such in the fresh urine ; for benzoic acid is converted in the organism into hippuric acid ; and the hippuric acid known to exist in the urine of herbivora yields benzoic acid when it is decomposed ; and 3, the existence of hippuric acid may be clearly proved in even small quantities of fresh urine, by evaporating it to the consistence of syrup, mixing it with some hydrochloric acid, and agitating it with ether, which dissolves the hippuric acid*

The hippuric acid thus obtained, cannot be derived from the discomposition of benzoic acid taken in the food, (for probably none of man's food contains any ;) it is formed in the body from the non-nitrogenized aliments. The acetic acid does not exist in fresh urine ; but it and the resinous substance with which it is combined may be regarded as the products of the discomposition of the coloring matter of the urine.

C. The acid reaction of healthy urine is due to the presence of the acid phosphate of soda, and the mode in which this salt is produced is as follows : alkaline phosphates are taken in meat, flour and grains ; none of these contain any free alkali ; and it is from these phosphates, and not from any free alkali or alkaline carbonate, that the chyle, lymph, and blood, derive their alkaline reaction. Now, among the remarkable properties of the bibasic phosphates of soda and potass are their relations to uric and hippuric acids. Both these acids dissolve very easily in water, to which common phosphate of soda has been added, and with their solution, the phosphate loses its alkaline, and assumes an acid, reaction. And thus, when the uric and hippuric acids are formed in the organism, they combine with the soda of the alkaline phosphate, forming the highly soluble urate and hippurate of soda, and an acid phosphate of soda.

D. But besides this, there is another cause by which the acidity of the urine is maintained and increased. The urine of man and the carnivora contains a large quantity of sulphates ; but their food does not contain either those salts ready formed, or any oxygen-compound of sulphur. The sulphur which it does contain-or (which comes to the same thing,) the sulphur of the transformed tissues, must therefore combine with oxygen in the body, and the sulphuric acid thus formed, combining with part of the alkali of the alkaline phosphates, converts them into acid phosphates, and thus maintains and increases the acidity of the urine.

E. It follows that whether the urine will be acid or not, depends upon the nature and quantity of bases taken with the food. If the amount be sufficiently large to neutralize the uric, hippuric, and sulphuric acids formed by the organism, and the acids supplied by the food, the urine must be neutral ; if the amount be more than enough, the urine must be alkaline ; if less, acid. And hence, no physiological or pathological inference can be drawn from our examination of the urine, unless an account be taken of the inorganic acids, salts, and bases taken with the food.

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* Liebig estimates the quantity of benzoic acid in the urine to be equal to that of the uric acid. Dr. Golding Bird has never found it exceed one third of the quantity of the latter. (Med. Gazette, Aug. 23, 1841.)

An exception to the rule that carnivora alone produce uric acid exists in the case of butterflies, (and other lepidoptera ?). Heller has discovered that in proportion to the weight of their bodies they of all animals produce the greatest quantity of uric acid. Their urine is analogous to that of serpents and predatory birds, containing as a chief constituent urate of ammonia ; it is principally a product of the metamorphoses which go on in the pupa state, and the red or yellow fluid which they discharge soon after being hatched is chiefly urate of ammonia. The secretion continues in after life.*

VASCULAR GLANDS.†

Spleen. Dr. Julian Evans,‡ together with many confirmations of the descriptions of Malpighi, Müller, and others, has given a more complete account than yet existed of the cells of the human spleen. They are, in comparison with its parenchyma, smaller and less capable of distinction than in the graminivora ; probably from one third to half a line in diameter, and pentagonal or hexagonal in form. They are frequently found filled with coagulated blood; and, in this condition they give the spleen a granular appearance. But the chief peculiarity of his account is, that he describes [what certainly needs confirmation, for they must be very difficult to demonstrate] a set of transparent vessels, of less diameter then the small splenic corpuscles, (i. e. less then 1-7000th of an inch,) and appareutly arising from them : he believes these to be lympathic vessels, and that they gradually unite and form trunks which can be traced trom the parenchyma into the Malpighian bodies, (which he considers to be lymphatic glands,) from which, after numerous convolutions, they emerge fewer in number but larger, and pass through the pedicle by which the body is attached.

A case is recorded§ in which a man lived in good health for thirteen years after removal of the spleen.

Schwager Bardeleben,|| from his experiments on the extirpation of the spleens of animals, has obtained (as others before him have,) scarcely more then negative results. Those who survive the operation appear quite unaffected by it ; so also it is with those from which the thyroid body has been removed, (except that in one rabbit the venereal appetite seemed to be increased.) Even when both have been removed, no evident effect is produced upon any of the functions of the animals that survive ; but indeed few survive both operations. The removal of the spleen does not produce impotence ; and the author has never seen the spleen reproduced. But, according to the continued investigations of Mayer,¶ some slight changes are produced. He says that after the spleen has been extirpated it is usual to find all the mesenteric glands more or less swollen and blood-red, blue, or blackish. Moreover he has often found the small lymphatic glands, near the part where the splenic artery has been tied, marked with the bloody spots, in which at last capilary networks form ; and he believes that then several such glands unite and form the new spleen.

SKELETON.

M. Bretchet** says that he has seen ten cases in which the human malar bone was composed of two pieces, a superior or orbital, and an inferior, jugal, or zygomatic portion ; presenting an analogy with normal formation in some of the quadrumana and in several other mammalia, and making it probable that the bone is developed from two osseous nuclei.

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* Oesterreichische Med. Wochenschrift, Sept. 1844.

† Among the notices of these mysterious organs recently published, is an account of the microscopic structure of them all. by Dr. Oesterlin, in his Beitrage zur Physiologie, Jena 1843, 8vo. The results he has arrived at are not however, so definite or important but that the further account of them may be deferred till, in the next Report, an account is given of the work of Mr. Simon on the Thymus and Thyroid Glands:

‡ Lancet, April 6, 1844.

§ Gazette Medicale, No. 28, 1844 ; and Oesterr. Wochenschrift, Sept. 21, 1844.

Gazette Medicale, 23 Mars, 1844 Repoert of the Academie des Sciences ; Seance de 18 Mars.

¶ Med. Correspondenz-Blatt Rhein. und Westphal. Aerzte, 1843, No. 5 ; and Schmidt's Jahrbucher Januar. 15, 1844. ** Annales des Sciences Naturelles, Janvier, 1844.

Dr. Wilbrand* describes and figures a case in which a *supra-condyloid process*, like that described by Dr. Knox,† but less perfect, was found on the human humerus. It arose from the front and inner aspect of the humerus, a short distance above the inner condyle, and, like a rose-thorn in shape, about six lines long and two lines in diameter it extended towards the inner condyle, to which its extremity was connected by a fibrous band; the brachial artery and median nerves after running past the inner side of the process went under the ligament to their ordinary position at the bend of their elbow.

He describes also a process on a femur, which he calls the supra-condyloid process of the femur, and considers as the analogue of the process found in certain edentata, rodentia, pachydermata, and others. In these there is a process of considerable size on the outer aspect of the thigh, at the middle, or a little above or below the middle of the shaft. The subject in whom he found a rudiment of a simular process was a strong man. The process was situated at the attachment of the short portion of the biceps femoris. It was one and a half inches long, four lines thick, and projected outwards nearly three quarters of an inch. It was covered by the persistence of the femur, and a rather large nutritive artery of the femur passed through; it had not at all the aspect of a morbid exostosis.

VOICE.

MM. Pétrequin and Diday,‡ after, as they believe, satisfactorily disproving all previous explanations of the falsetto voice, maintain that in it "the glottis places itself in such a state that the vocal cords can no longer vibrate like reeds. Its contour represents the embouchure of a flute, and it is not by the vibrations of the aperature, but by those of the air, that the sound is produced." They do not explain how the assumed rigidity of the lips of the glottis by which these vibrations are prevented is produced; [it would be very difficult to do so:] but they maintain their theory by the following statements: 1. There is an analogy between the tones of the falsetto voice and those of the flute, from which the former are often called *fluty*. 2. There are but two modes in which voice can be produced: by the vibration of the vocal cords and by that of the air; and since the chest-notes are produced by the former, the falsetto must be produced by the latter. 3. Bass voices have commonly no falsetto notes [?]; because the aperture of the glottis is too large for the air to be thrown into vibrations in passing through it. 4. High chest-notes easily pass into the corresponding falsetto notes when we try to soften them; for when we wish to diminish the loudness of any note we are singing, the glottis is instinctively constricted to prevent the note from falling in consequence of the diminished force of the current of air; but when the note which is being sung is high, and the ligaments already very tense, a reduced current of air could not make them vibrate if they were still more tense; the air therefore, instead of making them vibrate, vibrates itself as it passes between them, and the glottis is changed from a reed-like to a flute-like instrument. 5. In the same manner when we try to strengthen a low falsetto note, it unavoidably assumes the character of a chest note, by the lips of the glottis passing from the rigid state to that of vibration,—from the state in which the air alone vibrates in passing between them to that in which themselves vibrate. 6. The difficulty of passing imperceptibly in ascending or descending scale, to or from the falsetto notes, indicates that the state of the glottis in the two kinds of voice is wholly different. 7. The supposed change, from the vibrating to the rigid state of the lips of the glottis, may be imitated and illustrated with a reed-instrument, such as bassoon. Its ordinary notes are like chest notes—but if, while sounding them, the reed be suddenly taken hold of and held with forceps so as to prevent its vibrations (though nothing else be altered,) the notes become acute, soft, and

* Ueber Processus supra condyloideus Humeri et Femoris; Giessen, 1843, 4to.

† Edinburgh Medical and Surgical Journal, 1841, vol. lvi; and Medical Gazette, July 7, 1843. See also, on similar subjects, his numerous and interesting "Contributions to Anatomy and Physiology" in the Med Gaz., Nov. 4, &c., 1843.

‡ Gazette Medicale, Fevr. 23, and Mars 2, 1844.

whistling—they pass from reed-notes to flute notes—from those like the chest-notes to those like the falsetto.

NERVOUS SYSTEM.

General anatomy. Structure of the nerve fibres. Reichert* confirms Volkmann and Bidder's account of the speciality of the sympathetic nerve-fibres, their distinctness in size and structure from the cerebro-spinal.† He believes also that the tissue which invests the smallest fasciculi of the sympathetic fibres of the higher vertebrata, (that which has been variously described as filamentous epithelium, fibro-cellular tissue, *formatio granulosa*, &c.,) is a transparent, finely granulated membrane, which has a peculiar tendency to wrinkle, and sometimes to separate, in a longitudinal direction, so as to assume the appearance of a fibrous texture.‡

Central ends of nerve-fibres. In a monster without either brain, medulla oblongata, or spinal cord, Dr. Lonsdale§ examined the central extremities of the roots of several of the cerebral nerves, (from the 5th downwards,) which were hanging free and unattached in the cranial cavity. The extremities were enveloped in a delicate membrane of filamentous tissue, but after removing this and numerous granules which surrounded the nerve-fibres, it was quite evident that the latter, in every instance in which they could be examined, formed loops. Each fibre, after passing to the central end of the fasciculus, then turned back upon itself, and could be traced down the fasciculus towards the periphery. The fact has peculiar interest in that it adds probability to the opinion of Valentin, Carus, and Klencke,‖ that in the normal state the nerves in their cerebral central extremities (as they are called,) form loops, analogous to those formed at their peripheral extremities.

But that this is not the only mode in which the nerve-fibres are related to the centres would appear from the observations of Dr. Will¶ on the nervous ganglia, and the origins of the nerves in invertebrata. He states that the primitive nervous fibrés terminate or commence in certain nerve-corpuscles or ganglion-corpuscles within the cephalic and other ganglia. The general results of his examinations** (so far as the are likely to be soon applicable in human physiology,) are, that the nerves enter and leave the ganglia through constricted apertures or meshes in its external investment, the fibres of which are continuous with those of their neurilemma, and with others by which the ganglia are partitioned. The ganglia contains besides the nervous fibres and the nerve- (or ganglion-) corpuscles, a granular substance filling up the spaces between them, and often coloured by pigment and various cells. There are two kinds of ganglion corpuscles. In the one kind, the space between the investing membrance (the secondary-cell of Henle,) and the cell is filled by a pellucid hyaline substance, which coagulates by the action of water or acids. Each of these corpuscles has always one appendage, a simple tube, which never divides into branches, but may be traced into direct continuity with a primitive nerve-fibre, or tube. In the ganglion-corpus-

* Muller's Archiv, 1844; Jahresbericht, p. ccvi.

† See Report, 1842, p. 34. But Valentin still maintains the absence of any distinction between the two sets of fibres, in Muller's Archiv. 1844, Heft iv, p. 395.

‡ In like manner he describes the tissue connecting the vessels and other elementary parts of the kidney, as Mr. Bowman does, who calls it the matrix, as a uniform structureless substance which has even less tendency than the similar connecting tissue in most parts of the body has to wrinkle itself or break up so uniformly as to appear like fibro-cellular tissue. In all this his view coincides with that of Dr. Todd and Mr. Bowman. (See last Report, p. 5.)

§ Edinburgh Med. and Surg. Journal, Jan. 1844.

‖ Report, 1842, p. 36.

¶ Muller's Archiv, 1844, Heft ii, p. 76. His account is very similar to that of Helmholtz, who, in his dissertation 'De fabrica syst, nervosi evertebratorum,' Berol. 1842, (analysed in Muller's Archiv. 1844, Jahresber,) has described the like structure in many more species, and especially the direct passage of the nerve-fibres into the ganglion-corpuscles of the cephalic ganglion of the leech.

** The chief subjects of examination were the leech, Helix pomatia, Astacus fluviatilis, and Lymnæus stagnalis. Details of the modes of preparing the nerves for examination are fully given in the paper.

cles of the second kind, there are numerous minute cells without nuclei in the clear substance within the investing membrane; and they have generally several appendages, which neither are nor contain tubes, but are striated, and consist throughout of fine fibres. In their course they divide into two or three branches, which may again branch, and even break up into their component fibres. The larger branches have, at various distances, varicose enlargements; and the smallest ones run together into very small ganglion-like swellings, with dark central spots like nuclei, from which again similar fibres proceed in all directions.

When the nerves enter at the cephalic margin of the ganglion, after passing through the constricted aperture, they spread out a little, and enlarge, and finely granular substance lies between them; when they leave it the become closer again, and similar granular matter accompanies them for some distance in their sheath. They remain separate in their course through the ganglion, lying immediately beneath its dorsal surface, and give off from the outer margins the lateral nerves, which, like themselves, leave the ganglion by constricted apertures iu its neurilemma, and are accompanied by granular matter.

The tubular appendages of the ganglion-corpuscles of the first kind are all directed towards the exit of the nerves, aud become connected with them near or at the point at which they emerge. From their attachment to the corpuscles, the appendages gradually become narrow, till they attain a certain permanent thickness nearly equal to that of the primitive nervous fibres into which they are continued.

Peripheral terminations of the nerve-fibres. The investigations of Henle and Kölliker* have proved a new and peculiar mode of termination of the nerve-fibres in the little bodies, seated especially in the nerves of the fingers and toes, which were discovered and to a certain point well described by Pacini of Padua, in 1830. These bodies (to which the name of *Pacinian corpuscles*† is now given,) are found in man at all ages after the twenty-second week of fœtal life, and under all circumstances, and in many mammalia. They are most numerous on the cutaneous nerves of the hands and feet; but they occur also sometimes on other sensitive cerebro-spinal nerves, and on the sympathetic plexuses in the mesentery and mesocolon, and about the pancreas; where they are especially numerous in cats. In man, from 150 to 350 may be counted on a single limb: and they are chiefly abundant on the branches of the digital nerves just penetrating the cutis; to which they are attached singly, or in pairs, or, sometimes in groups, by little fibro-cellular pedicles. Through the pedicle of each, a single primitive nerve-fibril passes into the corpuscle.

The corpuscles are of various form, elliptic, ovate, obovate, crescentic, or reniform: they measure, (in parts of a line,) from ·66 to 1·2 in length, and from ·45 to ·6 in breadth. They are semitransparent, slightly glistening, and appear as if a central cord passed through them. Each of them is composed of from 40 to 60 very thin coats, arranged round a central canal or cavity, like so many capsuls inclosed one within another; and each coat or capsule is composed of two layers of fibro-cellular tissue, an outer layer with circular, and an inner with longtitudinal fibres. Between each two adjacent layers or capsules, there is an albuminous fluid; it is most abundant between the outer capsules which are less compactly arranged then the central ones. The outermost of all the capsules in each corpuscle is connected by cellular tissue with the adjacent parts, from which also blood-vessels penetrate inwards through more than half the layers. Here and there the adjacent capsules appear connected by partial septa

* Ueber die Pacinischen Korperchen; Zurich, 1844, 4to.

† A minute account of them will be found in the last January Part of this Journal; A further account of them in several mammalia is given by Pacini, in the Annali Univer. di Medicina, Gennaio, 1844. They are described as lacteal organs by M. Lacauchie, the nerve-fibril being taken for a chyle vessel, in his communication to the the French Academy of Sciences, Oct. 30, 1843; and, more recently, by Mayer, as glands with excretory ducts passing into the nerves, in the Oesterr. Med. Wochenschrift, Sep. 14, 1844; from the Med. Corresp. Rhein. und Westphal. Aerzte, No. 3, 1844.

extending across the spaces containing the fluid, and this is especially the case at the end opposite to the pedicle. The canal or cavity in the axis of each corpuscle contains a fluid like that between the capsules, and, in this fluid, a primitive nerve-fibril. The nerve-fibril after traversing the pedicle of the corpuscle and a conical prolongation from the end of the pedicle through the substance of the lower part of the corpuscle, enters the cavity, and at once becomes smaller, paler, and flatter. It passes along the cavity, and at or near its distal end, terminates in a knob, or by bifurcating: in no case is anything formed like the *terminal loops* of nerves, and it is very rarely that more then one nervous fibril enters a corpuscle ; neither does the terminal enlargement of the nerve-fibril resemble a ganglion corpuscle.

Of the use of these bodies little can be said. It is suggested that as their construction with alternate layers of membrane and fluid is rather like that of the electric organs of the electric ray, &c., these also may be electric organs, and, according to Pacini, the chief agents in mesmeric operations. But Henle and Kôlliker could find no manifestations of free electricity in them during life. Their not ocurring upon any known motor nerves, would appear to prove that they have nothing to do with motion ; but their existence on many nerves of the sympathetic system, and their non-existence on many sensitive nerves, make it also probable that they are not connected with acutenes of sensation. They may be electric organs, as their peculiar structure snggests ; but before they can be concluded to have any relation to animal magnetism, it would be advisable to prove that *that* has any relation (exept in name,) to physical magnetism or any form of electricity.

GENERAL PHYSIOLOGY OF THE NERVOUS SYSTEM.

The publication of the collected observation of MM, Matteucci and Savi is an important fact in this year's history of the progress of nerve physiology ; but the contents of their work* are not of this year's growth, consisting as they do, almost entirely, of the observations which have been a long time in course of publication,† and are now collected and arranged. They cannot therefore be brought within the range of this Report ; though the observations which M. Matteucci has made since the publication of his work properly fall within it.

Of these continued observations, some were made with M. Longet‡ to determine the influence of electric currents on the anterior roots of the spinal nerves and the anterior and lateral columns of the cord. The result was that their influence is wholly different when exercised on centrifugal fibres alone, from what it is when exercised on mixed fibres. In the latter case, (according to the experiment performed before the last,) contractions take place only at the commencement of a current directed centrifugally, and at the interruption of a current directed centripetally ; in the former case the contractions ensue only at the closure of the circle with a centripetal current, and the opening of one with a centrifugal current. The anterior (and, though with less energy, the lateral) columns of the cord act, in these respects, like the motor nerve fibres.

Other researches§ show that there is a direct proportion between the quantity of electricity employed in repeatedly exciting (through the lumbar nerves,) muscular contractions in the posterior extremities of frogs, and the amount of force exerted by the muscles thus contracting. The proportionate quantities of electricity were measured by the quantity of zinc which was dissolved in its production in the several experiments ; and the muscular force by the distance through which a weight attached to the feet was drawn. The muscular force was reduced to one half and to one third, when the current was reduced, in different experiments, to those amounts. The amount of force exercised by the muscles

* Traite des Phenomenes electro-physiologiques des Animaux ; Paris, 1844, 8vo.

† In the Bibliotheque Universelle de Geneve, whence extracts have been commonly published in other journals.

‡ Gazette Medicale, 14 Sept. 1844. Report from the Acad. des Sciences, 9 Sept. 1844

§ Gazette Medicale, 21. Sept. 1844. Report from the Academie des Sciences, 16. Sept.; and Annales de Chemie et de Physique, Aout, 1844.

was six times as great as would be obtained by the combustion of the same quantity of zinc, or by using the same current in an electro-magnetic apparatus.*

In an endeavour to establish a theory that the cerebro-spinal nerves (excepting nerves of special sense,) should be classed as muscular and cutaneous nerves, and not as motor and sensitive, Dr. J. W. Arnold† (Heidelberg,) relates some experiments which render it possible that the mind derives the consciousness ot the position of a muscle at any given time through the medium of the motor, and not of the sensitive nerves, or at least through the medium of filaments which are included in the anterior spinal roots. The chief fact is that when the posterior roots of the nerves of the posterior extremities of a frog are divided, although no external stimulus of either hind-leg excites movements of it, yet when by exciting other and sensitive parts, the frog is induced to move its hind-legs, it always first puts them into a position adapted for the performance of the intended movement. *E. g.*, if one of the hind-legs of a frog has had its sensitive (posterior) nerve-roots cut, and this leg be extended, when the frog wishes to leap, it first draws this leg up, and than leaps with it as well as with the others, all the nerves of which are entire; as if, though the leg could convey no sensation of objects, it still was able to give the subjective sensation of the position of its muscles.

[The fact is singular, but far from sufficient to prove the theory. It is contradicted also, so far as man is concerned, by the cases of persons who having lost the sensation, but not the voluntary motion of the arms, are so unaware of the position and state of there muscles, that they are obliged to look at what they are holding, lest they should let them fall. The other part of the theory, namely, that those commonly called sensitive nerves should be called cutaneous nerves, is founded on the notion that when the skin is strippad off a frog's hind-leg, the limb is in a state similar to one of which the posterior nerve-roots are divided. Doubtless in such a state the frog does not wiliingly move its leg; but is the leg therefore insensible ?]

To prove the *functional independence* of the sympathetic nervous system, that is, that it is independent of, and essentially different from, the cerebro-spinal system in the discharge of its functions, Volkmann and Bidder‡ have published an extensive series of experiments on the effects of removing from frogs the brain, or spinal cord, or both, leaving in every case the medulla oblongata so that the respiratory movements might continue. The general result was to show the strongest contrast between the effects of the destruction in the parts supplied by cerebro-spinal nerves, and its effects on those supplied by the sympathetic. In the former, all the muscles were rendered at once incapable of contracting upon either voluntary or reflex stimulus; in the latter, it was long before any effect was produced. The circulation in the web continued unimpaired two weeks after crushing the cord, fourteen days after destruction of the brain, and five days after destroying both at the same time; and the pulsations were as frequently and vigorous as in healthy frogs. Sufficient evidence was also afforded, that whether the brain or cord or both were removed, the processes of exudation and absorption were very well carried on in the limbs. [The contrary results obtained by Valentin and Stilling, are shown to have been due to the improper mode in which they kept the frogs.] The intestinal canal also continued, to the time of death, to be active and irritable, and both it and the heart remained

* There are further observations in this subject in the just-published third part of the 2d volume of Valentin's Lehrbuch der Physiologie, of which a report will have to be given next year.

† Ueber die Verrichtung der wurzelnder Rueckenmarksnerven; Heidelberg, 1844, 8vo. Analysis in Schmidt's Jahrbuecher, April 1. 1844. Mr. Swan appears also to think that the sensitive nerves are not the conductors of our impressions of muscular fibre. "The sensory judges of slight changes produced by motion on the skin;" but the forces of "motive action are appreciated by a specific centre, probably by the striated body;" "The principal offices of the brain, &c." 1844, p. 11.

‡ Mueller's Archiv. 1844, Heft iv, p. 359.

capable of being excited to healthy movements for some time after the voluntary muscles had ceased to be excitable by any kind of stimulus. Urine also was secreted in natural quantity, but was retained in the bladder, which by the destruction of the central parts of its cerebro-spinal nerves had lost its power of contraction. Food also was digested completely, and in the ordinary period, in the stomach; so that, on the whole, no organic function was materially disturbed by the destruction of the brain and spinal cord, although, by the integrity of the medulla obluugata and the respiration, the animal lived many days. Since during the same time all automatic and other movements really dependent on the cerebro-spinal centres ceased, it is deduced [and with great probability, at least, in frogs,] that the functions of the sympathetic nerve are independent of its connexions with the brain and spinal cord.*

The experiments just cited regarding the secretion of urine after the loss of influence from the brain and cord, are corroborated by M. Segalas.† His experiments, and the cases of two patients recorded by him, tend to prove (against the experiments of Krimer,) that division or complete destruction of the lower part of the cervical region of the spinal cord, and of any or all parts below it, does not check the secretion of urine; and that destruction of the upper part of the cervical portion does not affect it, provided artifical respiration be completely and early established. When the urine of numerous rabbits, guinea-pigs and dogs, secreted after such division or destruction, was analysed, it was found sometimes of healthy constitution, and when altered, the alterations were slight and not uniform. M. Segalas, therefore, deduces that the serious changes observed in the urine of men at a late period after injuries of the spine, are due to the products of inflammation excited in the coats of the bladder, either by its long-continued distension, or by the irritation of catheters kept in it. He further shows by experiments, that the secretion of semen is not directly affected either in quantity or in composition, by injury or division of the spinal cord.

ANATOMY AND PHYSIOLOGY OF THE NERVOUS CENTRES.‡

Spinal cord. In his description of the spinal cord and its membranes, M. Foville describes the following structures, none of which have, I believe, been hitherto noticed. 1. The existence of a thin external cortical layer surrounding the whole cord, formed by the more intimate approximation of the fibres. It is this which being penetrated by the roots of the nerves makes them appear as if they

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* The paper contains many experiments indirectly bearing on the subject, and notices of the diseases of experimental frogs by which perhaps many failures of researches may be explained. As to the conclusions drawn from it,—it may show that the sympathetic nerves and ganglia will exercise ther wonted influcnee upon nutrition, secretion, &c., long after the separation of their connexion with the cerebro-spinal nervous-centres, but the facts do not prove that the influence of the sympathetic, on the parts in which its nerves are distributed, is different essentially from that exercised by the cerebro spinal system on the parts which it supplies. They afford no evidence that, for example, an influence is exercised on nutrition or secretion, in one part of the body, through sympathetic fibres, more or otherwise than it is exercised in another part, through cerebro-spinal fibres; the connexion, also, which exists between the two systems and the sensations and movements of the parts in which they are severally chiefly distributed, differs in degree, but not, so far as yet is proved, in kind.

† Bull. de l'Acad. de Medecine, Sept. 15-30, 1834; and Medical Times, Sept. and Oct. 1844.

‡ On the comparative weights of the parts of the nervous centres, see M. Bourgery's communication to the Acad. des Sciences 23. Sept. 1844, reported fully in the Gazette Medicale, 5. Oct.; and, for many interesting considerations on their several functions, see Mr Swan's work, "The principal offices of the brain and other centres," London, 8vo. 1844. The principal part of the contents of M. Foville's great work, 'Traite complet de l'Anatomie . . . . . du Systeme nerveux cerebro-spinal,' 1re partie, Paris, 1844, 8vo, were published before this year in his article Eneephale in the Dict. de Med. et de Chir. Pratique, and in the reports on his memoir on the structure of the brain and skull, made to the Paris Academy of Sciences by M. de Blainville in June 1823 and May 1840; and to the Paris Academy of Medecine in the latter year by M. Blandin. I have therefore reported nothing but those views of M. Foville which are, I believe, peculiar to him, and which appeared first in the volume lately published; they relate exclusively to the structure of the spinal cord and the origins of nerves.

arose from a groove.* 2. Cords, rather less white and firmer then the rest of the white substance, extending all down the spinal cord, at the junctions of the central angles of its interior and posterior columns with the margin of the corresponding commissure.† 3. The continuation of the posterior-internal (posterior pyramidal) tract, as low down as the lumber enlargement of the cord.‡ 4. The similar continuation of the thin lateral tract (that which is seen on the medulla oblongata, just in front of the restiform body,) down the whole length of the cord. 5. The arrangement of the longtitudinal fibres of the cord in lamellæ whose edges radiate to and from the central axis of the cord, and which, by their connexion with the gray matter, may be said to be all fixed to the wall of its central ventricle.§ 6. The decussation of the anterior tracts through the whole length of the anterior commissure. 7. The mode of this decussation and of that at and above the anterior pyramids, by the layers or fibres passing not only from side to side, but from before backwards, so that those (for example,) from the right side become, in successive layers, the lateral parts of the grey matter of the left side.|| 8. Prolongations of the pia mater passing in from various parts of the surface of the spitnal cord, and converging to its centre.¶ 9. The commencement of the lumbar enlargement of the cord by an elongated narrow eminence, (like an olivary body,) at the antero-lateral angle of its lower portion.**

Dr. Budge†† has examined the connexion between the spinal nerve-roots and the cord of frogs, and wholly opposes his observations to those of Stilling and Wallich, who he believes have been deceived by using too low microscopic powers, and by not observing the changes of direction of the nerve-filaments at the torn or cut portion of the cord. He traces tbe filaments of the nerve-roots passing straight upwards in the cord, those of the upper roots overlaying those of the lower, and all becoming rather smaller. He believes that there are no longitudinal fibres in the cord which are not continuations of the nerve-fibres. Transverse fibres are found in it, but are obscure; he considers many of them to be fibres connecting [like bridges,] the ganglion globules, or their membranous envelopes; but is doubtful whether some are not independent transverse nerve-fibres. He was unable to trace any of the nerve-fibres to the brain.

[These results are directly opposed to those experiments of Van Deen,‡‡ quoted in the last Report, which seemed to prove that the nervous fibres do not proceed to the brain, but terminate in the spinal cord; for impressions did not appear to pass much, if at all, beyond the part of the cord on which the several nerves irritated were connected with it. The result of some experiments which I made with Dr. Baly upon turtles were as contrary to the conclusions of Van Deen as these examinations by Budge are. All the spinal roots of both sides, in the cervical and superior dorsal regions, were divided so that a portion of the cord six inches long had no nerve connected with its sides. Its connexion with the lower part of the cord being unimpaired, I found that every irritation of the upper end, or any other part of this loose portion of the cord, produced vived movements in the hind limbs and tail. The movements were perhaps rather more vivid when the posterior columns were irritated, so as to produce them by reflex influence, than when they were exited directly by irritation of the anterior columns. I very gently cut slices from the cord [as in Van Deen's experiments,] and at every cut the movements of the hinder parts were produced.]

Dr. Poletti,§§ unaware of the experiments which had already been made by Dr. Engelhardt, has observed the effects produced on the posture of the limbs of frogs, by dividing [not crushing, as in Engelhardt's experiments,] the spinal

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* L. c. pp, 136, 232, &c. † L. c. pp. 284-5. ‡ L. c. p. 283, &c.

§ L. c. p. 291, || L, c. pp. 294-6, 318, 324, &c.

¶ L. c. p. 342. These must be, or contain, the vessels injected by Mr, Smee, and described by Dr. Todd, (Cycl. of Anatomy, art. Nervous System, p. 708.

** L. c. p. 138.

†† Mueller's Archiv. 1844, Heft ii. The examinations appear to have been carefully made; the greater part of the long paper is a detail of the precautions to eusure accuracy.

‡‡ Related in Tijdschrift voor naturliche Geschiedeni en Physiologie, 1842, D. ix.

§§ Il filiatre sebezio, Dec. 1843.

cord in different parts. When it is divided just below the occiput, the limbs are permanently flexed ; the flexion becomes less decided the lower the cord is divided, till the division comes to the level of the space between the fourth and fifth vertebræ. When the cord is divided below this point, the limbs which before were flexed, are somtimes suddenly forcibly extended ; they remain for some time fixed parallel to one another and to the axis of the trunk, and then gradually relax.

*Brain.** An excellent paper on the structure of the cerebellum has been published by Dr. C. Handfield Jones,† in which many parts of Reil's obscure description are explained or rectified. The sum of his account [so far as it may be made intelligible without diagrams,] is as follows: the medullary or fibrous nucleus of the cerebellum has not [as Reil and Foville suppose,] a separate "exterior shell," or "laminated stratum," from which alone the medullary axes or stems of the lobules are derived, but consists throughout of similar medullary plates, which decussate with each other as they pass onwards from the base or junction of the crura, and which are all destined to pass into the lobules. The central medullary stem of each lobe or lobule consists, first and mainly, of the medullary plates which are given off in succession from the exterior of the medullary nucleus of the hemispheres, or the lobe, as the case may be. But, besides these, the stem of each lobe or lobule has 2dly, [what are, probably, a part of that which Foville describes as the white nervous lining of the cortical substance,] commissural plates which pass to and from the lobe or lobule next to it on each side. The first of those sets of medullary plates, which lie in the middle of the stem of each lobule, are not [as Reil supposed them] derived from the *ridge* on which that very lobule stands, and with which the *furrow* at its base forms the *articulation*; but they are those plates which from the highest part of the ridge of the lobule next preceding it. The same may be said of the lobes and their ridges. The ridge below the base of each lobule is thus formed by the elevation of those plates of the stem of the lobe which will compose the greater part of the stem of the next following lobule ; and the furrow at the base of each lobule is bounded at its *proximal* side [*i. e.* the side at which the plates or fibres, if traced from the greater to the less divisions of the cerebellum would first arrive,] by the plates given to its medullary stem from the stem of the lobe, and in its distal side by the commissural plates going from itself to the lobule next beyond it.

The same plan of arrangement is repeated in the construction of the several parts of a lobule. As its medullary stem passes along its centre, it gives off in succession subordinate plates which pass into the middle of each lamina or subordinate lobule ; those which go to the lamina on the *distal* side of the lobule having decussated across the fissure in its axis. And besides these plates which form the axis of the stem of each lamina, each has also, like each lobule, commisural plates by which its medullary stem is connected with those of the laminæ next before and after (or above and below) it. Moreover, as with the lobules, so with laminæ, the plates of the medullary stem of each are not those which from its own ridge, but those which formed the ridge of the lamina next preceding it, and the plates which form its ridge are those which will pass into the central stem of the lamina next beyond it.

ANATOMY AND PHYSIOLOGY OF THE NERVES.

Olfactory nerve. M. Foville‡ maintains that nearly all the deep origins that have ever been assigned by different anatomists to this nerve, do really belong

* An account of numerous careful repetitions of Magendie's and other experiments on the pulse-like movements of the brain and spinal cord has been published by Dr. Alex. Ecker, in his "Physiol. Unters. uber die Bewegungen des Gehirns und Ruckenmarkes; Stuttgart, 8vo., 1844. His conclusions fully confirm those of Magendie ; especially as regards that of the respiratory movements of the brain being mainly due to the ascent and descent of the cerebro-spinal subarachnoid fluid as it is alternately subjected to and freed from the pressure of the vertebral sinuses and other veins, which become alternately full and empty in the acts of expiration and of inspiration.

† London Medical Gazette, March 29, 1844.

‡ Traite, &c, pp. 518, 525.

to it; and that it is attached to the gray matter of the convolutions, the band of the convolution of the corpus callosum, the anterior crura of the fornix, the surface of the locus perforatus anticus, the fibres continued from the posterior tract of the medulla oblongata into this *locus*, the external portion of the gray matter of the corpus striatum, and the fibrous layer which invests it; and, lastly, to the anterior commissure. [But he does not appear to have direct evidence of all these origins in the human subject.]

Optic nerve. M. Foville* rightly describes the connexion between the commissure of the optic nerves and the lamina cinerea as being more intimate than, since it was described by Vicq. d'Azyr, it has been generally considered. He describes the gray matter at this part as the *anterior gray root* [the posterior being that commonly known, from the infundibulum]; it is connected with the anterior pillars of the fornix; it is covered by a very thin white layer which extends from the commissure over the locus perforatus to the superficial layer of the adjacent convolutions; and some of the gray matter passes into the substance of the nerves. He says also that the continuation of the optic tract is not only spread over the whole upper surface of the thalamus and corpora quadrigemina, but is connected with the direct median and external prolongations of the posterior tract of the medulla oblongata with the tænia semicircularis, and by a "little nervous membrane (?)" with the temporal tuberosity of the convolution of the corpus callosum.

Fifth nerve.† The large sensitive portion of this nerve is said by M. Foville‡ to pass obliquely to the outer edge of the restiform body through part of the substance of the middle crus cerebelli. From the posteririor edge of this prolongation of its roots a nervous membrane passes into the nucleus of the cerebellum; and from its anterior edge fibres pass which form some of the transverse arches of the pons, or are sometimes continued into the transverse fasciculi on the floor of the fourth ventricle, where, as well as in the nervous expansion which he supposes to line the cortical substance of the cerebellum, these roots are closely connected with those of the auditory nerve.

The roots of the small motor portion are traceable he thinks,§ to those fibres of the lateral tract of the medulla oblongata which are given from it to the middle crus cerebelli.

Auditory nerve. To this, also, M. Foville‖ assigns an origin not less complex then those of the nerves already mentioned. He says that a fine white membranous nervous tissue is prolonged from its roots, from those of the fifth, and from the surface of the restiform body; which tissue, after surrounding the crus cerebelli [in which it is not difficult to see it], forms a lining (*doublure*) to the whole of the cortical substance of the cerebellum. Another nervous membrane, continuous with the preceding, lines the whole of the walls of the fourth ventricle, and is continued into some of the transverse striæ; and other are combined with the medullary velum and pass to the corpus dentatum. Lastly, other emanations from these roots pass forwards and inwards, and are confounded with the transverse arches of the pons.

Facial nerve. Dr. Hargrave¶ has related a case in which, as in that by M. Diday, mentioned in the last Report, the uvula was drawn to the left side in paralysis of the facial nerve, and recovered its median position when the disease ceased. [Both this case, and one of a similar kind by Dr. Williams,** afford additional evidence for believing that the facial nerve is the motor nerve of the levator palati and azygos uvulæ muscles, and sends its fibres to them throgh the superficial petrosal branch of the vidian nerve and the spheno-palatine ganglion. And this opinion must, I think, be retained concerning the arrangement of these

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* L. c. pp. 182, 510, &c.

† Some facts relating to the branches of the fifth and facial supposed to supply the muscles of the palate, will be found in the following account of the experiments of Dr. Hein.

‡ L. c. p. 507. § L. c. p. 531. ‖ L. c. p. 505, &c.

¶ Dublin Medical Press, Dec. 6, 1843. ** Ibid, March 5, 1844 nerves in men, although apparently contradicted by the experiments of Hein.*] He could never in dogs, calves, and goats, produce contraction of any of the muscles of the palate by irritating the root of the facial nerve, neither was the contraction which he produced by irritating other roots effected by destroying the petrosal nerves going to the spheno-palatine ganglion. He succeeded, indeed, in tracing a branch of the middle posterior palatine nerve (through which the petrous filaments of the facial are supposed to pass after traversing the spheno-palatine ganglion,) into the substance of the tensor palati and azygos uvulæ : but this branch enters the former muscle at its tendinous part, while that of the pneumo-gastric and accessory, which his experiments indicate to be the real motor nerves of these muscles, enters the tensor in its belly.

Nerves of the eighth pair. Among the nerves most examined during the last year are the glosso-pharyngeal, pneumo-gastric, and accessory ; on the offices of which observations have been made lately by Van Kempen,† Krause,‡ Bernard,§ Bischoff,|| and Hein.¶ The chief questions to be determined were— 1st. Wherther each or all of these nerves be composed at their origin of wholly sensitive, or wholly motor, or mixed, fibres ? 2d. What muscles derive their motor nerves from each or either of them ? The present state of the facts is as follows.

1. The *glosso-pharyngeal nerve* is, by general consent, chiefly sensitive ; having filaments for both taste and common sensation. But it is probably, also, a motor nerve. Penizza, John Reid,** and Longet,†† have maintained that it is not ; having failed to discern movements in any muscle when irritating its roots within the skull, and referring the movements produced by irritating it outside the skull to filaments of other nerves mixed with it, or to reflex acts following the impressions conveyed through its centripetal fibres to the medulla oblongata. But other and more certain experiments are in favour of its having a direct motor influence. These include the experiments of Muller,‡‡ Volkmann,§§ and Hein. The last-named, whose experiments were very carefully performed, states that his results completely agree with those of Volkmann. When the roots of the glosso-pharyngeal nerve were irritated in the recently cut off-heads of calves and dogs, after removing the brain and medulla oblongata, and separating these roots from those of the pneumo gastric, contractions always ensued in the stylo-pharyngeus muscle. He believes, also, that the terminal branches of the glosso-pharyngeal give motor filaments to the palato-glossus muscle, although he could not make it contract by stimulating either this or any other nerve ; for glosso-pharyngeal filaments alone can be traced to this muscle, and its not contracting may be due to the disturbance of connexions by the dissections necessary for the experiment.

2. *Pneumo-gastric and accessory nerves.* The pneumo-gastric nerve is, by general consent, chiefly sensitive : but it is questioned whether the motor fibres, which many of its apparent branches contain, are derived directly from some of its own roots, or from those of the accessory, through its internal branch, which unites with the trunk of the pneumo-gastric soon after the formation of the jugular ganglion. The opinion that the roots of the pneumo-gastric are wholly sensitive, and those of the accessory wholly motor, and that the two bear to each

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* Muller's Arch. 1844, heft iii-iv. See also his account of the nerves of the palate, p. 583.

† Essai experim. sur la nature fonctionnelle du nerf pneumogastrique ; Louvain, 1842 8vo; analysed by Bischoff in Muller's Archiv. 1843, heft vi.

‡ Handbuch der menschl. Anatomie, Bd. ii, 1843.

§ Arch. Gen. de Med., Avril et Mai, 1844.

|| Muller's Archiv. 1843, Jahresbericht, p. clv.

¶ Ib. 1844, heft iii, iv, pp 297-358; from an inaugural dissertation, Heidelberg, 1843

** Experimental Inquiry, &c.; Edinburgh Medic. and Surg. Journ., 1838, vol xlix

†† Anat. et Phys. du Systeme Nervoux, t. ii, p. 220

‡‡ Physiologie des Menschen, Band i, p. 630, Ed. i.

§§ Ueber die mortor.-wirkungen der Kopf- und Halsnerven. Muller's Arch. 1840 p. 485.

other the same relation as the anterior and posterior roots of the spinal nerves, has been especially maintained by Valentin* Longet,† and Morganti,‡ following in the steps of Scarpa, Arnold, and Bischoff. But the experiments of all others, and especially those of more recent date, though they agree in few things besides, are all opposed to this opinion. Among these are the experiments of Müller,§ Volkmann,‖ John Reid,¶ Stilling,** Van Kempen,†† Bernard,‡‡ and Hein,‖‖ of which those of the last four must be included in the history of this year.

Stilling's experiments tend to show—1st, That irritation of the roots of the pneumo-gastric produces movements in the pharynx, glottis, stomach, (and heart): 2d, That irritation of those of the accessory nerve produces no movement in any of these parts: and 3dly, That irritation of the trunk of the pneumo-gastric, and of the superior and inferior laryngeal nerves, produces movements in the pharynx. He concludes from these and others that the fibres of the superior laryngeal nerve are (as to the larynx) wholly centripetal; those of the inferior laryngeal mixed, giving centripetal fibres to the trachea, and motor fibres to all the muscles of the larynx; and that the fibres of the internal branch of the accessory which joins the vagus are all centripetal.

Van Kempen's experiments on the roots of the pneumo-gastric and accessory agree with Stilling's, in showing that in the roots of the pneumo gastric there are both sensitive and motor fibres; and that the accessory nerves give motor fibres to no muscles besides the sternomastoid and the trapezius. He concludes also that the pneumo-gastric nerve is the motor nerve of the constrictors of the pharynx, the palato-pharyngeus, the œsophagus, and the interior muscles of the larynx; and that irritation of its roots, as well as of those of the accessory, is wholly without influence on the movements of either the heart or the stomach. But he differs from Stilling, and agrees with all others, in holding that the superior laryngeal contains a few motor fibres, and that with these it supplies the cri-thyroideus muscle.

On the other hand, many others are agreed, against Van Kempen, that irritation of the pneumo-gastric does produce movements of the stomach, at least while digestion is going on; and in affirmation of this, Longet's experiments appear conclusive.¶¶ And, as to the influence of the pneumo-gastric on the muscles of the palate, both Hein's experiments, presently to be mentioned, and Bischoff's,*** agree that in domestic animals irritation of its roots produces movements not only of the palato-pharyngeus, but also of the levator palati and azygos uvulæ.

The experiments of M. Claude Bernard, on the roots of the pneumo-gastric and accessory, were made by seizing the accessory at its exit from the skull, and plucking it out by its roots.

This mode of experimenting has the advantage of not rapidly destroying life; the animals lived for many days or weeks, till they were killed for the sake of the autopsy. But it is, even more then the others, subject to fallacy, in consequence of the frequent mingling of the fibres of the pneumo-grastric and accessory, before they leave the skull, as well, perhaps, as in consequence of the injury that may be done to the medulla oblongata.

The results of these experiments were—1st, That all the roots of the accessory are insensible, except those which arise from the medulla oblongata; these appear to have "a certain dose" of sensibility, and to have the more the nearer

* De functionibus nervorum, p. 45 † L. c. t. ii, p. 262, e. s.

‡ See last Report, and Archives Gen. de Med. Nov. 1843, where his experiment are fully detailed. § Phisiologie, Band i, p. 641. ‖ L. c. ¶ L. c.

** Haeser's Arch. Heft iii, 1842; and Report of the Scientific Meeting at Brunswick in Muellers Arch. 1843, heft vi; Bischoffs Jahresbericht, p. cliv.

†† L. c. †† L. c. ‡‡ L. v.

‖‖ Bidder, in Muellers Archiv. heft iv, 1844.

¶¶ L. c. p. 322. They are lately confirmed by Bischoff, Muellers Arch. 1843, Jhrbr. p. clv. and by Stilling and Valentin, as far as the FACT, but not the explanation, of the movements is concerned. *** L. c.

they are to the origin of the pneumo-gastric. The external branch of the nerve, which, with Morganti and others, he considers to be formed exclusively from its lower roots, is wholly insensible, till it has mingled with filaments of the cervical nerves. 2d. It is only from the upper roots of the accessory nerve that its laryngeal filaments are derived; when they are destroyed the voice is completely lost; when they remain the voice is not effected by the destruction of all the lower roots. 3d. The effects of the complete destruction of the roots of the accessory on both sides are, complete loss of voice, some difficulty of deglutition, shortness and interruption of expiration when the animal tries to cry out, panting during great movements and efforts, and sometimes irregularity of gait: but when the animal is at rest, respiration, circulation, and digestion are performéd as perfectly as in health. 4th. It is hence evident that the accessory nerves do not furnish all the motor filaments to those parts to which the branches of the pneumo-gastric nerves are distributed, and it is made probable that they furnish very few, if any, filaments to the organs below the upper part of the œsophagus.

[In Longet's experiments of galvanising the trunk of the accessory, no movements of the stomach, trachea, bronchi, or heart were ever seen; and M. Bernard justly obsêrves, that it is hardly imaginable that so small a branch as the internal branch of the accessory should supply filaments to be distributed in all the parts to which motor influences are conveyed through the branches of the pneumo-gastric.]

5th. It is, further, made nearly certain that the accessory nerves do not supply all the motor filaments of the larynx and pharynx. For, as regards the larynx, the effects of the division or destruction of the accessory nerves, are very different from those of dividing the trunks of the pneumo-gastric nerves below their junction with the internal branches of the accessory. The latter operation completely paralyses the muscles of the larynx, and young animals die at once suffocated by the closure of the glottis; by the former, the voice is destroyed, but quiet respiration is performed with ease, and when the larynx is exposed, the glottis, though it remains widely open and is nearly immoveable, yet moves as much as it does in ordinary quiet respiration, and the animals life is hardly endangered. 6th. It is not experimentally determined what largyngeal muscles, besides the cricothyroid, in which paralysis was proved, are paralysed by destroying the accessory; but the power of closing the glottis is lost, and vocalization is impossible. Consequently, since the power of keeping the glottis open against the influence of the atmospheric pressure in respiration, is not lost, (as it is when the pneumogastric are divided,) it is presumable that the arytenoid, the thyro-arytenoid, and the lateral crico-arytenoid are also more or less paralysed, while the posterior crico-arytenoid retain their power.* 7th. With regard to the movements of the pharynx, the animals which had lost their accessory nerves, after eating, perhaps, rapidly for a time, were obliged to eat slowly, stopping and raising the head at each effort of deglutition; and if they were hurried, portions of food often passed into the larynx. [This may most probably be ascribed to the partial paralysis of the muscles of the pharynx, but Mr. Bernard supposes that as the larynx has, in his opinion, both a vocal and a respiratory motor nerve, so also the pharynx has some branches from the pneumo-gastric for the simple movements of deglutition, and some from the accessory nerves for those movements by which in deglutition the glottis is closed. The conclusion is improbable, and the experiments show that there were difficulties in deglutition, independent of the non-closure of the glottis.]

* M. Bernard strangely misinterprets his facts to support an hypothesis the that accessory nerves are the special vocal, and the pneumo-gastric the respiratory nerves of the larynx. But coughing is no vocal act; yet it is impossible when the accessory nerves alone are devided, because the closure of the clottis is not possible. M. Bernard says nothing difinite about coughing, or any similar complex respiratory act; but no doubt the interrupted efforts of expiration of which he speaks were attempts at coughing, not recognised be cause the glottis was not closed and there was no explosive noise

8th. By the division of the external branch alone of the accessory nerves no effect was produced on the voice or deglutition, but the animal was put into the condition of a *short-breathed* person; its cries became short and interrupted; it was quickly *blown* when it made violent muscular efforts, and when the respiration was hurried; there was sometimes imperfection in the movements of the fore limbs. Of this effects the shortness of the cry is ascribed, by M. Bernard, to the loss of power in the sterno-mastoid and trapesius muscles, by the contraction of which the descent of the sternum and upper ribs is for a time hindered in healthy vocal expiration. In like manner, the paralysis of these muscles (and of others dependent for fixed points on parts to which they are attached) hinders the fixing of the walls of the chest which is necessary for efforts. 9th. But the paralysis is not complete. M. Bernard relates an experiment in which, after division of the external branches of the right accessory nerve, both sterno-mastoid muscles contracted with equal force in forcible respiration, but the left alone contracted when the animal cried out.*

Hein's experiments relate almost exclusively to the nerves of the palate. They consisted in irritating the roots of the several nerves within the skull of an animal just beheaded, after removing the brain, so that there was very little danger of a part of one root being mistaken for another. His conclusions regarding the motor fibres of the glosso-pharyngeal are already mentioned. As to those of the pneumo-gastric and accessory nerves, they tend to show that these nerves send branches, through the medium of the pharyngeal branches of the glosso-pharyngeal, to the levator palati, azygos uvulæ, and palato-pharyngeus. These branches, near their distribution, are almost too minute for dissection; their existence was proved chiefly by the results of the experiments, which at last guided to the detection of them running just under the mucous membrane to the muscles. But he does not pretend to distinguish between the effects (on the muscles of the palate,) of irritating the pneumo-gastric and the accessory; he believes that their fibres are from the roots mingled, and that they both influence all the muscles supplied from the branches of either.

[From all these facts the first conclusion must be, I think, that the precise functions of these nerves are not yet determined. The most probable account may be—

1. That the glosso-pharyngeal is chiefly the nerve of the sense of taste, and, in less a degree, a nerve of common sensation.

2. That the glosso-pharyngeal is, according to the experiments of Müller and Hein, the motor nerve of the stylo-pharyngeus, and probably, also, of the palato-glossus. The branches which it gives to the digastricus, stylo-hyoideus, and constrictors of the pharynx, appear, according to the experiments, to be sensitive, or else derived from the facial and accessory nerves, with which it has previously united.

3. The pneumogastric is, from its origin, composed of both sensitive and motor fibres. But it cannot be decided at present, whether it alone supplies any particular muscles; or whether, in all its muscular branches, and especially in those given off above the œsophageal, there are filaments from the accessory as well as from its own roots.

4. The accessory nerve contains in all its lower roots motor fibres alone; in its upper roots it is not improbable that there are some sensitive fibres also. It is a motor nerve of the sterno-mastoid and trapezius muscles; and it is very probable that it gives, by its internal branch and other communications, motor fibres to the pneumo-gastric, from which they are subsequently distributed to

* Hence he supposes that these muscles, and the trapezii, like those of the larynx, have a vocal and a common respiratory function, and a separate nerve for each; so that while they act as common inspiratory muscles under the influence of the cervical nerves, they arrest the respiratory acts when they are under the influence of the accessory nerves. He would, therefore, call these nerves the antagonist rather than the accessory nerves of respiration.

some or all of the muscles of the larynx,* and pharynx ; and, in some animals to muscles of the palate.

It is evident that the chief difficulty is in distinguishing the functions of those roots which may be either the uppermost roots of the accessory, or the lowest of the pneumo-gastric. In the last Report an observation by Mr. Spence of a root which he considered to be the motor root of the pneumo-gastric was mentioned. That root had its origin midway between the pneumo-gastric and the accessory roots ; and both Krause and Hein agree that it is not unfrequent to find that several of the upper roots of the accessory (as the are usually described,) are united to and intercharge filaments at the jugular ganglion of the pneumo-gastric. In a preparation which I lately made, several of this minute roots are commingled with roots of the pneumogastric, even before the ganglion is formed on it. It is therefore difficult to say whether these are roots of the pneumo-gastric or of the accessory ; and the more difficult, because the upper roots of the accessory arise more and more nearly from the posterior columns of the medulla oblongata ; and, moreover, whatever these roots may be, such communications as I have observed might effect an exchange of filaments between the two nerves before their trunks are formed. It is probable, also, that there are differences in these points in different species, and in different individuals of each ; so that before we can hope to distinguish precisely the physiological properties of these two nerves, we must learn to distinguish them (if indeed they are two nerves,) anatomically.]

Inferior laryngeal nerve. Mr. Jackson† has recorded a case of aneurism of the distal two-thirds of the arch of the aorta, in which the left inferior laryngeal nerve was involved, and a firm manly voice was changed to a squeaking whisper. Severe paroxysms of dyspnœa ensued on exertion, and sometimes during swallowing—signs referrable to paralysis of all the muscles of the left side of the larynx, except the crico-thyroidei and thyro-hyoidei, by the unopposed action of which the vocal ligament was kept unnaturally tense, while in violent inpiration the arytenoid cartilage was not hindered from being, as in Dr. J. Reid's experiments, forced down like a valve over the glottis.

Professor Barkow‡ (Breslau) describes a small ganglion, scarcely a line in diameter, on that terminal branch of the inferior laryngeal nerve, which after ascending over the posterior surface of the cricoid cartilage lies upon or among the fibres of the posterior arytenoid muscle. He calls it the arytenoid ganglion ulncr nerve.

Dr. Poletti§ find that another branch of the deep palmar branch of the ulnar nerve, constantly exists in addition to those described by former anatomists. It is as large as the filaments given to the lumbricales muscles, and arises from the concavity of the arch which the nerve forms, opposite the lower margin of the os unciforme, or a little further on. After giving off a few secondary branches, when it is opposite the articulation, between the os unciforme and os magnum, it passes through the interosseous ligament, and goes to be distributed in the synovial membrane of the metacarpal joints.

ORGANS OF SPECIAL SENSES.

Eye. M. Maunoir,‖ of Geneva, to support the view which he has so long maintained of the existence of the sphincter muscle around the pupil, and the radiated muscle in the rest of the iris, states that galvanism excites contraction of both

* The arrangement of its internal branch internal in the chimpanzé, discovered by W Vrolik, (see last Report,) in very imporrant evidence for this view ; and Bernard's experiments are not less so, for in them he could not destroy much more than the roots of the accessory, though he may sometimes have left some of them uninjured.

† Medical Gazette, Dec. 22, 1843.

‡ Report from the Academie des Sciences, Seance du 26 Aout,, in the Gaz Medicale, 31 Aout 1844.

§ Il filiatae Sebezio, Dec. 1843. It is probably the same which Valentin describes as "filaments to the articulation." (Sommering's Anatomie, Fr. edit. iv, p. 517)

‖ Report from the Acad. des Sciences, 19 Fevr. 1844 in the Gaz. Medicale, 24 Fev.

these muscles, and that in a recent case of accident he had an opportunity of observing their antogonist action. A man received a small penetrating wound through the lower part of the cornea, and a point just higher up in the iris. When the cornea healed, a triangular false pupil was found at the wound of the iris. Both this and the natural pupil could contract and dilate; but, on the contact of light, the false pupil dilated and enlarged when the natural one contracted, and the false pupil contracted when, in the absence of light, the natural one was dilated. [The result of a number of observations in cases of artificial pupil, wounds of the iris, &c., by E. H. Weber,* went to prove, that the movements which occured could not be explained by supposing that the iris has circular or radiated fibres, or both, but only by believing that its irritable part consists of fibres variously woven together without any definite derection.

A good discussion of this subject has been published by Dr. C. R. Hall.† He considers that the iris has a circular muscle for the contraction of the pupil; that the dilatation of the pupil depends, probably, on the cellular tissue or the blood-vessels of the iris having an unusual vital contractility; and that the only effect of the elasticity of the tissues of the iris is to accommodate it to changes of size, and to restore it from extremes to a medium state—i. e. to the size which it usually has after death.

Valentin ‡ has found, in two rabbits, that four months after the extraction of the lens, a new body has been produced smaller than it, yet presenting to microscopic examination, the peculiar cells and fibres of the natural lens in stages of development similar to those through which they naturally pass. These were mixed, however, with minute granules; their arrangement was not quite regular in all parts: and in both cases there was at the lower part of the new lens, opposite the injured part of the capsule, a yellow turbid portion formed of a finely granular substance quite different from the lens-subtance. In both cases the capsule of the lens was transparent, without any trace of blood-vessels. The appearences indicated that the new lens was formed from cytoblastematous substance oozing into the emptied capsule, and that thus its harder central parts were those which had been longest engaged in development, and were most perfect. Ernst Brüke § has pointed out an ingenious mode of demonstrating the structure of the vitreous humour, derived from the fact that when two solutions which will precipitate each other, permeate, or are imbibed in, membranous septa, the precipitate first forms in and upon the membrane. By placing the vitreous humour of an eye cut vertically and transversely behind the lens in a concentrated solution of acetate of lead; he found its surface soon covered with a white substance, and when, some hours after, a portion was cut off from the posterior part, the section was seen marked by fine milk-white streaks, which were edges of layers of which the outermost lay parallel to the retina, the innermost parallel to the posterior surface of the lens. The distances between the streaks were greatest in the axis of the eye, and became the less towards the zonula, where they were about $\frac{1}{250}$ of an inch apart. The margin of the layer appeared to be connected with the hyaloid membrane near the zonula, but in what way could not be determined. The vitreous bodies thus treated with lead separate more easily in the direction of the layers than in any other.

Eyelids: Dr. Zeis ‖ has found that the eyelids of young animals born blind (and probably, also, those of the human fetus in utero,) are held together by a thin layer of gelatinous substance, which is interposed between their adjacent margins, and bleeds when torn across very soon after birth. Every day after birth this layer (displayed by vertical sections through the eyelids,) becomes

* Hildebrandt's Anatemie, Bd. iv, p. 80. † Edinb. Med. & Surg. Journ. July 1844.
‡ Oesterr. medic. Wochenschrift, Feb. 10, '44; Henle & Pfeuffers Zeitschr. Bd. 1, h. 2.
§ Mueller's Archiv. 1843, heft iv, p. 344. Pappenheim [Specielle Gewebelehre des Anges, 1842, p. 182,) said that the vitrious humour, when treated by carbonate of potash "may be stripped in concentric layers almost like a bulb," but did not demonstrate any of these layers of membrane. ‖ Walther & Ammon's Journ. fur Chirurgie, bd. ii, st. ii.

thinner; at the same time, the fissures between the eyelids grows deeper, making progress from before backwards, as if by the gradual eversion of the skin of of their adjacent margins; and after the first day, vessels are not detected in the intermediate substance by either bleeding or minute injections.

Lachrymal Gland Mr. Gosselin,* after numerous attempts to inject mercury into the lachrymal ducts, has made himself nearly sure that there are only two ducts which go to the principal mass of the gland, and that the other six or eight adjacent apertures in the conjunctiva are the orifices of ducts belonging to as many isolated small glands, which form what some have described under the name of the palpebral portion of the gland. Some only of these smaller glands appear to have ducts which open separately; the ducts of others open into one of the larger ducts. By the existence of these separate portions, M. Gosselin explains the fact that after removal of what was supposed to be the whole lachrymal gland, some secretion of tears has still continued, and the eye has not lost its brilliancy; tears being secreted by the remaining small glands.

Tongue. Mayer † has published many brief notes on the comparative anatomy of the tongue, from which he deduces that papilla exists on it, not in mammalia alone, but variously developed in birds, amphibia, and fish; and that the varieties of form which the papillæ present are due to the various arrangements of the same elementary clavate or fungiform shape. He thinks that all the papillæ in the human tongue may be organs of taste; and that those which are so chiefly are the smaller ones most thinly invested by epithelium. He says the hypoglossal nerve has often in man, and generally in horses, from one to three ganglia; and that in *many* mammalia it has a posterior ganglionic root.

STRUCTURE AND FUNCTIONS OF THE GENITAL ORGANS.

Male genital organs. In an excellent treatise by Dr. Kobelt, ‡ the following descriptions are chiefly worthy of notice. 1. Before uniting with the other, each crus penis enlarges into a bulb-like swelling, the *bulbus corporis cavernosi penis.* The arteria profunda penis sends a special branch into this bulb; in which also, the divisions of this branch diverge, one backwards to the apex of the crus penis, the other forwards to anastomose with the trunk of the A. profunda penis. 2. The A. profundæ of the two sides form an anastomotic arch near the junction of the crura penis, and it is from the convexity of this arch that the A. corporis cavernosi of each side is given off. 3. Among the veins of the corpus cavernosum, several ascend from the groove between it and the anterior part of the C. spongiosum to the V. dorsalis penis; and others of the same kind, coming out from the groove nearer the pubes, ascend to the abdominal cutaneous veins. 4. The posterior part of the bulb of the C. spongiosum urethræ exhibits, more or less distinctly, two lateral hemispheres, (the representatives of the completely divided bulb of the marsupials,) which are separated by a narrow fibrous partition gradually ceasing as it proceeds forwards. And above and between these in the middle line it has a third lobe (*coliculus bulbi*), through which pass the membranous part of the urethra, the vessels of the bulb, Cowper's gland-ducts, &c. 5. The erectile tissue is continued from the bulb, in a less developed form, round the membranous and prostatic parts of the urethra into the neck of the bladder. It is well developed in the caput gallinaginis, which probably serves, in its erect state, to prevent the reflux of semen into the bladder. 6. The artery which runs forward in the upper part of the corpus spongiosum to the glands is usually a branch of the pudic, not of the artery of the bulb. 7. Among the many veins proceeding from the C. spongiosum are a set which pass in two rows through its upper part, emerge from the groove between it and the C. cavernosum, run downwards and unite in a plexus with scrotal veins, from which their blood is carried to the inguinal cutaneous

* Arch. Gen. de Med, Octobre 1843.

† Ueber die Zunge als Geschmacksorgan in the Acad. Nat. Curios. vol. xx. p 723

‡ Die maenl. und weibl. Wohlustorgane des Menschen, &c., Freiburg, 4to 1844.

and other veins. 8. The fibres of the M. bulbo-cavernosus are in three sets. The superficial posterior three-fourths of them end in a dendinous lamella which insinuates itself between the crus penis and bulb, and then passing over the latter, unites with its fellow; so that this part of the muscle surrounds the bulb, and will compress it from below upwards, and from behind forwards. The anterior superficial fibres have a similar thin tendinous layer which winds over the side of the C. cavernosum, and unites with its fellow over the dorsal vessels and nerves; and the fibres of this division will compress not only the anterior part of the bulb, but the body of the penis and its dorsal vessels. The third or deep set of fibres immediately cover and surround the hemispheres of the bulb, like a muscular hood, and form their proper compressor. 9. The M. bulbo-carvernosi serve especially to maintain and increase the turgescence of the glans both by retarding the current in its veins and accelerating that in its arteries. The arrangement just described enables their contracting fibres to compress all the veins returning blood from the glans. There is also a peculiar excito-motor connexion between them and the nerves of the glans, *provided* the latter be first made irritable by a partial turgescence of the vessels of the glans. If, in a dog just strangled, the glands be irritated, the bulbo-cavernosi twitch responsively, and sometimes make a rhythmical succession of contractions; and in each contraction the blood is forced from the vessels of the bulb in a rapid jet through those of the corpus spongiosum into those of the glans, which is thus each time made tense and glistening. A similar contraction of the muscles may be felt in the perineum of the dog during copulation. By the increased turgescence of the glans, the subjective sensation of its nerves is in turn increased, and thus the sensitive glans and the irritable muscular apparatus of the bulb are mutual and alternate excitors of each other. 10. The ischio-cavernosus is a hollow or bivalve-shell-shaped muscle, which covers the whole free surface of the crus penis and its bulb; its contraction, by compressing all the blood within the crus, increases and completes the turgescence of the C. cavernosum. These muscles also, contracted with the bulbo-cavernosi as often as the turgid glans of the strangled dog was irritated; and probably in all cases the two pairs of muscles act together to increase the turgescence of the whole penis.

Female genital organs. In the female organs, Dr. Kobelt shows the exact analogy of the glans clitoridis to the glans penis, in more respects than are usually considered. As the analogue of the bulb he demonstrates a leech-shaped venous plexiform mass attached on each side to the ramus of the pubes, and covered by the (so-called) constrictor vaginæ muscle, the analogue of the bulbo-cavernosus; and for the analogue of the corpus cavernosum, he describes a venous plexus (*pars intermedia*) connecting the veins of each part of this bipartite female bulb with the veins of the glans clitoridis. But for the full account of these and for some more interesting matter, the book itself must be referred to.

Uterus. Dr. Pappenheim* has published a very minute description of the arrangement of the muscular fibres of the uterus, of which it is impossible to give a short intelligible account. For this reason alone it is that I thus pass over both it and the elaborate 'Contribution to the Physiology of the Human Ovary,' by Dr. Ritchie,† as well as the essays on the same subject by Dr. Knox.‡

Gestation. It is stated to be the result of researches (but no details are given) by M. Berthold,§ that after healthy gestation, delivery takes place at the time when the tenth menstruation from the time of conception should occur; in other words, when the ovary is preparing for the tenth menstruation. And hence, according as the periods of menstruation are in each case at all more or less than the ordinary time of four weeks apart, so in each case will the period of gestation be in the same proportion longer or shorter than usual. Consequent-

* Vorlæuf, Meittheil. ueber den Vorlauf der Muskelfasern in der schwangern menschl. Gebarmutter in Roser and Wunderlich's Archiv, and Schmidt's Jahrbucher Mai 1844
† Medical Gazette, Dec. 22, 1843, to June 14, 1844 ‡ Ibid
§ Report of the Acad. des Sciences, Seance du 27 Mai. Arch. Gen. de med. Juin 1844

ly, the most probable estimate of the duration of gestation would be the number of days that elapsed during the last preceding ten menstruations.

PHYSIOLOGY OF THE OVUM AND EMBRYO.

Separation, transit, and impregnation of the ovum. In further evidence of the separation of the mature ova in mammalia independent of impregnation or copulation, M. Bischoff* has added to his observations recorded in the last Report, of some which are, perhaps, yet more convincing. For example, a ewe, which had never copulated, was killed twenty-four hours after the beginning of heat, and Bischoff found a Graafian vesicle ruptured in the right ovary, and the ovum in the oviduct nearly half an inch from its ovarian orifice. He cut out the left ovary of a bitch that was in heat, but had not yet copulated, and found Graafian vesicles unopened in it: five days afterwards, no copulation having been permitted, he examined the right ovary, and found four well marked corporea lutea and the four ova far advanced in the oviduct. Similar observations were also made on a sow, and on a rat.

Dr. Zeigler† has proved that the rut of the roe is in July or August. At this time also he has found ova in the tubes; they are at the beginning of the tube towards the end of August, and at the end of it about the beginning of November. In December they pass into the uterus; on the 16th he found them as vesicles from ½ to 1½ lines in diameter; on the 26th the diameter was six lines. The smallest embryos were found in the beginning of January, ¾ths of an inch long; in February, 2 inches; in March 6 inches long. The whole period of gestation is 40 weeks, of which three months are thus spent in the passage through the tubes. It is only from April to November that spermatozoa are found in the bucks.

Some observations by Professor Czermak‡ on the development of the black salamander (salamandra atra,) have interest in regard to human physiology, and especially among them these three; 1. After impregnation a certain number of ova are found in the posterior portion of each oviduct, of which one on each side is developed; but while this development is going on, a certain number of ovules grow and are developed in the ovaries; and these, after the discharge of the two young salamanders formed from the first set, pass in their turns into the oviducts, and, without a second impregnation, are developed into a second pair of young ones. Thus there is what the author calls *an incomplete super-fecundation;* a single impregnation causes two sets of ovules to be in distinct periods developed. 2. Of the ovules which pass in each period into the oviducts (or uterine horns,) although all are at first alike, yet only that one which lies on each side nearest to the cloaca is developed into an embryo; the others (*embryonotrophic ovules*), together with the albuminous envelopes which they acquire in their passage into the uterus, are fused into a pulpy, dirty-yellowish, and ropy substance, in which the elements of their yelks may be descerned. And it is on this substance that the embryo, after consuming the yelk of its own vesicle, is supported. So long as the embryo is retained in its own membranes its intestine (a simple straight pouch) is pretty full of its own yelk; but after a time it makes its way out of its membranes, and its stomach is now found distended by the yelk-substance which it has swallowed, while the greater part of the intestinal canal is empty. 3. By the time that nearly the whole of the yelk substance in which the embryo was immersed has been consumed by it, the branchiæ have obtained the highest stage of development; and the author believes that they now serve for the absorption of a nutritive fluid, secreted from the

* Beweis der von der Begattung unabhangigen periodischen Reifung und Loslosung der Eier; Giessen' 1844. Translated in the Annales des Scienccs Natnrelles, Aout 1844; and by Mr. Henry Smith, in the Medical Gazette, Jan 3, 17, &c. 1845

† Beobachtungen ueber die Bremst und den Embryo der Rehe, Hannov. 1843. Abstract in Mueller's Archiv. 1844, Jahrbesricht by Bischoff, who generally confirms the account.

‡ Oesterreschische Medicin. Jahrbucher, Oct 1843

closely adjacent vessels of the uterus, which are developed into a long-meshed network.

Development in general. MM. Provost and Lebert ‡ have published sundry observations on the development of the ovum of the frog, with the view, chiefly of illustrating the development of the organs of the circulation in the batrachians and birds. In their account of the contents of the ovum before and soon after impregnation, they accord with Reichert, but their account is more circumstantial. They describe the ciliated surface of the young tadpole, and the ciliæ as not set on any peculiar epithelium-cells; the pigment-cells, formed from primary-cells, (the "smaller predisposed vitelline-cells" of Reichbert, the "organo-plastic globules" of Prevost and Lebert,) of which some, in the pigmentum nigrum, retain their form, and others become star-like, and form canal.communications; the development of muscular fibres from similar cells elongating themselves in groups, and having their granules developed into muscular filaments; and the development of the chorda dorsalis. In regard to the development of the blood corpuscles they confirm the account of Baumgärter, Schultz, Reichert, and Bischoff, that in the frog, they are formed, like the tissues, out of the smaller vitelline-cells, which gradually pass from the round to the oval form have their contained granules resolved, and acquire the red colouring matter. The development of the heart is described as by Reichert and others; and in the first formation of blood-vessels, they believe that the cells and other constituents of the tissue in which they are to be formed separate so as to leave the spaces which are to be occupied by the vessels. The capillaries, they add, are never formed independently of the general circulation, but always centrifugally under the influence of the circulation, by arches passing from a minute artery to a corresponding vein.

Development of the tissues. Of bone. Bidder* has given a minute account of the development of bone, from examinations of the long bones of kittens just born. In vertical sections, he finds just below the synovial membrane on the head of the bone, six or eight layers of the usual flattened cartilage-corpuscles, below these is a layer of cells of irregular shape and size, irregularly and thinly scattered in the intercellular substance, with clear, dark outlines and finely granular contours; nearer the ossified part the corpuscles are almost all oblong, larger, proportionally, more numerous, and they have more distinct granules, which in some have assumed the form of distinct nuclei. After these, in the layer immediately adjacent to the bone, the corpuscles, as Miescher showed, are arranged longitudinally in rows vertical to the ossified surface; they are oblong, with their long axes transverse to the direction of the rows, and in the several rows are very closely set. The smallest of them retain their finely-granular grumous contents, and appear opaque and jagged, or crenate at their margins; others, and these always the larger, are clear and contain bright nuclei. The last layer of cartilage next the bone is further distinguished. The rows of corpuscles are less regular. (The nuclei and the contents of the cells are so alterable that they must be examined directly after death, especially in young animals.) The corpuscles, as in the previous row, may be divided into two kinds, and they occupy three-fourths of the whole substance. Some are large cells with one, two, or three clear distinct nuclei, and are either oblong or tolerably round or angular, with, sometimes, a double contour. Their nuclei also contain from one to four nucleus-corpuscles. The cells of this kind are formed by the gradual fusion of two, four, or more of the clear corpuscles last described, which when they first coalesce, form dentated

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* Annales des Sciences Nat. Avril. 1845. Possibly there is more of novelty in this paper than I have stated, but the account given is obscure, and is made the more so by the adoption of new names, and the avoidance of any referrence to those by which others have been called the same things. In the following number of the Annales is a paper of the same kind, on the development of the chick. See also the remarks by Vogt on the former paper in the same Journal. July 1844

* Mueller's Archiv, Heft iv-v, 1843

dark corpuscles with granular contents, but granular contents, but gradually clear up, become rounder, obtain the distinct double contonr, and the one or more nuclei; that is from the conflux of several cartilage cavities, there are formed complete cells with nuclei and nucleoli. Others of the corpuscles at this part have opaque granular contents and irregularly crenated edges, and agree pretty nearly with the crenat-cells before mentioned; and these may sometimes appear to be inclosed in cells of the same kind as the clear nucleated ones last described; but they are certainly distinct and independant.

In the ossified part it is clear that the ossification first makes progess in the intercellular substance, between the vertical rows ef cells, and then passes transversely between them, so as to form cancelli, each of which incloses one or more of the true enlarged and nucleated cartilage-corpuscles; while the dentated corpuscles are themselves inclosed in the deposits and impregnated with the calcareous matter. (In different specimens, however, the degrees of regularity of the arrangement of these cartilage-corpuscles are very uncertain, and many must be examined to afford the complete view.)

It thus appears that of the original cartilage-corpuscles, some are developed and some retrograde. The former increase to three times their original size, at first by growth, and at last by coalescence; their walls, at first not distinguishable from the intercellular substance, become thicker and more distinct, crenated, and then round with a double contour; their contents, at first clear, become turbid and granular, and then again clear, with distinct nuclei and nucleoli; they they arrange themselves in regular rows, and seem to grow at the expense of the intercellular substance.

The latter (retrograding) corpuscles are those which do not attain a higher grade than that of the dentated and crenated corpuscles; they are smaller than the former, their dark granular contents give no indication of the formation of new cells, they do not grow, and at last are ossified together with the intercellular substance.

The intercellular substance does not in these changes remain unaltered. Before ossification, it becomes laminated in the large-celled layer of cartilage immediately adjacent to the ossified part; and this laminar arrangement is the forerunner of the osseous lamellæ, which in a later stage surround the vasular (Haversian) canals. These vascular canals are unquestionably produced by the fusion of the cancelli formed from the more perfect cells: they are not derived from canals previously formed in the cartillage, but result from the absorption of the partitions between cancelli, of which some lay in longitndinal, others in oblique or transverse rows. The canals in cartilage, from which those of bone were supposed to be formed, are far from constant in ossifying cartilage, even when ossification has made much progress, and when they do exist they scarcely ever reach so far as the ossifying part, but stop short near its surface.

The other kind of corpuscles, the small dentated ones, become the bone corpuscles as earthy matter is deposited all around them; and the author inclines to Henle's view, that the *canals of the corpuscles* are formed as *intereellular spaces* in substance deposited in laminæ within the corpuscles. Bnt in a note appended to the paper, Müller says that in enchondroma he has often seen the transition from the nucleus of tho distinct cell into the toothed and branched nucleus. So that, as Bidder himself admits, this part of the subject must be still very doubtful.

Development of arteries. H. Rathke* has published a very complete account of the development of the arteries which proceed from the arch of the aorta in mammalia and man. His examinations were made on embryos of pigs, sheep, and oxen.

When the pharyngeal (or branchial) fissures are still open, the heart lies behind and below them; its single ventricle is continued anteriorly into a single canal, which corresponds with the trunk of the branchial arteries of fish, but

* Mueller's Archiv, Heft iii-iv, 1843

has no bulb; this, as it passes forwards, gives off or divides into five arterial arches on each side, of which four pass between the pharyngeal fissures, and the fifth passes behind the last fissure. Above the fissures of all these arches unite into one aorta, which thus, as in fish, arises with two divided roots from each side, and is continued down the vertebral column.

When the ventricular septum has formed, the single trunk proceeding from the originally single ventricle also divides into two trunks, partitions continuous with that of the ventricle growing from opposite parts of its walls, and uniting within it. Of these two trunks, the right passes at its further end rather behind the left; and when the partition between them is completed, the left trunk appears, as the ascending aorta, the right as the trunk of the pulmonary artery, which, enlarging at its lower (or proximal) part, forms the *conus arteriosus*, and here constitutes a part of the heart itself. No similar enlargement of the aorta, ever takes place.

But before these changes are finished, it is fonnd that of the five arterial arches on each side, the anterior pair (the most distant from the heart) disappears almost entirely: but the proximal portion of each of these arches, from which there is already given off a branch to the brain, remains; and the remnant of the trunk, together with this branch, now disappears like a branch from the second arch. A little later the second pair of arches also disappears, their proximal portion only remaining. And the same is soon repeated with the third pair of arches. Thus, there now remain only the fourth and fifth pair of arches, and a vessel formed on each side out of parts of the first three pairs, which looks like a branch given off from the enlarged and now more divergent fourth pair. This vessel is the common carotid; it is S-shaped, having a proximal curve turned towards the mouth, and a distal curve directed backwards; a branch is given off from it near its origin, and passes to the anterior fissure, where the ear is formed. This branch is the facial (external) carotid; the continued trunk, including the branch before mentioned as given off from the first arch, is the cerebral (or internal) carotid.

All the fissures, except the anterior, are now closed; the partition of the single arterial trunk from the heart is soon completed; and at length the fourth and fifth pairs of arches having increased greatly in size and strength, the right ventricle sends all its blood into the fifth pair, the left into the fourth pair and the carotids.

The present condition of parts, therefore is this. 1. The trunk from the left ventricle divides into two branches, which go to each side, and ascend arching upwards and backwards, then run converging for some distance further backwards, and between the œsophagus and vertebral column, pass at an acute angle into the trunk of the (descending) aorta. Of these branches, the right is, in its whole length, but especially towards its distal end, smaller than the left, which is to be developed into the arch of the aorta, and its convex portion lies a little further forwards than that of the left does. Each of these branches, also, at some distance from its origin, give off a carotid branch. 2. The current from the right ventricle, passing into the trunk of the pulmonary artery, in like manner divides into two branches, which ascend arching upwards and backwards, and of which the right is smaller and rather more anterior than the left. Both these branches are placed behind those of the former trunk, into which, at their distal ends, they pass at an acute angle, and than which they are rather smaller.

The fifth arch, on the left side, now gives off a branch which goes backwards to the lungs, and ramifies in them. This branch becomes the distal half of the trunk of the pulmonary artery; the proximal part of the same left fifth arch becomes the proximal half of the pulmonary artery; and the distal part of the arch (that part which is beyond the branch) becomes the ductus arteriosus. The fifth arch of the right side gives off no such branch, and gradually wastes away.

The vertebral arteries arise very early from the distal part of each fourth arch just before it passes into the dorsal aorta: that of the left side arises just before the junction of the ductus arteriosus with the fourth arch. They run up on the six upper vertebræ, and soon became inclosed in short double processes (the cervical transverse processes) which unite, first together andthen with the vertebræ, and form foramina. On the left side the vertebral artery sends off a branch which goes far backwards to the anterior extremity, and constitutes the distal half of the left subsclavian artery, of which the proximal half is afterwards formed by the lower part of the vertebral enlarging so much, in corespondence with the development of the anterior extremity, that its upper half appears like a branch from it. The right subclavian is given off as a branch from the distal half of the right fourth arch, at some distance beyond the origin of the right vertebral from the same. It also bends far backwards to reach the right anterior extremity.

The part of the fourth arch on the right side which lay beyond the subclavian branch just mentioned, next wastes away, and the rest of this arch, having grown less then the corresponding arch on the left side, with which it is connected at their common origin from the aortic trunk, now assumes the appearance of an arteria innominata given off from the commencement of the left fourth arch. This left fourth arch, together with the aortic trunk, thus becomes the proper arch of the aortic, giving off, first, the innominata, then the left carotid, and then the branch from which the left vertebral and subclavian are derived.

After this, probably, the only important alterations which take place in the human embryo, are the change in the direction of the subclavian arteries as the heart retreats further into the cavity of the chest, the elongation of the carotids in correspondence with the growth of the neck, the enlargement of the common trunk of the left subclavian and left vertebral, so that the former, which at first appeared like a branch of the latter, now appears like a trunk giving it off, and the elongation of that part of the arch of the aorta which lies just before the orlgin of the left subclavian.

In the embryos of the mammalia which were examined, further changes effect the coalition of the three branches from the arch of the aorto into two or one ; and the plan of these changes is pointed out at the end of the essay.

Development of the nervous system, From Bischoff* we have some further observations on the development of the nervous centres, which confirm his own and V. Baer's former descriptions, and add them to some interesting minute details. In the examination of four ova, from a bitch which had conceived three weeks previously, he found, in the first, the dorsal laminæ formed by accumulations of formative substance on each margin of a groove (the *primitive groove* or *streak*) which lay in the long axis of the pyriform germinal area, and was not covered in by even the most delicate membrane. In the second and third ova which were removed from the living uterus twenty four hours after the removal of the first, the groove was still open, although the rudiments of six vertebral arches were formed in the dorsal laminæ on each side of it; at the head-end its margin were separated in three successive dilatations, in the middle they were nearly in contact, and at the tail end they had a lancet-form ; and, which war most interesting, the inner borders of these dorsal plates, and thus the innee layer of the substance forming the primitive groove, presented distinctly the transparent aspect of nervous matter. In a fourth ovum, removed from the uterus twelve hours latter, the groove was almost all closed in, and the borderd of the nervous matter having united along almost their whole length, presentes the appearance of a medullary tube, dilated and bent downwards anteriorly, and having the sacculi for the eyes considerably developed.

The conclusion, therefore, is that of the substance which accumulates by the sides of the primitive groove, and forms the dorsal laminæ, the inmost layer forms the nervous tube which is the rudiment of the central nervous system; that the rest of this substance is developed into the dorsal part of the trunk ;

* Mueller's Archiv, 1843, Heft iii-iv.

and that the nervous substance is formed even before the closing in of the groove.

Respiration of the embryo. The researches of MM. Baudrimont and Martin St. Ange* on the chemical changes produced in the incubation of the eggs of birds have shown that in all cases oxygen is absorbed and water and carbonic acid are given off by the eggs. The evolution of these substances appears essential to the progress of development; and they are probably derived from the combustion of carbon and hydrogen within the ovum. The quantity of carbonic acid which is given off increases with the period of impregnation; that of water is nearly the same at all the periods. From this combustion, moreover, eggs must have a proper temperature independent of that communicated by the mother, and the authors suggest that it might have been in part the source of the increased heat (amounting to from 12° to 19° above the surrounding temperature,) observed by M. Valenciennes† to be developed during the incubation of the eggs of the boa (python birittatus.)

Food of the embryo. Experiments by Signor Capezzuoli‡ appear to prove that the fatty matter of the vitellus is especially consumed by the chick during the last days of incubation; and that much of that contained in the chick itself is consumed by it shortly after it is hatched.

Excretions of the embryo. Dr. John Davy§ has analysed the meconium and vernix caseosa. The former contains

Mucus and epithelium scales	23·6
Cholestearine (in plates,) and margarine	·7
Colouring and sapid matter of bile, and oleine	3
Water	72·7

Its ashes also contained peroxide of iron and magnesia, with a trace of phosphate of lime and chloride of sodium.

In the vernix caseosa he has found,—

Epithelium (epidermis,) plates	13·25
Oleine	5·75
Margarine	3·13
Water	77 37

and in the very minute quantity of ashes which remained he found traces of phosphate of lime and sulphur.

The subjoined list includes references to the more valuable of the physiological Essays published last year, of which, for various reasons, no notice is contained in the body of the Report. They are arranged according in the subjects to which they relate, in the same manner as the Report itself.

Prof. Harting—Observations on the original forms and subsequent alterations of organic and inorganic precipitates; on the phenomena of the formation of crystals; &c. in this Tijdschrift voor Naturel. Gesehied. en Physiologie, 1843, St. 2, 3.

Mr. T. W. Jones—Muscle a neuro-magnetic apparatus, in the Medical Gazette, Oct. 20, 1843; and Dr. Letheby's Remarks on the same in the Physiological Journal, Dec, 1843.

Dr. Houston—On the circulation in acardiac fœtuses; in the Dublln Journal of Med. Sciences, Jan. 1844; and Dr. Marshall Hall's reply, in the same, Sept; 1844.

Dr. Thompson, Prof. Draper, and others—On the diffusion of gases, in the Philosophical Magazine, Jnly 4, 1844.

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* Annales des Sciences Naturelles, t. xvi, p. 65.

† Report from the Academie des Science, Seance de Dec. 26, in the Gazette Medicale, Dec. 30, 1843, and confirmed by more recent and numerous experiments, in the Report from the Academy for December 16, 1844.

‡ Annali univ. di Medicina; Gennaio, 1844; see also the foregoing observations by Czermak.

§ Medico-Chirurgical Transactions, vol. xxvii, p. 189. 1844.

Mr. Sibson—On the changes induced in the situation and structure of the internal organs in health and disease; in the Trans. of the Prov. Med. and Surg. Association, vol. xii, 1844; [a very important paper, the title of which led me to suppose, till it was too late to correct my error, what it could have little interest in physiology.]

S. Rusconi—On the respiratory organs of the proteus. Oken's Isis, viii, 1844.

Prof. Nasse—On the application of percussion to the study of gastric digestion, in the Med. Corresp.—Blatt. Rhein, und Westphal. Aerzte. No xvii, 1843.

Dr. Heintz—On a supposed new acid contributing to give the acid reaction of urine: in Poggendorf's Annalen, No. viii. 1844.

M. Parise—On a new method of bleaching bones, by alkaline and alcoholic injections of their texture through small holes bored in their walls; in the Ann. de la Chirurgie, Fevr. 1844.

Dr. Todd—The article *Nervous Centres*, in the Cyclop. of Anatomy.

M. Flourens—The new edition of his researches on the nervous centres.

Dr. Wigan—On the duality of the mind; in the Lancet, March 30, 1844.

Dr. Playfair—On sleep and some of its concomitant phenomena; in the Northern Journal of Medicine, May, 1844.

M. de Haldat—The summary of his researches on the crystaline lens; and M. Prevost, on single vision with two eyes; in Poggendorf's Annalen, 1844, Heft, viii.

S. Rusconi—On the non-erectile tissue of the tongue of the chamelion and its protrusion by a muscular apparatus; in the Ann. des Sciences Naturelles. Mars 1844.

Dr. J. Y. Sympson—On the fruitfulness of females born co-twins with males; in the Edinburgh Med. and Surg. Journ. Jan. 1844.

M. Serres—On the corpora Wolffiana and allantois [refered to in the last Report,] in the Ann. des Sciences Nat. 1843. p, 4, Pt. i.

Lund—On fossilized human bones in chalk caves in Brazil, [the caves contain bones of several extinct species, but admit each year the waters of adjacent lakes;] in the Edinb. New Philos. Journal. Jan. 1844.

Various papers on anthropology and ethnology, from the proceedings of the Ethnological Society; in the numbers of the Edinb. New Philos. Journal.

Dr. Laycock—Contributions to proleptics, in several numbers of the Lancet. A summary of them was given in this Journal, July 1844.

Part Fourth.

MEDICAL INTELLIGENCE.

FOREIGN.

1.—*A paper on the formation of fat in the Animal Organism.* By Dr. Justus Liebig. (From the London Lancet.)

The discussion respecting the origin of fat in the animal organism, which has now lasted three years, is at length brought to an end. The hypothesis, that all the fat found in an animal body is derived from plants, in which it first existed, and that in the state of fat, is now definitely abandoned by both its advocates. MM. Boussingault and Payen have each in his own way arrived at the conclusion that their theory is no longer defensible.

After Messrs. Dumas and Milne Edwards had satisfied themselves that the experiments of Gundlach, of Cassel, on the formation of bees'-wax, were correct; after the discovery of butyric acid as a product of the fermentation of sugar, by Pelouze and Gelis; after the decisive experiments of Persoz on the fattening of geese, and those of Playfair on the production of milk in the cow—Boussingault still deemed it necessary to institute especial experiments to remove the doubts he still entertained. The results of these experiments he communicated to the French Academy by letter. These are as follows:—1st. "My experiments," says M. Boussingault, "prove that pigs at eight months old have no more fat in their bodies then they can have received in their food." 2nd. Fed on potatoes alone for six months, they will have no more fat than that contained in the potatoes. 3rd. In the process of fattening pigs, more fat is formed than is found in their food. 4th. Aliments which cannot themselves be converted into fat, greatly nourish the body of pigs when mixed with fat, although fat alone enfeebles the animal, 5th. Those kinds of food which produce fat, and which only contain traces of fat ready formed, are always rich in the constituents of blood.

"I have," continues Boussingault, "fattened geese, and have found, as Persoz formerly did, that the fat formed in the stomach of these animals far exceeds the amount of oil contained in the maize. Upon this point my experiments decisively confirm those which Persoz communicated to the Academy, which I feel bound to bring to notice particularly, being a member of the committee appointed to report on his labours.

"I will describe to you, briefly, in what manner I proved the rapid influence which fat, added to other articles of food, has upon the formation of fat.

"Some ducks were fattened with rice, which only contains a few thousandth parts of fatty substances; others were fed with the same rice mixed with butter Those fed on rice alone remained in their primitive state; the others, whose food was rice and butter, became, in a few days, like balls of fat.

"In all my experiments, I have always found the formation of flesh accompany the production of fat."

As regards these results of Boussingault, I beg to remark that, for some portions to be clearly understood, we must await the publication of his mode of experimenting. I do not understand, for instance, how it was possible for him to ascertain that pigs which were fed on potatoes for six months did not yield a larger amount of fat than was originally contained in the potatoes; this amount of fat must, I suppose, comprise that resinous or wax-like substance which ether dissolves out of potatoes; the amount present is about 3-1000ths of the weight of the potato, and may be estimated; but the amount of fat contained in the pig before the experiment cannot be ascertained. Neither can I conceive how, in the ducks that are fed with rice and butter, and become like balls of fat, the formation of flesh could keep equal pace with the deposition of the fat.

In my publication, entitled, "Animal Chemistry compared with Animal Physiology; Heidelberg, 1844," I have sufficiently exposed these erroneous methods which have commonly been adopted to decide the question of the convertion of starch and sugar into fat, and we may congratulate science that Boussingault has at last found the only proper method.

In those animals in which fat is to be produced and deposited, the amount of carbon taken in (in the shape of flesh or fat) must be greater than the amount given off, (i. e., by respiration, by motion and exertion;) they must be in that normal healthy condition which permits of their passing the nourishment received over into the organism; the nourishment must contain the components of blood in great quantities, which are necessary for the maintenance of the healthy state, and the unimpeded demonstration of organic action upon which depends the transformation of the constituents of the food free from nitrogen.

With a diet which produces death from starvation, or extreme enervation, fat is neither assimilated, nor does the organism, in this case, transform sugar or starch into fat. The fat is inclosed in cells in the animal body, the substance of which is richer in nitrogen than the components of blood, and if the nourishment,—potatoes, for instance,—only contains so much of the constituents of blood as the animal requires to renew the daily consumption and deficiency, nothing can remain to form the cells necessary for the reception of the fat. In my book, entitled, "Chemistry in its application to Agriculture," pp. 137, 138, I have mentioned that, by the use of a great proportion of amylaceous food, such as potatoes, the excrements contain a large amount of perfectly unchanged globules of starch, which are never perceptible when the proportion of flesh taken as food is greater.

Nobody has ever, at any time, doubted that the presence of fat in the food has an influence upon the formation of fat. If, as has been proved by the most evident facts, in cases of starvation, the fat already deposited can leave the cells, return to the circulation, and be used to support respiration, it cannot be deemed remarkable, if that fat, which enters from the stomach through the chyle, into the circulation, should behave as if it were a portion of the substance of the body itself. If the conditions necessary for the deposition of the fat are present it remains in the body, and adds bulk to the mass of fat already present A small quantity of salt, added to the food, prevents this deposition; the fat is then to be found in the fæces.

The last experiment described by Boussingault has, in these scrofulous times, been made some thousands of times by our medical men with cod-liver oil, and no one is astonished if, on the application of certain doses of the oil, his patients assume exactly the same state as the ducks of Boussingault, which he fed with rice and butter. Indeed, I have often been asked how, particularly in gouty cases, tbe accumulation of fat might be avoided without diminishing the medicinal action of the oil.

The question relating to the formation of fat in the animal body is, however in danger of taking quite an absurd turn, by Mr. Milne Edwards' interpretation of the results of Boussingault; I must claim the reader's attention to this for a moment. Milne Edwards expresses himself in the following manner: "In our experiments (i. e., those made by himself and Dumas) on the production of wax by the bee, we found that they made no wax as long as we attempted to nourish them exclusively with sugar and water. The production of wax followed, however, on the addition of a certain amount of honey (which contains a certain quantity of wax-like matter) to the sugar. The quantity of wax contained, in this case, in the food of the bees, added to the amount of fat contained in the body, did not correspond to the quantity of wax formed during the experiment; we were therefore compelled to acknowledge the formation of wax from sugar. We did not in our experiment seek for the matter, which acted in this transformation as a sort of ferment; if, however, the views of Boussingault are adopted, these differences may be easily accounted for. From this may be seen that the formation of fat in insects in the same as in mammalia."

The ideas of Milne Edwards, contained In the above statement, are as weak as they are confused. At the commencement, he would seem to be of opinion that the wax contained in the honey has an influence upon the formation of wax from the sugar; the latter part of his statement shows, however, that he attributes to an unknown ferment, present in the honey, the formation of the wax. Milne Edwards speaks, at the close of his statement, of the views of Boussingault; the latter has given no views, but only communicated the result of his experiments. It would have been more judicious if Milne Edwards had remembered that honey is the normal food of the bee, which contains in its sugar, not only the condition for the formation of wax, but also in its nitrogenous constituents, which are never wanting in honey, the conditions for the sustenance of their life and health.

That bees are, however, able to produce wax from pure sugar (from cane sugar as well as from starch sugar) without any addition of wax, the latter experiments of Gundlach have given satisfactory proofs. It is true, from what he says, that bees only have recourse to pure sugar for food when in a state of starvation, and although they by this means become unhealthy, they do most certainly still make wax, and this wax is of the same composition as that made from honey. Mr. Brodie, who has, at my suggestion, analyzed these two kinds of wax, will shortly publish his investigations on this subject.

The other principal opposer of the fact that fat is formed in the animal body, (M. Payen,) who, until lately, contented himself with inventing proof to support his notions, has, as might be expected from his usual flippancy, adopted the resulth of Boussingault. Nothing that he has advanced himself remains, and yet he behaves before the Academy as if he had always entertained his present opinion. One portion of the addition which he makes to M. Boussingault's letter, is, however, too remarkable to be omitted here.

"The whole of these experiments, says Payen, would seem to prove, in opposition to the opinion of a foreign chemist, that no effects can be expected from the employment of potatoes in feeding animals, similar to those obtained by the employment of bran, maize, oil-cakes, and others substances rich in oil. Thus the traditions of practice are again justified.

"As regards the purely scientific question, it seems to be completely solved, and the truth is just the medium of those assumptions which the opposite views had exclusively adopted, and in this respects all our labours have contributed to the definite solution of the question."

Nothing of the labors of Payen, concerning the formation of fat, has been made known, excepting an analysis of maize, performed nine years ago. I can scarcely believe that he considers his random guesses of the transformation of the wax-like and resinous constituents of hay into butter; or of the formation of milk, butter, and the sugar of milk, in the body of a starving nurse, or of the transformation of the fat of bees into wax, to be labours. If Payen, by his "foreign chemist," means *me*, he has again committed the error into which ill-educated people so often fall,—the same fault which he committed, when he classed, in my name, whales and guinea-pigs with herbivorous animals. I have never recommended potatoes as fattening food, much less have I compared its nourishing properties to those of bran, oil-cakes, or maize. When I have spoken of potatoes, it has just been in the opposite manner to that which Payen attributed to me. I remember, however, having read, that one of the *colleagues* of Payen attributed to potatoes the property of producing flesh.

2.—*Sulphate of Quinine not absolved when applied Endemically.* By M. Martin-Solan. (*Bulletin de Théraputique.*) December, 1844.—Many medicines, when applied to the skin, either whole or deprived of its cuticle, act energetically on the economy, and may be detected in the secretions, thus showing they have been absorbed. Sulphate of quinine, when given internally in the dose of one grain, may easily be detected in the urine by means of the ordinary tests, as iodide of potassium, &c. Martin Solan, however, has made many experiments on twenty individuals affected with various maladies, relative to this medicine being absorbed when applied to the skin, and in no case has he succeeded in detecting the slightest traces of the medicine in the urine. The sulphate of quinine was applied by friction to the sound skin, and to that denuded of cuticle, in baths, and by means of ointments. The effect was null in all.

3—. *Case of a large calculus, voided from the female urethra, encrusted upon a hair-pin, which had been swallowed twenty-seven months ago.*—Margaret Leigh, aged, 26 years, was admitted into the Salford Workhouse, on the 12th of August, 1845, in the last month of her pregnancy of an illegimate child. For several weeks she suffered from pain in the sides and back; from September 10th to 14th, complained of constant pain in the urethra, difficult micturition, a feeling of tension and bearing-down as from the passage of a hard body of considerable size. On the 14th, after much painful straining and bearing-down when voiding her urine, she parted with a calculus, without any manual interference, of which the accopanying sketch is an exact representation.

Its weight is four drachms and forty-four grains, its length two inches and a quarter, breadth one inch and a quarter, and thickness five eigths of an inch. It is of a flattened oblong figure. Its nucleus is a common hair-pin, the points of which as well as the convex extremity, are equally evident to the sight. Its probable composition consists of phosphate of lime, and the triple phosphate of magnesia and ammonia.

She has had no pain since parting with the above, expresses herself greatly relieved, and is well as women usually are towards the close of gestation. On the 21st, (one week after,) she gave birth to a full-grown child, after an easy natural labor, from which she quickly recovered. The woman is still in the workhouse, and the specimen in my possession.

Upon tracing the history of this case, I find from the evidence of three persons, then present, that the pin was really swallowed on June 6th, 1843. The woman was straightening her hair, with the hair-pin between her teeth, when one of her companions pulled her hair behind causing her to laugh and throw her head back, when the pin slipped down the œsophagus. During the first twelve months she felt little inconvenience, with the exception of slight pain in the bowels, attended with constipation. On the 26th of April, 1845, she was admitted a home-patient of the Chorlton Dispensary, under the care of the house-surgeon. She remained under this institution five weeks, during which timo she complained of continued acute pain in the left inguinal region, of incontinence and increased flow of urine, a profuse purulent discharge from the urethra, scalding, and obstinate constipation, attended with frequent discharges of blood with the fæces. At the recommendation of the house-surgeon, as her case was considered more proper for the Manchester Infirmary, she gained admittance into that institution. There she remained two months, suffering from the same symptoms, only the urine was much increased in quantity, but gave no evidence upon being tested of saccharine matter. She frequently parted with six quarts of urine during the night, and generally seven quarts in the twenty-four hours. She complained of pricking pain in the left groin, in creased on bending the body forward, and on sitting down, but was never perfectly free from it, excepting when in the recumbent position. She was much relieved while under the Infirmary, but as her confinement was evident'y near approaching, was obliged to leave and gain admission into the Salfcrd Workhouse to lie-in

I must here remark that this patient never once mentioned to any of her medical advisers the circumstance of having swallowed the hair-pin lest, according to her statement, she should be compelled to undergo an operation for its removal. She being pregnant and unmarried, I once suspected she might, for the purpose of procuring abortion, have introduced the pin into the vagina, but from the nature of the evidence I have since been put in possession of, I am now fully convinced to the contrary. Being interested in her case, I referred to the medical gentlemen who had the treatment of her, and from them have gleaned the above statements. Since her confinement I have made an examination per vaginam, but without discovering any alteration of structure that would indicate its course into the bladder. She still complains of pain upon pressure at the lower and left side of the abdomen and groin. From the symptoms above related, I think it most probable the pin passed from the sigmoid flexure of the colon into the left side of the bladder. *Prov. M. & Sur. Jl.*

4.—*Expulsion of a piece of bone from the gullet at the end of eight months.*—The following case of prolonged lodgment of a foreign body, for the period of eight months, in the upper part of the œsophagus, giving rise to severe respiratory embarrassment, but without occasioning any impediment to deglutition, is is sufficiently curious to merit publicity.

Mrs. Backhouse, æt. 62, whilst eating a beef-steak pie, in January, 1845, accidently swallowed a piece of bone. She was much distressed at the time, by efforts to vomit, during which some blood was brought up. Embarrassed breathing, cough, muco-purulent, and occasionally hæmoptic expectoration, with loss of strength, and emaciation, soon followed. Ausculation failed in discovering the cause of so much pulmonary disturbance.

This deranged condition of health continued until the morning of Thursday, August 28th, 1845, when, during a very severe paroxysm of coughing, she brought up a large mouthful of phlegm—experienced the sensation of something giving way in the throat, speedily succeeded by the feelings of a sharp substance across the passage. During a fit of vomiting, she succeeded, by the help of her finger, in removing the accompanying piece of bone.

A week subsequent to the discharge of the bone, all traces of previous irritation were entirely removed.

[*Northern Journal of Medicine, Edinburgh, Nov.*, 1845.]

AMERICAN MEDICAL INTELLIGENCE,

MISSISSIPPI MEDICAL CONVENTION.

In our last number, we noticed the proposal for holding this Convention, and published such preliminary proceedings of physicians in different places as had reached us. Pursuant to notice, forty physicians, chiefly from the middle and Southern parts of the States, met at Jackson, the seat of Government, on the 12th of January, and the Convention was organized by the election of Dr. Metcalf, of Natchez, President; and Dr. W. B. Williamson, of Clinton, Secretary.

We have received from the Secretary the entire proceedings for publication, but as we cannot conveniently make room for the whole report, and believing moreover, that it would not be desired by the mass of our readers, we shall only give such extracts as we deem of interest and importance. We will here express our thanks to the members of the Convention, for their kind and flattering notice of the New Orleans Medical and Surgical Journal, and shall be most happy to have their co-operation in its support. It is very gratifying to us to see our well-meant, though humble efforts to subserve the best interests of the Profession, so highly appreciated; it will stimulate us to increased exertions in the prosecution of our object.

We learn, that after the Convention was organized, Dr. S. A. Cartwright, of Natchez, was invited to "present his views upon the subjects for which the Convention had met," and on the occasion delivered a very able and interesting address, a copy of which we have just received from the author. We are pleased to find the views heretofore proclaimed in this Journal, in regard to the peculiarity of Southern diseases, and the importance of Southern medical education, so strongly sustained by this able writer. This address abounds in sensible and judicious views; and if not published in full, will receive a more elaborate notice in our next number.

A committee of five, consisting of Drs. Lipscombe, of Columbus; Kilpatrick, of Woodville; Stokes, of Hinds; Banks, of Clinton; and Barnett, of Yazoo City, was appointed "to examine and report on the views contained in the paper read by Dr. Cartwright." By request, Dr. A. C. Dayton, of Columbus, next delivered a lecture, "On the importance of a more particular attention to the diseases of the teeth, in the ordinary practice of medicine;" for which he was complimented with the thanks of the Convention.

Dr. Lipscombe, chairman of the committee on Dr. Cartwright's address, made a report, accompanied by resolutions, embracing the following points:— 1st, Requesting a copy of the address for publication. 2nd, To petition the Legislature to pass a law for the establishment of a registry of births and deaths throughout the State. 3rd, In favour of the establishment of a Lunatic Asylum. 4th, To petition for a tax on the venders of nostrums and quack medicines. 5th, Recommending the establishment of a State Medical Society, and to have it a department of the State University, now about being organized. 6th, Adopting the code of medical Ethics of the Natchez Medical Society.

A committee of five, consisting of Drs. Cartwright, Pickett, Langley, Copes, and Barnett, was appointed to draft a constitution and by-laws, for the State Medical Society. On the 14th, Dr. Copes, on behalf of this committee, offered an excellent constitution, which was unanimously adopted. The State Medical Society was then organized, by incorporating all the members of the Convention, and they proceeded to the election of officers; whereupon the following gentlemen were elected: viz, S. A. Cartwright, M. D., of Natchez, President. W. Langley, M. D., of Jackson, 1st Vice President. J. S. Copes, M. D., of Jackson, 2nd Vice President; and W. B. Williamson, M. D., of Clinton, Secretary.

On motion of Dr. Kilpatrick, of Woodville, the following Resolutions were adopted by the Convention:—

1st.—*Resolved*, That we deem it of the utmost importance that physicians who intend to practice in the South, should, in order to be more expert in the management of the diseases peculiar to our climate, receive as much of their medical education in the South, as can be obained there; and that no physician should enter upon the duties of the profession, before attending, if convenient, a course of study, either in a Southern hospital, or a Southern college.

2nd.—*Resolved*, That we hail with great pleasure, the establishment of the Medical College of Louisiana, as the emporium of Southern medical knowledge, and the proper *alma mater* of Southern physicians.

3rd.—*Resolved*, That we will always uphold the Louisiana, Medical College, as long as conducted in its present dignified and creditable manner, and will encourage it by every means in our power.

4th.—*Resolved*, That we hail the establishment of the New Orleans Medical and Surgical Journal as a praiseworthy and honorable work, every way worthy of our cordial support; and that we will aid it with our pens and purses.

Previous to adjournment, on motion of Dr. Kilpatrick, the Convention adopted other preamble and resolutions highly complimentary to the Louisiana Medical College; and proposing to establish in conjunction with some Southern Literary College, a Summer School of Medicine, to which said Medical College is invited to furnish one or more Professors, together with such apparatus "as may be thought proper and necessary to elucidate the several departments and subjects of study." It was also proposed to establish an Infirmary in connexion with the Summer School. A committee of five, consisting of Drs. Kilpatrick, Dayton, Banks, Langley, and Emanuel, was appointed to confer with the Professors of the Louisiana Medical College upon this subject. The committee then adjourned *sine die*.

We are greatly surprized that this Convention should have adjourned without taking any action whatever in regard to the proposed National Convention at New York. It surely must have been an oversight; we understood it to be one of the objects for which the Convention had assembled; and it was alluded to in Dr. Cartwright's address. We hope Mississippi will not be unrepresented in that highly important Convention. We are much pleased to see the physicians of the South awaking from their lethargy, and we tender to our friends in Mississippi our best wishes for their success in the cultivation of medical science.

NEW-ORLEANS, MARCH 1, 1846.

HEALTH OF THE CITY.

Since our last report the customary winter diseases have prevailed to a rather greater extent than previously. Scarletina, pneumonia, typhoid fever, cartarrh, sore throat, bowel complaints &c. &c. have been the predominant affections. The amount of sickness however has been by no means great, and we know nothing to remark of special attention.

These has been, so far as we have been able to learn, less small pox than usual about the city this winter, yet we see by our exchanges that this disease prevails to a considerable extent at the North; especially about Philadelphia and Baltimore. We cannot omit the occasion to reiterate the importance of strict attention to vaccination.

The weather has been uniformly cold, with a good deal of rain. The winter has been one of the coldest experienced here for a number of years. We write at the period for the opening of spring, yet scarcely any evidences of its approach are to be seen.

The River has continued very low, but is now slowly rising.

HEALTH OF THE COUNTRY.

We will chide our correspondents once more for their neglect of our desire to be supplied regularly with correct information in regard to the existing state of health throughout the country, and if it fail to arouse them to greater punctuality, we can but be thrown back upon one old resource, *dame Rumour*. We regret this very much, as we have learned from various sources that the letters of our "health correspondents" are usually read with much interest, and it surely would be very desirable to give in each number of our Journal, a glance at the state of health throughout the south-west. To the few of our kind friends who have uniformly and punctually supplied us with the desired information, we return our sincere thanks, and hope they will continue their favours.

So far as we have been able to ascertain the health of the country, has been generally good during the winter. We have heard of some cases of small pox about Memphis, and in the country back of Natchez. We understand also that epidemic erysipelas has prevailed to some extent in the Western District of Tennessee, and about Yazoo City, in Mississippi. The following are the only letters we have received from our correspondents.

St. Marys, La. Feb. 17, 1846.—Our correspondent says; "Since my last communication this region of country has been generally healthy. The autumnal months were passed with fewer cases of fever than usual; but few cases of congestive fever occurred. During the month of January occassional cases of pleuritis and pneumonia occurred. At present tehy are more numerous, and catarrhs, and catarrhal fevers are prevailing extensively, especially among children. Negro children seem to be suffering most from these affections. We have had an extremely unpleasant winter; frequent and excessive rains, and unusually cold and changeable weather. It is the frequent remark of our oldest inhabitants that this has been the coldest winter known in this latitude for many years. It has been especially a hard winter upon stock of all kinds. Our prairies are coverred with dead cattle and horses, from want of food and exposure; and if a high temperature prevailed, the atmosphere would be loaded with noxions effluvia, and I doubt not pestilence would be stalking abroad at noon-day."

Columbus, Miss., Feb. 11, 1846.—"The last time you heard from us, (Aug. 15. 1845.) we had the pleasure of informing you that our section of the country had been remarkably healthy during the whole year. But about the 10th of Sept. disease became prevalent all at once. I find from my notes that the last ten days of Aug. and the first few days of Sept. we had rain nearly every day. 'Sept. 11th, fine, pleasant this morning.' '13th, Bilious fever very prevalent; confined to the low, flat sandy land.' '16th, continues.' '29th, more sickness this fall than any since 1840.'

"It is worthy of notice that the south-western country was unusualy healthy during the past summer, and that Bilious-remittent and intermittent fevers became prevalent throughout this large extent of country, nearly to a day.

"It is thought that our winter has been more severe than any since 1831. The mercury has been down frequently to 16°.

"It has been remarkably healthy during the last few months. Our physicians are all idle at this time."

Montgomery, Ala., Feb. 15, 1846.—We have had an unusual amount of cold weather, and the vicissitudes have been very sudden; still we have less sickness than has been customary with us during the winter season. Our winter sickness seems to leave a pretty exact proposition to the amount of fever during the summer and fall,—from whence the influence, that the causes of the latter, are also measurably the predisponents of the former. Catarrheal effections principally prevail at this time though they are neither numerous nor fatal. I have seen one case of genuine primary croup, a rare disease here. I have heard of two cases of traumatic tetanus, one of which was treated with success ; and I have myself seen a case of remittent fever, complicated *with symptoms of a tetanic character* during the exacerbations.

Galveston, Texas, Feb. 17, 1846.—I owe you an apology for not having made a report of the health of this city in time for publication in the last No. of your Journal, and the only excuse I can offer is that I have had nothing of the slightest importance to communicate in relation to the diseases that have prevailed.

The general health of this city, as well as of the entire country since the period of my last report has been very good, the measles I believe is the only disease that has prevailed to any extent, and it has generally been so mild in its character as to require no medical treatment whatever.

It is rumoured that the small pox has made its appearance amongst us, as yet I have seen but one case that at all resembles it, and I am inclined to believe that the rumour is intirely groundless. "

Nashville, Tenn. Feb. 13, 1846.—I have received the Journal, and see the error in the dates of my letters to you. In this I have nothing to communicate in regard to sickness except that since the 1st of Dec. we have here suffering in Nashville with small pox. The first cases which occurred were of the most maliegrant kind and we have had 6 or 7 deaths out of about 20 or 25 cases. I have no doubt it has been epidemic, although I am sustained in that opinion by only a part of the profession here. Indubitable evidences have been added, to the vast amount before collected, of the complete protection afforded by perfect vaccination.

In other respects our community is perfectly healthy, scarcely the usual number of cases resulting from cold, as pleurisies, pheumonia, &c."

Woodville, Miss. Feb. 14, 1846.—I am really sorry that I can say nothing worth publication. Our country is still very healthy, I suppose seldom, if ever more so. The unusually severe winter has caused some cases of pleuritis and phneumonia and accelerated the progress of a few cases of tubercular phthisis. The pleuritic and pneumonic cases have been manageable and no deaths have occurred from those diseases since I wrote last. One death in the county from phthisis on the 3d inst. after about eighteen months duration, in a young man aged 25. "

POPULAR LECTURES IN NEW ORLEANS.

We have observed with great satisfaction the regularity with which the lectures at the " People's Lyceum " and " the Young Men's Free Library Association " have been kept up during the winter, the ability displayed by the lecturers, and the attentive interest of the large and most respectable audiences.

The People's Lyceum is an association of intelligent and influential gentlemen, who have engaged some of the most talented men belonging to the different professions in the city to deliver lectures once a week, upon such subjects as they may think proper. Judge Eustis, Prof. Riddell, Judge Carlton, Rev. Mr. Nicholson, Rev. Mr. Hamilton, of Mobile, Mr. Schmidt, and Mr. Dimitry, have already lectured, and the Hon. S. S. Prentiss and others are to follow. The spacious and magnificent hall of the Washington Armory, where the lectures are delivered, is generally crowded to overflowing by the most enlightened and respectable audiences, of which our fair and intelligent ladies form a large part. There could be no better evidence than this of the high estimation placed upon the efforts of these learned gentlemen, nor of the rapidly developing scientific taste among the citizens of New Orleans. It augurs well for coming events; and the prospect is most cheering to the philanthropist, the patriot, and the scholar. A brighter day is dawning upon us; and it is not folly to look forward to the time when New Orleans will rival her sister cities, *in the arts, sciences, and in literature*, as she now does in commerce. "*Tempora mutantur; et nos mutamur in illis.*"

The Young Men's Free Library Association is a praiseworthy institution gotten up by the young men of the city. They have collected by donation and purchase, a very handsome library, the use of which, together with their neat and beautiful room, is freely offered to all who wish to read. Lectures are delivered here, also, once a week, and are attended by most respectable audiences. Twelve months ago we noticed the scientific lectures then delivered in the city, and it is with pleasure we are allowed to refer to them again, and to mark the growing interest in the subject. When our new Municipal Hall shall be completed, and our Public School Lyceum and Library organised, we may expect to see *popular lectures* become by far the most attractive amusement of the city.

BOARD OF MEDICAL EXAMINERS—THE TAX ON PHYSICIANS.

We feel it to be our duty to lay the following communication from one of our most respectable country practitioners, before our readers, and to invite the particular attention of the physicians of the State to its contents. In regard to the abolition of the Western Board of Medical Examiners, the remarks of our correspondent are doubtless well grounded, and his suggestion may be very good; but we will take occasion to express our regret at *the negligence of the entire body*. In this state we have already on our statute books good laws for the regulation and protection of the Profession, yet from the supineness of the medical boards, they remain virtually *a dead letter*. Throughout the state—even in this city, under the very eyes of the Eastern Board—scores of physicians and druggists are pursuing the profession contrary to law. In short the operation of the law at present only serves to impose a tax of twenty dollars on such gentlemen as have too much pride to practise without obtaining license. Thus a burthen is imposed upon such as would do honor to the profession and the state, whilst quacks and impostors are permitted to carry on their unholy and outrageous traffic in human life, unmolested. This evil cries aloud for reform; and if public sentiment will not sustain the better part of the medical Profession in the discharge of the high responsibilities that devolve upon them, we hesiate not to say that the laws regulating the profession had as well be repealed.

As to the state tax upon the income of practising physicians, we are at a loss to conceive how any legislator at all familiar with the duties and toils of the profession could have ever proposed it. After retiring from the profession, he must have lost all sympathy with it. This tax may be onerous and odious enough in the country, but what will be thought when the fact is proclaimed: that the physicians of New Orleans annually attend, without charge, and in the service of the State, upwards of six thousand invalids, besides an immense number in private practice from whom they get nothing! This law should by all means be repealed; but let us hear what our correspondent says.

"Whilst writing I will mention a subject which especially interests the profession in the Western district of this State, viz.: the difficulty which physicians settling in this part of the State have to encounter in procuring their licenses to practice medicine from the Board of Examining Physicians in the Western district. This Board is constituted of physicians residing at a distance from each other, and it is almost impossible ever to get a sufficient number of them together to form a quorum to grant licenses conformably with the law of the State. This difficulty is made an excuse by physicians for not complying with the law and they here practice their profession with impunity, in open violation of the law. I have conversed with many physicians in this section of the State, and all agree, even the members of the Board themselves, that this Western Board is perfectly useless, nay, a positive injury, and should be abolished; and that we should have but one Board in the state, and that at New Orleans. Not one physician in fifty residing in the Western district, has complied with the law, and I doubt whether the penalties of the law could be inflicted for its violation upon physicians residing in this district, in consequence of the difficulty of getting the Board together for the purpose of obtaining licenses. This Board was constituted for the convenience of physicians residing in the Western district. But most physicians, on coming to the state, first visit New Orleans, and those resident in the country generally visit the city once a-year, or at least in two years, and a quorum of the city Board can always be obtained 24 on hours notice. This subject ought to be brought before the legislature immediately; the Western Board ought to be abolished, and in every parish one or two medical men should be appointed, whose duty it shall be to present to the grand juries of the several courts the names of all physicians who practice medicine in violation of the law.

"Another subject to which I will allude is that of the tax of $10 per year imposed by the last session of the legislature upon physicians practising in this state, whose professional income shall exceed $500 per annum. This tax is looked upon by the profession and by the community at large, as unequal, unjust and illiberal:—unequal, because the poor country doctor who happens to receive his scanty pittance of $500 per year for his days of hard riding, and nights of toil, watching and anxiety, pays the same tax as the man who owns ten likely slaves of the value of 10,000:—unjust, becanse the legislature has no more right to tax physicians than they have to tax planters or school teachers, or persons engaged in any other useful and honorable occupation:—iliberal, because if there is benefit to society in the exercise of our noble profession, we ought not taxed for the privilege of doing good, and relieving the ills of suffering humanity. A profession whose object is to mitigate "ills to which flesh is heir," ought not certainly to be taxed for the exercise of it, but on the contrary should be fostered and encouraged by every aid which a wise legislature can contribute. The lot of the physician has enough of toil and trouble, and hardship, without the addition of an onerous tax. His services at best are but poorly paid, and no class of men distribute gratuitously so large a portion of their time, their labor, and I may add, their hard-earned money. For at least one third of their services, physicians in the country receive nothing, not even the thanks of a grat eful community; and yet they are taxed. We are taxed 20 dollars on coming into the State for permission to practice; we are taxed 10 dollars per year for practising if we happen to clear 500 dollars; and we are taxed 5 dollars per annum for the vehicle in which we ride on our errands of mercy. Verily, in their treatment of an enlightened and liberal profession, our last legislature were governed by very *illiberal* views! And strange to say, this very bill for taxing physicians was introduced by a member of the last legislature, himself a member of this liberal profession, and a retired practitioner of medicine.

I hope in the next number of your valuable Journal, you will bring these two subjects to the notice of the profession; and now, whilst the legislature is in session, I trust that active measures may be used to have the Western Board abolished, this odious tax repealed, and the character of the medical profession elevated.

We, in the country, calculate upon the co-operation of the city physicians in the accomplishment of these objects. J. B. D.

MORTALITY OF NEW ORLEANS IN 1845.

We are happy at last to have it in our power to compare the mortality of New Orleans with that of other large cities. We have had regular reports from all the cemeteries during the year, and have made out from them the following statistics, which, we doubt not, will be interesting to our readers.

The total number of interments in the city of New Orleans for the year 1845, was 2783; classified as follows: viz, white adults, 1287; coloured adults, 386; white children, 683; coloured children, 381; not distinctly marked, 46.

We have made out the following list of the principal diseases and causes of death: viz, phthisis, 360; enteritis, 90; bilious fever, 20; conjestive fever, 49; typhoid fever, 76; scarlet fever, 121; pneumonia, 57: delirium tremens, 73; convulsions, 78; tetanus, 26; trismus nascentium, 109; dysentery, 81; diarrhœa, 56; still-born, 176; old age, 31.

The number of deaths from consumption would appear to be large for the South, but it must be recollected that a great many come from a distance to this place, in search of a more genial climate, and here fall victims. The number of cases that originate in this city is comparatively small. The next most frequent cause of death appears to be *trismus nascentium*. This affliction is doubtless much more common here than any where else in the United States, and demands careful investigation. The number of deaths marked from *old age* is large; and we think the term somewhat indefinite, as we observe some of the ages on the bills as low as 63 years. Yet there may be instances of délicate persons in whom the constitution is worn out, and the vital powers exhausted at that period of life, and death occur without any distinct or well marked disease. On examination we find that the age of the greater number thus marked, ranges from 80 to 100 years, and in one case, 110 years. In numerous cases the age is not stated; these were doubtless old negroes who did not know their age. There is but little doubt that a greater number of extremely old people may be found in Louisiana than any other State in the Union, as was shown by the last census; which proves that how much soever *immigrants* may suffer whilst undergoing acclimation, the climate *per se* is by no means inimical to longevity. We invite particular attention to the comparative mortality of the white and coloured population: the statistics here given would indicate the deaths to have been greater amongst the coloured than the white, in proportion to numbers.

By reference to our Hospital reports, it may be seen what a difference the residence of even a few years makes in the number of admissions. Of 6136 persons admitted into the main building, in 1845, the number which had resided in New Orleans less than three years, was 4944; and more than three years, 1192.

Boston is the only city whose mortality, for 1845, we have yet learned; The following statement we take from the Boston Medical and Surgical Journal:

" *Mortality of Boston in* 1845.—From the General Abstract of the Bill of Mortality of the City of Boston, just published from the Records kept at the health Office, it appears that the whole number of deaths during the last year was 1585, being 344 more than during the year previous. Of this number there were, still-born, 245; under 1 year, 481 deaths; under 5 years, 1096; and over 60 years, 278. These numbers vary somewhat from the totals of the weekly report published in this Journal, which has been faithfully made up from the copy furnished us from the Health Office. The difference, however, is in no instance great. The whole number of deaths from consumption, as above, was 426; in our report, 422. Scarlet fever is given as the cause of death in 152 cases; lung fever, 135; typhus fever, 97; smallpox, 31; delirium tremens, only 4. Taking the population of Boston to be 114,000, the above report shown the mortality of the city during the last year to have been 1 in 44.10, or 2.26 pr. ct.

We have long been under the impression, that when a fair comparison should be instituted, the annual mortality of New Orleans would be found to be as small in proportion to the population as any large city in the Union; or indeed, the world. The results of 1845 will show the fact with regard to Boston, which is esteemed one of our healthiest American cities, and we hope to be able to give in our next number, the comparison with New York, Philadelphia, and Baltimore. We learn from a late number of the British American Journal of the Medical and Physical Sciences, that the mortality of Montreal, Canada, a city of about 50,000 inhabitants, for the month of December last, was 270; whilst the mortality in New Orleans for the same month, was 246.

Now, taking the average annual population of New Orleans at 150,000, (which is probably considerably short of the mark,) our mortality of 2783 would be about 1 in 54, or within a fraction of 1.85 per cent.

Our estimation of the population will surely appear most *moderate*, if it be recollected that the census of 1840, taken in the summer, gave us 102,000; that for three decennial periods previous, the population had been nearly doubled; that since 1840 the increase has probably been as great as at any time before; and that during six months of the year we are supposed to have a floating or transient population amounting to 40, or 50,000.

New Orleans is looked upon abroad as a perfect *charnel house*,—a *Golgotha*—a " whited sepulchre," fair enough without, but within, "filled with dead men's bones;" but the results of careful investigation would lead to a very different opinion. Will it be said that we are fortunate in having one of our *most healthy years* for the comparison? It is true we had no epidemic last summer; but we had as much other sickness as usual when yellow fever does not prevail. And as for the other seasons of the year, we had about as much sickness as customary. Indeed, we had one epidemic, (scarletina,) throughout the year. The fact is, yellow fever is the only disease we have any occasion to dread in New Orleans; and when that does not visit us, no city is blessed with a larger share of health.

In the Boston statement, we find only the following *causes of death* specified, and have placed in comparison the same from the New Orleans statement:—

Consumption, in Boston,	426, —	in New Orleans, - - -	360.
Scarlet Fever, - - -	152, —	- - - - - - - - - -	121.
Lung Fever, - - -	135, —	Pneumonia - - - -	57.
Typhus Fever, - - -	97, —	Typhus and Typhoid Fever	76.
Small Pox, - - -	31, —	- - - - - - - - - -	1.
Delirium Tremens - -	4, —	Ditto, and Intemperance,	73.
Still-born, - - - -	245, —	- - - - - - - - - -	176.

As none of the cases on our bills of mortality are marked *lung fever*, we have placed in comparison, our pneumonias and pleuro-pneumonias, thinking them probably the same complaint, though we may be mistaken. We have likewise added to our list of *delirium tremens* all the deaths marked from *intemperance*, although this may have been but the *remote cause* in many instances. The statistics on this point would go to show quite a contrast between the intemperance of Boston and New-Orleans, which may be very just. We will not deny that there is great room for improvement in this respect in our city.

It may be asked whether there is any evidence of progressive improvement in the general healthfulness of New-Orleans. We should say that since 1841, there certainly is, though a longer time may be required to determine this. The low grounds in and around the city are being elevated, the pavememts extended, and if the proposed plan of waternig the city be carried into execution, we shall certainly be authorized to expect great improvement in health. But all this will not enable the stranger to come and live amongst us without more or less danger during the first one or two years. His constitution must undergo acclimation before he is safe.

If we had a registry of births and marriages properly attended to, our statistics might now be much more complete and satisfactory; but we regret to say, that instead of having the present law in relation to these subjects, carried into execution, there is a bill now before the legislature, for repealing it. The only reason we have heard advanced in favor of its repeal is, that it has been found impossible to get the people to attend to this duty, and they detest being annoyed by suits and fines, as has been done recently. But the truth is, the public mind is not yet prepared to appreciate the importance of this matter; and we must confess that a law had as well be repealed, as to remain a *dead letter* on the Statute Book.

PRIZE ESSAY OF THE LOUISIANA MEDICO-CHIRURGICAL SOCIETY.

We are requested to announce to the profession, that at a recent meeting of this Society it was resolved to offer a gold medal, of the value of one hundred dollars, for the best essay *on Strictures of the Urethra, with their treatment.* This prize is offered to the competition of the profession in all countries; but the essays must be written in the English or French language. The communications must be accompanied with a letter and corresponding mottoes, to the President of the Louisiana Medico-Chirurgical Society, New Orleans, La., and should be received by the 1st day of February, 1847.

The medical press throughout the country is respectfully requested to give publicity to this notice.

Here is the offer of a splendid prize, and we doubt not it will call forth the competition of great talent. The prize will be awarded at the anniversary meeting of the Society, on the first Wednesday of April 1847.

HOSPITAL REPORTS FOR 1845.

We have selected from the Annual Report of the New Orleans Charity Hostpital for the year 1845, the following statistics, which will probably be interesting to our readers. We expected by this time to have obtained Reports from the large Hospitals of our Northern Cities, so as to have drawn a comparison between them, but have not as yet. We will not delay the publication of our statistics; the comparison can be made at a future time.

Annual Report of Patients in the New Orleans Charity Hospital for the year 1845.

MAIN BUILDING

Admitted: 6136. Discharged: 5446. Died: 563.
Whites Admitted, 5993. Colored do., 143.
Residence in New Orleans under 3 years, 4944.
do. do. over 3 years, 1192.
Number of patients remaining on the 1st Jan. 1846, 401.

PLACE OF NATIVITY.

The United States	1350.
Foreign Countries	4704.
Unknown	82.

FROM THE DIFFERENT STATES OE THE UNION.

New York,	231	Pennsylvania,	217
Louisiana,	119	North Carolina,	39
South Carolina,	26	Ohio,	113
Massachusetts,	76	Kentucky,	66
Maryland.	64	Maine,	43
Missouri,	18	Rhode Island,	15
Connecticut,	26	New Jersey,	22
Indiana,	24	Illinois,	12
Vermont,	14	Delaware,	15
New Hampshire,	24	Tennessee,	40
Mississippi,	11	Arkansas,	2
Georgia,	17	Alabama,	7
Florida,	4	Michigan,	2
Virginia,	100	TOTAL,	1350.

FROM FOREIGN COUNTRIES.

Ireland,	2477	Germany,	710
France,	357	England,	330
Scotland,	128	Spain,	131
Prussia,	141	Portugal,	49
Switzerland.	74	West Indies,	16
Canada,	41	Sweden,	41
Norway,	14	Denmark,	25
Sicily,	9	Italy,	46
Belgium,	11	Holland,	13
Sardinia,	9	New Foundland,	4
Austria,	15	Mexico,	15
East Indies,	4	Poland,	6
Nova Scotia,	10	Venezuela,	2
Africa,	1	Texas,	2
Malta,	6	China,	1
Greece,	3	Island of St. Helena,	1
Russia,	3	New Brunswick,	5
Hayti,	3	Peru,	1
		TOTAL,	4704.

N. B. It is not to be understood that all these admissions are separate individuals; many persons have been admitted two or more times.

PRINCIPAL DISEASES.

The following is a list of the principal diseases for which patients were admitted into the main building :—

	ADMITTED.	DISCHARGED.	DIED.
Ascites	25	21	12
Abscess	41	39	1
Bronchitis	150	137	11
Contusion	194	189	6
Colitis	77	70	21
Catarrh	60	67	4
Conjunctivitis	59	62	0
Diarrhœa	259	221	36
Dysentery	173	149	39
Delirium Tremens	47	40	4
Enteritis	72	52	9
Intermittent Fever	1497	1399	3
Typhoid Fever	154	99	50
Bilious Remittent Fever	165	149	5
Pernicious Fever	5	2	3
Yellow Fever	1	1	0
Bilious Fever	14	13	0
Nervous Fever	8	12	0
Congestive Fever	21	10	0
Scarlet Fever	2	2	0
Gastro Enteritis	72	53	20
Gonorrhœa	87	87	0
Coup de Soleil,	27	10	17
Mania â Potu,	35	46	5
Phthisis Pulmonalis,	164	58	107
Pleuritis,	39	36	6

Pneumonia,	64	45	16
Rheumatism,	346	344	0
Syphilis,	329	293	4
Ulcers,	347	312	1
Wounds of all kinds,	111	101	5
Fractures of all kinds,	73	49	15

LUNATIC ASYLUM.

Admitted, 343. Discharged, 279. Died, 55.

Remaining on the 1st of January, 1846, 60.

N.B. It must be remembered, that the patients admitted into the Asylum are not all lunatics; cases of *delirium tremens*, and others with boisterous delirium are admitted, to prevent disturbance in the main building.

Remarks.—It would thus appear that the total admissions into this Hospital, (main Building and Lunatic Asylum) for twelve months, have been 6479, and the deaths 618, a proportion of $10\frac{1}{2}$ per cent.

This is surely a satisfactory result, more especially when it is considered that during the year, probably not less than *fifty* are admitted in a *moribund state*, entirely beyond the reach of medical aid. The year, it is true, has been a healthy one: that is, we have had no epidemic of yellow fever; but this only renders it more suitable for a fair comparison with Northern hospitals, which are seldom visited by such a scourge. We thus frankly publish the results of New Orleans Hospital practice, and invite comparison with that of any city in the world. We have, upon previous occasions, alluded to several errors and defects in the management of this Hospital, which demand reform; but as the subject is of a local nature, we will not trouble our readers abroad with it.

The numberof annual admissions into this Hospital is very large; but look at the sources from whence they come: first, our large shipping list; second, our immense inland navigation; third, the large scope of country around the city, which sends patients to this hospital from a distance of fifty or a hundred miles. A large number of these patients are not *proper objects of charity*, and the State should not be taxed with their medical expenses; yet they have free access to this hospital. Upon reviewing the States and countries from which the patients of this hospital have come, one cannot fail to be struck with the very small number of natives of Louisiana: viz, 119, out of 6479; but it must not be forgotten that a much larger number, though not natives, are now citizens of the State.

In conclusion, we cannot omit this occasion to remark, that whilst the physicians of New Orleans are attending annually *in the service of the State*, between six and seven thousand invalids, without charge, the Legislature, in its wisdom and liberality, thought proper at its last session to impose a tax upon the Profession.

HOSPITAL REPORTS.

NEW ORLEANS CHARITY HOSPITAL.

Monthly Report for December and January.

MAIN BUILDING.

December—Admitted : Males, 505 ; Females, 94 ; Total, 599.
Discharged : Males, 424 ; Females, 67; Total, 491.
Died : Males, 54 ; Females, 9 ; Total, 63.
Remaining on 1st. January, 401.

INSANE DEPARTMENT.

Admitted : Males, 30 ; Females, 11 ; Total, 41.
Discharged ; Males, 31 : Females, 7 ; Total, 38.
Died : Males, 6 ; Females, 1 ; Total, 7.
Remaining on 1st January, 60.

MAIN BUILDING.

January—Admitted : Males, 422 ; Females, 98 ; Total, 520.
Discharged : Males, 358 ; Females, 76 ; Total, 424.
Died : Males, 55 ; Females, 14 ; Total, 69.
Remaining on 1st February, 418.

INSANE DEPARTMENT.

Admitted : Males, 24 ; Females, 9 ; Total, 33.
Discharged, Males, 16 ; Females, 9 ; Total, 25.
Died : Males, 4 ; Females, 0 ; Total, 4.
Remaining 1st February, 63.

MEDICAL WARDS.

The admissions into these wards have been numerous as usual, and among them many very interesting cases, but we are not supplied with any reports by the attending physicians. In our casual visits to the Hospital we have observed cases of typhoid fever, typhoid pneumonia, erysipelas, phthisis, bronchitis, hepatic abscess, disease of the heart, dropsy, &c.

SURGICAL WARDS.

The admisslons into these wards have been of the customary character. Very few surgical operations have been pertormed this winter. Amongst the most interesting cases in Dr. Stone's wards we have observed two of *traumatic tetanus*, one of which has been discharged cured, and the other is now convalescent ; and one of *empyema*, in which *paracentisis thoracis* was performed, and the fluid discharged ; the patient is now convalescent. These cases will probably be reported at a future time.

In Dr. Wedderburn's wards there has been some interesting cases, but we have no reports. Dr. W. has continued his observations on the local application of sulphate of quinine to ulcers, and will report progress at a future time.

MONTHLY MORTALITY IN THE CITY OF NEW ORLEANS.

December, 1845. Total number of interments, 246.
January, 1846. - - - - - - - - - - - 242.

METEOROLOGICAL OBSERVATIONS NOR 1845.

We have selected from MR. LILLIE's table the following items, which may be worth remembering:—

NO. OF RAINY DAYS.	QUANTITY OF RAIN.
January, 7, (*inches.*)	3.097
February, 3,	1.498
March, 10,	4.671
April, 6,	1.413
May, 15,	4.959
June, 7,	2.795
July, 10,	1.592
August, 12,	1.838
September, 9,	1.430
October, 10,	7.145
November, 6,	8.810
December, (20th,) 11,	5.877.
1ST. QUARTER, 19,	9.266
2ND. DITTO, 28,	9.167
3RD. DITTO, 31,	4.860
4TH. DITTO, 27,	21.832.

Warmest day of the year 23d July; maximum of thermometer, 92° 7′.
Coldest day of ditto 1st December; minimum of thermometer, 25° 0.
Annual range - - - - - - - 67° 7′.

ABSTRACT OF A METEOROLOGICAL JOURNAL FOR 1846.

BY D. T. LILLIE, AT THE CITY OF NEW-ORLEANS

Lat. 29° 57′ Lon. 90° 7′ west of Greenwich.

weekly.	Thermometer. MAX.	Thermometer. MIN.	Thermometer. RANGE.	Barometer. MAX.	Barometer. MIN.	Barometer. RANGE.	COURSE OF WINDS.	Force of Wind in ratio 1 to 10.	RAINY DAYS.	QUANT. OF RAIN. INCHES.	QUANT. OF RAIN. THOUSANDTHS
1845.											
De. 27	63.5	27.0	36.5	30.44	30.10	.34	N.W.	2½	2	0	325
1846.											
Jan. 3	66.7	32.0	34.7	30.36	30.02	.34	N.	2½	1	0	025
10	59.5	32.5	27.0	30.37	29.86	.51	N.W.	3	1	2	325
17	60.0	35.0	25.0	30.28	29.83	.45	N.W.	2½	1	0	910
24	58.0	39.5	18.5	30.54	29.51	.03	N.W.	3½	2	5	505
31	70.0	43.0	27.0	30.44	29.91	.53	N.W.	2½	1	0	430
Feb. 7	61.7	45.0	16.7	30.28	29.88	.40	N.W.	2½	2	1	140
14	61.5	40.2	21.3	30.23	29.81	.42	S.W.	3½	4	3	560
21	64.2	40.0	24.2	30.26	29.90	.36	W.	2½	0	0	000

REMARKS.—The Thermometer used for these Observations is not attached to the Barometer, but is a self-registering one, and is placed in a fair exposure. Regular hours of observation, 8 A. M., 2 P. M., and 8 P. M.

The Barometer is located at an elevation of 19 feet above the level of the ocean, and is suspended clear of the wall of the building.

The Rain Guage is graduated to the thousandth part of an inch, and the receiver is elevated 40 feet from the ground.

THE NEW ORLEANS

MEDICAL AND SURGICAL JOURNAL.

MAY, 1846.

Part First.

ORIGINAL COMMUNICATIONS.

Art. I.—*On the Periodical Maturation and Discharge of Ova, in Man, and other Mammiferæ.* By W. M. Carpenter, A.M., M.D., Professor in the Medical College of Louisiana. [Concluded from p. 572.]

Viewing menstruation as consisting of two principal acts, the maturation and discharge of ova, and the consequent accession of a hœmorrhage from the uterus, it becomes important for us to determine, in cases of suppression of this function, whether the suppression is merely of the hœmorrhage, which is the uterine and sympathetic portion of the function, or likewise extends to the ovarian element of the function, the development and discharge of the ova. The determination of this question, even with a degree of probability, would certainly modify our views in regard to the treatment of the cases. If, in any case, it is probable that ova are formed and discharged, and there is suppression merely of the hœmorrhage, with indications that the health of the patient is suffering, in consequence of the suppression, we are justified in the use of aloetics, and other medicines, usually regarded as emmenagogue. But, if the condition of the patient is such, that the ovarian portion of the function—the maturation and discharge of the ovum—is suppressed, it will be in vain that we resort to these means. We may, in such cases, by irritating the rectum by these means, induce, by proximity, a degree of excitation of the uterine system, and even induce hœmorrhage, but this treatment can avail nothing in correcting the radical lesion, and is frequently most injurious in its consequences.

Unfortunately, we have no means of arriving at a complete and certain diagnosis in these cases; and when the menses are suppressed, we are obliged, in our present state of knowledge, to satisfy ourselves with a degrêe of probability, as to whether the function is suppressed in all its elements, or merely in its uterine element—its external manifestation.

In forming an opinion on cases of amenorrhœa, we are obliged to rely for our diagnosis principally upon rational signs, and observations of the general condition of the individual, and to keep in view the known effects of such condition, in producing the modifications of the functions under consideration. It is known that, in persons enjoying good health, in plethoric persons generally, and in most acute forms of disease, the ova are periodically discharged, and consequently we may generally infer that the suppression, in such persons, merely extends to a cessation of the elimination of the external discharge, and we adapt the treatment accordingly.

But in many other cases, the general health and condition of the patient interfering with the general nutrition of the ovaries, prevents the fulfilment of the ovarian function, of the production and discharge of ova, and the menstrual function is then suppressed in all of its parts; and the only thing to be done is to repair the condition of the general system, thus restoring the functions of the ovaries, the fundamental and essential part of the menstrual function.

In the autopsy of women who menstruate regularly, and who have died of any, excepting some chronic forms of disease, the ovaries are found to exhibit Graafian follicles, in all states of development, as well as cicatrices which indicate the continued performance of the ovarian function. But there are some forms of disease which seem especially to affect this function; such are various cachectic conditions in which there is a diminution of the nutritive qualities of the blood, or which result in great general debility of the system, either of which are incompatible with the performance of the ovarian portion of the menstrual function. In persons who have died in conditions such as these, the ovaries are generally found pale, if not atrophied, with a smooth and even surface, or if any large vesicles are found they are pale, and without the injection which indicates a progressive state; and the cicatrices are either wanting, or of such an appearance as to show that they are not recent. Among the diseases which have been observed to produce this complete suspension of the entire menstrual function, are phthisis, chronic bowel complaints, profuse suppurations or other discharges, dropsies, &c., besides various forms of chlorotic disease. Suppression of menstruation being a common concomitant of such affections, and as, in such cases, it has been discovered that the suppression extended to the radical or ovarian portion of the function; and as there is every reason to suppose, by analogy, that those general conditions are competent causes of the suppression, it would always be most safe, in cases of amenorrhœa accompanying such conditions, for us to regard the general condition as the primary one, and the suppression as a consequence. If such be the case, we can only combat the suppression successfully by the employment of such means as will relieve the system of the general condition which has induced the suspension of the menstrual function. Consistently with these views, we find that, in cases of this kind, such remedies act most efficiently as emmenagogues, as have the effect of promoting the general vigor of the economy, and restoriug the physiological balance of the functions. General principles will, of course, guide us in this treatment, which must vary according to the indications to be fulfilled. In chlorotic cases, however, the treatment is almost exclusively *analeptic*, or limited to the use of means directed to the promotion of the assimilative functions. Among these means are tonics, but especially the chalybeates, a suitable, generally a generous diet, free air and exercise, with the use of any other means which may facilitate the attainment of the same end.

In cases of retarded appearance of the menses, in young persons, and in many cases of suppression, these views will be applicable. Raciborski* observes, that, in these cases, "hygiene affords the best emmenagogues."

It has been frequently observed, that many women were subject to frequent suppressions, and some even did not menstruate at all, but were, notwithstanding, in good health, and retained their fresh and rosy complexion. In these cases the suppression was, of course, limited to the uterine part of the function, and their cessation might or might not require treatment. It is certain, that the common emmenagogues are too indiscriminately used, and are frequently resorted to when Nature could have managed the matter as well, or even better. It has been remarked,† that "more than one-half of the Irish and German emigrants who arrive on our shores have amenorrhœa, and it is difficult, through the various treatments adopted, to cause them to return, till they have been residing in this country from six to seven months, when they return naturally."

The same remarks apply equally to many other cases. In such cases I have generally limited my interference to recommending mild purgatives, and hip-baths to be used about the periods when menstruation should come on; and it is only when other consequences are threatened, that I generally resort to more active means.

The great abuse of emmenagogues has arisen from false views of the nature and relations of the menstrual discharge. Bearing in mind the immense diversity of the causes upon which its suppression may depend, and that the function is a compound one, of which the ovarian part may be performed, while the uterine, or external manifestation, is suppressed, or the suppression may affect the entire function, it is easy to see that the indiscriminate employment of emmenagogues in all cases, must give an unfavorable result.

The function of menstruation has been regarded, and is perhaps still regarded by many, as having for its object the elimination of certain injurious or superfluous materials from the system; and its cessation or diminution would imply, according to those persons, an accumulation of these morbific materials in the economy. It will be hardly necessary, after the remarks already made, to say any thing in refutation of this opinion. That plethoric persons may be benefitted by, and even require such a drain, might seem possible, were it not for the fact that many women continue plethoric, and nevertheless continue to enjoy good health after the cessation of the menses.

From what I have read, and from my own very limited experience, I would incline to the inference that the large proportion of the serious consequences which are generally attributed to suppressed menstruation might, with much greater justice, be referred to conditions, either local or general, which a nicer discrimination might likewise regard in the light of causes of the suppression. Viewed in this light, suppression

* Raciborski, op. cit.

† Kennedy on Obstetric Auscultation.

would, in many cases, be regarded merely as one of the symptoms of a morbid condition which, in the progress of its ravages, affected the vital manifestations of the uterine system, in common with those of other organs. The treatment of those primary conditions according to general principles, would be the measure most directly calculated to restore the menstrual function.

Chlorosis has been regarded, by most writers, from the earliest times, as an effect of suppression, or retardation of the appearance of the menses. But when we consider chlorosis in its proper light, that is to say, as a condition characterized by a deficiency in the globules of the blood, the most important of its nutritive elements; and bear in mind the necessary influence of such a condition on the nutrition, and elaborative functions of all the organs of the economy, it is impossible to deny that the suppression or diminution of the ovarian and other reproductive functions is exactly what should be expected from such a condition. But we are not under the necessity of relying exclusively upon theory in this matter, for the pale, smooth condition of the ovaries, and the arrested development of the Graafian vesicles, in these cases, as described by Raciborski,* leave no doubt that the function of menstruation is affected in its radical and essential part, and that this diminution of function is directly referable to the vitiation of the nutritive qualities of the blood.

The above remarks apply strictly to those cases which depend upon conditions of the system in which a general anœmic or debilitated state affects the menstrual function; but there are many other cases to which the same remarks apply with nearly equal force. There exist various gradations of conditions of the general system, characterized by a tendency to local congestions, or rather by a disturbance of the equilibrium of the circulation, and often partaking more or less of the intermittent type. In conditions of this kind, amenorrhœa generally extends only to the suppression of the uterine discharge, and this suppression is probably due to an undue engorgement of the uterine vessels, which affects the secretory functions of that organ in the same manner as engorgement would affect any other secreting apparatus.

The treatment of such cases, to be decidedly successful, demands that our measures should be directed against the primary condition, rather than directly against the suppression itself. In cases of this kind, the preparations of iron, but especially of quinine, produce the happiest effects. The citrate of quinine and iron is the remedy that seems to be best adapted to such cases. The quinine seems to produce its striking effects in consequence of its powerful influence in relieving local plethora, and restoring the equilibrium of the general circulation; and when administered about the time the menses should appear, facilitates the performance of the function, by moderating the periodical engorgement of the uterus, and restraining it within the physiological or secretory limits. I have used no class of remedies in these, and some chronic engorgements of the uterus, with so much advantage as some of the quinine compounds.

The few remarks that I have made in this paper are by no means intended to cover all cases of amenorrhœa, nor to comment on or criti-

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* Raciborski, op. cit., 246.

cise the treatment which has been found, for so long a period, to be so well adapted to many cases; but I wish it to be distinctly understood, that they are made only in reference to the cases and varieties specified.

RELATIONS OF THIS THEORY TO MEDICAL JURISPRUDENCE.

There are cases on record in which the detection of corpora lutea, in post mortem examinations, have been made the basis of criminal prosecutions, and have been received in courts of justice as the most direct and satisfactory evidence of seduction or rape, and even as circumstantial evidence of murder committed to conceal these crimes, and avoid their consequences. It is well known, that according to most authorities, up to this time, it has been supposed that an ovum escaped from the ovary, only as a consequence of coition and conception, and the detection of a corpus luteum was regarded, therefore, as a sure indication of conception. Recent researches, by demonstrating the periodical development and discharge of ova, have shown that the corpora lutea left in cases of fecundation can, by no means, be distinguished from those which remain after the spontaneous discharge of ova. As ova are discharged at each menstrual epoch, we would find them probably after each menstruation.

A recent case occurred in England, in which a *corpus luteum* was found in the ovary of a woman who had been assassinated, and which gave rise to much discussion as to the value of this fact, and to a controversy upon this subject, between Drs. R. Lee and Patterson, which was published in the London Medical Gazette for 1842, the November number, p. 198, and the December number, p. 365.

Cases of this kind are continually liable to occur, from a variety of circumstances. If we were to regard corpora lutea as indicating conception, their detection in post mortem examinations of females might often give rise to or confirm suspicions most unjust to the character of the individual, and injurious to others. It would be regarded as evidence of seduction or rape, and the inference that the deceased came to her death by murder or suicide would be a natural, if not an inevitable one. But, in accordance with the views already advanced, the existence of corpora lutea indicate, merely, that the woman menstruated at a short period prior to her decease, and is really a sign of no value, as regards the probability of conception, or of sexual intercourse.

Another question which sometimes arises in the course of judicial investigation, is, whether a woman may conceive without having menstruated? There are numerous facts of this kind on record, in some of which women bore several children without having ever menstruated; while in one case,* the woman, aged twenty-five, bore a child without having ever menstruated previously, but the menses occurred regularly afterwards. These cases are perfectly explained by supposing the ovaries to elaborate and discharge their ova, while the menstrual hæmorrhage alone is suppressed. In order that conception should occur, the ovarian portion of the menstrual function is the only one that is absolutely essential; the menstrual discharge may be absent, without necessarily inducing barrenness.

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* See Lancet for 1842, the February number.

ART. II.—*Observations on the Nutritive Process, tending to show that it is one of the Forces of the Circulation.* By JOHN HARRISON, Professor of Physiology and Pathology in the Medical College of Louisiana.

[Read before the Physico-Medical Society of New Orleans, Feb. 7th, 1846, and published by request.]

It was soon discovered, by the earlier physiologists, that the course of the blood in the substance of the tissues was under the control of other powers than those of the heart. They observed that the skin became more red, sensitive, and swollen in certain parts, while it remained healthy elsewhere: the heat of the part was also found to be somewhat increased. To this phenomenon they gave the name of inflammation; and it was obvious to them, that more blood existed in the parts when inflamed, than when in a state of health. They laid bare the serous membranes of animals, and, irritating them, saw the blood accumulate at the point of irritation—the spot becoming intensely red and vascular. "Haller, Spallanzani, and others, by means of the microscope, saw the globules advance, recede, move in many different directions in animals with red and cold blood, when they irritated the mesentery, or any other transparent part." * Again, the phenomena of blushing, and the erection of certain tissues from mental emotions, evidenced the same fact. They found, when they cut into an inflamed organ, that the hœmorrhage was far more violent than if the same organ was cut in a healthy state. Now, as the heart, by its pulsation, must send the blood by a single force into all the arterial trunks, we cannot find in that organ, a reason for the partial distribution of blood which occurs in the phenomena above-mentioned;—we must seek elsewhere for an explanation.

What, then, is this cause? By what means or agency is it, that an organ can exert such a control over the arterial blood as to draw that fluid to itself from the surrounding parts, and retain it there? By consulting works on Physiology, even the most recent, it will be seen that the causes of these phenomena lie still in great obscurity. By some, they are attributed to the arteries; by others, to the capillaries. Bichat has explained the phenomena by means of his "Insensible Organic Contractility." † "Every tissue," says he, "is possessed of an organic sensibility, by which it feels the impression of fluids, with which its fibres are in contact; and of an insensible organic contractility, by virtue of which it reacts upon them, to favor their course." ‡ In the present paper I shall endeavor to show that none of these opinions are correct, and that the phenomena have quite a different origin.

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* Bichat: Gen. Anat., vol. ii., p. 37.

† "La contractilité organique insensible est celle en vertu de laquelle les conduits excréteurs réagissent sur leurs fluides respectifs, les organes sécrétoires sur le sang qui y aborde, les parties où s'opère la nutrition sur leurs sucs nourriciers, les lymphatiques sur les substances qui excitent leurs extrémités ouvertes, etc. Partout où les fluides sont disseminés en pétites masses, où ils sont très-divisés, là se développe cette seconde espèce de contractilité."

Recherches, etc., sur la vie et la mort.

‡ Gen. Anat., vol. i., p. 14, *et passim.*

Before, however, proceeding with this subject, let us settle our notions about the arteries and capillaries.

The arteries, as is well known, are composed of three tunics. The external or cellular, and the internal or serous, we may pass over. The middle coat is that upon which the functions of these organs chiefly depend, and the nature of which has been the great subject of dispute. By some it is contended, that this tunic has a power of contraction independent of the action of the heart, and that by this power (seated principally, it is supposed, in the smaller arteries) it is, that the blood vessels are emptied after death;—that inflammation, erection, and analogous phenomena, are produced.

We may here state, that the arteries unquestionably contract in two very different ways, and that the causes of these contractions are very different. One species of contraction in the arterial tunics, is simply the result of elasticity; the heart throws its contents into the aorta, already filled with blood; distension, to a certain extent, of the whole arterial tunics takes place; and, during the diastole of the heart, the tunics returning to their proper situation, press upon the blood, and propel it onward. This is purely a mechanical effect; easily imitated in the dead body, or in tubes similarly constructed. The other species of contraction may be witnessed when any violent impression is made upon the nervous system. Thus, under certain strong mental emotions, the pulse becomes small; that is, the arteries diminish in volume. The same thing may happen from the influence of poisons, or from mechanical injuries.

As of contraction, so also of the expansion of the arterial tunics. They expand (to a slight degree, it is true,) when the blood gushes forth from the left ventricle during the systole of the heart; and they expand also in certain conditions of the body, such as fever, inflammation, &c.; for we have, in these cases, the pulse not only stronger and quicker, but fuller than in health.

But it is obvious that neither of these species of contraction or expansion can explain the phenomena under consideration. The reaction of elasticity is general in the arterial tunics, and cannot, therefore, account for the partial accumulation of blood; and the other species is of a tonic or persistent character,—the whole phenomena being simply a diminution or increase in the calibre of the arteries.

The only action, on the part of the arteries, which it is possible to conceive capable of producing a partial accumulation of blood in the tissues, is that of alternate constriction and relaxation; in other words, of muscular action.

But observation shows that they possess no such action. When laid bare in the living body, all that can be observed is the very slight expansion produced by the blood thrown in from the heart, and the very slight contraction caused by the return of the tunics to their proper situation. Moreover, no stimuli, galvanic or mechanical, can produce contractions in the middle tunic of the arteries, as they do in muscles. To be sure, it has been asserted, that caustic potash, sulphuric acid, &c., will constrict the arteries, if applied directly to their coats: but they constrict them only at the spot where they are applied, and will produce the same effect if applied to arteries preserved for years in alcohol, as likewise to the skin and several other tissues. The constriction is evidently produced by the chemical action of the agent upon the animal substance. As well might we infer that a piece of paper possesses muscular power, because it curls up before a fire.

The constitution, too, and all the properties, physical and chemical, of the middle tunic, differ from muscular fibre: the latter contains phosphorus and sulphur;—the other does not; the first is highly elastic and resistant;—the other, soft and compressible. The organic muscular fibre possesses characteristic striæ upon its surface; the arterial fibre possesses none.

Unable to establish the muscularity of the arteries in the higher animals, comparative anatomy has been resorted to by the advocates of this doctrine. They have instanced the contraction visible in the dorsal vessel of insects; in the *bulbus aortæ* of the frog; in the aorta of fish, &c.; and Dr. Marshall Hall supposed that he had positive proof of muscular action in the arteries, from observing a vessel which continued to pulsate after the heart had been removed from a frog. "But," says Müller, "in this he was mistaken; there is in this situation in these animals a peculiar pulsating lymphatic heart, which is not, however, connected with an artery, but with a vein." It was indeed a strange attempt, this of Marshall Hall, to establish so grave a physiological principle from such obscure phenomena.

As for the other instances mentioned, such as the pulsation of the aorta in fishes, &c., it is sufficient to say that, in reality, they are *hearts*, and are, of course, supplied with muscular fibres.

The impossibility of explaining inflammation and similar phenomena by means of the capillary vessels, and the irreconcilability of the supposed action of these vessels to the facts of observation, have driven many inquirers to the arteries, as the only source left for an explanation; just as those who were dissatisfied with the theory of arterial action, resorted to the capillary vessels.

The coats of the arteries most admirably fulfil their functions. The serous, by its lubricity, facilitates the onward flow of the blood; the cellular gives strength and connection to the whole; but it is upon the middle coat that the most important offices devolve. This tunic consists of a peculiar tissue—the elastic, or yellow fibrous; which, both chemically and physically, is entirely distinct from the white fibrous, or muscular. Its function is to react on the blood during the diastole of the heart, and it performs its office by virtue of its two properties, *dilatability and elasticity*. These properties are most nicely proportioned to each other, for any increase or diminution of the first would be attended with a loss of power in the second, and consequently, with derangement in the circulation. We know, from observation, that the blood continues to flow to the tissues during the interval between the strokes of the heart, and there can be no vacuum between the column of blood and the valves of the aorta. Now, upon these two facts depend the rhythm and regularity of the circulation; and they are owing to the properties of the elastic tissue. Were the arteries rigid tubes, or even as dilatable as the veins, they could not perform their functions.

Facts, therefore, are against the arterial theory: let us now turn to the capillaries.

These vessels may be considered either as the termination of the arteries, or the commencement of the veins. It was supposed by Wedemeyer, that "the inner coat of an artery diminishes in strength and thickness along with the fibrous coat. At length, both gradually terminate altogether in membraneless canals formed in the substance of the tissues. The blood in the finest capillaries no longer flows within actual vessels, whose parietes are formed by a membranous substance distinguished from the adjoining cellular tissue by its texture and compactness, but in simple furrows or canals, whose walls are formed by the surrounding cellular tissue."

According to this view, the blood flows directly from the arteries into the tissues; but later observations have shown that Wedemeyer was in error. Müller adduces against this opinion, "the facts that fluids injected into the arteries pass into the veins without extravasation, and that currents cross above and below each other without uniting. The number of the currents, and, indeed, the smallness of the islets of solid matter between them in the pulmonary membrane of the frog and salamander, also tend to prove that membranous tubes must exist; for these small islets would otherwise be themselves sometimes involved in the currents. But there are also direct means of proving the existence of the membranous tubes around the capillary streams. For this purpose, we must select a very delicate parenchyma, which easily softens and dissolves in water, so as to leave behind the net-work of capillaries. In a piece of the cortical substance of the kidney of a squirrel, which had been laid in water for a short time only, but long enough to have become softened, the capillary vessels which are interlaced around the tubuli uriniferi appeared to me, when I examined them by the microscope, to be independent parts. In the choroid, iris, and ciliary processes, the capillaries are still more evidently independent parts."

The capillaries may therefore be regarded as very small arteries reduced to their serous coat, anastomosing thousands of times in a very small space, and finally ending gradatim in the smaller veins. But between these capillaries—in those little islets or spaces of the net-work, what exists? Surely and unquestionably the solid components of the tissues.

From the time of Cullen, and perhaps earlier, it has been common to hear of nutrition and secretion as being performed by the action of vessels; inflammation, with all its consequences, is attributed to vascular action; the secretion of bile and urine—the deposition of all the tissues, are said to be owing to the action of the capillary vessels; "vascular action is modified by nervous action;" &c. &c. But the capillaries are merely conduits for the blood; they are blood vessels, and differ from the larger only in possessing a smaller caliber, and tunics of a slighter texture. We know that the tissues and secretions are formed from the blood; in other words, that the arterial blood undergoes complete transformations, the products differing from each other in different parts of the body. We know, besides, that the oxygen taken in by the respiratory apparatus is perpetually consuming the body; for if food be withheld, the animal wastes away rapidly, and finally perishes. Now, can any man's imagination conceive how mere motion in a set of vessels—complicate and vary that motion as you please—can convert blood into bone in one part, into nervous matter in another, and so on? The action is mechanical, and the effects must be so likewise. The true results, however, are chemical.

Upon this subject, let us hear Broussais.* "Since," says he, "it is proved that fluids penetrate every where, to say that the blood is always enclosed in these vessels, (the capillaries) is to assert that the body of animals is altogether vascular, which is repugnant to our belief. Undoubtedly there is a capillary net-work, which penetrates all parts; but when these capillaries have reached a certain degree of thinness they disappear, and the blood they contained is actually extravasated. It circulates in the interstices of the fixed animal matter, (which is every where porous) no longer in large masses, nor even in small columns, but molecule by molecule, in immediate contact with those of the fixed matter, and it is there that the phenomena of nutrition, composition, and decomposition must take place; and there also is verified, as respects living bodies, the axioms of the chemist, '*Corpora non agunt, nisi sint soluta aut fluida.*' It is by the successive diminution of the filiforme structure that nature accomplishes this purpose, and we may conceive the last can no longer be organized coats, themselves containing other vessels. A glance at the different orders of animals ought to throw some light on this question.

"The lower order of animals, such as the infusoria and the polypi, afford no evidence of vessels. They are formed altogether of a homogeneous and porous animal matter, always identical in its organization, whatever may be its size. It absorbs and admits into its interstices the nutritive materials; it appropriates them, throws off the superfluous part, and secretes its calcareous phosphate, without requiring the aid of vessels or nerves. Behold, here, the type of the parenchymatous system, or of the proper tissue of the organs: it is from this that it is necessary to set out, in order to form an idea of the circulation."

Again: "The small cylinders which form the capillary net-work are immersed in the midst of animal matter not vascular; they are themselves formed of it; they are to pour out in the interstices of its molecules the fluids necessary to its nutrition and functions; they always furnish it; they receive continually the effete portion, which changes the quality of the circulating blood, while accelerating greatly its progress towards the veins."

We cannot well understand, from these extracts, the ideas which Broussais had formed concerning the structure of the capillary vessels. He says in one place, that, when "they have reached a certain degree of thinness they disappear, and the blood they contained is actually extravasated." This would lead us to suppose that he considered each capillary vessel as ending in a fine point, pouring out its contents into the parenchyma of the tissues—the blood, after being changed in the nutritive process, finding its way into the veins. But from what has been said, we find that the capillaries are not so constructed: we know that they are tubes continuous from the arteries to the veins.

The phenomena of endosmose and exosmose were not understood in the time of Brouissais.

Then, the question arises, that, as the capillaries are formed of mem-

* Physiology—"functions of the capillaries."

branous cylinders continuous from the arteries into the veins, how is it that they are able to pour out into the non-vascular matter the fluids necessary for nutrition, and to receive back the effete portion?

The permeability of membranes to liquids and gases, is a fact now universally recognized. The oxygen, and the liquid portions of the arterial blood necessary for nutrition, penetrate intermolecularly the membrane of the capillary vessel at the same moment that the effete liquid portion penetrates it in a contrary direction. In short, there is a simple play of endosmose and exosmose; the blood having lost certain principles and received others—being now venous blood, which is swept onward by the *vis à tergo* in its course to the heart. In other words, a similar but reverse action takes place in the systemic capillaries, as contrasted with those of the lungs. In the latter, oxygen penetrates inwardly, and carbonic acid gas outwardly; in the former, oxygen penetrates outwardly, and carbonic acid gas into the vessels.*

According to the authority of Prévost and Dumas, as likewise of Müller, quoted in the foot-note, the conversion of blood into muscular substance and nervous matter, must take place *extra vasis*. The vessels themselves, then, can have nothing to do with it, except in so far as they are channels by which the blood arrives in the neighborhood of the organic molecules which compose the tissues.

The action of a vessel—be that action what it may—is mechanical, and mechanical only. It might possibly act upon the current of the circulation, increasing or retarding it, but in no other way. In other words, it might act upon the mass; but the change, undergone by the blood, is molecular, and therefore of a chemical character.

Certain phenomena—such as pallor of the countenance under certain moral emotions, and the coldness and pallor of the whole surface from the effects of poisons or mechanical injuries—have been appealed to as proofs of a power of contractility on the part of the capillaries. But the same objection may be made here as in the case of the arteries. The contraction is tonic and persistent—not alternating with relaxation; and therefore the whole effect is simply a diminution in the caliber of the vessels, and of the quantity of blood in them.

* "The muscular fibres are held together by an adipose cellular tissue, and are traversed in various directions by vessels and nerves which seem to pervade the muscle, without having any easily observable connections with it. We cannot now enter into the history of the circulation peculiar to these organs, and shall therefore only observe, that if there exists a material communication between the muscular fibres and the blood-vessels, it can only be conceived on the supposition of transudation taking place through the coats of the vessels. The passage from the arteries to the veins is easily traced, and does not present the extreme divisions which would be indispensable to the nutrition of the organ, if it took place as it is generally imagined."
—*Prévost and Dumas.*

"All these different elementary tissues, glandular ducts, muscular fibres, and nervous fibrils, are surrounded and connected together by a net-work of capillaries. The primitive fibres of muscles, and those of the nerves, are not themselves traversed by any blood-vessels, for they are smaller than the finest capillaries. In examining recent and well-injected specimens of these parts, no other capillary vessels are seen than those which are distributed in the interstices of the primitive fibres, and it is the same with regard to the minute ducts of glands."—*Müller—Physiology.*

Such a species of contraction would unquestionably explain the pallor, but in a few hours the very reverse may take place; we may have inflammation and fever—the parts previously cold and pale may become red and hot—nay, certain other moral emotions, such as shame or sexual desires, may produce blushing and erection. Now, how explain these facts on the hypothesis that the capillary vessels are the agents producing them? It may be said that the capillaries, in this case, expand; but what causes their expansion? And even should we admit such an explanation, it would by no means get us out of the difficulty. We know that the capillaries are enlarged under two very different circumstances—for instance, in passive congestion and in inflammation. Yet the phenomena and effects of these two conditions, how different! At any rate, neither expansion nor contraction of the capillary vessels, if they be (as all observation assures us they are) of a tonic and persistent character, can ever explain the emptiness of the arteries after death; the fuller and more bounding pulse of the radial artery leading to a hand afflicted with whitlow, and many other phenomena. Expansion of these vessels may explain congestion, but not inflammation.

Bichat mistook the capillary vessels for the immediate seat of nutrition and secretion—and committed a fundamental error; for, having in his mind this notion, and being besides convinced, from observation, that the blood in the tissues obeyed other powers than those of the heart, how could he conceive of action there in any other way than he did?—that is, that there occurred an alternate contraction and relaxation of these vessels? A vessel, to act independently of the heart, could have no other action.

But mere contraction and relaxation of a set of vessels, however varied they may be, can never explain why bone is deposited in one place, nervous matter in another, and muscular fibre, &c., in a third. To reach the explanation of these facts, the "Organic Sensibility," and "Insensible Organic Contractility," were introduced.

Of the first it may be sufficient to say, that, to talk of sensibility which results in no sensation—of a property, in other words, that never manifests itself in the effect from which it derives its name—is a vile abuse of language. We know that, in the higher animals, the property of sensibility is possessed by a certain portion of the encephalon, and by that only. If a nerve going to any part be divided, we may burn, prick, or tear the tissues without a sensation being the result: they cannot, therefore, possess sensibility, in the proper sense of the term, for it is a word merely expressive of the capability of a part to experience sensations.

It is true, that when mechanical or chemical agents come in contact with the tissues, certain changes are effected upon those tissues. But these changes may or may not be accompanied by sensation. To say that all such effects are dependant upon "organic sensibility," is going beyond the province of observation. We might just as well insist that a needle possesses sensibility, and therefore experiences a sensation, when it is attracted by the magnet.

But how much more would the adoption of the "insensible organic contractility" aid us in explaining the phenomena now under consideration—such as blushing, erection, inflammation, &c.? Suppose we laid bare the peritoneum of an animal, and irritated the part: we should see the blood rush to the spot, and accumulate there. But how could this happen if the vessels underwent contraction? It is plain, that if all the tissue contracted simultaneously, the blood would be forced back into the arterial system, as much as it would be forced onwards into the veins: the action would be precisely analogous to that of squeezing a wet sponge in the hand:—the fluid would flow out equally at both ends.

The emptiness of the arteries after death has been more than once alluded to, and it is a very instructive phenomenon;—one that must be explained by other powers than of the heart, arteries, or capillaries. Now, if a certain set of vessels contracted in their full extent and then relaxed, it is evident, that the contraction would as much drive back the arterial blood, as the relaxation would permit its entrance into the tissues. Arnott, who was too well acquainted with the laws of physics not to perceive this consequence, has given us another explanation. "A *muscular* capillary tube, strong enough to shut itself against the arterial current from the heart, is also strong enough to propel the blood to the heart through the veins, even if the resistance on that side were as great as the force on the other; for, if we suppose the first circular fibre of the tube to close itself completely, it would, of course, be exerting the same repellent force on both sides, or as regarded both the artery and the vein. If, then, the series of ring fibres forming the tube were to contract in succession towards the vein, as the fibres of the intestinal canal contract in propelling the food, it is evident that all the blood in the capillary would thereby be pressed into the vein towards the heart. If, after this, the capillary relaxed on the side of the artery, so as to admit more blood, and again contracted towards the vein as before, it might produce a forward motion of the blood in the vein, independently of the heart."*

So much for the "insensible organic contractility" of Bichat, as an hypothesis adequate to explain the phenomena we are now considering. As such, I have been examining it. It now remains for me to add, that if it be insensible—that is, imperceptible—it is, of course, beyond our powers of observation; and it has been aptly asked by Magendie, "If it be insensible, who discovered it?" In truth, any species of action on the part of the capillaries, considered as mere vessels, is totally inadequate to explain the facts. Adopt the opinion, that they are the causes of the singular motions and phenomena observable in the fluids which permeate the tissues of living beings, and we must add hypothesis to hypothesis, as we have just seen in the quotation from Arnott; for, beginning with supposing contraction to take place in a set of small vessels, he is obliged to suppose further that they afterwards relax, then that they are muscular, and then, that they contract in a vermicular manner.

But all observation is opposed to any such hypotheses; for if we except the tonic and persistent contraction and expansion already spoken of, these vessels present no other. "The capillaries," says Wedemeyer, "do not possess the power of contraction in their parietes, at least, I have never been able to develope it by the strongest chemical or mechanical stimulants, or by galvanism." So, likewise, Müller: "The

* Elements of Physics.

pulsatory motion of the blood in the capillaries" (seen in animals suffering from great exhaustion) "cannot be attributed to an action in these vessels themselves, for when the animal is tranquil, they present not the slightest change in their diameter."

In this opinion, a host of other physiologists concur. In the foot notes I have cited the remarks of Spallanzani and Magendie.* But, in truth, any one who is willing to take the trouble, may satisfy himself of its correctness by observation.

Finally, some physiologists have contended that the blood is endowed with a power of self-propulsion. It is hardly worth while to spend time in criticising such opinions. Their source is obvious—the impossibility, which their authors labored under, of explaining the phenomena in any other way. Any thing, to be *self-propelling*, must possess volition and muscular power.

From the foregoing remarks, it follows, that the phenomena under consideration cannot depend upon the action of the heart, arteries, capillaries, or upon a self-propelling power in the blood. We must therefore seek for other causes.

To account for the phenomena already mentioned, and others to be hereafter reviewed, the theory proposed should be of such general principles as to embrace all living beings; for the existence of a nutritive fluid, and solid or semi-solid tissues, is essential to all such beings: without their co-existence, there can be no such thing as life. We see, moreover, phenomena analogous to those which now occupy our attention, in beings in which no vessels whatever exist. The theory, then, which is manifestly to be preferred, is that which, upon the same principles, explains the ascent and descent of the sap of plants, the emptiness

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* "On ne peut nier que l'hypothèse de l'attraction et de la force oscillatoire ne soit très ingéuieuse: mais si les vaisseaux éprouvaient les effets de ces deux puissauces, c'est-à-dire une alternative de contraction et de la dilatation, et une accélération de vîtesse, l'un et l'autre phénomènes devraient être sensibles; néanmoins le resultat de *toutes mes observations* établit une parfaite immobilité dans les parois des rameaux capillaires, et un retard plutôt qu'une augmentation de vélocité dans le fluide qui s'y meut."—*Spallanzani—Experiences sur la Circulation: ouvrage traduit par J. Tourdes.*

"Les grosses artères ne présentant pas de contraction, on devait croire que les petites n'en auraient pas présenté davantage; mais comme parmi les physiologists qui rejettent l'irritabilité des troncs arteriels, les uns, comme Haller, ne parlent pas des branches, les autres leur accordent la contractilité, il fallait soumettre cette question à l'expérience; or, ces petits vaisseaux, comme les vaisseaux plus grands, sont restés parfaitement immobiles sous l'action du scalpel, des caustiques et du courant galvanique.

"L'irritabilité n'existe donc ni dans les grosses ni dans les petites artères. Quant aux dernières divisions artèrielles, comme les vaisseaux qui le forment sont si petits, qu'ils ne tombent point sous les sens, au moins dans l'état de santé, personne ne peut affirmer ni nier qu'ils soient irritables, cependant, si on s'en rapporte à l'analogie, on doit croire qu'elles n'ont aucun mouvement sensible. Dans les animaux à sang froid, en effet, il est facile de voir le sang circuler dans ces vaisseaux, et même passer dans les veines; or, les vaisseaux eux-mêmes n'offrent aucun indice de contraction."

—*Magendie—Notes sur Bichat.*

or fullness of the arteries after death, and, in short, all other like phenomena. It must embrace vegetable life as well as animal.

It is an axiom in philosophy to refer the explanation of phenomena to general principles known to exist, when we can do so consistently with reason and fact, rather than admit the introduction of hypotheses, however well they may explain the phenomena. It has been proved, I hope, that the hypotheses heretofore called in to explain certain phenomena which occur in the tissues of living beings are inadequate, and opposed to observation. It will be seen presently, that I have introduced no hypothesis whatever; and that, if the explanation be erroneous, the error consists in the misapplication of known principles.

Ascent of Sap.—One of the most striking phenomena in vegetable life, is the ascent of the sap. Plants may be divided into two classes—the cellular and the vascular. The cellular plants are composed of closed vesicles or cells, through which, or between the interstices of which, the sap coming from the roots percolates, and passes to all parts for the purposes of nutrition, secretion, &c. The vascular vegetable, as its name denotes, possesses vessels. These organs are the remains of old cells, and are formed by the partitions between the cells having been broken down. Those which carry the ascending sap are found in the *alburnum*, or new-wood, and without branching go directly to the leaves; those which carry the descending sap, inosculate frequently together, and are found in the *liber*, or thin layers of bark. Cellular vegetables are all of low stature; the class being comprised of such plants as mushrooms, mosses, &c.: hence, the distance the sap has to flow from the root to the top is exceedingly short, when compared with most of the vascular plants. The vessels, therefore, in vascular vegetables serve the purpose of permitting a freer and less obstructed flow of sap than can take place in cellular plants.

In vascular vegetables the sap ascends through the ligneous portion, having been absorbed from the earth by the extremities of the roots, which extremities are composed of areolar tissue, and are called *spongioles.* The sap, imbibed by the spongiole, ascends through the alburnum and enters the parenchyma of the leaves. Here it undergoes a change from the actions of the atmosphere, heat, and light; and becomes the *descending* sap, which is the nutritive fluid of plants, and therefore analagous to the arterial blood of animals. This fluid descends along the thinner layers of the bark, passes off laterally into the lignine or woody portion, and by so doing mingles in part with the ascending sap. Another portion, continuing in the bark, passes down to the extremities of the roots: by this course, it furnishes material for the nutrition of the plant, and for all its various secretions.

This, then, is a short account of the phenomena. By what agency is it that the sap ascends, against the principle of gravity, to the top of the plant? By what other agency is it, that the nutritive fluid courses along the bark, and thus interrupts not the flow of the ascending sap? Attempts have been made to explain the facts by that property which all plants, and indeed many minerals, possess, of absorbing water, and which is called by the French, "hygroscopicité." Attempts have also been made to explain them by capillary attraction. Dutrochet explained them upon the principles of endosmosis and exosmosis. But there is one reply to all these explanations, which is conclusive. The capillarity, hygroscopicity, and powers of endosmosis of a dead plant are as great as those of a living one: the sap, therefore, should ascend in a dead plant, if it were placed in water, which is not the case. These explanations failing, it was said that the sap ascended by the *vital* action of the plant; which is but an indirect way of saying that we are ignorant of the whole matter. As to Bichat's "Insensible Organic Contractility," the same objections exist here, which oppose the reception of that hypothesis when applied to the explanation of analogous phenomena in animals. A solution of the problem is, therefore, yet to be sought for.

Bodies, we all know, attract other bodies: the attraction of gravitation is an instance in point. But attraction is not confined to masses, it likewise occurs among the particles of matter. When evidenced in homogeneous matter, it is termed *cohesion;* and is greatest in solids, less in liquids, and least in gases. When evidenced among the particles of heterogeneous bodies, provided no change of properties takes place, it goes under different names; such as *aggregation*, *solution*, *imbibition*, *absorption*, *adhesion*, &c. We have many familiar examples of this species of attraction; and it may occur in various ways, as between solids and liquids—such, for instance, as the imbibition of water by dry wood or dry bladder; or, between the particles of liquids of different specific gravity—such as alcohol and water; between gases and liquids; between gases and solids; or between gases themselves;—and this is well evidenced if the gases be of different specific gravity, such as carbonic acid and hydrogen.

When a change of properties takes place among the particles of heterogeneous matter, their attraction is termed *chemical affinity*.

Suppose, then, an attraction exists between a certain liquid and certain solids; and suppose, further, that the solid is penetrable by the liquid, as we know most organic solids to be; what will take place when they are brought in contact? Why, if the solid is capable of being dissolved, a solution will be effected; if it be insoluble, the liquid, to a certain amount, will be retained there by the force of attraction exerted upon it by the solid. To be sure, under a change of circumstances, the liquid may be again taken from the solid, but this is effected by other forces similar in nature.

In these remarks we are merely putting a case of simple absorption or solution, that is, a case in which no chemical change is effected upon the particles of either liquid or solid. But it is easy to conceive that a chemical change may take place between the liquid and solids—nay, that, in thousands of instances that might be mentioned, it does take place.

We may now put the final case:—suppose that, in the chemical action between the liquid and solids, the fluid has free access to the solids; what will be the results? They will depend upon the products of the chemical action: if they be gaseous, they will fly off and escape; if liquid, they will also be driven off, provided there be means for conducting them away; if solid, they will remain fixed.

Why, under favorable circumstances, gases should leave the point of action, requires no explanation; but it may reasonably be asked, what is the force driving off the product when it is liquid? We answer, that it is the chemical action going on between the first-named liquid and the solid particles. The particles of both combine under a strong force of attraction; the liquid, therefore, being free to move, is drawn to the solid, and as particle after particle undergoes the change, and as the particles of homogeneous liquids attract each other by the force of cohesion, it is plain that there must be a molecular movement towards the point of action throughout the whole extent of the liquid. It is this current of the liquid about to undergo the change, which drives before it that which is the result of the chemical action, and which, of course, has no longer any attraction for the molecules of the solid. We may, therefore, (to quote a passage from a work published by me in January, 1844) lay it down as a law, "*that when liquids and solids act chemically upon each other, the absorption of the liquid by the solid will be proportional to the intensity of the chemical action; for it is plain, that the velocity with which the liquids flow to the spot wherein the chemical change occurs, will be in exact ratio to the rapidity with which it is removed; and as it is removed only after it has changed its chemical constitution, it is clear that the absorption must be proportional to the action by which that change is effected.*

It may now be asked, if there be any phenomenon in the inorganic world in which the postulates we have asked are fulfilled? It is answered, yes, and a very familiar one. What, I may ask, is the cause of the ascent of oil in the wick of a lamp? An obvious answer is, "Capillary attraction." But this is a very unsatisfactory reply, because, if the lamp be not lighted, the oil will not ascend, after the wick has been once soaked in it. The real cause of its ascending, then, is the combustion going on in a single spot—the extremity of the wick. The capillarity of the wick is only a secondary agent. Therefore, arrest the combustion—put a stop to the *chemical action* going on, and the oil (the liquid undergoing the chemical change) will no longer ascend: and we shall find, too, that the velocity with which the oil ascends is just in proportion to the intensity of the combustion. In oxygen gas, it ascends far more rapidly than in atmospheric air.

We have now to apply these principles to living beings. Are there any chemical actions going on in a living being?—and do those actions take place between a certain liquid and the solids? It is answered, yes, and it is precisely that action which constitutes the difference between a living being and a dead one: it is the Nutritive Process.

It cannot be expected of me, in this place, to enter upon the fundamental truths of physiology. All acquainted with the subject in the slightest degree, know, that from the descending sap of vegetables, the plant is formed, and increases in size; and that, from the arterial blood of animals, the tissues are formed; and that the body increases in size from birth to adult life. We all know, that if the nutritive fluid be withheld, the phenomena of life cease sooner or later. But I may remark, that this great process is essentially different in plants and animals. In the former, it consists simply in the conversion of the descending sap into the tissues and different secretions of the vegetable; in other words, of assimilation only. In the latter, a process of decomposition is going on in the solids at the same time with assimilation. The solids are converted into carbonic acid, water, and the different products we find in the urine, and are removed from the system as excretions. This waste is effected by the action of oxygen gas, taken into the blood during respiration.

I have said that the nutritive process is a chemical one. I must aver, that I cannot conceive it can be any thing else; for what is a chemical action? It is action and reaction among the molecules of matter, resulting in a change of properties in one or more of the substances brought in contact. It is thus distinguished from sciences which treat of the action of matter in masses, or in other words, from Physics. Now, surely no one will maintain that the different tissues and secretions of animal and vegetable life result from any sort of mechanical action. From the sap we have starch, lignine, oils, resins, &c.; from the arterial blood we have bone, cellular tissue, nervous matter, bile, urine, &c. These substances are not to be found in the descending sap or the arterial blood. They are formed, to be sure, from those liquids, but are not found, as such, in them. How, then, could they be formed, except by new combinations of the principles of the liquids—by transformations occurring among their particles; in other words, by chemical action?

It has, indeed, been said, that the *Vital Principle* performs all these wonders. Now, although I consider the introduction of such an hypothesis as a flagrant breach of the most simple rules of logic, it matters not, for our present purposes, whether it be admitted or rejected. The transformation of sap and blood into the tissues and secretions, are facts derived from observation, and whether they be effected by the agency of the Vital Principle, or the ordinary forces of matter, is here a matter of no consequence.

Let us now return to the ascension of sap. Observe the trees of the forest at the commencement of Spring. As soon as the influence of the sun is felt on their frame, the sap, which had almost ceased to flow during the winter, is called to the boughs and extremities in the greatest abundance. Bore a hole into the woody portion, and this will be made apparent. But in the winter, if this experiment be performed, no such effects will follow.

The influence of Heat over chemical phenomena is not well understood, because we do not positively know what heat is,—whether it be material; or, as some have supposed, the mere evidence of matter in action. We know, however, that it exerts a powerful control over all chemical phenomena, organic or inorganic. Sulphur, for instance, will not combine with oxygen under ordinary circumstances; but combines readily if heat be present. The like may be said of phosphorus, hydrogen, the metals, &c. So likewise does heat exert a powerful control over the bodies of animals, as is very plainly evidenced by the pallor of the skin when long exposed to cold; by the ruddiness which ensues when heat is applied in a moderate degree, as when we hold the back of the hand before a fire; by increase of perspiration in warm weather; and by many other phenomena.

As soon, then, as the warmth of the Spring is applied to the material of a living plant, those chemical actions, constituting the nutritive process, are many degrees increased. What, from the principles laid down, must necessarily be the consequence of this? Assuredly, an afflux of the fluid to those parts in which the increase of action takes place. Yet this augmentation in the intensity of the nutritive process may be general or partial. We shall see that it is greater in certain parts of the plant than in others.

The sap ascending in the spring consists principally of water and a few salts derived from the soil. On its way, it meets with granules of starch deposited in the tubers, pith, bark, and other parts of the plant, by the descending sap of the preceding year. This material is converted into gum or sugar, or both, and thus becomes soluble. Taken up in solution, it constitutes the nutrient principle for the cells of the buds, which finally are developed into flowers and leaves. These developments take place in a surprisingly short time. Now, if we consider the number of leaves and flowers which some plants put forth, together with the rapidity of their development, we may gain some notion of the intensity of the chemical actions going on. Does not this well explain why the flow of sap is so much greater before the leaves are developed than afterwards? The leaves are put forth with great rapidity; but after their development the ascending sap is converted by them into the nutritive fluid, which is engaged in the general nutrition of the plant, and the formation of its various secretions—results which are accomplished by much slower actions. Therefore, the rapidity with which the sap ascends is proportional to the intensity of the nutritive process.

After the leaves are put forth, and the supply of starch exhausted, the plant must of course be supplied from another source with its nutritive fluid. This is furnished by the agency of the leaves themselves. Under the influence of the sun's light and heat, they decompose the carbonic acid gas of the atmosphere; the carbon unites with the ascending sap; gum and sugar are formed, and the nutritive fluid (the descending sap) is prepared. It must be obvious, from what has been said, that this chemical action between the ascending sap and carbonic acid gas is calculated to produce the same effects as those which the nutritive process—a similar chemical action—produces elsewhere; namely, an afflux of the ascending sap to the seat of action.

And now ensues another beautiful result, flowing, of necessity, from the principles laid down. The influence of light in effecting a union between carbon and water, is exerted on the upper surface of the leaf where the green matter is in the greatest abundance, and which, indeed, is necessary to the effect. Near the under surface, contained in branching vessels, is found the descending sap, destined to nourish the plant. This sap flows downwards to the farthest extremities of the roots. What is the power by which it descends? It was once supposed that it descended by the mere force of gravity; but the fact that in certain plants, such as the weeping willow, for instance, it at first really *ascends*, was sufficient for the overthrow of such a conjecture. There are two causes for the descent of the sap; first, the nutritive process, by which it is converted into the various solids that compose the plant; and, secondly, the *vis à tergo* of the ascending sap. The descending sap has no attraction for the carbon of the atmosphere; the ascending sap has, and in consequence is attracted into the leaf. In other words, the descending sap is the ascending sap after having fulfilled its affinities. The ascending current must, therefore, drive the descending before it.

I have alluded to the ascension of oil in the wick of a lamp; the upward flow, we have seen, is due to the chemical action—to the combustion. If we increase or diminish the intensity of the chemical action, the rapidity of the current up the wick is increased or diminished. It is the same with plants; for if the trunk be removed (the seat of nutrition), the flow of sap is arrested. However fast the flow of sap ascends in the tree, scarcely any flows from the stump after the tree is cut down. The cases are perfectly analogous; in both (though by different means), those chemical actions on which the phenomena depend, are arrested.

Moreover, examine attentively those motes of dust which are usually found in oil, and it will be seen that, during the combustion of the wick, they display all those phenomena described by microscopic observers as occurring in the Chara Hispida, the lesser Celandine, and other plants; as, likewise, in the capillary vessels of animals. They will be seen to advance, recede, and move in many different directions, accordingly as they do or do not meet with obstructions; just as Haller and Spallanzani saw the globules of the blood move about in the capillaries. Extinguish the lamp, and all this motion is put an end to.

We are told by De Candolle, that branches of willow, as likewise those of many other trees, if cut and placed in water in an inverted position, will absorb the fluid; and, that the liquid will ascend in a direction contrary to the natural one. Here is a fact irreconcileable with the hypothesis of contraction, but easily explicable upon the principles now advocated: for, it is plain, that the water must flow to the spot where it undergoes its chemical change. With the lamp it is the same—the oil must ascend, whichever end of the wick be fitted in the lamp and set on fire.

Before concluding this subject, it may be proper to advert to an experiment of Hales; which, at first sight would appear adverse to the principles we are advocating. "In the beginning of April, he cut off a vine stem at the distance of thirty-three inches from the ground. The stem had no lateral branches, and its cut surface, which was nearly circular, had a diameter of seven-eighths of an inch. To this section, he adapted a reversed syphon; and things being so disposed, he poured in a quantity of mercury, which, after a time, and from the effect of the pressure exerted by the sap as it escaped, rose in one of the arms of the syphon, and remained stationary at the height of thirty-eight inches above its original level. This column of mercury, it is obvious, represents a pressure very much greater than that of our atmosphere,"*

There are three points in this experiment to be attended to. First, that it was made in the beginning of April. The ascending sap, at this period, by taking up the gum and sugar into which the starch is converted, constitutes the nutritive fluid of plants. Secondly, that as the nutritive process goes on between this fluid and the rest of the plant, it necessarily follows, that expansion of the tissues must take place, because the nutritive fluid is strongly attracted into the parenchyma of the solids, and therefore acts as a distending force. When the top of the vine is cut off, the fluid, pressed upon by the elastic reaction of the tissues, naturally escapes at the point of least pressure. Similar phenomena occur in

* Boussaingault—Rural Economy.

animals. Thus, we are told by Kaltenbrunner, that "when a large artery is divided, a considerable hœmorrhage ensues from the two ends, and the blood of the neighboring arteries is seen moving towards the wound, as towards a centre." This is doubtless due to the elastic contraction of the arterial tunics upon the blood. Thirdly, that as the starch is resolved into gum and sugar, there exists in the vesicles of the plant, and also in the intervesicular spaces, a liquid of a different density, and of a different chemical constitution from that out of the plant; that is, from that existing in the soil. From the laws of endosmosis, what must be the effects of such a state of things? We may read the answer in the experiments of Dutrochet. He found, that, with syrup and water, the column of mercury in his endosmometer was sometimes as high as forty-five inches. He calculated that a syrup of 1.3 density would produce an endosmose capable of raising a column of 127 inches of mercury, or the weight of 4½ atmospheres. But no membrane can support this weight. Dr. Mitchell found that the weight of a column higher than sixty-three inches broke the membrane, yet at the time this occurred, "the weight of the column did not seem to very sensibly affect the rate of entrance."

We must now turn our attention to animal life. Animals, at least those high in the scale of life, differ from plants in possessing a mechanical agent—the heart—which propels the blood, by regular and constant action into all the tissues. But as the effect of its action is entirely mechanical, we may pass it over. It has already been shown, by the citation of many facts, that the arterial blood in the tissues is controlled by other powers as well as by the heart.

Animals, again, differ from plants in the possession of a nervous system, which being closely—I may say, molecularly, connected with all the other tissues, exerts a powerful control over the phenomena of nutrition and secretion. This last point will require our serious attention.

The nutritive process of plants can be increased or diminished by three external agents, and by these only; namely, Light, Heat and Electricity. By this remark, I, of course, exclude the action of manures, &c., because they really act internally, by contributing to the formation of the nutritive fluid. But in animals there is interposed the nervous system; impressions upon which will produce similar effects.

The influence of Heat upon vegetation has been already adverted to. In winter we see the trees leafless, and, in cold climates, all the annual plants destroyed. The warmth of the spring renews the life which had lain, as it were, in the grave.

It is plain, then, that if heat exercises such influence over the phenomena of vegetation, it should produce local effects as well as general. If applied to only one part of a plant, whilst the rest is exposed to a far inferior temperature, we should have different results. This is found to be the case.

I remember that, while attending a course of lectures in the University of Maryland, there was a bush of sweet-briar, which, growing just beneath one of the windows of the lecture room, had either sent some of its branches through crevices, or had been retained inside of the room when the sash had been lowered. All was winter without—not a leaf on the plant. But that portion inside, protected from the weather, and excited by the warmth of the room, retained its green leaves. As the window looked southward, the plant could receive the benefit of the sun's rays; and it is evident that the sap was carried to the branches within the room by the combined action of light and heat exciting the nutritive process, and the decomposition of carbonic acid gas by the leaves. Richerand, in his Physiology, mentions a similar case.

The same influence of heat—general and local—may be witnessed in animal life. When the temperature of the air approximates *zero*, the skin is pale and shrivelled; the pulse low; and artificial warmth must be resorted to, to sustain life. On the other hand, the warmth of the summer oppresses us by uneasy feelings, and by profuse perspiration.

As in vegetables;—we may, by a simple experiment, evidence likewise its local effects. Place one hand in iced-water, and the other in water as warm as it can be borne; and we shall find the first pale and shrivelled, with the pulse small and contracted; whilst the other will present a red surface, fully expanded, with a pulse large and strong.

Apply a moderate degree of heat to any part of the body whilst the other parts remain at an inferior temperature, and, in a short time, morbid phenomena are generated. The like may be said of cold.

But heat is not the only agent which modifies the nutritive process in animals; among others, the nervous system is that which principally demands our attention. Concerning the manner in which the nervous system acts in producing the effects alluded to, I shall not here attempt to speculate. It suffices, that the facts are notorious in physiology—such facts as the secretion of tears, blushing, erection, secretion of watery urine, and a hundred other phenomena, from the influence of moral emotions.

Under the iufluence of the nervous system, affected either by mechanical injuries or from moral emotions, we see the surface become pale, cold, clammy; we find the pulse almost gone, and sensibility, in a measure, lost. Under other circumstances, the sensibility is highly increased, the pulse bounding, the skin red, hot and dry. These opposite conditions are obviously due—the first, to a diminution of the nutritive process; the second, to an increase of its intensity.

When the arterial blood reaches the capillaries, it is separated from the little islets of organic matter which lay in the net-work which those vessels form, by a thin membrane of serous tissue. The *liquor sanguinis*, holding in solution the oxygen obtained in the respiratory process from the atmospheric air, penetrates the membrane of the capillary vessel, and assimilation and decomposition take place by the mutual action and reaction of the fluid and solids. Carbonic acid gas, among other products, is formed, which, by endosmosis, repenetrates the membranes of the capillary vessels, and is carried on by the veins to be exhaled from the lungs. The current of the blood in the veins is usually attributed to the sole power of the heart; but from the principles laid down, it is evident that the nutritive process must exert a power here as in vegetable life. The phenomena of disease prove this conclusively.

We have already said that the nutritive process may be increased or diminished, in regard to its intensity. In animals, among other causes, it may be interfered with by impressions made upon the nervous system. Now, as the nerves have no connection with the blood, which is continually moving, we are forced to the conclusion that these impressions produce some molecular changes in the nerves themselves, which are propagated to the solids, and by which the chemical relations of the latter to the arterial blood are altered. In other words, the tissues must undergo a chemical change; for it is impossible to conceive, that if no such change took place, a change of action could occur.

A change then occurs in the chemical constitution of the tissues, by which the relation existing between the arterial blood and the solids is broken up. The nutritive process is, of course, interfered with: it may be increased, or diminished, or simply perverted. Let us first suppose that it is diminished in intensity. What must be the necessary consequences of such a condition?

The nutritive fluid being attracted into the tissues, with a certain force must act by removing the particles farther from each other—in other words, it must distend them in all directions; just as wood swells when it absorbs water. This force being diminished, a distending cause is removed, and the tissues contract from their natural elasticity. Hence we have paleness, and subsidence or shrinking of the parts; and as the nutritive process, upon which the elimination of animal heat depends, is impaired, there must also ensue coldness of the parts. If the affection be merely local, these will constitute the phenomena; but if it be general, we shall have a small pulse, clammy perspiration over the body, together, it may be, with diarrhœa and a flux of limpid urine.

The small pulse is evidently owing to the nutritive process, being diminished in the arterial tunics:—they contract just like the other tissues. The clammy perspiration, diarrhœa and flow of limpid urine, are mere exhalations—not secretions. They consist of the more watery portions of the blood being driven through the tissues by the action of the heart;—phenomena, which we may imitate in the dead body by injecting warm water with a strong force into the aorta.

Just all the reverse of this will happen, if the change in the relations of the solids and arterial blood be such as to augment the nutritive process. It can scarcely be worth while to go into details. It is obvious, that the conditions being reversed, the effects must be opposite. Instead of pallor &c. we shall have redness, tumefaction, increase of heat, full large pulse, with diminution or total stoppage of the exhalations.

Here then we have the prominent phenomena which characterize *blushing*, *erection*, and all the varieties of *inflammation*. But I would not be understood to say that inflammation consists in increase of nutritive action merely. Together with increase, there is also *perversion* of this process; and this perversion may take various directions, and from many and various causes.

I must leave to my intelligent hearers, the application of the principles laid down, to many other interesting phenomena which might be cited. I shall speak of one only.

The emptiness of the arteries after death is a phenomenon that has not yet been satisfactorily accounted for. The difficulty of explaining the fact has led some, even, to deny the circulation of the blood. "For how is it possible to imagine," it was asked, "that the last feeble contraction of a dying heart is sufficient to drive the blood into the venous system. 'Such an effect would not occur if the action of a dying heart were the strongest possible—while, in reality, it is so feeble, that the pulse for some time ceases to be perceptible at the extremities and the diminished circulation lets them become cold.*' " Various theories have been given to the world in explanation of this phenomenon. Some have contended that the arteries themselves contract upon their contents and thus expel the blood into the veins; but we have shown that the arteries have no other powers of contraction than those of elasticity, and of the tonic persistent kind, common to all the tissues. Arnott's explanation has been already examined. It may be remembered that he pushed through the difficulty by the aid of three hypotheses; viz: 1stly. That the capillaries contracted and then relaxed. 2dly. That they were muscular; 3rdly. That they contracted in a vermicular manner. Some others—Drs. Barry and Carson for instance—have attributed the course of the blood in the veins to the suction power of the heart, and the vacuum formed in the thorax, by what Carson calls, the resilience of the lungs; that is, by their contraction after inspiration. But "no kind of pump can lift fluids through pliant tubes, free to collape like the veins;" and secondly, "the suction power of the chest in healthy respiration is too weak to lift liquids even one inch through tubes of any kind.†" And even were the veins not pliant tubes, and the suction power of the heart and chest sufficient to lift a column of blood, it is plain that these powers could only fill the right auricle and ventricle, and the pulmonary artery. The quantity of blood which these cavities could hold, may be about six ounces;—a trifling amount, when compared with that contained in the arterial system of a living being.

The emptiness of the arteries after death is one of the most interesting facts in physiology. Indeed it is the fact which so long retarded the discovery of the circulation of the blood. The ancients finding them constantly empty, believed them to be air-vessels;—and hence the word *artery.*

We know from observation that nutrition aud absorption go on to a certain extent after the heart has stopped and the respiratory movements have ceased. The beard continues to grow; the chyle of animals suddenly killed, is absorbed; collections of pus &c. are removed. In many cases too, as for instance in apoplectic patients, the heat of the surface is remarkably exalted after death.‡ Now, if such processes as these go on, it is plain, that the attraction existing between the nutritive fluid and the

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* *Arnott*, Elements of Physics.

† Ibid.

‡ It has been well ascertained, that a real process of secretion not unfrequently continues after general or somatic death; urine has been poured out by the ureters, sweat exuded from the skin, and other peculiar secretions formed by their glands, and these changes could not have taken place unless the capillary circulation were still continuing. In the early embryonic condition of the highest animals, the movement of blood seems to be unquestionably due to some diffused power, independent of any central impulsion; for it may be seen to commence in the vascular area, before the developement of the heart; the first movement is towards instead of from the centre; and even for some time after the circulation is fairly established, the walls of the heart consist merely of vesicles loosely attached together, and can hardly be supposed to have any great contractile power." *Carpenter's* Physiology.—P. 506.

solid molecules, must continue to operate even after the more visible evidences of life have ceased to exist. It is simply, then, this continuance of the nutritive process, which empties the arteries of their contents, and converts, even after death, the arterial into venous blood.* If we tie an artery in the living subject, the like effect will be produced: it will be emptied beyond the ligature, and by the same causes.†

But if from any affection of the nervous system, mechanical or chemical, the nutritive process in the periphery—in the tissues generally—should cease before the action of the heart, what from the premises laid down should be the necessary result? Why, plainly, that the blood should remain in the arteries. Is not this exemplified in cholera? Examine the cold and blue tongue; the clammy and corpse-like surface of patients in collapse; and conviction must ensue at once, that the processes of nutrition and secretion scarcely go on. The patient dies in this state; and what do we find? That which we ought to expect: the arteries filled with blood.

The principles now advocated, were put forth in a work published by me in January 1844. They are indeed not new—scattered, here and there in various works on physiology we find the fundamental principle touched upon, but only to be lost the next moment. One is surprised at the enunciation of the proof in one page, and astonished to find that it has been totally neglected in its application to phenomena in the next. The following extracts from Müller bear upon this subject.

"Although it be denied that the circulation is in any way aided by an attraction between the blood and the capillaries, the existence of such an attraction or affinity may, nevertheless, be admitted in the instance of the 'turgescense, turgor vitalis, or orgasm,' which is observed to take place in certain parts of the body, independent of the action of the heart. This condition of turgescence in animals is analogous to phenomena which are so evident in plants, such as the afflux of sap to the fruit-bud, which contains the impregnated ovum."

"The mutual vital action or affinity between the blood and the tissues of the body, which is an essential part of the process of nutrition, is, under many circumstances, greatly increased; and an accumulation of blood in the dilated vessels of the organ is the result. It is seen, for example, in the genitals during the state of sexual desire, in the uterus during pregnancy, in the stomach during digestion, and in the processes of the cranial bones, on which the stag's antlers afterwards rest, during the reproduction of these parts. The local accumulation of blood, with the dilatation of old and the formation of new vessels, is however, seen most

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* It has been proved by the experiments of Wilson Philip and others, that the blood continues to move freely in the smaller arteries long after apparent death. See Experiments—24, 75, 76. *Experimental Inquiry into the vital Functions.*

† Other physiologists have taught that an artery is capable of contracting directly upon its contents, so as to expel even the last drop; but large arteries when emptying, do not contract *roundly* like an intestine; they become *flat* like elastic tubes of leather *sucked* empty, and no contractile action of the vessel itself could bring its sides together in such a manner." — *Arnott*, op. cit.

frequently in the embryo, in which new organs are developed in succession by a process of this kind; while, on the other hand, other organs, such as the branchiæ of the salamander and frog, and the tail of the latter animal, become atrophied and perish as soon as the vital affinity which existed between the blood and their tissues, ceases to be exerted.

"The phenomena of turgescence have been supposed to be dependent on an increased action or contraction in the arteries. But arteries present no periodic contractions of muscular nature; and a persistant contraction of arteries, unless it were progressive,—vermicular, as it were,—or aided by valves arranged in a determinate direction, would be quite inadequate to produce a state of turgescence in any part."

"To explain the state of orgasm of the uterus during pregnancy, and of the bony processes which bear the antlers of the stag, we must presuppose the existence of an increased affinity between the blood and the tissue of the organ. This condition may be excited very suddenly, as is seen in the instantaneous injection of the cheeks with blood in the act of blushing, and of the whole head under the influence of violent passions; in both of which instances the local phenomena are evidently induced by nervous influence. The active congestion of certain organs, of the brain, for example, while they are in a state of excitement, is a similar phenomenon"

"If the organ which is susceptible of the increased affinity between the blood and the tissue, is, at the same time, capable of considerable distention, tumefaction and *erection* take place."

It will be seen from the above and from other passages to be found in his Physiology, that Müller denies any attraction to exist between the blood and the *capillaries*, though he admits "the mutual vital action or affinity between the blood and the *tissues* of the body." In this, I partly concur, for the chief office of the capillaries is, to conduct the blood into the tissues in which they are imbedded; but they are likewise tissues themselves, in which, as in all others, nutritive action is continually going on.

But, how useless was this great fact to Müller, and into what confusion and inconsistencies he has fallen, may readily be perceived by any one who will attentively read his chapters on the circulation.

Art. III.—*Address of* Samuel A. Cartwright, M. D., *of Natchez, Mississippi; delivered before the Medical Convention, in the city of Jackson, January* 13, 1846.

[We invite the special attention of our readers to the following able and interesting address. Dr. Cartwright's extensive experience in the treatment of Southern diseases, his literary and scientific attainments, and his valuable contributions to practical medicine, entitle his observations to profound respect. His views in regard to the *peculiarities* of Southern diseases, as well as Southern constitutions, are such as have been proclaimed by this Journal from its commencement. These views have been opposed, and even violently assailed from the North, but as far as we have been able to ascertain, they are universally entertained in this region. Dr. Cartwright's notice of the *impurity* and the *impotency* of a large amount of the drugs and medicines sold by Southern apothecaries, as also his bold and manly exposure of the abuses and outragous impositions inflicted upon the Southern people by the Northern manufacturers of *quack nostrums*, entitle him to the thanks of the Profession. He says truly "that the empyrics have *polluted* the press in making it daily tell Munchausen stories of cures and miracles never performed." In connection with this subject we cannot forego making the following extract from an Anniversary address to the New-York Medical and Surgical Society, by F. Campbell Stewart, M. D., delivered in January last. Dr. S. says :—

"The influence of the Press is severely felt by our profession as being frequently exerted to its prejudice and discredit.

The most prominent and striking manner in which this injury is experienced, is through the facility afforded to Quack advertisers, for inserting in popular and extensively circulated papers, notices of their secret remedies, and astonishingly successful modes of treatment. To such an extent has this evil grown, that whole columns of our most respectable newspapers are filled with them. A recent writer declares that he had counted eleven out of twenty columns comprised in one paper, filled with these quack notices.

It is wholly impossible to calculate the amount of injury that accrues to the community, as well as to us, from this pernicious and degrading practice. For, besides lending to impostors, facilities for puffing and making themselves known, these advertisements are frequently of a character both indelicate and criminal, producing a baneful influence on public morality, and often leading to the commission of offences against the laws both of God and the State.

I am aware that the space occupied in newspapers in the manner indicated, is well paid for, and that it may appear unfair and unreasonable to expect their proprietors to deprive themselves of so fertile a source of revenue. But have these gentlemen no other object than pecuniary gain? Are they not bound, as directors of public opinion, and as guardians of the public interests, to abstain from pursuing a course calculated not only to injure us, but directly prejudicial to the public weal? Is it so clear that they would necessarily incur a loss by refusing to promulgate information such as is usually contained in these notices? If they could be induced to devote the space occupied in this disreputable manner to miscellaneous information of a general character, they would soon find their subscribers to increase in a proportion amply sufficient to compensate them. The experiment has already been tried abroad, and we hear of no complaints of loss from those interested in such journals as have refused to serve as a medium for imposition.

The opinion of the publisher of one of these papers on the evils resulting from the course generally pursued, is expressed in such strong and forcible language, that I cannot resist the temptation to quote it. He says,—

"We fell into the current and followed the bad example of the pre-existing periodicals; but reflection has led us to see our mistake, and we hasten to repair it, assured that we shall give satisfaction to all our readers, who properly estimate the true character of modern quackery, which is one of the vilest and foulest of all foul and vile vocations, and is sustained to an incredible extent by fraud, forgery and falsehood, and fraught with delusion, disease and death. To publish their nostrums is to partake of their deeds. To receive their money is to share their spoils and aid them in making war upon mankind."

May we not hope that the example set by this Editor may be speedily and generally followed on this side of the Atlantic, and that editors here may likewise be found—

"Cheerfully to abandon the publication of all advertisements of quack-medicines, which will be an act of homage to their own taste and judgment, no less than a concession to the strongly expressed opinions of some of their best friends who, with ourselves, deeply deplore the disease and mortality occasioned by the nostrums of medical quacks, published daily in this great metropolis."

By following so independent and wise an example, this foul blot on the body of our Press will soon be entirely and permanently erased."

But this is not the only complaint that we have to make against publishers and editors in this country. We have to find fault with many of them for their editorial encouragement of new systems, and innovations on our established methods of practice, by which, if possible, more than by inserting advertisements, they injure us, and make dupes of the public. The frequent, and often well written articles which appear in the editorial columns of our newspapers, in praise of various quack systems of medical practice, and urging upon the community their value and importance, do much harm. For, besides being more generally read, they carry with them a great degree of weight, as emanating from individuals supposed to be impartial, disinterested, and capable of forming a correct judgment.]

Gentlemen: We have met together to see if something cannot be done to increase the usefulness and respectability of the medical profession in the state of Mississippi. Those of us, who look to the hand of legislative power for aid in this good work, may meet with disappointment. It becomes us, therefore, to consider what we can do for ourselves without trusting too much to Hercules. But we should not despair of having something done for the encouragement of medical science, considering that a large majority of all classes of society, from the governor down, warmly espouse the cause of education, and advocate the diffusion of knowledge, and the cultivation of the elementary sciences. While all the other sciences and every species of useful knowledge meet with so much encouragement, it is not too much to hope, that the day is not far distant, when the addition of medical science to elementary learning, will not act as a disqualifying circumstance for popular and legislative favor. It becomes us, the votaries of that science, to assert its rights. They have but to be told to be seen. No argument is necessary to prove, that that species of knowledge by which diseases are prevented or cured, pains relieved, and life prolonged, deserves as much protection and encouragement as that other species of knowledge connected with the rights of property. Medicine deals with the man himself; the law with what he possesses. Yet law, as a profession, is protected, while medicine is not. None but the learned in the law can practise without a licence, while every ignoramus, every deceiver, every dupe of a false or one-sided theory, can practise medicine. Uncivilized and savage nations suffer more for the want of skilful and learned physicians, than for the want of learned or dexterous jurists. The ravages that the small pox has made among our Indian tribes, and the terrible havoc of the cholera among the ignorant nations of the East, are sufficient proofs, if there were no others, that mankind cannot safely dispense with the services of physicians of learning and skill. Hippocrates, the father and founder of rational medicine, more then three centuries before the christian era, met and refuted the fallacious theory of cold and heat, and the dogmas founded upon it, by the most lucid and unanswerable arguments, yet the theory that heat is life, and cold death, has been revived of late years, and made the moving spring of action of a numerous class of irregular practitioners of the present day in Mississippi—while the unanswerable refutation of that fallacy in medicine, is slumbering, in Greek, in the tomes of Hippocrates. But it is to be hoped that it is not to slumber long. The time is ripe for its translation into the English language. It is not denied that some good may be done under a false theory, and by ignorant pretenders. The false theory in politics of the divine right of kings, did not oppress and do harm to all, yet the general result of government founded on that political fallacy, was degrading and oppressive to the great masses of mankind; being a public evil with only partial good. The same may be said of quackery in medicine. The exclusiveness of empirics in confining themselves to the vegetable kingdom alone, argues ignorance and narrow mindedness. Modern chemistry, which has done so much for agriculture and the mechanical arts, is the child of medicine. Why should physicians therefore reject the aid of chemistry and every medicinal agent drawn from the mineral kingdom? But the worst and most numerous class of empirics are the makers, venders and consumers of patent medicines and secret nostrums. Every one who swallows a nostrum of unknown composition, is an empiric, making an experiment upon himself, and whether it kills or cures, does harm or good, nothing is added to medical knowledge by the experiment, as the thing experimented with, that does the good or evil, is unknown. All who fabricate such secret nostrums, must be the greatest deceivers or the most heartless and inhuman of mankind—heartless and inhuman if they have actually discovered a remedy that will cure all diseases, or even one disease, and will not let its composition be known—deceivers and traitors to mankind, if they pretend to have discovered such a remedy when they have not. The empirics have polluted the press in making it daily tell Münchhausen stories of cures and miracles never performed. The quack medicines whose miraculous virtues are daily set forth by the press in standing advertisements, are mostly made up of inert or poisonous ingredients, or of the ordinary drugs of the shops, disguised and sold as having virtues, which the medicines composing them are known not to possess. Yet, hit or miss, these quack compounds are palmed off on the public at extravagant prices, without a thought of the harm they do, in injuring two, where they cure one, but purely with the view of making money out of our people. But who reaps the profits? Not the citizens of Mississippi, but an abandoned set of impostors in the northern states, who fabricate the trash for our market. Our apothecaries are not benefitted but injured by it. Those of them who act as agents for the northern imposters, receive a small per centage, but that per centage is not sufficient to compensate them for the injury, their legitimate business sustains, in making quack medicines supply the place of the regular articles in the materia medica. Worse than all the fabricators of quack medicines are, in too many instances, the very persons who supply our apothecaries and planters with the regular articles of the materia medica. Interest and avarice offer them a double encouragement to sophisticate the medicines that physicians commonly use. In the first place, impure, stale, or adulterated drugs cost them little or nothing, and in the second place, the sale of all adulterated physic tends to bring the medical profession into disrepute with the public, and thereby to increase the demand and open a market for quack medicines. The patient who looses his teeth from adulterated or badly made calomel, blames the physician who prescribed it, or attributes the evil to some inherent property in the article, and not to its sophistications or careless manufacture, and the next time he is indisposed, purchases quack medicines, made probably, by the very man who made or adulterated the bad calomel that salivated him. Calomel rarely salivates when properly prepared. Thus, the fabricators of quack medicines, by being the wholesale venders of the regular medicines of the shops, get between the physicians and their patients, making the physicians powerless for good, by given them bad tools to work with, and destroying all confidence in them, and leading the people on, against their will and better judgment, into a forced market of secret compounds—and when they fall ill, having no confidence in physicians, or in the efficacy of any known medicine, they are too apt to consult the newspaper advertisements for some vaunted cure-all, and neglect calling on a physician until it is perhaps too late. The public press as well as the physicians' store houses, or in other words, apothecary shops, are made unwilling instruments to spread and diffuse quackery over the land, and to subject the people to an enormous indirect tax—not to build railroads and found schools, but to convert the unworthy northern empirics into the lordly owners of extensive blocks of buildings in our Atlantic cities. Now if no legislative remedy can be devised against these evils, it will, I think, become a proper subject of inquiry for the physicians here assembled in convention, to consider what remedy they can provide themselves. We ask no exclusive privileges from the legislature, we ask no tax to be imposed upon the people for the encouragement of medical science, but may we not with confidence ask the legislature, as the state is in want of money to found schools and colleges, and for other purposes, to impose a tax on every printed libel in the form of a quack advertisement, and on all those persons in the state, engaged in the business of vending nostrums for northern impostors. Those impostors derive an immense annual revenue from the people of Mississippi, and pay no taxes to the support of the state that pampers them. The cotton planters and every other class of citizens, in proportion to their wealth, are compelled to pay taxes to the state, while the more wealthy non-resident empirics, who derive so large a portion of their wealth from our people, contribute nothing to the revenue of the country from which they derive their wealth. If the people are to be imposed upon by secret nostrums, it would be far better to manufacture them at home, and keep the money in the country, instead of sending it to a set of impostors, a thousand miles off, for their secret medicines to cure our diseases. What do the people a thousand miles off, know about the diseases of the south, that they should take out patents to cure them? We tax auctioneers, we tax showmen, we tax the retailers of spirituous liquors, but we impose no tax on the wares of those who are reaping large profits from our people by falsehood and quackery, reduced to a system, and carried on as a most lucrative trade. If classic learning be worthy of encouragement, and schools and colleges be founded for its diffusion, and the boys at school be not pressed into the service of the state, why should physicians, after they have added medical knowledge to classical learning, be interrupted in their professional pursuits, and pressed into the service of the state to act as jurors, and subjected to all the little annoyances arising from their being a body of men exercising a profession unknown to the law? We do not ask for the establishment of a board of medical censors, but we might ask for the passage of such a law as would put it in the power of a majority of the people of any county, that might be so disposed, to elect medical censors to act for the county. Thus leaving the question, whether medicine as a science should be regulated or not, entirely with the people of the several counties —each county to act independently for itself. But as I said before, we are not to expect too much from the legislature. Our main dependence is upon ourselves. We can hold our ground against the Goths and Vandals of empiricism if we act in concert, and clothe ourselves with the whole armor of science. We should set our faces against those medical schools which turn out half educated men. We should recommend no pupils to them, and thereby we would do much to put down that new trade of *doctor-making*, which promises, if not arrested, to make a diploma from a medical school of no more value than a blank piece of parchment. Something might be done by the establishment of a state medical society, to inquire into the qualifications of those who come among us to locate themselves in the practice of medicine. A state society composed of members residing at a distance from one another, would not be subjected to the influences of those petty jealosies so prejudicial to medical societies in all little villages overstocked with doctors. The society would be the most competent authority to draw the line between the physicians proper and the half educated or ignorant pretenders, and the errors of the latter would cease to be visited on the heads of the former. "Life is short, art is long." Hence the necessity of society, or some organization to enable us to consult with our contemporaries in the state, and to make ourselves masters of their united knowledge. The adding to our lives, the lives of others who have gone before us, by studying their works and profiting by their experience, together with free communion with our cotemporaries, will do much to set reason free from the chains of prejudice, and the baneful influence of favorite but fallacious and short sighted theories. It would make us more cautious and prudent when on unknown ground, and will give us more confidence—when we see that we are on the road that the world's experience has marked out. Medicine is not a fortune making business, gentlemen: but an honorable profession, to be followed more for the good that we make it do to others, than for the gains it may bring us. While the profits from the exercise of our profession should always be sufficient to yield its votaries a handsome competency, we should never seek to increase them so much as to make our professional fees so oppressive to the people, as to drive them from the employment of physicians. The patient and his physician should be bound together by a reciprocal interest. The patient, if able, should pay such a sum as would be sufficient to command the willing and prompt services of his physician when needed, and at the same time should not be taxed by his medical adviser beyond his ability. For the want of some central society, or organization among physicians, our profession is in danger of falling into disrepute by undercharging and underbidding on the one hand, and exorbitant bills on the other. Physicians have to deal principally with three things—the patient, the disease and the medicine. In regard to the patient, his constitution, habits, temperament, occupation, and even the race of mankind to which he belongs, claim serious attention. A very large portion of the practice of the physicians of our state is among a race almost unknown to medical books and schools. The peculiarities of that race, and the diseases incident to it, make it the more necessary that there should be a greater facility of communication among our medical men, enabling them to profit by one another's experience. A physician may be ever so learned in other respects, but if unacquainted with the negro character and the peculiarities of the diseases of that race, he will not be likely to win the practice from overseers. In regard to the second matter claiming the attention of physicians, namely, *disease*, there is a sad deficiency of books and information on the diseases of this climate. The writers of our medical libraries are mostly Europeans, and the medical writers of our own country, mostly reside in the northern cities, in a climate very different from that of Mississippi. In vain do we look in their works for a description of a large portion of the diseases we daily meet with in practice. The diseases of the northern latitudes, and of the paupers of European hospitals, are delineated with the most minute accuracy, and the remedies, which the experience of all countries and ages has proved to be the most successful in their treatment, can be brought to bear upon them. Not so, however, with the bilious pneumonia, the bilious and congestive fevers, which kill our negroes, planters, lawyers and legislators. In vain do we search medical books for an accurate description of those affections, as they occur in our practice, or for the most successful methods of treating them. Hence the necessity of a society, or some organization among ourselves, for learning the laws of those affections, and the remedies that each of us has found, by dear experience, to be the most successful in their treatment. It is to be hoped that if the legistature will give us no aid, it will not cross our path, as it now does, by taxing us with jury service, by drawing no line between the physician and the empiric, and making no distinction between the time honored remedies of the materia medica and the quack medicine, that the northern impostors impose upon our people. I know of but one book that gives any thing like an accurate description of the diseases we daily meet with in practice. It was written by Hippocrates, and has never been translated into the English language. Dr. J. R. Cox, of Philadelphia, is now engaged in translating into English, certain portions of the work. If this convention could prevail on him to give us the entire work, omitting not a word, it would not have assembled in vain. If Dr. Cox publish a part of the work, he lives so far north that he will be sure to omit the most important parts to us. Medicine originated or became a science, and advanced to a high state of maturity and perfection in a climate very similar to ours. When civilization travelled north, medicine went with it, and from a science rapidly approaching perfection in the latitude of Greece, it became a very imperfect science when transplanted in the high latitudes of the north of Europe. It was there reformed to suit the climate. From the physicians of the great cities of Paris, London, Dublin and Edinburgh, in the north of Europe, our northern professors, teachers and writers mostly derive their medical knowledge, and we of the south from them. On coming south, our physicians finding that the diseases, presenting themselves in their daily practice, correspond so little with the diseases of the medical schools and books, that they are too apt to throw away books as faithless guides, and rely too much on their own individual experience. Much more valuable knowledge can be obtained, in my opinion, in regard to the laws that our southern diseases are under, by cutting across direct to Greece, and drinking at fountain head, than by obtaining it through the circuitous route of Edinburgh, London and Paris. The north has improved in the medical agents with which we work, but no improvement has been made in elucidating the laws that govern diseases, as we find them in this climate, since the days of Hippocrates, and I doubt if any can be made. The truth itself cannot be improved.

The third matter for consideration, is the remedies used in the treatment of diseases. The importance of having these good and genuine, must be apparent to every one. Yet, no part connected with our science in the south, seems to have attracted so little attention. How often, when we prescribe calomel, does the patient take corrosive sublimate mixed with it; black lead with iodine; cinchona, starch, and such things, with quinine; lime with magnesia; slack and bittern, with salts; and so on with every article of the materia medica? To open the way to remedy some of these evils, after in vain crying out against them for years, I have lately made some experiments to test the question, whether genuine medicines cannot be imported direct to Mississippi, from first hands, from the manufacturers of the highest celebrity in the world, as cheap or cheaper, than what we pay for those questionable, and often grossly adulterated articles, that the northern venders of patent medicines, throw upon the southern market. I am happy to inform you, it can be done, and I would advise all our apothecaries to adopt the plan of direct importations from first hands.

The science of medicine can never take its true stand in Mississippi, and our physicians can never hope to overthrow quackery, until they insist upon our apothecaries providing them with medicine of unquestionable quality; and if they will not do so, I would advise all physicians to keep their own medicine. The apothecary business should be a handmaid to our profession, and not an opposition interest, as it has in too many instances, become. Another means I suggest to increase the usefulness and respectability of our profession, is an agreement among ourselves, not to take any students destitute of natural talents and a good preliminary education, or lacking in industry or deficient in moral honesty.

Six things, says the sage of Cos, are necessary to make a good physician: 1, Natural talent; 2d, a good preliminary education; 3, moral honesty; 4, to begin young; 5, industry; 6, length of time. Another means, is the careful separation of science from mere opinion, or things known from things imagined. That which hurts or helps, should never be lost sight of in the too eager pursuit of a favorite theory; and we should condemn no remedy that experience has found to be useful, merely on theoretical grounds, or because quacks may happen to abuse it, by using it too much. I lost favor with some of my medical acquaintances at the north, by using pepper in cholera. But I have the authority of Hippocrates for the use of both the *piperum longum and piperum nigrum*, in some congestive affections; and I am not disposed to give up the experience of centuries, in a climate like ours, for a medical theory or mere opinion. Drs. PERRINE, McPHEETERS, and Dr. METCALF, our worthy president, and your humble speaker, nearly twenty years ago, introduced the practice of giving quinine, in our paroxysmal fevers, much in opposition to northern opinion. In 1826 Dr. PERRINE in his own case, took, during the febrile paroxysm, eight grs. of sulphate of quinine at a dose which reduced the pulse. Soon afterwards Dr. McPHEETERS adopted the practice of giving quinine during the febrile paroxysm. In 1827 Dr. McPHEETERS and myself used the quinine very extensively in large doses, 6 or 8 grs. in an epidemic fever characterized by a very frequent pulse and great determination to the head.—We gave it in the febrile paroxysm and found it to be more effectual than in the remission in that particular fever. We want some means to be devised, of posting up all the useful information extant, derived from the teachings of the school of experience, in the state of Mississippi. Hence the necessity of a State society, which might be made a department of Oxford university. It need not be in the little town of Oxford, however, but located in any convenient part of the state—to that we might all send up our experience, in manuscript, giving a description of the diseases we meet with, and the method we pursued in treating them, whether good or bad. Much of this matter might be published in that excellent medical paper, the New Orleans Medical and Surgical Journal. We would thus soon have a manuscript library, and lay the foundation of a better system of practice, than any now extant. Some of us might probably, at some healthy period of the year, find it convenient to assemble to hear and deliver lectures exclusively devoted to practical matters connected with our profession. Every contributor to the manuscript library, should be advised to read Hippocrates, or at least Lord Bacon, before he took pen in hand. There is a branch of our science which has been very little cultivated, since the days of Hippocrates. It is the science of Prosoposcopia, or the physiognomy of disease. It is particularly useful in the treatment of the diseases of children who cannot, and females who often will not tell, what is the matter with them. To be able to tell a large portion of the diseases of women and children, by merely looking at the patient, is of no small advantage in the practice of medicine. Many diseases of the lungs, the stomach, liver, spleen, and uterus, can be recognised at a glance by the practised eye of any physician who has paid much attention to prosoposcopia. Indeed, many forms of congestive and yellow fevers, are often difficult to recognise by any other means than the expression of the patient's countenance. The passions are pictured in the countenance, and so are diseases. Prosoposcopia is also very valuable in prognosis, the expression of the countenance alone often determining how the question of life or death is about to be decided. A correct diagnosis and prognosis, go very far to give confidence in the medical profession. To be able to tell, in advance, what medicine can do and what it cannot do—to discriminate between the curable and incurable complaints, and to foresee and predict the result of many dangerous affections, requires high professional skill, and goes very far toward relieving the profession of any reproach in those cases that terminate unfortunately. Though vanquished, the physician may show the powers of his art; and it would be as absurd to condemn Medicine as powerless in all cases, because it occasionally encountered diseases of superior power, as it would be to condemn the British forces under Packenham as powerless inefficient legions, because they were whipped by western militia. The adoption of some code of etiquette, seems likewise to be called for; and the election of delegates to the medical convention to assemble in New York in May next; as also a request from the legislature, to pass some laws necessary for an accurate register of births and deaths in the state.

Art. IV.—*Cases showing the Effect of Yellow Fever on the System for a long period after an attack, and aiding to elucidate the character of that Fever.* By C. H. Stone, M. D., Natchez, Miss.

If observations made at post mortem examinations have not led to more fixed opinions respecting the organs implicated, as cause or effect, in yellow fever, it is, in part, because, in estimating the value of the appearances, due attention has not been given to the rapidity with which death has occurred. If, in the most malignant cases, terminating rapidly in death, the liver has not presented the strongly marked alteration claimed by some as characteristic, it is forgotten that this event happened before sufficient time had elapsed for that lesion to have taken place. Dr. Gilkrest has not fallen into this error, for he has noted that, "in cases (of yellow fever) of extraordinary malignity and rapid mortality, this change of color in the liver was seldom well marked; but in a considerable proportion of these cases, it was sufficiently so to show that the organ *was passing into that state*" which Louis asserts to be the anatomical characteristic, and which Gilkrest, Imray, and others, declare to be so common.—See notice of Dr. Gilkrest's paper on the Gibraltar fever of 1828, in the Med. Chir. Review for November, 1830.

The state referred to is one of diminished rather than increased volume, of a dry, crumbling texture, with yellow color, &c.

Much, also, of the uncertainty, may be the result of too great reliance on the ability to see, with the eye alone, changes in the organization, for which the power of the microscope must be necessary, and perhaps may be sufficient.

This is recognized in investigating the abnormal states of the blood and other fluids, and it has led to an elucidation of several diseases which before were totally misconceived. Until this mode of investigation has been more extensively used, it is unfit to declare an organ not deviating from a healthy condition, in opposition to the strongest language it can use. Not less unsuitable it would be to say, from a visual examination of venous blood taken from various parts of the system, or from different individuals or species of animals, that its *composition* or its *corpuscles* were exactly the same. It must receive a chemical and microscopic examination before any opinion can be justly made up.

To what important results might we not arrive, if these modes of investigation were more applied to the blood, bile, urine—indeed, all the secretions—all the tissues of patients affected with yellow and other fevers, as far as possible, in all periods of exposure to the poison, and after an attack—during life, and after death. As New Orleans and Mobile can claim so many men of ability in the profession, it is to be hoped some attention will be given to this investigation.

It will be admitted that post mortem examinations, *necessary as they are*, require a knowledge few can justly claim. Palpable changes all can see; but a practised eye is required to detect others, and then it is no easy matter to read them rightly. Instance Dr. Imray describing the stomach, in the yellow fever at Dominica in 1838. (See London Lancet, vol. i, 1839–'40). He speaks of congestion, inflammation, or disorganization, being so common, that the gastric affection was a necessary consequence, or an essential part of the fever, and describes, as proof, such appearances, nearly all of which Magendie would, more justly perhaps, declare the result of the diseased and fluid state of the blood, and such as he could at any time produce.

Notwithstanding, as writers most generally agree in describing the liver as more uniformly exhibiting deviations from health than any other organ, and as the very condition upon which Louis lays so much stress, and which others prove to be so common in this organ, is *most improbably* caused by inflammation, and is *not* of a nature, but the *reverse*, to result from the fluidity of the blood, it is fair to infer, that in the impaired *function* of the liver must we look for much, if not all, of the peculiarity and danger of this fever; and more strongly are we urged to this conclusion from the *symptoms* which are observed *before*, *during*, and for a long period *after* an attack.

Considerable organic lesion may exist in this organ; the same may be said of the kidneys, without serious impairment of the secretions; and the system will suffer less, comparatively very little, than if the diseased action be seated in the secretory surface. On the contrary, if the secretory vessels be impaired, destroyed, or deranged, secreting bile imperfectly constituted, however copious, the most serious consequences will rapidly result, if no other organ acts vicariously to relieve the system—the blood—from the poisoning effects of those materials, which these vessels can no longer, or only partially remove. It may, therefore, be very true, that disease may take great liberty with the liver, without the system suffering much, but it must be very careful with what part it tampers; the secretory vessels will not be interfered with, with impunity; and the liver is not unique in this; the same is true of the kidneys, and other organs.

It is not to cases of intense severity that we should look for a development of symptoms that would point to the organs implicated; they being overwhelmed, at the onset, give obscure or no signs, as they give little or no information after death, if this has occurred rapidly.

It is rather from those of a milder character, and of longer continuance, that we may expect more information. In the early part of the epidemic of 1844, which prevailed at Woodville, we had a great proportion of such; and, with the opinion long entertained, that the liver was more sinned against than sinning, in the pathology and treatment of this country, I saw the strongest proof that this was the organ mainly implicated; and since, in observing the impression left on the system (I hope not through the medium of *yellow* spectacles), I have found the most conclusive evidence of the same.

Not being able to find that similar observations have been published, I am induced to describe a few cases, in which, during a period ranging from four to sixteen months after an attack of yellow fever, the liver showed the impression which had been made on it by the poison.

Dr. Procter's case, as given by himself, as follows:

Case 1.—"I was attacked with yellow fever early in September, 1844, and my recovery was protracted through the following winter. In February my health seemed completely restored, and was good until the middle of June, when it began to fail, and I continued more or less unwell till the following August.

"I suffered almost constantly with head-ache, had little or no appetite, furred tongue, and constipated bowels; the skin was harsh, dry, and of a dirty yellowish hue; the conjunctivæ injected and tinged with yellow; the urine variable both in color and quantity, being at times of a deep yellow, and again almost colorless; the alvine dejections clay-colored; a sense of uneasiness in the right side, hardly amounting to pain even on pressure; much languor and debility, and with considerable depression of mind.

"You will perceive that I was precisely in the condition which, in February last, you predicted that I and others might experience during the summer. During the persistence of the foregoing symptoms, I used blue mass repeatedly: it produced copious evacuations of bile and mucous, and caused, for a short time, an abatement of all the symptoms, in proportion as the evacuations changed from an intense yellow to a more natural hue; the relief, however, was only temporary, as the same condition — suspended secretion of the liver, &c.— returned again and again. As, in your opinion, the continued use of mercurials alone was insufficient for the restoration of my health, I went to Pascagoula for sea-bathing, &c. Your advice was productive of the happiest results, and I am now in the enjoyment of as good health as at any former period.

"Yours, &c.,

"*January*, 1846. "Stephen Procter.

"*To Dr. Stone.*"

Case 2.—*Furnished by Dr. A. C. Holt.*

"I was called, on the 5th July, to visit J. R. Lyons, whom I found with these symptoms, viz.: intense head-ache; pain in the back and lower extremities; pulse full and hard; eyes and skin deeply tinged, the latter approaching a mahogany color; great thirst, with violent vomiting.

"*Treatment:* V. S. Mustard foot-bath; sinapism to the gastric region; ice water, in small quantities, and 20 grs. calomel in six hours.

"The following morning he had a decided remission, having had several copious bilious evacuations. Ordered 8 grs. quinine, to be repeated at intervals of three hours. Convalescent in three days.

"On the 29th July was called to the same patient. I found him suffering from a similar train of symptoms as in the former attack.

"A recurrence, in his case, of the peculiar state of eyes and skin, and the severity of his attacks during a healthy period in our community, called to mind a remark of yours, that 'we should, in this summer's sickness, discover the effects of last year's epidemic.'

"In a few weeks, I was summoned a third time to the same. On this occasion, his nervous system received a greater shock than before, his system being suddenly prostrated with alarming congestion. After reaction was established, a mild mercurial, followed at the proper time by 20 grs. of quinine, and some slight additional treatment, brought about a happy termination of the case. Previous to all his attacks, he had been constipated, and his evacuations exhibited inaction of the liver.

"*To Dr. Stone.*" "A. C. Holt.

Case 3.—Caroline Stewart, ten years of age, had yellow fever in 1844, and passed the summer of 1845 in Massachusetts, where she had an attack of fever of fourteen days' continuance, with irregular exacerbations. Tongue, at first white, became very red; urine, deep yellow; skin deadly pale (which was the case in her attack of yellow fever); the appearance of the eyes not recollected. Alvine discharges were produced with difficulty, and were very pale. She had a great desire for salt, which she licked from her hands greedily, it having been rubbed on the skin, and this desire she had in the attack of 1844. Mrs. Carrier, the mother from whom I obtain the history of the case, gave, in the absence of the medical attendant, a full dose of calomel, from which the liver began to secrete freely, deep yellow and full stools resulting, and then her recovery began. Her case presented such distinctive characters from those of former attacks, that Mrs. C., who is conversant with fevers, was forcibly impressed, and mentioned the peculiarity to me on her return.

Case 4.—Antoinette Kaigler, eleven years old, dark complexion, and a yellow fever subject of '44, had an attack of fever in the summer of '45, of much severity, exhibiting the following characters: Pulse full and firm; tongue deep red, after the first few days; the skin brownish yellow, this color increasing as the exacerbations took place, and lessening as the fever moderated, which it did at regular intervals. The urine was generally yellow, though not deep colored; the stools became a deep yellow, soon after giving a 10 and a 6 gr. dose of calomel, and continued so throughout, though no more was given. This case lasted ten days, and for as many more she exhibited proof of deranged action of the liver, in the skin, urine and stools.

She was treated by one general, and several local bleedings. Blister and enemata. Only one vigorous trial of quinine at the first remission, which made her decidedly worse.

I think, in this case, as in all the others, more was required than to check the paroxysms, which in all was well marked; there was required, as in original yellow fever, a *depuration of the blood through the steady and increasing healthful action of the liver.* This was the second case in which I gave quinine, and was disappointed in both.

Case 5.—In February, '45, I attended N. Dixon, in similar condition as respects the liver, and having distinct daily paroxysms, with *chill* preceding each, and the fever subsiding almost completely. He steadily improved under calomel, local bleedings, baths, blister, &c., without quinine. By this I was satisfied I should have impeded his recovery, and produced a continued fever, by aggravating and fixing the sanguineous irritations; at least, I preferred to profit by my past experience of *this remedy* in yellow fever.

Case 6.—Mr. Ferguson was in Woodville a few hours in September, 1844: he had a *mild* attack of fever in consequence, and did not recover a tolerable health till January, 1845, having deep yellow skin (complexion naturally dark), yellow urine, debility, &c. Without being in good health, he attended to his business, as overseer, till August 28th, 1845, when he had an attack of fever.

Without knowing the preceding history, I gave an emetic of ipecac., a mercurial purge, and ordered quinine freely as soon as the fever subsided. He had no second paroxysm, but remained from that time till January, '46, with strongly marked evidence of deranged action of the liver, as brownish yellow skin, deep reddish yellow urine, constipated bowels, &c. He experienced only temporary relief from mercurials, and only recovered perfectly when he abandoned his business, and used repeated tepid salt baths, friction, &c.

I could enumerate six or seven cases more of similar character, including one of uterine engorgement, in which this condition of the system was so great an obstacle, that I had to send the patient to the sea shore during the summer.

The year 1845 was remarkable for health in this section, and yet I have not, during eighteen years' practice in the South-west, seen as many cases in which the liver played so conspicuous a part as since the yellow fever of 1844, *and confined to those who had been attacked during that epidemic mildly or severely.* The aspect of these cases differed from common biliary derangements, difficult to describe, and yet as striking as the appearance of persons only a short time recovered, of whom it is common to remark that they present the appearance of having had yellow fever. Dr. Procter, judging from his own case, confirms this remark.

The action of mercurials was not so much to be relied upon, for a permanent effect, as in common bilious derangements, the persistence of the diseased condition attributable, perhaps, to some modification in the blood still remaining, or, more probably, *again* induced by the *peculiar functional derangement of the liver*, which slight causes might reproduce, or which may not have been *entirely* overcome.

I say *peculiar* derangement, for if we repudiate the better word specific, we must allow the *fact* which it is intended to express; the whole history of disease and of remedies attests this.

Dr. Brown informs me that he has had numerous cases similar to mine, and besides my own, I have seen many others.

It is a great mistake to suppose the liver suffers particularly in our fevers, or that its chronic diseases are common here. Formerly nearly every sick person had "the liver complaint," often goaded to complain by repeated mercurials; but now the gastro-duodemal irritations, *sanguineous* and *nervous*, are duly recognised, a blessing, more limited, but, in other respects, equally great with the present knowledge of the power of sedative doses of quinine in periodical fevers.*

Art. V.—*Case of Strangulated Hernia—Operation—Sloughing of the Intestine—Recovery.* Reported by Charles McManus, M.D., of New Orleans.

A negro boy, aged 13, of good constitution, was admitted to the Maison de Santé on the 16th January last, with an irreducible inguinal hernia. For two years previous he had worn a truss; but as he was running on an errand, the day before his admission, he felt pain from its pressure, and took it off, when protrusion occurred. The tumour was now tense, and painful; the pulse frequent; the abdomen, when pressed on, was free from pain, and the stomach undisturbed. The usual means for reduction were resorted to, and continued for five hours without success; yet an operation was not deemed advisable at that time, as the constitutional symptoms of strangulation were not evident, and the bowels, or at least the large intestine, responded freely to emollient enemata; which led to the opinion that the hernia might be merely omental. An anodyne draught, and application of pounded ice to the tumour, were prescribed for the night.

The tumour was undiminished on the 17th. The boy had rested well during the night: he complained of pain in the diaphragm. Blood was taken from the arm, and leeches applied to the tumour; the symptoms, however, increased, and towards noon there was tenderness and distension of the abdomen, accompanied with vomiting. The operation, from which alone relief could be expected, was decided on, and Doctor Stone proceeded to perform it. On reaching the sac, it was found tense, and of a purple color. A small opening was made in its lower part, through which about an ounce of dark fœtid fluid escaped: a division of it was made upwards to the ring, and the intestine exposed. This, which was the small one, was protruded to the extent of about four inches, in a mortified state, and already giving way to the pressure of its contents. As reduction, under these circumstances, would have been absurd, an incision was made through the mortified gut, and the stricture dilated, to permit the evacuation of the intestinal contents. The subsequent progress of the case was left to nature, as affording, perhaps, the best

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* We think the title of this paper unwisely chosen. It had better have been "the effects of *Calomel* on the system." Some years ago we had many similar *effects* in New Orleans, but nothing of the sort of late years. Edtrs.

chance of the continuity of the intestinal canal becoming established again. The severe symptoms ceased after the operation: the fœces were discharged from the open intestine; in three days the protruded part had sloughed off: the discharge, from this time, diminished gradually, and the fœces again passed on to the rectum. During the progress of the cure, acute peritonitis had to be combatted. In six weeks after the operation, the part was healed by granulation. The patient was discharged in good health, without even a fistulous opening.

ART. VI.—*Remarks on Trismus Nascentium.* By F. M. FITZHUGH, M.D., of Madison County, Mississippi; and H. V. WOOTEN, M.D., of Loundesborough, Ala.

[In compliance with the call made in our January number, for information concerning this fatal disease, we have been favored with the following letters, which we take pleasure in laying before our readers. We have but little doubt that the cause of the disease is intimately connected with the management of the umbilicus, and therefore think our correspondent's suggestion valuable. In connection with this subject we will mention, that a Creole professional friend recently informed us, that *Trismus Nascentium* was much more common amongst the plantations below the city a few years ago, than it is now; and that the present exemption is attributed to a plan of managing the navel adopted within the last few years: it consists simply *in dressing it with balsam copaiba.* We learn, that since the adoption of this, the disease is scarcely known in that neighborhood. We should be pleased to receive farther facts and suggestions on the subject.—We had expected to insert Dr. Fitzhugh's communication in our last number, but it was unavoidably postponed. We now have the pleasure of adding Dr. Wooten's interesting letter on the same subject, which has arrived just in time for the press. Dr. W. seems to doubt the correctness of the opinion we expressed in our last number, that this disease is more common in this city than any where else in the United States. Our remark was solely based upon the large nnmber of cases found among our bills of mortality. We should be pleased to receive farther facts and observations in relation to its prevalence throughout the southern States, and especially as to its comparative frequency among white and black infants. The disease certainly occurs amongst both classes in this city, but to what relative extent we are not prepared to state at present. We repeat our invitation for farther facts.—EDS.]

Messrs. Editors:— Having observed, in your valuable journal, the great mortality in your city from *Trismus Infantum*, or *Tetanus Nascentium*, I am induced to offer the following view of its cause.

I have seen three cases only, all of which proved fatal. The symptoms were precisely those stated by Dr. Eberle in your January number; I therefore deem it unnecessary to repeat them.

The treatment which I adopted in these cases was somewhat different in each. I, however, blistered the abdomen, gave calomel, tinct. opii, oil, &c., without any good effect whatever. I doubt not, if taken in time, the treatment recommended by Dr. Eberle would be very efficient.

It is the cause of this alarming disease, however, which I wish to consider. Believing it proceeded from some mismanagement in dressing the navel, I requested the owner of that plantation to let me know when the next accouchement took place. He did so. I attended particularly to the dressing of the navel; following plan usually pursued, that is, folding a piece of linen three or four times, making a hole in it for umbilical cord, then applying bandage three inches wide, and three or four feet long, drawing it pretty tightly, so as to give sufficient support to abdomen, passing it around two or three times. This child never had an attack of *fits*, as they termed it, and is now in fine health. In a few weeks I had another opportunity, which resulted, under the same management, with equal success. Previous to this there had been some fifteen or twenty deaths from this disease on this plantation. I have every reason to believe that it depends upon the manner in which the abdominal bandage is applied.

The dressing of the infant is generally—I might say always—left to some female present, and, for fear of *hurting the navel*, she applies the bandage too loosely; the consequences are, when the child cries, the umbilicus is protruded, producing, in some cases, slight umbilical hernia, and otherwise irritating the navel, although it may externally appear perfectly cicatrized.

The navel should not be troubled or examined under four days, after removing it: the bandage should again be applied, and worn for three or four weeks, or as many months would not be amiss.

It has been supposed by some that the disease was produced by using old rusty scissors to divide the cord: this I am confident is not so, having known the nicest instruments used with no better success.

I hope physicians having an opportunity of testing this, will give it their attention. Respectfully, F. M. Fitzhugh.

Madison County, Miss., Jan. 20, 1846.

Lowndesboro', Ala., March 20th, 1846.

Messrs. Editors:—After stating the mortality of New Orleans in 1845, you remark concerning *Trismus Nascentium*, that "this affliction is doubtless much more common here, than anywhere else in the United States, and demands careful investigation."

After assuring you that the results of such "careful investigation" would form a chapter in your Journal of the highest interest to me, at least, I must beg your indulgence in the expression of a doubt of the correctness of your opinion in regard to the disease being more common in New Orleans than any where else in the United States. It is a disease of fearful frequency on the cotton plantations in this section of Alabama. I am not prepared to compare it with other maladies in respect to frequency, but I believe that it destroys more negroes than any other single disease, in this region of country. In a practice of ten years amongst these plantations, I have seen a great many cases. Sometimes, I have found it of such frequent occurrence, as to present the appearance of an epidemic. Yet *I have never seen a white child afflicted with the disease.* Is this the case in New Orleans?

Again: I have never seen a case of *decided* Trismus Nascentium, that did not prove fatal. Indeed, so well is this characteristic of the disease now known, that it is very generally deemed utterly useless to call in medical aid, after the initiatory symptoms are well developed.

I have tried every plan of treatment which books, or the most anxious study on my part could suggest, but all wholly in vain.

I have made post mortem examinations in several cases, and found the pathological appearances as uniform as in any other disease. They are as follows: — Heavy vascular engorgement of the peritoneum throughout its whole extent, denoting the highest inflammation. All the portion surrounding the entrance of the umbilical cord into the abdomen, for a circumference of from one to three inches, was in a gangrenous condition. The liver was unnaturally heavy and stiff, with its veins fully injected with fluid blood. There was also heavy engorgement of the substance and membranes of the base of the brain, and along the medulla oblongata, and cervical portion of the spinal marrow.

I have usually observed the first symptoms to make their appearance about the time the umbilical cord comes away, and from this I at first supposed that it was the effect of awkwardness in dressing the navel by the ignorant midwives who usually attend on the plantations, but careful investigation led to nothing conclusive on this point.

Having taken the liberty to offer you these few facts, under the hope that they will not be entirely useless to you, allow me to subscribe myself,

Your obedient servant,

H. V. Wooten, M.D.

Part Second.

REVIEWS AND NOTICES OF NEW WORKS.

I.—*Lectures on the Nature and treatment of Deformities;* delivered at the Royal Orthopoedic Hospital, Bloomsbury square; by R. W. Tamplin, F. R. C. S. E. Surgeon to the Hospital. Philadelphia: E. D. Barrington and Geo. D. Haswell. 1846. 8vo. pp. 216.

These Lectures were originally published in the London Medical Gazette, and have just been put into a volume for the convenience of such members of the profession as may not possess that Journal. The American Edition now under notice constitutes a volume of Bell's Select Medical Library.

The study of deformities in all points of view may be regarded as belonging exclusively to the recent effort of medical history—until lately, deformities and monstruosities of all kinds were regarded as freaks of nature, mere accidental deviations of form, not governed by any laws, or in opposition to those which govern normal development; this view led them to be regarded as unsusceptible of classification, and their study totally useless, as it was supposed there was no principle or law to be discovered by such researches. The fallacy of these views is fully exhibited by the results of Saint Hillaire, Serres and others, as regards the theoretical bearings of the subject; and the great advances made in the treatment of deformities, in our own day, speak volumes in the practical results which have accrued from accurate investigations in this prolific field. It is more especially of the relations of this subject to medicine and surgery that the volume under consideration treats.

From the circumstance that many of these malformations are congenital, it was inferred that the bones were created of a given form, and adapted only to the peculiar arrangements and combinations existing in the individual cases. On these inferences was based another, that as the deformed came misshapen and monstrous from the hands of God, it was not in the power or province of man to interfere for their amelioration. It was regarded as an impious interference of man, with the "mysterious workings of Providence."

The deformed too were looked upon as a class who should be set apart from men as bearing marks of celestial displeasure, and there are too many instances in which the sensitive mind of these unfortunate people is made to smart under the keen reproach of parents or friends and the sarcasms of others.

"They have been regarded as loathsome in body, and depraved in mind; they have often isolated themselves from their fellow creatures; persecution of them has ever been sanctioned by the Mosaic ceremonial, which admitted the unblemished alone to the dignity of the priesthood. Possessing all the feelings, and susceptible of all the impulses which animate the breast of man, frequently morbidly sensitive from the consiousness of their deformity, adorned with genius, gifted with wit, graced by fortune and by birth, crowned with learning, — still is the deformed man exposed to the derision of the heartless, and is shut out from the world simply because nature has played some freak by which he differs from his fellow mortals."

Our author starts with the determination to limit his lectures to the consideration of facts and matters of practical bearing. The general principles by which orthopedists are now governed, are, according to him, the same that were laid down by Delpech in his Orthomorphie, published in Paris in 1828, and are comprised in the following rules:

1st. "A tendon to be divided must not be exposed; and its division should be made by turning the instrument on one side, so that the line of the incision may not be parallel to the division of the skin; without this precaution risk of exfoliation of the tendon is incurred."

2nd. Immediately after division of the tendon, the divided ends should be brought into contact with each other, and kept in this position by a suitable apparatus during the entire period necessary for their union."

3rd. "Inasmuch as it can only take place by the intervention of an intermediate fibrous substance, this substance, before it has become firm, can, and should be extended gradually and carefully, until it has assumed a degree of length equal to the shortened muscle."

4th. "When this degree of extension has been effected, the parts should always be fixed in the position, and kept so until the new substance has acquired its requisite degree of consolidation."

In regard to the mode in which tenotomy increases the length of the muscle there is some difference of opinion. Stromeyer dissents from the opinion of Delpech, that it is always by the formation of a new portion of tendon between the separated extremities of the divided tendon; he says:

"Delpech laid it down as a rule, that the surgeon should encourage the formation of sufficient substance between the two divided ends of the tendon to maintain the function of the muscle, and should not destroy the new union by immediate extension, but commence extension some days after the operation: this rule is most important for the safe performance of orthopædic operations, and its value should be duly estimated. The idea that the elongation of the muscle is effected through the cicatrix, is a false one; the extent of the substance or cicatrix is quite inadequate for this purpose. In some cases of pes equinus the gastrocnemii are two or three inches too short; and, in wry neck, the sterno-mastoid is equally short, yet the cicatrix, after the cure, is but a few lines long. The elongation of the muscle must be effected in these cases at the cost of its contractility, and thus the incision of its tendon acts not only on its mechanical, but also on its vital properties, and, by the temporary diminution of its irritability, its contractile power is diminished, and any increase of it prevented. This view is confirmed by observations made in cases where the tendons of sound muscles have been lacerated. It is a remarkable circumstance, that when the tendo-Achilles unites in an imperfect manner after injury, the foot is not drawn up by the flexor muscles, but hangs like a loosely connected part, showing that the diminished irritability of such a mass as the calf of the leg exerts a weakening influence on the entire extremity. That any person should commence extention immediately after the operation, and attempt to restore the limb to its natural position, using like Sartorius, a degree of violence which makes us shudder to think of, is neither necessary nor advisable."

"The immediate restoration of a limb in its natural position is not to be recommended, for by extension before the healing of the wound in the skin, the parts are liable to inflame or suppurate; by gradual extension, the contractility of the muscle, the tendons of which have been divided, is interrupted for a time, and restored by the stretching and motion of the parts when the foot is again used. 'In all my cases,' adds Stromeyer, 'of division of the tendo-Achilles, the use of the muscles of the calf has been completely restored. Dr. Weiss informs me that at Paris the use of the muscle of the calf is not always restored. This may probably depend on the immediate separation of the ends of the tendon, which does not prevent its healing, but hinders the restoration of the function of the part; it is also possible that, by extension of the intervening substance, which, from immediate extension, is very thin, the cicatrix itself may be lacerated.'"

Mr. Tamplin makes the following remarks in reference to this matter:

"The views of Stromeyer are, I believe, on the whole correct. I am of opinion, however, that the new substance will and does admit of sufficient extension to compensate for the greatest amount of shortening met with in any deformity; for I have repeatedly, in cases of talipes equinus, found the new connecting medium full two inches in length within three or four weeks following the operation, especially in those cases where there is no resistance in the joint, and where the foot admits of being rapidly brought in its normal position. Again, how often is it that, in fracture of the patella, or in rupture of its ligament, the new and permanent uniting medium is full three inches in length, sometimes more, and sufficiently strong itself, were it not that the muscle, from its greatly increased length, loses its power of action."

"It is true, as a general rule, that the cicatrix after the cure is effected, measures but a few lines, oftentimes only a line, in the thickness, provided proper care has been taken during the treatment, which arises from the circumstance, that every new substance, after it has fulfilled the purpose for which it was generated, viz. the restorative process adopted by nature to remedy any wound or injury, loses its vascularity, except so far as is necessary for its individual vitality, contracts upon itself, and draws with it, if unrestrained, the part with which it is connected."

"In the foot I have found the permanent cicatrix full two inches in length (after the operation of talipes equinus;) and a worse distortion produced, viz: the talipes calcaneus valgus, where the foot was placed in position immediately after the operation, and kept for a long time in the flexed position; this position was not however, maintained, except during the exercise of volition. The elongation in congenital cases, and also in noncongenital cases, of young subjects, is undoubtedly effected eventually at the cost of the contractility of the muscle, but not primarily: this is a secondary result, for no uniting medium can by possibility possess the power of drawing down the muscle, any more than the newly formed granulations following a burn can do; the cicatrix, as it contracts, certainly does so, but after the foot is brought into position; and hence the linear cicatrix. The force of the contraction of a cicatrix is sufficiently evident in the instances of burns alluded to, not only to draw down and overcome one muscle, but even a set of muscles—as the extensors of the head and neck, or of the arm, and other parts."

After giving some general views of the physiology of this branch of surgery, our author proceeds to the particular deformations of the lower extremities, the spine, including those of the neck, and finally to those of the superior extremities.

We cannot but think that Mr. Tamplin deserves the thanks of the profession for his silence on the subject of strabismus and stuttering; for unless he could say something to redeem these operations from the hands of men scarcely better than our "corn-doctors" it would be better to say nothing of them, as appertaining to our profession.

Not having made a particular study of the nature and treatment of the class of disease, to which this work refers, we are illy prepared to give a decided opinion respecting its merits;— but regarding this as a legitimate and rapidly progressive branch of surgery, greatly calculated to promote the happiness of our fellow beings, it affords us pleasure to see works of such extent, and such apparent value, given to the profession, at so moderate a price. As far as we have compared this work, with others on the same subject, the comparison has been most favorable to the former, and we do not hesitate to recommend it above any we have seen, to those who desire a correct and accurate knowledge of the deformities of which the author treats W.M.C.

II.—*Homœopathy, Allopathy, and "Young Physic."* By John Forbes, M.D., F.R.S., &c. &c. Philadelphia: Lindsay & Blakiston; 1846: pp. 121.

This is a republication of one of the most interesting papers that has met our eye for many a day. It appeared originally in the 41st number of the British and Foreign Medical Review, and we must commend the judgment of the American publishers in putting it in a separate and cheap form, calculated to give it an extensive circulation. Dr. Forbes is one of the readiest and ablest writers of the profession, and this paper contains so many bold and rational views in regard to medical science, and its state at the present day, that we doubt not it will be read with profit by all who may have the good fortune to meet with it. Being itself a review of *eight works* relating to Homœopathy, we cannot be expected to give it a minute analysis; its importance, however, demands as much attention as our limits will allow us to bestow upon it.

Dr. Forbes sets out with a philosophic review of the new system called Homœopathy, which he hesitates not to pronounce "as ingenious as many that preceded it, and destined, probably, to be the remote, if not the immediate, cause of more important fundamental changes in the practice of the healing art, than have resulted from any promulgated since the days of Galen himself." He then touches upon the common medical doctrine termed *Allopathy*, in contradistinction to Homœopathy; showing, boldly and independently, its errors and imperfections. He then comes to the dawn of a brighter era in medical science, just now springing up, which he sportively personifies under the title of "*Young Physic.*" We shall notice each of these subjects in the same order.

1. *Homœopathy.*—Dr. Forbes says, that Hahnemann, the founder of this system, was undoubtedly a man of genius and a scholar—a man of indefatigable industry, and of undaunted energy. He thinks him one of the greatest systematists and theorists that has appeared in the history of medicine; "nor will the overthrow of his system, as a system, deprive him of his fame, so long as Paracelsus, and Stahl, and Sylvius, and Boerhaave, and Brown, and the hundred others of theoretical renown are remembered by their successors in the schools of medicine." Of the influence, and general estimation of Homœopathy among physicians, Dr. Forbes says:

"The thoroughly radical change in the theories and practice of medicine, propounded in the system of Hahnemann,—a change equivalent to a total reversal and subversion of almost all that had preceded it,—naturally roused great and general opposition to it in the minds of medical men. This, and the seemingly-monstrous extravagance of one of its main dogmas—that of infinitesimal doses—so abhorrent at first sight to common sense, and so obnoxious to the attacks of a facile ridicule,—has, up to this day, prevented common justice being done to the new system, and to its author and his successors. By most medical men it was taken for granted that the system was one, not only visionary in itself, but was the result of a mere fanciful hypothesis, disconnected with facts of any kind, and supported by no processes of ratiocination or logical inference; while its author, and his apostles and successors, were looked upon either as visionaries or quacks, or both. And yet nothing can be further from the truth. Whoever examines the homœopathic doctrines as enounced and expounded in the original writings of Hahnemann, and of many of his followers, must admit, not only that the system is an ingenious one, but that it professes to be based on a most formidable array of facts and experiments, and that these are woven into a complete code of doctrine with singular dexterity and much apparent fairness. And it is but an act of simple justice to admit, that there exist no grounds for doubting that Hahnemann was as sincere in his belief of the truth of his doctrines as any of the medical systematists who preceded him, and that many, at least, among his followers, have been and are sincere, honest, and learned men. That there are charlatans and impostors among the practitioners of Homœopathy cannot be doubted; but, alas, can it be doubted, any more, that there are such, and many such, among the professors of orthodox physic?

"On these grounds, then, it appears to us reasonable, that the claims of Homœopathy, regarded as a system of medical doctrine, ought to be admitted so far as to entitle it to investigation, at least; and, in undertaking such an investigation, we have no more right to reject the evidence supplied in its favor by its professors, than we have of rejecting any other evidence in favor of any other medical doctrine, theoretical or practical."

Hahnemann conceived the fundamental idea of his new doctrine from observing the effects of Peruvian bark upon himself. He was at the time engaged in translating Cullen's Materia Medica into German, and being dissatisfied with the author's attempt to explain the action of this medicine in curing intermittent fevers, he resolved to try its effect upon his own person—he being, at the time, in perfect health. Having taken a sufficient quantity of this drug, he affirms that he was speedily attacked with symptoms resembling those of ague, and forthwith arose in his mind the conception that the virtues of this and all other medicines depend upon their capability to excite in the healthy body the diseases which they cure, when occurring naturally in the diseased body. He made numerous experiments upon himself and others, with many other medicines, and always, it is said, with the same result, and thus established satisfactorily to his own mind, and promulgated, what he considered the grand and universal law, that "every (dynamic) disease is best cured by that medicine which is capable of producing in the healthy body similar symptoms, or a similar disease—*similia similibus curantur.*" Possessed of this, as he conceived, unfailing clue to all the mysteries of therapeutics, he and his disciples commenced an extensive and long-continued series of experiments with various medicines on their own persons, all resulting in the confirmation of *the great law.*

Hahnemann gave as the *rationale* of cure, "that of two similar actions developed in the same part, the stronger destroys the weaker; but he regarded his doctrine as substantially based *on experience*, and therefore as independent of any theoretical explanations. The curing of diseases *homœopathically* was, he maintained, *a fact* which could not be disputed, whether the theory invented to explain it were true or false." We will pass over the reasoning and observations which induced the homœopathists to use their infinitesimal doses of medicines: this, too, they say, is the result of *experience*, but it savors awfully of *humbuggery*, and the candid, rational inquirer can scarcely forego the conclusion, that if it possess *any merit*, it must depend upon leaving the disease fairly and completely to the *vis medicatrix naturæ.* It would require a vast number of experiments upon the sick and well to settle this question—doubtless, more than any man, or set of men, will ever faithfully make; yet, if a fair trial could be made between the *homœopathic* doses, and *no dose at all*, stripped of all adventitious influences, we are inclined to think that the great system of Hahnemann would find its place amongst the amulets, charms, quack nostrums, and the like impostures, that have infested mankind since the earliest periods of society. See what Dr. Forbes says of the infinitesimal doses.

"Every one has heard of this incomprehensible posology; but we are inclined to believe that few, if any, but the homœopathists themselves, or those who have read their books, (and only a small number have,) are aware of its infinite and astonishing minuteness. What passes respecting it, in common medical parlance, is regarded as a playful exaggeration of the truth, garnished good-humoredly for the nonce, like the ornamental facts of the story-teller.

And it is no wonder that this is so. Mere imagination, working primarily on its own ground, could never have reached such a climax of the marvellous. Here, assuredly, if anywhere, the truth, if truth it be, is stranger than fiction.

"So minute are the doses prescribed by the Hahnemannic school, that they are scarcely conceivable by the human mind. They defy all the powers of chemistry and physics to detect in them any trace of the remedial substances which they profess to contain, and they almost confound arithmetic in reckoning their amount. We are not ashamed to confess, that our own powers are inadequate to put down in figures an ordinary homœopathic dose, and we suspect that many of the homœopathists themselves would find themselves in the same predicament on trial. The following are the different *attenuations* or doses used.

" First	= one hundredth of a drop or grain.	
" Second	= one ten-thousandth	do.
" Third	= one millionth	do.
" Sixth	= one billionth	do.
" Ninth	= one trillionth	do.
" Twelfth	= one quadrillionth	do.
" Fifteenth	= one quintillionth	do.
" Eighteenth	= one sextillionth	do.
" Twenty-first	= one septillionth	do.
" Twenty-fourth	= one octillionth	do.
" Twenty-seventh	= one nonillionth	do.
" Thirtieth	= one decillionth	do.

"The primary dilutions or attenuations are used comparatively rarely; the higher ones, as the sixth, twelfth, twenty-first, and thirtieth, very commonly. It may be worth a moment's trouble to try how far we really understand or comprehend these numbers. Looking at the first of these, we have no difficulty. The *hundredth* (100th) part of a grain, is intelligible enough; the *ten-thousandth* (10,000th) is comprehensible, but begins to waver before the mental view; while the *millionth* (1,000,000th) part of a grain puts our powers of comprehension on the rack, and leaves us in a chaos of undefined entities or non-entities, we know not which. We fancy that we grasp the reality, and then it instantly vanishes as a phantom, even beyond the sphere of imagination itself. Having got so far, the additional subdivisions, or attenuations, scarcely add to our difficulties. The mind, in any such case, is occupied by a word more than a thing,—and whether the word be a millionth, billionth, or decillionth, the power of comprehension seems the same. And yet the *actual difference* between these quantities is immense,—so immense as to be almost as inconceivable as the actual things themselves. This will be more intelligible, we think, by setting it down in words, thus:

" One thousand thousands, is	A Million.
" One million millions*	A Billion.
" One million billions	A Trillion.
" One million trillions	A Quadrillion.
" And so on to	A Decillion.

"Imagine, if you can, a grain of silica, or charcoal, or oyster-shell, (powerful remedies, sccording to Hahnemann and his followers, in this attenuated condition,) divided into this number of parts: and one of these parts is not only a fit and proper dose to be given as a remedy for severe diseases, but is an agent of such potent influence on the animal economy, that *one dose* of this amount will continue acting for thirty, forty, or fifty days, and must not be interfered with by any repetition of it, for fear of deranging or destroying its curative virtue! Thus, Hahnemann tells us, that a sextillionth of a grain of

* This is according to the English mode of calculation. The French calculate by thousands—not by millions; e. g., with them a billion is a thousand millions only.

carbonate of ammonia will act beneficially upwards of thirty-six days;† that the decillionth of a grain of oyster-shell (*calcarea*) will require forty, fifty, and even more days, "to effect all the good it is capable of;‡ that a similar dose of plumbago (*graphites*) will act for at least from thirty-six to forty-eight days;§ and a like dose of phosphorus, at least forty days." ||

Hahnemann and his followers only attribute such powers to their infinitesimal doses when the remedies are prepared in a *peculiar* manner, and this peculiarity consists altogether in the mode of solution, the precise number of shakes given to the phial, &c., all which is most minutely directed. New properties and powers are developed by the frictions and shakings to which the medicines are subjected; the evidence in support of which opinion "is entirely derived from *experience*, they say." The quality of the evidence which they can adduce under the name of *experience*, deserves to be well considered. In regard to this, Dr. F. remarks:—

"If they adduce no other proof but the fact of diseases ceasing on or after the employment of their medicines, the fact, though repeated *ad infinitum*, if standing simply by itself, must go for nothing in the way of proof. If they can show a sufficiently large number of two parallel series of diseases, the one series treated homœopathically, the other left to nature, and show that all or the vast majority of the one set were cured or benefitted, and the other set not, —then, indeed, we shall be prepared to admit the conclusiveness of the argument based on experience. And in this case we must concede to the Homœopathists, that no argument based on the mere ground of a positive inconceivableness of a dose, or a supposed impossibility of its action, will have any weight. 'Empty declamations,' to repeat Hahnemann's own words, 'must give way before the might of infallible experience.'"

We come now to Hahnemann's doctrine of *the Origin and Nature of Chronic Diseases;* and here we shall be as forcibly struck with his strange hypothetical genius, as we were with his ideas of the nature and power of remedies. Hahnemann maintains, that all chronic diseases, or with hardly an exception, are derived from three cutaneous diseases, *syphilis*, *sycosis*, and *psora*, or common itch. Of the whole class of chronic diseases, he attributes one-eighth part to the two former maladies, and the remaining seven-eighths to the last. Think of that, ye delicate and fastidious invalids who can show forth such a variety of chronic complaints! Must not your *amour propre* be greatly offended at the solemn announcement that you are the victims of that vulgar disease, *the itch?* Look back upon your past lives, and trace up the *filthy association* from which you must have imbibed this loathsome affection; for you cannot elude the awful discovery. We confess there is but little satisfaction in tracing such a host of diseases only back to other diseases. Our curiosity leads us to inquire into the first link of the morbid chain, and we should like Hahnemann to tell us the causes of his prolific *syphilis*, *sycosis*, and *psora*. This we desire more particularly, because we are informed that "the real object of consideration with the physician, and the thing to be cured, is not the ostensible diseases, but their all-pervading cause and basis." Dr. Forbes informs us, that Hahnemann considers *psora* to be originally a disease of the

† Die Chronischen Krankheiten, Band ii., p. 20.
‡ Ib., p. 67. § Ib., p. 148. || Ib., iii., p. 48.

whole system, which only shows itself locally on the surface, in its progress. But this is rather a summary way to get over difficulties, and at variance with the commonly received and pretty well established opinion that itch depends upon animalculæ which originate in neglect of personal cleanliness—that the disease is confined to the cutaneous surface, and is spread from person to person by direct contact. This simple and rational explanation, however, would not suit the mysterious pathology of Hahnemann's system. He informs us, that if the itch be cured, in its superficial form, in any other way than *homœopathically*, it infallibly gets worse internally, and may subsist in the system for years, without assuming the semblance of any formal chronic disease—the patient, all the time, being unaware of being out of health. The list of diseases said to be derived from itch comprises nearly all our common chronic diseases, of which 120 are formally named. The total number of antipsoric remedies detailed is twenty-two.

Such is an imperfect sketch of Dr. Forbes' "Brief outline of the main doctrines of Homœopathy." In his review he displays towards it a great deal of fairness and liberality. He admits its ingenuity—that it may be defended by feasible arguments, like all other medical systems, but says, "it is quite another consideration whether it be *true*." Here Dr. F. indicates a few of the most important objections to Hahnemann's system, and we cannot do better than to give them in his own words:

"1. We hold the great alleged fact from which the doctrine took its rise, to be no fact at all; or, at least, not to be a fact of that generality of manifestation, which a theory said to be of universal applicability ought to rest upon. We deny, on the one hand, that many of the medicines said by Hahnemann to be capable of exciting artificial diseases, or the symptoms of diseases, in the healthy body, are really possessed of such powers. We instance, in proof of our assertion, the very medicine which gave rise to the idea of the doctrine in its author's mind—cinchona. We deny that it will produce ague, or anything like ague, or any other form of fever, in the majority of human beings; and so of a large proportion of the homœopathic remedies in most common use. On the other hand, we affirm, that some medicines are capable of curing morbid conditions of the body which are incapable of exciting any such condition in the healthy body.

"2. We affirm, that a large proportion of the experiments performed by Hahnemann and his friends, with the object of ascertaining the therapeutic properties of medicines, are altogether fallacious; and that the alleged facts thereby elicited are not facts at all. We believe that of the numerous—we had almost said, innumerable—symptoms recorded in their trials, the vast majority bore no other relation to the medicaments swallowed, than the relation of sequence. Not a shadow of *proof* exists that the symptoms were the consequence or direct effect of the medicine; while a thousand reasons can be adduced for supposing the contrary to be the fact. As the doses administered in these trials—at least, in the later and principal trials—were administered in infinitesimal doses, we are fully warranted in even denying entirely that *any* effect was produced by them. Before we can be called on to admit the recorded phenomena as *consequences* of the medicines, we have a right, as in the case of the treatment of diseases, to call for a parallel series of healthy persons set down to record all their sensations for days, after taking *no* medicines. This the homœopathists cannot give us. In these experiments it seems to be taken for granted that every bodily or mental change, every sensation, every action that occurred subsequently to the medicine being taken, was *caused* by the medicine. Every feeling and occurrence were recorded, and every thing is admitted as matter of course Yet no unprejudiced person, who examines these records even superficially, can for a moment believe that one-half or one-tenth of the symptoms recorded were, or could be, produced by the medicaments swallowed. The very *number* of the symptoms stated to be produced, independently of their character, suffices to show the absurdity of the conclusions drawn. Thus, for example, 1090 symptoms are recorded as effects of oyster-shells (calcarea); 590 as produced by plumbago; 1242 as the effects of the ink of the cuttle-fish (sepia). If we had room to give specimens of the various symptoms, no doubt could remain on any candid mind as to the utter want of any necessary connection between a vast proportion of them and their alleged causes. Among these symptoms, thus alleged to be produced, we have almost every sensation which man or woman can feel, derangement of nearly every function of the body, and many formal diseases, *surgical*, as well as medical.

"3. Even in the cases where positive effects are produced on the healthy body by medicines in sensible doses, these effects (except in a very small number of instances) bear a most imperfect resemblance to any natural malady, or even to the *symptoms* of any malady. Several formidable diseases may, indeed, be said to have no symptoms—as, for instance, those diseases which are called *latent*. How many diseases have been detected only on dissection after death, and which have escaped the recognition of the most experienced physicians? Every physician, for example, has met with cases of chronic pleurisy with extensive effusion into the chest, which presented *no pectoral* symptoms, and which were only detected by auscultation. How could the fitting remedy for such cases be selected on the principle of *similia similibus?*

"4. Many persons deny the truth of the homœopathic therapeutics, on the mere ground of the extreme improbability of the *theory* of disease adopted by the homœopathists. We do not admit the validity of this objection. If we once admit that the homœopathic doses possess a medicinal potency, and that this potency exerts itself in exciting actions analogous to those of certain diseases, we see nothing unfeasible in the doctrine that the new artificial action should destroy the previous natural or morbid one. At least, this is as good and rational a theory as most of our orthodox medical theories. And, indeed, it is supported by several strong analogies afforded both by pathology and (allopathic) therapeutics.

"5. But to admit the potency of the homœopathic medicaments is not so easy. Indeed, it is so difficult, that all the arguments that have hitherto been adduced in support of the affirmative of the proposition, are incapable of making any impression on ordinary minds, while the glaring improbability of the fact lies open before them. All the arguments of weight seem to be on the other side; and nothing but the demonstration of the truth—if truth it is—by positive physical facts within the sphere of the senses, can ever win assent to it. The reasons against the doctrine are so manifold and obvious, that it is almost unnecessary to state them. That substances possessing a power of acting on the animal economy in doses of a certain appreciable amount, and which are found to lose their power when administered in quantities still appreciable but less than this amount, should once more acquire the same or similar properties, when this lesser quantity is rubbed for a few minutes in a mortar, or shaken for a second or two in a phial, would be a thing most strange and unaccountable. That when the quantity was reduced not merely below an appreciable amount, but so far below this as to vanish utterly from the senses, and set at defiance all power of detection, and almost of calculation; nay, that when attenuated to such a degree as to be inconceivable by the human mind, the substance should not only regain the potency it had lost, but a potency vastly greater—would surely be still stranger, and still more unaccountable. But when, going far beyond all this, we find the homœopathist maintaining that substances utterly powerless in a state of sensible bulk, even in the greatest amount, acquire astonishing powers by mere subdivision, without any discoverable change in their physical or chemical properties,*—can any proposition be submitted to human apprehension that seems more utterly improbable—more ludicrously absurd? To be called on to believe that the decillionth of a grain of charcoal or oyster-shell is capable of producing hundreds of the most formidable symptoms, and of curing, as by magic, the most inveterate diseases—while we can take ounces, nay, pounds, of the very same substance into our stomachs with no other inconvenience than its mechanical bulk—seems so gratuitous an outrage to human reason, that the mind instinctively recoils from the proposition."

Dr. Forbes then gives us a good specimen of the precise nature of the manipulations to which the homœopathic medicines are subjected, and under which their marvellous powers are developed; but observes, that "it is *impossible* for any human being, during the course of a long life, much less in the course of a few years, to have performed a sufficient number of experiments, or made a sufficient number of comparative trials, to enable him to state, with any degree of certainty, that those particular manipulations, and none others, were the exact and exclusive means to produce the effect desired. Thousands and tens of thousands of instances would be insufficient, as could be shown mathematically, to enable the experimenter to decide whether there should not, for example, be *three* shakings instead of *two*, or whether the triturations and the scrapings should not be each of *five* minutes, instead of the one being *six* and the other *four*." Add to this the precise time of rubbing and shaking, the turning of the phial on its axis, &c., and we can but see the *humbug* sticking out most palpably.

If Homœopathy came before us only as a *theory*, it would be unnecessary to waste any more time in the discussion of its merits, as Doctor Forbes well observes; but it assumes a much more imposing aspect, and claims the notice of mankind on the grounds of its superior power of curing diseases, and preserving human life.

"If, as is maintained by its advocates, it is indeed true, that with its infinitesimal doses it cures diseases; nay more, that it cures them exactly according to the ancient *beau ideal* formula, *tuto cito et jucunde;* and cures them also in a larger proportion than is done by ordinary treatment; it matters but little whether its theory is false or true." "If it can *prove* that it does all these things," says Dr. Forbes, "we are bound to admit, and we are prepared to admit, that this is a kind of evidence sufficient to overthrow all the arguments we can bring against it, however strong, and all our reasonings, however just: improbabilities, however glaring, and even what seem impossibilities, must go for nothing." Dr. Henderson, of Edinburgh, one of its most distinguished advocates, observes, "The question now is, not whether it originated in a mere speculation, or an induction of facts, but whether it be, as actually employed in the treatment of diseases, a valuable acquisition to the practice of medicine." In this point of view, what has Homœopathy to present to us? Dr. Forbes here divides the subject into two parts: 1st, As to the *absolute power* of Homœopathy to cure diseases; 2dly, As to its *power relatively to that of Allopathy.* Under the first head, Dr. F. thinks, "we are justified in stating that no unquestionable evi-

"* It will be afterwards seen, that Hahnemann says the chemical properties *are* changed by attenuation; but the arguments he brings in proof are invalid."

dence exists as to the absolute power of Homœopathy to cure diseases." As before remarked, the only way to settle this question would be by a sufficient number of fair experiments. "Such experiments as have been made — and several have been made in the German hospitals — must be considered, as far as they go, as unfavorable to the claims of Homœopathy." It appears, also, that such experiments as have been made in private practice, "tell against Homœopathy."

On the second head, *i.e.*, the comparative power of Homœopathy and Allopathy, the evidence is very different, both in character and amount. We have the results of upwards of 6000 cases, of all sorts of diseases, treated in the Hospital of the Sisters of Charity in Vienna, during a period of eight years, from 1835 to 1843, by Dr. Fleischmann, "a regular and well-educated physician;" and reports of 122 cases by Dr. Henderson, professor of Pathology in the Edinburgh University.

Dr. Forbes says, that —

> "These tables (Dr. Fleischmann's) substantiate this momentous fact, that all our ordinary curable diseases are cured, in a fair proportion, under the homœopathic method of treatment. Not merely do we see thus cured all the slighter diseases, whether acute or chronic, which most men of experience know to be readily susceptible of cure under every variety of treatment, and under no treatment at all; but even all the severer and more dangerous diseases, which most physicians, of whatever school, have been accustomed to consider as not only needing the interposition of art to assist nature in bringing them to a favorable and speedy termination, but demanding the employment of prompt and strong measures to prevent a fatal issue in a considerable portion of cases."

From the general result, no conclusion can be drawn peculiarly favorable to the *new practice*, yet it cannot be denied that it was as successful generally as the old.

"But the results presented to us in the severer internal inflammations are certainly not such as most practical physicians would have expected to be obtained under the exclusive administration of a thousandth, a millionth, or a billionth part of a grain of phosphorus, every two, three, or four hours." It is attested by a respectable physician (not a homœopath), that he watched several of Dr. Fleischmann's cases of pneumonia, and traced their progress, by the physical signs, through the different stages of congestion, hepatization, and resolution, up to a perfect cure, within a period of time which would have appeared short under the most energetic treatment of allopathy." The relative proportion of cures, and the relative mortality of the different diseases, are found to be about the same in Dr. Fleischmann's report, as in ordinary practice. Inasmuch, therefore, as both the *kind* and *amount* of cures are found to be about the same under each system, Dr. Forbes asks the following pertinent question: — "Does not this fact, common to both, seem to point to a *community of power, or want of power*, in the two classes of agents, rather than to a speciality of action and potency in one?"

Dr. Forbes says that, "the materials furnished by Professor Henderson, towards the solution of the practical portion of the question agitated between the old and new systems, are very inferior in amount and in intrinsic value to those of Dr. Fleischmann," but that they are not unimportant. He then reviews some of Dr. Henderson's cases in detail, but we find we shall not have space to follow him. Dr. Henderson, it appears, has adopted the homœopathic doctrine and practice in full; but, nevertheless, has been honored with the chair of Pathology in the Edinburgh University. This is certainly a high distinction for the new school.

In regard to the homœopathic treatment of diarrhœa, a case of which is taken from Dr. Henderson's book, Dr. Forbes gives us a striking observation from his own experience. He states that, "many years ago, when in charge of a large body of men in the public service, he had occasion to treat an epidemic diarrhœa of considerable violence, but not dangerous." Finding that the patients recovered under one plan of treatment as well as another, he concluded to try the effect of what he considered equivalent to *no medicine at all*. Accordingly, he put one-half of the remaining patients on a course of orthodox physic, and the other on homœopathic doses of flour, in the form of bread-pills: he observes, "and it puzzled us sadly to say which was the most successful treatment." This is rather a *poser* for the Homœopathists, but we are inclined to think thousands like it might be easily obtained.

Dr. Forbes does not hesitate to declare, that the amount of success obtained by Dr. Henderson, in the treatment of his cases, would have been considered by himself as very satisfactory, had he been treating the same cases according to the rules of ordinary medicine; at the same time, in making these admissions in respect to the cases supplied by Drs. Fleischmann and Henderson, he is carefully guarded against admitting, as a corollary, "that the result of the homœopathic treatment *generally* is, and will be, as successful as the result of the ordinary treatment generally." This may *possibly* be the case, but it remains to be substantiated. Dr. Forbes says:—

"But many of our readers, we expect, will be of opinion that, in admitting what we have done, we are betraying the cause of legitimate medicine, and lending our aid to extend the heresy of homœopathy. If such be the result of our admissions, we cannot help it. We have said only what we believe to be true; and if what we believe is in reality the truth, the promulgation of it cannot lead to evil. Truth is good. If the art of medicine, as we profess and practise it, cannot bear investigation, and shrinks before the light of truth, from whatsoever quarter it may come, it is high time that it should cease to be sanctioned and upheld by philosophers and honest men. If, on the contrary, it be true and good—even if it be only but partially true and moderately good—the stirring touch of inquiry and the stimulus of opposition cannot fail to benefit it in the end.

"What, then, it will naturally be asked, is the explanation of the momentous fact we have announced, that a considerable number of diseases have been, and perhaps continue to be, treated as successfully by homœopathists as by allopathists? *Is it, that the one kind of treatment is as good as the other?* Is IT, THAT HOMŒOPATHY IS TRUE?

"To both of these queries we give an unequivocal and decided negative, so far, at least, as this can be given in a case where we have, as yet, no demonstrative proof on one of the sides of the question. We may, indeed, have proof sufficient to satisfy any reasonable mind, that the theory or doctrines, or principles of Homœopathy are false; but as yet we have no demonstrative evidence that it is false in its practical bearings—false, that is, powerless, as a means of curing diseases. It will not be disputed by any one conversant with the history of medicine, that these two things are not only distinct, but independent of each other. We can, however, assert with the greatest positiveness, that, as far as the evidence supplied by the documents now before us, or the evidence we have been able to gather from other published writings of the new school, goes,—there exists not a tittle of actual PROOF that Homœopathy is true in this aspect. On the other hand, we have not a little positive evidence to prove that it has often failed to cure in cases where, according to its principles, and the alleged experience of its professors, it ought to have cured, and in which allopathy did effect a cure. Still, this is only negative proof, and might be accounted for or explained away on grounds that would not necessarily compromise the existence of Homœopathy as a means of cure. In a case so extraordinary, so marvelous, it may be said, as that of Homœopathy, nothing short of the most positive and demonstrative evidence of its curative powers can be accepted; nothing, in short, will suffice but the *experimentum crucis* of a comparative trial, on the large scale, of *its* powers, on the one hand, and of *nature's* powers, on the other. Until it can be proved, by clinical experience on an extended field, and on two parallel groups of similar diseases, that Homœopathy cures better than nature, we are warranted by every principle of philosophy, not merely in doubting, but in denying its truth."

Here follow some excellent reflections on the nature and treatment of diseases, the errors of medical science, the powers of quack nostrums, and the remedial powers of *nature*. We agree fully with Dr. Forbes, that there is too much interference with diseases, which prevents us from learning their *natural progress* and *natural history;* and that we are not sufficiently acquainted with the actual powers of remedies in controlling and modifying diseases. He reminds us, too, of the established fact, that scientific physicians of all ages, men of philosophic minds, have generally, in their old age and mature experience, "abandoned much of the energetic and perturbing medication of their early practice, and trusted greatly to the remedial powers of nature." He illustrates this by the remark of a learned and highly respected physician of Edinburgh, who, when some one boasted before him of the marvelous cures wrought by the small doses of the homœopathists, said, "this was no peculiar cause for boasting, as he himself had, for the last two years, been curing his patients with even less, viz., with *nothing at all!*"

Dr. Forbes observes that, "the candid consideration of what precedes will, we hope, go far to satisfy the minds of most men, of the justness of the conclusion previously come to by us—viz., that the curative powers of nature suffice to explain all the triumphs of Homœopathy." He mentions additional influences essentially connected with the new system, which, he thinks, "must entirely remove all doubt on the subject." These are—1, The abandonment of all previous medication; 2, A much stricter diet and regimen; 3, The influence of imagination, faith, hope, &c.; 4, The indirect influence of this faith, hope, &c., in inducing patience; for, as no immediate or obvious effect is promised, the sufferer awaits more patiently the final result.

"But (says Dr. Forbes) while we are thus exalting the powers of nature at the expense of Homœopathy, are we not, at the same time, laying bare the nakedness of our own cherished Allopathy? If it is nature that cures in homœopathy, and if homœopathy (as we have admitted) does thus cure, in certain cases, as well as allopathy, do we not, by this admission, inevitably expose ourselves defenceless to the shock of the tremendous inference,—that the treatment of many diseases on the ordinary plan must, at the very best, be useless; while it inflicts on our patients some serious evils that homœopathy is free from, such as the swallowing of disagreeable and expensive drugs, and the frequently painful and almost always unpleasant effects produced by them during their operation? This inference, and the dilemma it involves, are always held up by the homœopathists *in terrorem* to any allopathist who should think of using the argument of nature's *autocrateia* against their system; and they think the threat too terrible to be encountered with disregard, much less with defiance, by any man in the actual practice of allopathy."

He continues:—

"These threats do not deter us from accepting the horn of the dilemma presented to us; nor do we think it worth while caviling about the precise amount of the estimate involved in Dr. Henderson's inference. This may or may not be accurate; we believe that it is exaggerated; but be this as it may, we concede at once to him the truth of his general proposition; *and still adhere to* ALLOPATHY. In doing so, we consider, that though we are embracing a system extremely imperfect, we are, at least, embracing one which, with all its faults, contains a considerable amount of truth, and a yet greater amount of good; and which, above all, is, or may be made, in its exercise, consonant with the principles of science, and is capable of indefinite improvement; while in rejecting homœopathy, we consider that we are discarding what is, at once, false and bad—useless to the sufferer, and degrading to the physician."

Having finished his examination of the writings of the homœopathists, Dr. Forbes comes to the consideration of the second part of his subject, viz., *Allopathy*, or the present state of *general Therapeutics*. And here he does not shrink from admitting, and adopting the inferences—however unfavorable to Allopathy—which seemed necessarily to flow from the homœopathic treatment of diseases. They are recapitulated as follows:—

"1. That, in a large proportion of the cases treated by allopathic physicians, the disease is cured by nature, and not by them.

"2. That in a lesser, but still not a small proportion, the disease is cured by nature in spite of them; in other words, their interference opposing, instead of assisting the cure.

"3. That, consequently, in a considerable proportion of diseases, it would fare as well, or better, with patients, in the actual condition of the medical art, as more generally practised, if all remedies—at least, all active remedies—especially drugs, were abandoned."

Dr. F. is grieved at being compelled to admit these inferences, but believing them to be true, he cannot reject them until they are proved not to be so. "Although," says he, "Homœopathy has brought more signally into the common daylight this lamentable condition of medicine regarded as a practical art, it was well known before to all philosophical and experienced physicians."

Dr. Forbes says, he is far from denying that practical medicine has made considerable progress since it was first established as an art, or that we do not now cure more diseases, and save more lives, than our forefathers did; yet its imperfections at the present day are numerous, and most palpable.

"From these, our free confessions and bold denunciations of the feebleness and uncertainty of therapeutics, it may possibly be inferred, that we are entirely sceptical of the truth of medicine as a science, and think most meanly of it as a practical art. And yet this is not so. On the contrary, we look upon medicine, regarded in all its parts and all its bearings, as a noble and glorious profession, even in its present most imperfect state; and we believe it destined to become as truly grand and glorious in actual performance, as it now is in its essence, its aims, and its aspirations.

"It is an unquestionable truth that medicine, both as a science and as an art, is, on the whole, progressive; and its progress, compared with that of preceding times, has been immense during the last sixty years. In the fundamental parts of the science—in physiology, pathology, and diagnosis, great and manifold additions have been made to our knowledge, during this period; several positive improvements have been also introduced into the general mode of treating our patients; and we have acquired one or two unequivocal accessions to our stock of certain means of relieving or curing diseases. We believe we may also add, with truth, that the general style of practice in this country has become better,—is less guided by theory and tradition,—is more discriminating, less confident and bold, less perturbing and meddling. We have learned a good deal at home, and still more, perhaps, from home. The long peace, and the general intercourse of nations consequent thereon, have permitted every country to know what every other country possesses. British medicine has thus profited considerably, and most especially by the importation of some of the humbler notions and milder practices of our continental neighbors. In the treatment of acute diseases, we have attained somewhat nearer to the heroic virtue of patience, from an increased knowledge of the morbid processes going on; in chronic disorders we have become more regimenal and less druggish; in all cases, perhaps, we have grown a little more trustful of nature, and a little less trustful of art. Nevertheless, it cannot be denied that the more ordinary proceedings of a large proportion of the practitioners in this country differ from those of their predecessors, much more in their nature than in their effects; and that they are, to a lamentable extent, palpably and egregiously wrong. We doubt, therefore, if we should greatly, if at all, exceed the bounds of truth, if we said, that the progress of Therapeutics, during all the centuries that have elapsed since the days of Hippocrates, has been less than that achieved in the elementary sciences of medicine during the last fifty years. This department of medicine must, indeed, be regarded as yet in its merest infancy. It would, doubtless, be going far beyond the truth to assert, that there is no certainty in medicinal therapeutics, and that the whole practice of medicine, in as far as this consists in the administration of drugs, is a system of traditionary routine and conventionalism, hap-hazard, and guess-work; but it is not going beyond the truth to assert, that *much* of it is so. In the hands of men of scientific education, men of philosophical views and long experience, and who, from the position they occupy, and the confidence they inspire, are enabled to proceed exactly as they think best, Practical Medicine, we readily admit, is, even now, a rational and wise system, rarely productive of evil if it fails to benefit, and often benefitting in the highest degree. But in the hands of those who are differently circumstanced in every respect;—who either travel contentedly in the broad highways of tradition, or deviate into still more dangerous paths which they deem rational;—who, confounding therapeutics with medicinal formulæ, prescribe according to rule or according to fancy,—medicine is a very different art, and its practice productive of very different results."

Dr. Forbes says, that the foregoing elucidations disclose "a lamentable state of things;" yet it is one not to be despaired of, "much less concealed, as something disgraceful." In this, as in morals, *the confession of error is the beginning of reform;* and our author thinks he sees unquestionable evidences, signs and portents of a dawning reformation and regeneration in medicine. "Things have arrived at such a pitch, that they cannot be worse; they must mend or end."

We now approach the close of this interesting and valuable little work. Our notice of it has already been more extended than we designed, though, we are satisfied, not more than its importance demands; and we cannot refrain from laying before our readers Dr. Forbes' enumeration of the objects necessary in medical reform — in other words, the labors to be performed by "Young Physic"!

"COGITANDA — EXCOGITANDA — AGENDA.

"1. To endeavor to ascertain, much more precisely than has been done hitherto, the natural course and event of diseases, when uninterrupted by artificial interference; in other words, to attempt to establish a true Natural History of human diseases.

"2. To reconsider and study afresh the physiological and curative effects of all our therapeutic agents, with a view to obtain more positive results than we now possess.

"3. To endeavor to establish, as far as is practicable, what diseases are curable and what are not; what are capable of receiving benefit from medical treatment and what are not; what treatment is the best, the safest, the most agreeable; when it is proper to administer medicine, and when to refrain from administering it; &c. &c.

"4. To endeavor to introduce a more philosophical and accurate view of the relations of remedies to the animal economy and to diseases, so as to dissociate in the minds of practitioners the notions of *post hoc* and *propter hoc.*

"The general adoption by practitioners, in recording their experience, of the system known by the name of the *Numerical Method*, is essential to the attainment of the ends proposed in the preceding paragraphs, as well as in many that are to follow.

"5. To endeavor to banish from the treatment of acute and dangerous diseases, at least, the ancient axiom, *melius anceps remedium quam nullum*, and to substitute in its place the safer and wiser dogma — that where we are not certain of an indication, we should give Nature the best chance of doing the work herself, by leaving her operations undisturbed by those of art.

"6. To endeavor to substitute for the monstrous system of Polypharmacy now universally prevalent, one that is, at least, vastly more simple, more intelligible, more agreeable, and, it may be hoped, one more rational, more scientific, more certain, and more beneficial.

"7. To direct redoubled attention to hygiene, public and private, with the view of preventing diseases on the large scale, and individually in our sphere of practice. Here the surest and most glorious triumphs of medical science are achieving and to be achieved.

"8. To inculcate generally a milder and less energetic mode of practice, both in acute and chronic diseases; to encourage the Expectant preferably to the Heroic system — at least, where the indications of treatment are not manifest.

"9. To discountenance all active and powerful medication in the acute exanthemata and fevers of specific type, as small-pox, measles, scarlatina, typhus, &c., until we obtain some evidence that the course of these diseases can be beneficially modified by remedies.

"10. To discountenance, as much as possible, and eschew the habitual use (without any sufficient reason) of certain powerful medicines in large doses, in a multitude of different diseases, a practice now generally prevalent and fraught with the most baneful consequences.

"This is one of the besetting sins of English practice, and originates partly in false theory, and partly in the desire to see manifest and strong effects resulting from the action of medicines. Mercury, iodine, colchicum, antimony, also purgatives in general and blood-letting, are frightfully misused in this manner.

"11. To encourage the administration of simple, feeble, or altogether powerless, non-perturbing medicines, in all cases in which drugs are prescribed *pro forma*, for the satisfaction of the patient's mind, and not with the view of producing any direct remedial effect.

"One would hardly think such a caution necessary, were it not that every-day observation proves it to be so. The system of giving and also of *taking* drugs capable of producing some obvious effect—on the sensations, at least, if not on the functions—has become so inveterate in this country, that even our *placebos* have, in the hands of our modern doctors, lost their original quality of harmlessness, and often please their very patients more by being made unpleasant!

"12. To make every effort not merely to destroy the prevalent system of giving a vast quantity and variety of unnecessary and useless drugs, (to say the least of them,) but to encourage extreme simplicity in the prescription of medicines that seem to be requisite.

"Our system is here greatly and radically wrong. Our officinal formulæ are already most absurdly and mischievously complex, and our fashion is to double and redouble the existing complexities. This system is a most serious impediment in the way of ascertaining the precise and peculiar powers (if any) of the individual drugs, and thus interferes, in the most important manner, with the progress of therapeutics.

"We are aware of the arguments that are adduced in defence of medicinal combinations. We do not deny that some of these combinations are beneficial, and therefore proper; but there cannot be a question as to the enormous evils, speaking generally, resulting from them. Nothing has a greater tendency to dissociate practical medicine from science, and to stamp it as *a trade*, than this system of pharmaceutical artifice. It takes some years of the student's life to learn the very things which are to block up his path to future knowledge. A very elegant prescriber is seldom a good physician: and no wonder. Tailors, barbers, and dancing masters, however learned they may be in the externals of gentility, are not expected to be fine gentlemen or men of fashion.

"13. To endeavor to break through the routine habit, universally prevalent, of prescribing certain determinate remedies for certain determinate diseases or symptoms of diseases, merely because the prescriber has been taught to do so, and on no better grounds than conventional tradition.

"Even when the medicines so prescribed are innocuous, the routine proceeding impedes real knowledge by satisfying the mind, and thus producing inaction. When the drugs are potent, the crime of mischief-making is superadded to the folly of empiricism. In illustration, we need merely notice the usual reference, in this country, of almost all chronic diseases accompanied with derangement of the intestinal functions, to "affection of the liver," and the consequent prescription of *mercury* in some of its forms. We do not hesitate to say, that this theory is as far wrong as the practice founded on it is injurious; we can hardly further enhance the amount of its divarication from the truth.

"14. To place in a more prominent point of view the great value and importance of what may be termed the physiological, hygienic, or natural system of curing diseases, especially chronic diseases, in contradistinction to the pharmaceutical or empirical or drug-plan generally prevalent. This system, founded as it is on a more comprehensive inquiry into *all* the remote and exciting causes of disease, and on a more thorough appreciation of *all* the discoverable disorders existing in all the organs and functions of the body, does not, of course, exclude the use of drugs, but regards them (generally speaking) as subservient to hygienic, regimenal, and external means,—such as the rigid regulation of the diet, the temperature and purity of the air, clothing, the mental and bodily exercise, &c., baths, friction, change of air, travelling, change of occupation, &c. &c.

"15. To endeavor to introduce a more comprehensive and philosophical system of Nosology, at least in chronic diseases, whereby the practitioner may be led less to consider the name of a disease, or some one symptom or some one local affection in a disease, than the disease itself—that is, *the whole* of the derangements existing in the body, and which it is his object to remove, if possible.

"16. To teach teachers to teach the rising generation of medical men, that it is infinitely more *practical* to be master of the elements of medical science, and to know diseases thoroughly, than to know by rote a farrago of receipts, or to be aware that certain doctors, of old or of recent times, have said that certain medicines are good for certain diseases.

"17. Also, to teach students that no systematic or theoretical classification of diseases, or of therapeutic agents ever yet promulgated, is true, or anything like the truth, and that none can be adopted as a safe guide in practice. It is, however, well that these systems should be known; as most of them involve some pathological truths, and have left some practical good behind them.

"18. To endeavor to enlighten the public as to the actual powers of medicines, with a view to reconciling them to simpler and milder plans of treatment. To teach them the great importance of having their diseases treated in their earliest stages, in order to obtain a speedy and efficient cure; and, by some modifications in the relations between the patient and practitioner, to encourage and facilitate this early application for relief.

"19. To endeavor to abolish the system of medical practitioners being paid by the amount of medicines sent in to their patients; and even the practice of keeping and preparing medicines in their own houses.

"Were a proper system introduced for securing a good education to chemists and druggists, and for examining and licensing them—all of easy adoption—there could be no necessity for continuing even the latter practice; while the former is one so degrading to the medical character, and so frightfully injurious to medicine in a thousand ways, that it ought to be abolished forthwith, utterly and for ever.

"Lastly, and above all, to bring up the medical mind to the standard necessary for studying, comprehending, appreciating, and exercising the most complex and difficult of the arts that are based on a scientific foundation,—the art of Practical Medicine. And this can only be done by elevating, in a tenfold degree, the preliminary and fundamental education of the Medical practitioner.

"Such are a few of the labors in store for our young Herclues of Physic; a few samples of the varied contents of the stable he is called upon to cleanse; and a few pailfuls, it may be, of the veritable Alpheian he is to work withal:

> " ———— Mox in ovilia
> Demisit hostem vividus impetus;
> Nunc in reluctantes dracones
> Egit amor ————."

We have thus given an imperfect sketch of this unpretending but valuable little book. Would that it could be read by every physician in America, from the professor who teaches *ex cathedra*, to the *tyro* in the doctor's office. Whilst exposing the fallacies of Homœopathy, as a system of medicine, it reveals the wonderful powers of *nature* in the cure of diseases. In comparing its practice with that of Allopathy, or the ordinary practice, it shows with *how little medicine* the most dangerous diseases may get well, and the awful responsibility of dealing boldly with drugs potent of *evil* as well as *good;* it proclaims candidly, honestly, and fearlessly the fallacies and imperfections of medical science at the present day, and concludes with most valuable suggestions for its reformation and regeneration. If we are all wrong, the sooner we find it out the better; and it is for the master-spirits of the profession to point out the way to truth. *Truth is mighty, and will prevail!* while *error*, though sanctified by age, like a slow disease preying upon the *body-medical*, will impede its progress, and thwart the attainment of its high and holy aims! If *we* are wrong in relying *too much* upon medicines and *too little* upon the efforts of nature, are not the community also wrong in expecting too much of us and our drugs? The young physician is reproached for his heroic, kill-or-cure practice; but is he altogether to blame? How often is he forced by patients and their friends to give medicine when it is not plainly indicated, when he would gladly watch and await the efforts of nature? This privilege is denied him; he must cure *quickly*, or *give place to a rival.* Experienced physicians are lauded for their prudence, their caution, and the small amount of simple medicines with which they effect their cures; but let it be remembered, that *experienced* physicians alone can inspire sufficient confidence to be allowed to practise this judicious plan. According to the ordinary practice, diseases are so much interfered with, that many years are necessary to afford a correct knowledge of their *natural history*, and of the *actual power of therapeutic agents;* and we fully agree with Dr. Forbes in believing, that the homœopathic system, in furnishing a great deal of this important information, will confer a lasting benefit upon medical science, and mankind. We think this is about *all the good* that will arise from the new system; but this is amply sufficient to entitle it to consideration; and we therefore hope that Homœopathy, in future, will not be looked upon, by scientific physicians, as an *empty humbug*, but rather be used as a mirror, which, though opaque itself, may yet serve to throw light upon our errors.

We have thought proper to say thus much about Homœopathy because the subject is still comparatively new to many of our readers, and we wished to present it to them under the candid examination of so able and scientific a physician as Dr. Forbes. It is scarce a quarter of a century since the extraordinary doctrine of Hahnemann began to attract the attention of the scientific world. It was looked upon as a marvellous humbug—the *ne plus ultra* of German transcendentalism. But who can fail to be struck at its wonderful progress? Dr. Forbes informs us, that scarce a city or town of any note in Europe or America is now without one or more homœopathic physicians; and, however it may be accounted for, they are found competing successfully with the ablest of the faculty, and among the most intelligent society. It has already gotten into one of the most venerable and respectable medical schools in the world, and it is impossible any longer to disregard its claims to investigation. It is just now reaching our own region, and a discerning public will soon be called on to judge of its merits. Its preposterous *theories and dogmas* may be said to have already fallen into ruins; but the great teacher, *Experience*, appears to be building upon those ruins a wonderful and most salutary reformation in practical medicine. The enlightened Medical Profession will probably never be restricted to homœopathic doses of homœopathic remedies; but if we learn, by the operation of the new system, how to do *less harm* and *more good* with our valuable Materia Medica, Homœopathy will have conferred a blessing upon mankind. E. D. F.

III.—*A Manual of the Diseases of the Eye ; or Treatise on Opthalmology ;* by S. LITTELL, jr., M. D., one of the Surgeons of the Wills Hospital, Fellow of the College of Physicians of Philadelphia, &c. &c.—Second Edition, revised and enlarged. Philad., Hogan & Thompson, 1846. pp. 362.

The diseases of the eye have not received that attention on this side of the Atlantic which they deserve : at long intervals, a few members of the profession have written short and meagre essays, on some of the affections of that organ ; — but with the exception of Dr. Fricks of Baltimore, Wallace of New York, and Dr. Littell of Philadelphia ; little has been done to illustrate the pathology and treatment of the deseases of the eye in this country. Our desire to understand and manage the diseases of any particular organ of the body, should be commensurate with the importance of such an organ to the general well-being and happiness of man. Although this organ may be destroyed by disease, or crushed by a blow, without exerting any serious influence upon the constitution, yet as an "avenue to the brain,"—as a "window to the soul" it brings us in contact with the objects around us, and is a source at once of pleasure and information.

Most of the Treatises on opthalmology aim at too much — especially those written by the French : Scichell's work, is of this kind ; it makes distinctions which are more arbitrary than real, and is entirely too learned and elaborate for ordinary practice.

We believe the diseases of the eye should be arranged according to the anatomical structures of this organ ; here we have nearly all the elementary tissues of the body represented ; we have the orbit, a bony cavity—next a mucous tissue ; afterwards the ordinary integuments ; covering the lids &c. ;—then a fibrous, a muscular, a nervous, a vascular, in a word, an assemblage of all the tissues,—the basis of the entire organism. Pathological science teaches that certain diseases, or specific inflammations attack particular structures in preference to others ; thus the muscular and fibrous structures are liable to rheumatic inflammation &c. ; the iritic tissue to syphilitic irritation, and the same may be said of the other structures, not only of the eye, but of the other organs of the body.

Dr. Littell's work, although bearing the modest title of a "Manual," is defective, in our opinion, in one essential particular ;—like most works treating of a speciality, it should have contained, in the first part, a brief description of the anatomy of the eye.

This, without adding much to the size of the volume, would have enabled the reader to understand more clearly the different affections which the author has so graphically described.

To presume every practitioner familiar with the anatomy of the eye would be entirely gratuitous ; he, therefore, who ventures to instruct us in any branch of science, must begin with the elements of that science, and gradually advance with the subject.

Few general practitioners are familiar with the diseases of the eye : it is only the surgeon attached to large Hospitals, who has the best opportunity of studying this interesting class of affections. These usually present a great variety of cases, and the attentive observer can watch the progress and witness the termination of each particular case ; — compare one case with another,—study the effects of remedies and determine their relative value.

With few exceptions, the same remedial agents known and acknowledged to subvert or change diseased action in those tissues of the body analogous to the anatomical elements of the eye, will display equal power in controlling the morbid changes of this organ.

This little volume is replete with practical information, and will richly repay a careful perusal. It gives a condensed and lucid exposition of the various diseases of the eye, written in a style at once terse and comprehensive. The matter is as sound, as the manner in which it is expressed, is clear and conclusive. The symptoms of each particular affection is placed so plainly before the reader, and their distinctive features so well described, that the most common understanding may readily arrive at a correct diagnosis.

Dr. Littell writes like one who had often seen what he attempts to describe; avoiding on the one hand unnecessary prolixity, and on the other, that degree of sententiousness which would leave the mind dissatisfied with the subject. He has accomplished much,—he instructs without wearying, charming the reader with the neatness of his style and stimulating him to persevere by the valuable matter which he sets before him.

Prudent and cautious, yet confident in the efficacy of medicinal agents, our author recommends a system of treatment based at once upon a correct acquaintance with the pathological change and a just appreciation of the action of remedies. Without seeking to charge Dr. Littell as being too *sanguinary* in his method of treatment, we venture to assert that depletion both local and general in the diseases of the eye, is frequently pushed too far in this country, weakening the powers of the constitution, increasing the irritability of both the nervous and vascular systems, thereby favouring local determination; forgetting, it would seem, the pathological axiom;—that if the system contains but six ounces of blood,—a part, at least, will still flow to the original seat of irritation. It will not answer therefore, to continue the abstraction of blood, because the conjunctiva is red; it will not unload the vessels, though we bled *ad deliquium*. Over distention of the vascular tubes will soon end in atony—in absolute debility of these parts, — obviously, the best method is in such cases, to apply a local stimulant in order to whip up the arrested circulation, to enable the vessels to contract upon their contents and thus sustain a healthy circulation.—The practice recommended by our author, is in the main, unexceptionable; due regard is paid to the age, constitution, temperament and general strength of the patient. He does not push mercury so far as to poison the system and enervate the powers of the constitution; he stops short of ptyalism, except in certain cases. He advises us to combine the general with the topical treatment of opthalmic diseases; from observation and some little experience, we can unite with him in his recommendation of many of the local means which he employs. We however protest against certain combinations, frequently advised by Dr. Littell, as collyria in these affections. He often speaks of a formula to which serious objections may be urged, at least on the ground of *chemical* incompatibility; *par example*, one of his favorite collyria is composed of the sulphate of copper—or of zinc, united with the tinct. vel. vin. opii.

Does he not know, that the *meconic acid* of the opium will combine with the metallic base of the sulphates and form an insoluble compound, which when brought in contact with the inflamed and delicate tissues of the eye, must, by acting as so much sand—or other foreign body, irritate the part, and add seriously to the local mischief.

It is too much the custom among routine practitioners to prescribe compound collyria with but little regard to the chemical habitudes of the articles and to the pathological condition of the tissues affected.

From Dr. Littell's position, as Surgeon to the Wills Hospital, we certainly had expected the introduction of an occasional case in his book, in order to illustrate and enforce the principles and practise which he advocates. Certainly, we are no admirers of a *case-book*, filled with the dull details of a long and protracted series of uninstructive observations, eked out with a view to swell a neat Manual into an octavo; yet the particulars of a few cases, with the corresponding treatment, in each, as showing the best method of treatment, would have added materially to the interest of the volume, and given us a more definite view of the authors, practical sagacity. It is not enough to tell us that we must resort to both local and general depletion—to calomel and opium internally—to astringent collyria such as zinc, copper, lead &c., or to the nitrate of silver—or bichloride of mercury—or citrine ointment, or the red precipitate as an escarotic in certain diseases of the eye; these are based upon general principles, which all understand, yet few there be, who can apply them at the opportune moment, or avail themselves of the full benefits of such general specifications.

The treatment must be varied according to the stadium of the disease; we are not to hurry through the list and flatter ourselves that we have done all in our power, because we have tried every means advised by writers on ophtholmology in each particular affection. Our author only recommends emetics in diseases of the eye, when obvious gastric derangement exists; certainly a good practice, but this is confining a highly important class of therapeutic agents to a very few cases; emetics may be administered in many of these cases with the best effects when no such derangement can be detected. They, by acting revulsively upon the stomach, and determining to the surface, unload the vessels of the eye, if overcharged—lower the tone of the vascular system—and in cases of effusion, hasten its obsorption. The nitrate of silver, either solid or in solution, and the sulph. copper, as local applications, are the best, and in our opinion, almost the only means that should be used as such in the diseases of the eye.

By varying the strength, to suit the case, we can accomplish all that may be expected from the use of various astringent collyria, recommended by writers.

In purulent opthalmia, whether of children or grown persons,—where the pain is intense, the discharge, purulent and profuse, —the lids greatly swollen, with circumorbital pain &c., a solution of the nitrate of silver in the proportion of 20 *grs. to the ounce*, may be applied to the eye, with an effect so prompt, and with such a relief of *all* the symptoms, that to one who had not witnessed the practice, it would seem incredible. — The lids must be closed, and a dossel of lint, saturated with the solution as above, laid lightly over the part, renewing it as often as the lint becomes dry. This practice, after proper local and general depletion, can not be too highly recommended, as we have repeatedly witnessed its efficacy in such cases, not only in our own, but likewise in the practice of an eminent phisician of this city, who first suggested this method of treatment to us.

It is not a little surprising that our author makes no mention of the use of the microscope in elucidating and detecting the diseases of the eye; we certainly regard this as an oversight or an omission to which we would respectfully invite his attention. Its value in other departments of pathology, is now universally acknowledged; surely then it might be made available in studying the morbid affections of an organ, the diseases of which sometimes elude the keenest scrutiny.

As a Manual, this book will be highly appreciated by the practical part of the profession, and as a proof of its high value, the first edition was republished in England, with notes, by Dr. Houston, an evidence of merit, with which but few American medical or other works have been *honoured.* It contains much information in a small compass; this will certainly induce many to read it, who justly dread to encounter octavos of 800 and 1000 pages. The book closes with a glossary with Greek and other derivations, so necessary to explain the barbarous nomenclature of opthalmology. A. H.

IV.—*Lectures on the Operations of Surgery, and on Diseases and Accidents requiring Operation.* By ROBERT LISTON, Esq., F. R. S. &c., with numerous additions; by Thomas D. Mütter, M. D., &c. &c. &c.—Lea & Blanchard, Philad.

This work is a republication of the lectures of the eminent English Surgeon, whose name appears on the title page. They have appeared and are still appearing in the London Lancet. The editor tells us that he has contributed additional matter to the amount of nearly two hundred and fifty pages; — the whole work consisting of five hundred and sixty pages.

It would be useless in a bibliographical notice like the present, to call the attention of surgeons to any thing that emanates from Liston. He is too well known and appreciated to make such a course necessary. In a review of his work which we may occupy ourselves with when his lectures are completed, we shall examine his peculiar merits and doctrines.

The editor has made several valuable additions to the work — chiefly derived from his own personal experience. His remarks and precepts, we think, are in general judicious.

But would it not be better for American Physicians to come before the world as original authors, than to be thus attaching themselves as parasites to every work of merit which issues from the English or French press! How much talent and industry are thus frittered away. It may do well enough for those who can do nothing else than translate, or annotate—those annotations being almost always, not the result of personal experience, but pilferings from others. Is not this course, one, which if longer persisted in, will lead to the destruction of independence of thought and action among the members of the medical profession in the United

States? Is not such conduct a tacit acknowledgement of inferiority? Is it not adapted to make the great body of the profession distrust any thing that an American physician may write, however meritorious his productions may be?

Foreign authors, however, do not always get the best of it. Dr. Stokes of Dublin is one of the most eminent physicians living. His lectures have been republished in the United States, — the first edition bearing the title of "STOKES' & BELL'S Lectures." The last edition, however, is BELL & STOKES—*Ego et rex meus.*

We have been amused with the formidable array of titles with which the editor of the work before us bedizens himself. Like Nelson going into action at Trafalgar, he seems determined not to abate a ribbon or a button.

Liston speaks highly of the isinglass plaster, which he says "is now made in webs and spread on various tissues, oiled silk, gold beater's skin (the prepared peritoneum of the ox,) or silk gauze. The latter material is the best; it is first made waterproof by a coating of boiled oil, and then laid over with layers of isinglass *dissolved in spirit.*" If by *spirit* is meant *alcohol*, there is some mistake, and we are surprised that the editor who tells us that the isinglass plaster referred to by Mr. Liston is exceedingly well made by Mr. Husband of this city (that is Philadelphia), did not inform his readers that isinglass is not soluble in alcohol.

J. H.

ART. V.—*Twenty-fifth Annual Report of the Bloomingdale Asylum for the Insane. For the year* 1845.—By PLINY EARLE, M. D., Physician to the Asylum.

Third Annual Report of the Managers of the New-York State Lunatic Asylum. Made to the Legislature, January, 1846. AMARIAH BRIGHAM, M. D., Superintendent and Physician.

Report of the Pennsylvania Hospital for the Insane. For the year 1845. By THOMAS S. KIRKBRIDE, M. D., Physician to the Institution. Philadelphia, 1846.

We beg leave to return our grateful acknowledgments to the respective Authors, for the receipt of these valuable Reports. We have read them with interest, and rose from the perusal with feelings of mingled pride and satisfaction at the evidence they afford of the great benefits confered by our noble profession upon the poor unfortunate Lunatic. Who, but the enlightened Physician of modern times has rescued him from his hapless doom of chains and the dungeon—surrounded him again with comforts and enjoyments to which he seemed forever lost,—and restored the light of reason to his benighted soul? The true glory of these noble achievements belongs peculiarly to the medical Profession; we have pointed out to the philanthropist and the legislator *how much* might be done for these unfortunate fellow-beings; and they, with commendable liberality, have joined heartily in its accomplishment. When we contemplate the miserable condition of the poor maniac in former times, doomed to eke out a wretched existence, with no hope, no consolation, no relief, and then behold him at the present day provided with a magnificent mansion, every rational want and inclination sought for and ministered to, allowed the enjoyments of liberty, and society, music, literature, nay even science and religion, — to see his vicious inclinations and dangerous propensities overcome by love and kindness, and his mind wooed back from its aberations, into the path of reason by the moral influences of gentleness, patience and perseverance; we confess our astonishment, and conclude, with some degree of exultation, that this alone should entitle the medical Profession to the gratitude of mankind. At this day fully *one half* of the Lunatics sent to the asylums, are cured; and the proportion is much larger, where the proper treatment is resorted to soon after the attack.

The Reports before us are from three of the most extensive Insane Hospitals in the country, and their management and results reflect infinite honour upon their respective medical attendants. We are pleased to see that the arrangement of the statistics in all three is pretty much on the same plan; and also that the three distinguished physicians agree in nearly all their general observations and suggestions. As we cannot enter minutely into the details of these Reports, we shall select a few of the most interesting statistics and observations.

1. *The Bloomingdale Asylum.* This Institution is situated near the city of New York. Dr. Earle informs us that the number in the Asylum

on the 1st of January, 1845, was	104
Cases admitted during the year	138
Whole number in the Asylum during the year	242
Numbers of cases discharged and died	125
Remaining December 31st, 1845	117
Of the cases dischaged, there were cured	61
" " " " " much improved	12
" " " " " improved	20
" " " " " unimproved	20
" " " " " died	12

Of the discharged, four of the patients who are recorded as much improved, became entirely well soon after removal. Several others who were believed to be curable, were prematurely taken away, thus losing the advantage which might have accrued from a longer residence in the Asylum.

Dr. Earle makes the following remarks concerning the apparent *increase*, and the exciting causes of insanity: —

"The number of patients admitted during the past year, as compared with the annual admissions for several of the preceding years, being assumed as the data upon which to found an opinion, the necessary inference is, that mental disorders are increasing. Whether the increase be in a greater ratio than that of the population of the City and its adjacent country, is a proposition which cannot easily be demonstrated. However this may be, it is an unquestionable fact, that the exciting causes of mental alienation were never, in time of peace, more active, among any people, than at the present day among the inhabitants of the United States; and particularly so in the States which, bordering on the Atlantic, were the earliest peopled by European emigrants.

Intoxicating liquors are so cheap that the labor of a few hours will procure enough to addle the brain for a week, and prevent the healthy excercise of reason perhaps a much longer period. The avenues to wealth, place and power are open to all: the child of the cottager thus entering into the strife of competition with the son of the most wealthy citizen. The progress of civilization and refinement, and the comparative ease with which the products of both nature and art in every quarter of the globe are here obtained, have a direct tendency to foster a luxurious life. Hence human desires and human wants are greatly multiplied, while both mind and body are exerted to the utmost power of endurance to gratify the former and supply the latter. The almost unavoidable effect of the artificial mode of living thus produced, is either a debility of the system, or an augmentation of nervous excitability, either of which facilitates the invasion of mental disease.

Art has made advances so rapid towards the annihilation of time and space, that, if life be measured by the proper standard—the number of events, circumstances and conditions, seen, felt or perceived—the amount of pleasure enjoyed and pain endured—the people of the present generation have an existence of ten-fold longer duration than their forefathers. As if this were not enough, the mind is forced into an activity corresponding with the new era of art.—Children, before the body has acquired sufficient tone, or the brain sufficient firmness, to endure much mental exertion with impunity, are placed in schools where the intellectual faculties are unduly urged, while the physical exercise necessary to the due development of the frame is too often neglected. Under such circumstances, the head will expand, but the body cannot grow in size or vigor sufficiently to maintain 'a balance of power.'"

In regard to the management of patients labouring under mental derangement, Dr. Earle lays down the following important conclusions which are strongly supported by reason and observation, viz:

First.—*As a general rule, the first measure in the curative treatment of insanity, is to remove the patient from home, from acquaintances, and from all familiar scenes and associations.*

Second.—*When the insane are placed under proper curative treatment in the early stages of the disease, from* 75 *to* 90 *per cent recover.*

Third.—*On the contrary, if they be not put under treatment before the disease has continued a year or more,* 15 *to* 20 *per cent only are cured.*

That there are strong prejudices existing in some portions of the community against lunatic asylums, is but too well known; but, says Dr. Earle, "the tales of our fathers who saw the maniac manacled, fettered and chained, are not the criterion by which to judge of asylums as most of them are now conducted." He continues: "Hence, if these thruths be known, it is apprehended that a fearful reponsibility rests upon such as, having the control of persons becoming deranged, neglect, from any ordinary motives, immediately to place them in a situation most favorable to recovery."

This Report contains many curious and interesting observations under the heads of: *Moral Treatment—Religious Worship—Instruction—Lectures—Recreation—Amusements—Manual Labour*, and *Restraints*—the *tout ensemble* making up a beautiful picture of domestic happiness and enjoyment which would certainly appear passing strange within the precints of a Lunatic asylum.

Appended to this Report is a Meteorological Register for the year 1845.

2. *Annual Report of the New-York State Lunatic Asylum.* The first part of this pamphlet is occupied by the report of the Board of Managers to the Legislature, on the state and condition of the institution, its receipts, expenditures, &c.—The second part is from the Medical Super-

intendent, Dr. A. Brigham, one of the most respectable physicians in our country. It appears that the number of patients in this institution

at the beginning of the year was - - - - - -	260
Admitted during the year - - - - - -	293
Total number in the course of the year - - - - -	553
Of this number there have been	
Discharged, recovered - - - - - - -	135
" improved - - - - - - -	78
" unimproved - - - - - -	34
" died - - - - - - - -	21
Total discharges during the year - - - - -	268
Remaining on the 30th November, 1845 - - -	285

As in the last-mentioned Report, Dr. Brigham says that several of the patients discharged as *improved*, recovered soon after leaving, and remained well. He states farther as *a general fact*, that "while patients are apt to be removed too soon from an asylum, some, we are convinced are kept longer than is necessary." It is not always easy to discriminate, and he is "in favour of giving a patient who has nearly recovered, and then remains stationary for several months, a trial at home."

We cannot follow Dr. Brigham in his minute statistics, but must invite attention to some of his general observations. And first as regards the prevalence and alarming nature of insanity, he propounds to those who wish to promote the welfare of the human race, the following important question:—"Is there no way of arresting the increase of this disease?"

"In our opinion we should not remain satisfied with providing curative institutions and comfortable abodes for those affected by insanity, but we should endeavor to prevent it, by timely care and wise precautions, by avoiding whatever is likely to predispose to it or to excite it. With the hope that some suggestions on this subject, in this report, may be useful to the citizens of this State, we subjoin the following remarks on

THE PREVENTION OF INSANITY.

Under this head, we wish to include *measures for arresting the disease in its incipient state.*

Often insanity exists in a slight degree for months, and as we have said, is only noticed by the most intimate friends or relatives, and then *suddenly* assumes an alarming form, leading in some instances to homicide and in others to self-destruction. Of the latter, almost every newspaper contains accounts, and of the former we have known many instances. Three instances, within a few years, have fallen under our observation of mothers killing their own children. In all these cases, the insanity previous to the act, though observed by a few, was very slight and unknown to their neighbors, and in one instance only to a part of the members of the family in which the person resided. In two of these instances, the act seemed to have been committed from a sudden impulse; the other was premeditated, and done for the purpose of securing the happiness of the child, and also the death, by execution, of its mother, who wished not to live, but was unwilling to commit suicide. Contrary to our expectations, these cases have all recovered, and all have been well and with their friends for more than a year, and some of them longer. Two of them have since become mothers and still continue well.

The suicidal form of insanity often exists in a slight degree for a long time, but unfortunately attracts but little or no attention; and hence the frequency of suicide, which in this State has become truly alarming. We have the names of *seventy-four persons, forty-four* men and *thirty* women, who have committed suicide in this State during the last twelve months, but we do not suppose we have heard of all.

Most of those included in the foregoing list, are reported to have been insane. We presume this is correct, and that many of them might have been saved had their friends adopted early and judicious precautions.

Of this we feel confident from the fact, that of the 844 patients admitted into this asylum, 111 viz. 49 men and 62 women, were disposed to suicide when admitted, and several of them had repeatedly attempted it, and we have no doubt many of them would have accomplished their purpose but for the timely precaution of their friends in sending them to an asylum. Here they have not only been prevented from committing suicide, but many of them have recovered. The suicidal form of insanity is, we think, generally curable by early and judicious treatment."

Dr. Brigham cautions the community against *the recurrence* of insanity—he says it is important for them to know that *re-attacks* are very common. "Thus of 844 patients who have been at this asylum, 145 or more than one-sixth had been previously insane." All those habits and practices that are known to cause or perpetuate this disease should be avoided; especially every thing likely to cause loss of sleep. He says that the general practitioners throughout the country *neglect too much the study of insanity:* which is undoubtedly the case. They should be familiar with its causes and premonitory symptoms, so as to be able to give *timely advice* for its prevention and arrest.

"Physicians are often called upon to give their testimony in relation to the mental condition of individuals, and sometimes in cases where not only property, but *life* is at stake. In such cases, their responsibility is very great, and furnishes strong additional reasons for their applying themselves diligently to the study of mental diseases."

Under the head of "Predisposing Causes" he gives some excellent remarks on *hereditary transmission*, and *education and habits of life*. Of the former he says :—

"That a predisposition to insanity is very often transmitted, is a fact well established. Thus of 844 patients who have been in this Asylum, viz, 431 men and 413 women, 224 were known to have insane relatives. That many of the others were thus predisposed, we do not doubt, but we were not able to learn anything respecting their relatives. 104 were known to have insane parents, viz 58 men and 46 women.

It would appear from our inquiries, and they have been very carefully conducted, that insanity is a little more likely to be transmitted by the mother than by the father, and that mothers are considerably more likely to transmit it to daughters than to sons, while the fathers most frequently transmit it to the sons.

Thus out of the 58 men, 35 had insane fathers and 23 insane mothers, while of 46 women, 16 had insane fathers and 30 insane mothers. We have known, however, of repeated instances in which insanity was transmitted by one parent both to sons and daughters.

But a predisposition to insanity is also transmitted from parents, who though not actually insane, are remarkable for violent and ungovernable temper, eccentricity, wanderings of the imagination or weakness of mind. Mothers in whom the nervous system predominates, who are prone to hysteria and who have suffered much from affections of the nervous system, are very apt to transmit a tendency to similar diseases to their offspring, and sometimes to insanity; especially if they have during pregnancy experienced violent emotions, such as terror and extreme anxiety of mind.

Children begotten in old age, or when the difference in the ages of the parents is very great, and also the offspring of those that have been very intemperate, are believed to be predisposed to mental disorders. Sometimes great originality of mind in the parent, intense study and entire devotedness to a particular pursuit, appear to predispose the offspring to insanity or idiocy."

In regard to education, Dr. Brigham makes the following judicious remarks:

"That education which consults the good of the whole man, that tends to develope and strengthen in equal and just proportion the moral, intellectual and physical powers, is conducive to health of body and mind. But in all countries the intellect or some of the intellectual faculties are cultivated to the neglect of the moral qualities, while in others the feelings, appetites and propensities are too greatly indulged and cultivated, to the neglect of just intellectual improvement. Hence, arise unbalanced minds, which are prone to become disordered. They feel too intensely, and are too ardently devoted to the accomplishment of certain purposes to bear disappointment without injury. They have not been taught *self-denial*, without which all education is defective.

Thus we find that insanity prevails in all civilized countries, and the most in those where there is the greatest intellectual activity, where there is the most political and religious discussion, and where the strife for riches and honors is the most intense. In fact, insanity is almost wholly confined to the civilized race of man. It is rare among the uncivilized and uneducated Indians and Negroes. It is uncommon in China, Persia and Hindostan, and prevails but little in Turkey and Russia out of the large cities. No case was heard of by the United States Exploring Expedition among the natives of the South Seas. There is but little in Spain and Portugal, compared with England, France, Germany and the United States.

In all the countries last mentioned, it prevails extensively, and is uniformly increased by events that excite deep and general feeling among the inhabitants. The French Revolution increased it in France, the American Revolution in this country. The Reformation of Luther, the noted South Sea speculation in England, about 1720, and the wars of Bonaparte, augmented the number of the insane. When Napoleon made and unmade kings and queens with great rapidity, kings and queens increased in the mad-houses of France. When the Pope came to Paris, an event that excited the religious community of that country, cases of religious insanity became more numerous. "So great has been the influence of our political commotions," says Esquirol, "that I could give the history of France from the taking of the Bastile to the last appearance of Bonaparte, by that of the insane of the hospitals, whose delusions related to the different events of that long period of history."

In this country similar results have been realized from our political and religious commotions. Thus, the Antimasonic excitement, the United States Bank excitement, the Abolition excitement, the speculating excitement, and the Miller excitement, and other intensely anxious and protracted religious excitements, have each furnished the Asylums with inmates.

Dr. Brigham now calls the attention of the Legislature to the neglected and pitiable condition of Idiots in the State; of whom the census of 1845 shows the number to be 1610. He thinks their condition might be greatly meliorated, and suggests the propriety of establishing an asylum for them. Here closes his interesting Report.

3. *Pennsylvania Hospital for the Insane.* We now approach the valuable Report of Dr. Kirkbride; the last on our list, but possessing equal interest, if not superior, to either of the preceding. This institu-

tion is a branch of the venerable Pennsylvania Hospital, which was founded in Philadelphia, in 1752; "a private charitable institution receiving no assistance from the City or State, and expending all its income for the benevolent objects of its foundation." Up to 1841 patients of all kinds were received at this Hospital in the City, but at this period the insane were removed to their present location, two miles west of the City. Dr. Kirkbride says:—

"At the date of the last report there were 151 patients in the Hospital, since which, 177 have been admitted, and 159 have been discharged or died, leaving 169 under care at the close of the year. The highest number in the house at any one time was 174; the average number for the whole year has been 162, being more than at any previous period in the history of the Institution. Of those discharged, during the year 1845, were

Cured	80
Much improved	5
Improved	24
Stationary	30
Died	20
Total—	159"

Dr. Kirkbride joins the authors of the preceding Reports, in complaints against the *premature removal* of patients.

The following remarks concerning the long residence of some of the inmates of this institution, may not be uninteresting.

"Of the patients who *remain*, are a large number of curable cases and many whose stay in the Institution will be short. Near one hundred, however, are individuals who must, in all human probability, look to this or some similar establishment as their permanent home, and the spot in gathering around which every source of comfort and happiness, they have a deeper interest than in any other. Many of them have already been residents of this hospital and of that in the City of Philadelphia, ten, twenty or thirty years; one gentleman has been under the care of the Institution more than forty-one years, and a lady more than fifty-five years. Both these last are still blessed with an abundant share of physical health, and scarce look older than they did when I first knew them, fifteen years ago. The gentleman retains all the courtesy of character, polished manner and social disposition which eminently characterised him in youth, and which still make him one of the most welcome guests at all our parties and entertainments. In the lady, although her intellect may have suffered more, the affections still flourish in all their original vigour, and she is noted for her kindness, her warmth of feeling, and tender sympathy for all who suffer, and she has Christian sources of consolation, sufficient to atone for all her afflictions. Two others have been forty-two and forty-five years in the hospital.

A majority of these long residents are in the best physical health, happy and contented with their lot. Many are constantly employed, make themselves very useful, and feel no little interest in every thing that is done about the Institution."

Here follow a number of Statistical tables which we cannot pretend to analyse, for want of space. In regard to the age at which insanity is most prevalent, Dr. Kirkbride agrees with the preceding physicians in stating that the disease occurs most frequently between 20 and 30 years. "It must be remembered, however, that the number of persons in the community between twenty and thirty years of age, is actually greater than of either of the other ages designated, except between ten and twenty." He says, insanity is of rare occurrence under the age of fifteen; the youngest case he ever had was eight years of age; but it does appear even earlier in life.

We learn that since the opening of this institution at its present location, in 1841, there have been 769 patients admitted, of whom 600 have been discharged or died up to the date of this Report. Of the 600 discharges, 313 were cured—50, much improved—79, improved—88, stationary, and 70 have died. He defines his meaning of these terms, but we shall only give that of *cured.* "By '*cured*' is meant that the patient's mind has been restored to what was its natural state before the accession of the disease, which brought him to the hospital. Natural eccentricities of character do not constitute insanity, and these, of course, are not expected to be removed by a residence in any institution." In regard to the recurence of the disease, Dr. Kirkbride says:

"When a patient is really well, it is often a matter of great solicitude, whether there may not be a peculiar liability to other attacks of the same kind. All, perhaps, are rather more liable than if their minds had never been affected, but not more so than they would be to functional diseases of other organs, or to pleurisy or dysentery, from having been once sufferers from them. The large number of this class, who are now to be found in every section of the country, who have for a long term of years gone through all the vicissitudes of life, without a symptom of mental derangement, proves that if once cured, and common prudence be observed in avoiding obvious causes of excitement, he who has once been insane, has no reason to anticipate a return of the malady.

The latter part of Dr. Kirkbride's Report is occupied by some interesting observations on various topics, such as recreation, employment, instruction, religious exercises, restraint, &c. In regard to the last mentioned subject we are pleased to find all these reports uniting in the assertion, that *personal restraints of all kinds are almost completely abolished*—thus doing away with one of the most revolting and injurious practices of the ancients. It is to be hoped these reports obtain a general and extensive circulation; for they are calculated to gratify as well as enlighten the public mind in regard to one of the most melancholy and dreadful afflictions of mankind. Here are fruitful evidences of practical benevolence and philantropy, which do honour to our Age, and go far to atone for many of its errors and its follies. E. D. F.

Art. VI.—*A Treatise on Corns, Bunions, the Diseases of the Nails, and the General Management of the Feet.* By Lewis Durlacher, Surgeon Chiropodist (by special appointment) to the Queen. Philadelphia: Lea & Blanchard, 1845. Pp. 134.

We are indebted to Mr. J. B. Steel (14 Camp-street) for this neat little book. We suppose the author is a distinguished Chiropodist of London; and the neat style and good sense displayed in this work certainly entitle him to a rank far above the ordinary class of *Corn doctors.* In it may be found much useful information in regard to every variety of corns, bunions, warts, and diseased nails; together with some excellent advice concerning "the *management of the feet.*" It is well known, that a vast number of persons are afflicted with those troublesome annoyances, *corns;* and it is certainly very desirable to know how to manage them properly. Mr. Durlacher gives the following rational account of their origin:

"The friction and pressure to which all prominent parts of the extremities are exposed, cause, by the local irritation they produce, the effusion or secretion of a larger quantity of epidermis than can be got rid of by the ordinary processes of nature. The scales consequently accumulate, and lie layer upon layer, forming indurated masses of larger or smaller size, constituting corns, callosities, or other diseases of the part, according to their situation, and the severity of the pressure or friction."

A large amount of the suffering and inconvenience arising from corns may be justly attributed to the stupidity of shoemakers, who, for the most part, have not sufficient ingenuity to make a well-fitting shoe; yet it cannot be denied that much is also to be attributed to a foolish pride and vanity, which induce many persons to imprison their feet in shoes unreasonably small. But this is only one of the vast catalogue of evils inflicted upon us by the tyrant *Fashion.*

Mr. Durlacher could not have given a better evidence of his honesty and exemption from humbugery, than the following candid confession. He says: "Although I have devoted nearly thirty years' practical experience to the investigation, and have tried various chemical and other remedial agents, yet I have never been able to discover any certain cure for corns." This is one of the instances in which the old proverb—"*An ounce of prevention is better than a pound of cure,*" is strikingly verified. Corns may be easily prevented, but with difficulty cured. We would recommend this little book to the attention of the *corned.*

E. D. F.

Art. VII.—*The Principles and Practice of Obstetric Medicine and Surgery, in reference to the process of Parturition.* By Francis H. Ramsbotham, M.D., &c. &c. A new edition, from the enlarged and revised London edition: large 8vo.; pp. 519. Philadelphia: Lea & Blanchard, 1845.

In 1841, the first edition of this work was published in London; and nothing can better indicate the high confidence of the medical public in the merits of the work, than the fact, that this edition of two thousand five hundred copies, in England, and a very large edition in the United States, has been already exhausted, and a new edition called for, by the demands for the work. When the whole profession is thus unanimous in placing such a work in the very first rank as regards the extent and correctness of all the details of the theory and practice of so important a branch of learning, our commendation or condemnation would be of little consequence; but, regarding it as the most useful of all works of the kind, to those physicians who desire to step beyond the common knowledge of routine practice, we think it but an act of justice to urge its claims upon the profession.

W. M. C.

ART. VIII.—*The Half-Yearly Abstract of the Medical Sciences; being a practical and analytical Digest of the contents of the principal British and Continental Medical Works published in the preceding six months, &c.* Edited by W. H. RANKING, M.D., Cantab., &c. Part 2, vol. I.: July to December, 1845. New York: J. & H. G. Langley. Pp. 373, 8vo.

The second part of volume I. of this excellent work has just come to hand, and its rich and varied contents are calculated to sustain the high reputation that the first established. This will be one of the most useful works ever published, as it gives an analysis of all discoveries in the medical sciences; and to those who will read it with care, it will serve as an index to what is being accomplished abroad, and enable them at once to obtain reference to the original sources of information, on subjects which they may desire to pursue by further investigations.

Medical literature is now very extensive, and it is impossible for any physician to purchase all the books and journals published: a publication of this kind is therefore most important, as a means of communicating the intelligence of discoveries, &c., to all the profession. Its extremely low price recommends it to all, as it is the cheapest medical work of its size ever published; and it is to us impossible to conceive how so much valuable matter can be collected and given to the world at a price so apparently inadequate to defray even the expenses of printing.

W. M. C.

Part Third.

EXCERPTA.

1.—*A Case of Hydarthros of the Shoulder-Joint, treated by Iodine injections, with Remarks upon the treatment of Hydarthros in general.* By J. Roux. (Annal. de La Chirurg.)

The following case is regarded by M. Roux as so important and unique in science, that he has taken pains to give its details, concluding with some considerations upon the pathology and treatment of Hydarthros in general.

The subject of this case was a farmer, near Toulon, aged 47 years, of a sanguine temperament, well organized, of medium corpulency,—had been affected with chills during the winter, with slight bronchitis. In June, 1843, he was attacked with a quotidian intermittent: took purgatives repeatedly, but was not cured until November of the same year. In September, he felt a pain in the left shoulder, soon followed by swelling and inability to move the hand. He exposed the part affected to hot vapour charged with some aromatic plants, and twelve days afterwards he resumed his usual avocations. About the first of October, a month thereafter, he experienced an intense pain in the right hip-joint, and embarrassment in moving the leg of the same side; in twenty days, however, these pains spontaneously subsided.

About the first of November of the same year, when lifting a large stone, Fournier (this being the name of the subject) suddenly felt a very severe pain in his left shoulder-joint. This pain returning from time to time, when extended motion was attempted, persisted for six months, and finally became so severe, that in August, 1844, the patient was forced to suspend his ordinary occupation. During these eight months, the joint of the shoulder gradually enlarged; its motions were painful and more limited: he could use only the fore arm of the left side, and but partially without the opposite hand. During this period he had used, without avail, leeches, cataplasms, frictions with various ointments, blisters, and aromatic fumigations.

M. Roux saw, for the first time, the patient August 1st, 1844, eight months after the commencement of the disease. For some days previously he had felt violent pains in the affected shoulder; these being relieved, M. Roux examined and found the parts as follows:—The left was about one centimetre longer than the right arm, hanging down the side, and inclined from within to without, relatively to the axis of the body. The fore-arm of the same side, flexed almost at a right angle, reposed against the chest, the elbow projecting outwardly, was sustained by the right hand of the patient. The entire trunk was a little inclined to the right. The shoulder of that side was evidently depressed, considerably swelled, extending some distance down the arm, around in the axilla, and back upon the scapula. The head of the humerus could not be felt in its place. The skin was tender, shining, and quite thin. Fluctuation was everywhere manifest; most evident in the scapular region posterior to the axillary space, or rather, from the external scapular region to the two regions already mentioned. The arm could scarcely be moved. Extension was impossible, and abduction exceedingly limited; rotation quite obscure, and circumduction out of the question. Any attempt to move the arm, on the part of M. Roux, gave rise to great pain; pressure was likewise disagreeable to the patient.

M. Roux at once pronounced this a case of *hydarthros* of the shoulder joint, caused, perhaps, by rheumatism. The size of the swelling—the evident fluctuation, and the embarrassed state of the parts, could only be explained upon the supposition that the *hydarthros* had invaded the sheaths, &c., of the scapulo-humoral muscles: yet M. Roux admits, that several surgeons, rejecting this opinion, regarded it as an abscess developed around the joint.

Fourier, anxious to be relieved, consented to submit to any treatment deemed necessary. M. Roux accordingly determined to evacuate the fluid by the subcutaneous method, and to resort, afterwards, to compression. He then thrust a trochar, with a cock attached, into the part, at the base of the depression formed in the middle of the subscapula fossa. When the serous collection was reached by the instrument, it was withdrawn, and the canula allowed to remain; then, adapting a syringe to the canula, and opening the cock, M. Roux drew off about six hundred *grammes* of viscous, ropy and yellow-coloured synovia. The tumour shrank in all directions; the fluctuation gave way to a doughy feel, and a peculiar flaccidity of the muscles. These latter could not be made to contract regularly; the head of the humerus could then be felt by the fingers, and was easily thrown from the glenoid cavity. After this simple operation, which produced but slight pain, a plaster spread with gum was placed over the wound, and over this thick compresses, the whole maintained by appropriate bandages, &c. But little was gained by this operation: the wound healed by the first intention; but the fluid soon began to accumulate, and gradually the swelling became as large as before. In fifteen days after the operation, the joint was in the same condition as when first seen by M. Roux.

It was now decided to resort to iodine injections; and, in the presence of MM. Aubert and Levicaire, distinguished military surgeons, M. Roux again made a sub-cutaneous puncture in the inside of the point of the shoulder, near the accromian, about one *centimetre* from the cellular line which runs between the deltoid and great pectoral muscles, and drew off with the syringe 400 *grammes* of ropy, albuminous fluid, not quite so yellow as that of the first operation, yet possessing all the characteristics of synovia.

All the fluid could not be abstracted, yet M. Roux injected 300 grammes of water into the wound, containing 100 *grammes* of tinct. of iodine. When the joint was considerably distended, with the injection, it was withdrawn, and again thrown in, and this operation was repeated three times. In imitation of M. Velpeau, a small quantity of the fluid was allowed to remain in the joint. Appropriate dressings were applied, and the patient sent home. On the following night, a violent pain seized the joint; the patient became agitated; had insomnia and fever. In this state he continued thirty-six hours, when the local symptoms yielded to a large flax-seed cataplasm. On the 17th of August, three days afterwards, the small wound had almost completely cicatrized; the swelling of the joint was not so great; obscure fluctuation was detected in all points of the swelling, more evident, however, in the supra-scapular region, but less distinct at the point of the shoulder. This led M. Roux to think, that no communication existed between these two portions of the tumour. The patient complained of an intense pain about the tendon of the deltoid muscle: he was dieted and kept quiet. On the following days, the pain became more severe in the supra-scapula fossa, in the axilla, and along the bicipital groove; three places corresponding with the three expansions of the synovial capsule of the joint.

On the 23d the fever became intense, and a phlegmonous inflammation, with heat, redness and pain, declared itself at the three points indicated, without the point of the shoulder participating in the affection.

Finally, a circumscribed fluctuation being detected, M. Roux made three punctures, the one in the middle part of the supra-spinous fossa, the second in the axilla, the third within and a little above the insertion of the deltoid in the humerus. From all these punctures issued a serosity mixed with a little blood, pus, and albuminous flakes, and the patient was relieved. The wounds were dressed, but did not cicatrize until the end of September; but, six days afterwards, the same scene re-appeared. The three points indicated became the seat of a phlegmonous inflammation, which was in vain combatted by diet, leeches, cataplasms, and emollient lavements. Fluctuation became rapidly developed; and the fever, agitation and pains only yielded to three punctures practised in the inflamed points, and to a considerable evacuation of a sero-purulent fluid mixed with albuminous floculi. The shoulder joint itself remaining free from inflammation, M. Roux introduced into the wounds tents, in order to keep up the discharge. However, after a few days, the point of the shoulder near the accromian became painful; it was dissipated by leeches.

In December, the three fistulous wounds cicatrized: a blister was applied along the tract dividing the deltoid from the great pectoral; suppuration was encouraged for twenty days. After this time, the case progressed; the movements of the joint became more extended, and less painful. The arm could be extended at a right angle with the body, and in this motion the articulating surfaces could be distinctly heard gliding over each other. In September, 1845, Fournier was quite cured, and resumed his avocations.

The pathological history of *hydarthros* in general seems to have been confounded with the dropsy of the ginglymoid and condyloid joints, without entering into the consideration of those peculiar to a serous collection of the orbicular or enarthroidal articulations; whence it happens, that some of the traits of this disease will be wanting in the present description. In proof of this declaration, M. Roux refers to the works on pathology and to special essays, such as those of MM. Murat, Boyer, Dupuytren, Roche & Sanson, Brodie, Velpeau, Blandin, Cloquet, Vidal, Berard, Fabre, Bonnet, Lesauvage, &c. All these authors have taken, as a type for their descriptions, *hydarthros* of the knee, and been perfectly silent upon those of the shoulder and of the hips. Dropsy of the enarthroidal joints presents other characters, more prominent, and entirely special in their nature: these consist in a frequent elongation of the extra-capsula expansions with which the synovial capsule invests the tendons of the muscles, in order to facilitate their motion; the distension of the muscles which covers them; their flaccidity after the evacuation of the articular fluid; in both cases, their action upon the bony levers, and consequently upon the direction of the limbs; lastly, their inaptitude to contract, at least, in a complete and regular manner. Upon these latter points, the omission is so complete in works on pathology, that in his last treatise, published in 1845, *upon the diseases of the articulations*, M. Bonnet, in his chapter upon *hydarthros*, says nothing of this state of the muscles, nor of the phenomena which depend upon it: an omission comparable, according to M. Roux, to that committed by the author who, in treating of ascites, was silent upon the state of the muscles of the abdominal parietes, and consequently upon every thing relating to their distention by the liquid, and their relaxation consequent upon its evacuation. The gradual distention of the muscles which cover the orbicular articulations has the effect of exciting their contraction, at least during a certain time, and to cause them to exercise upon the effused fluid an incessant action, the result of which is to dilate the least resisting parts of the fibrous envelopes, and consequently to favor the elongation of the serous expansions extending beyond the articulating surfaces, of which mention has already been made. In this way, a sort of serous hernia is formed about dropsical joints, which may acquire a considerable size. In the shoulder-joint, the extra-capsular expansions which the synovial envelope sends to the tendon of the long head of the biceps, of the supra-spinous, and of the sub-scapular, may acquire such dimensions in hydarthros, that fluctuation may be detected at the middle part of the arm, in the axilla, and even in the supra-spinal fossa. Hence, in the hip-joint, where the fibrous capsule is so resisting, the fluid, acted on by the muscles, easily distends the expansion which the articular serous membrane projects under the tendon of the psoa-iliac muscles; that a large pear-shaped pouch or sack is developed in the midst of the muscles of that region; and that fluctuation is quite evident both in the upper and lower part of the limb, at some distance from the joint. These observations have been verified in two cases by M. Roux: one has already been described; the other occurred in a sailor, and may be thus described:—The disease seemed stationary, and fluctuation could be detected above Poupart's ligament, in the left iliac fossa, when suddenly he was attacked with coxalgia. A short time afterwards, M. Roux imagined he detected fluctuation at the superior and internal part of the thigh. Daily it became more manifest; and as this new collection of pus had no direct communication with that which existed above the crural arch, since the wave of the fluid could not be perceived from the one to the others, M. Roux thought the pus had gained entrance into the hip-joint through the corresponding part of the bones of the ilia, as happens in cases of intra-pelvic abscess. The pus, on getting into the hip-joint, and being pressed by the contraction of the muscles which cover it, had a more powerful tendency to force it within the thigh, by distending the serous expansion which the synovial capsule transmits to the tendons of the psoas-iliac muscles, than to expand the resisting capsule of the joint. The patient died in six months, after having undergone four sub-cutaneous punctures, which discharged, at each time, pus from the tumours, and on examination after death, five lumbar vertebræ were found carious; pus was infiltrated along the psoas muscle; and the cellular sack which it contained, resting against, Poupart's ligament, was much enlarged on this side looking towards the pelvic cavity. In this case, the pus had penetrated through the *levator ani* muscle, and the corresponding iliac bone, into the joint, destroying the articulating surfaces. The pus but partially distended the articular capsules, but exterior to the joint there existed a vast pouch, which communicated with it by a circular opening.

The same thing occurred in the case of hydarthros of the coxo-femoral joint, related and reported by M. Lesauvage.

From these observations, made by M. Roux, it follows—1st, That in orbicular hydarthros, the effused fluid distends not only the articular serous capsule itself, but also their extra-capsular expansions; 2d, That in hydarthros of the hip-joint, within the articulation and without, fluctuation should be detected first with more facility, notwithstanding the thickness of the soft parts; 3d, That dropsy may affect separately the extra-capsular expansions of the synovial membrane; 4th, That, in enarthroidal hydarthroses, the muscles are especially liable to alterations, whence proceeds, at least in part, the elongation of the extra-capsular expansions, the direction of the limb—the difficulty, and even impossibility of moving it.

If, now, we look at the therapeutics of this question, we must endeavor to discover the state of the science, in our classical productions, before the year 1838, touching the treatment of hydarthros, and what improvements have been adopted since. We find that general and local bleeding, tonics, resolvents, purgatives, sudorifics, baths, douches, blisters, caustics, compression, &c., were employed in the treatment of this affection.

Doubtless, by all these means, our ancestors cured articular dropsies as we now cure them. It is a matter of daily observation, that *traumatic hydarthros*, such as those produced by a simple acute arthritis, or connected with a blenorrhagia—with a rheumatism, or any other cause, but nevertheless recent, yield, or may yield, spontaneously, by perfect quietude of the parts, or the use of the simplest means. Experience likewise proves, that articular dropsies, of several months' duration, are successfully combatted by powerful counter-irritants or suppurations on the surface over the affected joint, or by the action of baths, douches, &c. A large blister, covering the entire joint, is very effectual in some cases. This is the practice of M. Velpeau, and M. Roux's experience confirms it.

All these means, however, fail in some chronic cases; then we must institute other modes of treatment. [Surgeons, for a long time, hesitated to perform any surgical operation upon the joints, justly apprehending that an invincible inflammation might be created. These fears are fast passing away, and we now open even the peritoneal sack, and remove large tumours from the abdomen, without producing death].

Experience having taught us, recently, that sub-cutaneous incisions were not attended with suppuration, the chief danger to be apprehended in wounds of the joints; and, besides, both practice and reasoning having led us to think that surgery should not remain idle when we have to do with a chronic hydarthros that deprives an individual of the use of his limbs because the disease may depend upon a peculiar diathesis, art has boldly ventured to interfere in such cases.

Jobert, Velpeau and Bennet have attempted the cure of dropsy of the joints by the same means as those used for the cure of hydrocele; viz., injecting fluid, medicated, or otherwise, into the sack. For this purpose, they used spirits, tinct. iodine, and other stimulating liquids, in many cases with complete success. In 1842, M. Goyrand, of Aix, in a case of dropsy of the knee-joint, opened the synovia by a sub-cutaneous incision, and discharged the fluid in the surrounding cellular tissue; he then resorted to methodical compression, and a perfect cure was effected.

About, or soon after this period, Dr. Müller passed a seton through a considerable hydarthros of the knee: in four weeks afterwards, he withdrew the seton, resorted to efficient compression, and a cure was accomplished. The same treatment succeeded, Müller assures us, in *nine* other cases of the same disease. From facts and reasoning, M. Roux thus concludes his subject:

1. That Dr. Gimelli's method, the purgative, and more recently the hydrosudopathic plan, by the influence which they exert upon the absorbents, must exercise a certain control over the general cause which produced the affection, constituting certainly a valuable means of treatment, and applicable to all hydarthroses, whatever may be their seat, cause, or duration.

2. That simple puncture, with or without scarifications of the serous surfaces, or aided by compression, seems to have been useful, though the difficulty of scarifying or of reaching, by accurate compression, all the points of the joint, expose us to the danger of relapses.

3. That the seton, notwithstanding the published success of a few cases, is not less dangerous, because it may determine ankyloses, or excite suppuration within the joint.

4. That the trifling danger attending iodine injections in the cure of hydrocele, in dropsies of the sub-cutaneous bursæ, of tendinous thecæ — of accidental serous cysts, of the articular cavities, all seem to authorize a similar practice in *hydarthros*. The injection, penetrating into all the cavities and sinuosities of the joint, exercises a modifying influence upon the parts, and favors a resolution of the existing local affection.

Remarks.—In the summer of 1845, a man, aged about 40, came under our care, to be treated for a disease of the left knee. He stated, that, in falling, he ran a large tenpenny-nail under the patella, to a considerable depth. On examination, we discovered a penetrating wound, entrance just over the outer condyle, and beneath the outer border of the rotula. The joint was greatly swollen, not very tender on pressure, elastic to the touch, but slightly discoloured, and the seat of some morbid heat. From the mouth of the wound, which was ragged, a quantity of lubricious, pale straw-coloured synovial fluid continued to flow almost incessantly. A demiflexed position of the limb was most agreeable; the pulse was hurried, the tongue white, and the patient was pale and anaemic. Local depletion, followed by evaporating lotions, methodical compression, blisters, &c., produced but partial relief. We pushed a solid stick of nitrate of silver to the depth of more than two inches along the track made by the nail: it created little or no pain. This diminished the discharge of synovia. We gave calomel and opium internally, followed by hydriod. potass, and other alteratives. Iodine ointment to the joint, aided by compression, ultimately reduced the swelling one-half; at this point it remained stationary. We then resorted to tartar emetic ointment, and kept up a heavy crop of suppurating pustules for two weeks, with small doses of quinine internally. Under this treatment, the discharge of synovia ceased; the wound healed; the tumefaction and stiffness disappeared; flexion and extension were quite practicable. In a short time, the patient was discharged cured of his *synovitis.*

2.—*On the Minute Anatomy and Pathology of Bright's Disease of the Kidney, and on the Relation of the Renal Disease to those Diseases of the Liver, Heart, and Arteries with which it is commonly associated.* By George Johnson, M.D., of King's College, London.—Read before the Royal Medical and Chirurgical Society, Tuesday, November 11, 1845: Dr. Chambers, President, in the Chair. [From the London Medical Gazette, November, 1845.]

The author began by stating, that the true nature of Bright's disease was, he believed, to be found in a diseased state of the secretory or epithelium-cells which line the urinary tubules. He arrived at this conclusion in the first week of July of the present year, at which period he demonstrated his preparations to Professors Todd and Partridge; and a paper containing the result of his researches was given into the hands of one of the secretaries of this Society on the 7th of August.

The author then stated, that he had ascertained that the secretory or epithelium-cells of the kidney contain naturally a minute quantity of oil in the shape of globules, such as are familiar to microscopical observers. The presence of these globules is constant in the kidney, but their quantity varies considerably within the limits of health.

Bright's disease, the author considers, may be described as *primarily and essentially an exaggeration of the fat which exists naturally in small quantities in the epithelium cells of the healthy gland*—a fatty degeneration of the kidney analogous to the fatty degeneration of the liver, as demonstrated by Mr. Bowman.*

This accumulation of fat in the secretory cells necessarily leads to the engorgement and dilatation of the tubules which they line, and one or more convoluted tubes thus gorged with fat, and projecting either on the surface of the gland, or on the surface of a section, constitutes one of the so-called "granulations of Bright." Some Malpighian bodies were observed to contain no fat, whilst others contained gorged fatty cells; but the author had never observed in these bodies an accumulation sufficient to produce destructive pressure on the Malpighian tuft of capillaries. The frequent connexion of albuminous and bloody urine with Bright's disease, and the atrophy of the kidney, are attributed by the author to the mechanical operation of the above-described fatty accumulation. Having alluded to the circulation of the gland as described by Mr. Bowman, he entered into a minute detail of the reasons which led him to the conclusion that the presence of albumen and blood in the urine, is, in this disease, a secondary phenomenon, dependent on the previous morbid changes.

In reference to the atrophy which the kidney so often undergoes in this disease, the author contrasted the well-known peculiarities of the vascular

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* Lancet, January, 1842.

organization of this gland with the very dissimilar arrangement of the vessels and secretory cells of the liver, an organ which appears to suffer but little from a similar engorgement in its cells.

In speaking of the *stages* of this disease, the author observed that he had no reason for believing in the existence of any congestive stage as necessarily preceding the morbid accumulation which he describes. The various forms about which so much has been said and written, he believes to depend, in great part, if not entirely, on the rapidity with which the disease advances. In cases of long duration, the kidney is generally found small, contracted, and granular. When the progress of the case has been rapid, the gland is large, smooth, and mottled.

The author then dwelt at some length on the frequent coincidence which he had observed of the disease in question, with a similar fatty degeneration of the liver, arteries, and valves of the heart.

From the above data, the author deduced the important *practical conclusion*, that these fatty degenerations so often conjoined are of constitutional origin, and that they must not be considered and treated as local disorders. He repudiated the notion of Bright's disease having any specific connexion with scarlatina; neither did he believe in its alleged relation to acute inflammatory dropsy.

The causes of the disease are, according to the author, essentially debilitating: in large towns the disease is prevalent and fatal; in country districts, it is comparatively rare. The disease has been artificially produced by Mr. Simon,* of King's College, in the lower animals, by their continued exposure to depressing influences.

With respect to the microscopical characters of the urine in this disease, the author remarks—1st, That the cylindrical bodies described by Dr. F. Simon are fibrinous casts of the tubes, frequently entangling blood-discs, oil-globules, or epithelial cells with fatty contents; 2d, That the presence of much fat in the urine is an alarming symptom. In an advanced stage of the disease, fat rarely abounds; but, from experiments on the lower animals, as well as from observation on the human subject, it seems probable that, in many cases of chronic ill-health during a period in which no especial attention is directed to the state of the urine, there may be eliminated with this secretion such an excess of fatty matter as would in reality mark the first stage of Bright's disease.

On the subject of treatment, the author stated, that the obvious indications were—

1. The pursuance of a general tonic regimen in respect of diet, atmosphere, exercise, and medicine.
2. The careful avoidance of all exhausting remedies.
3. To avoid, as articles of food, fat and other highly carbonized materials. And,
4. To relieve congestion of the gland by strict attention to the functions of the skin and bowels, and by such small blood-lettings as circumstances might demand.

Dr. Todd said, that he rose at that early period of the debate, for the purpose of bearing testimony to the accuracy of Dr. Johnson's statements. The admirable and lucid manner in which Dr. Johnson had expounded his views would, he was sure, be generally acknowledged. He (Dr. Todd) had had the opportunity of watching the progress of the investigation, from its commencement in July to the completion of the paper which had just been read, and he would add, that he had rarely witnessed a more interesting inquiry. Independently of the intrinsic merit of this investigation, he felt that it was peculiarly important, as tending to turn the attention away from questions of mere vascular reple-

* Observations on the Artificial Production of Scrofulous Diseases in the Lower Animals. (Unpublished.)

tion, or the opposite condition; and to direct it to the real state of the elements of textures—as of the kidney, in the present case—as being those parts in which the seeds of disease are sown. According to the views now brought forward, we must count three stages in Bright's disease:—In the first stage there is a morbid state of the primary and secondary assimilating processes, giving rise to a diseased state of the blood. At this stage there are no very marked signs of disorder readily recognizable by the physician. The second stage is accompanied by a change in the attraction between the gland and certain constituents of blood, so that fat, which in health passes off by the kidney only in small quantity, is now attracted largely to its elementary parts, the epithelium cells, and accumulates in them to overloading. These gorged cells collecting in the uriniferous tubes, press upon the capillary plexus of their walls, and throw back the blood on the Malpighian tufts, causing congestion or rupture of them. And this is the third stage, in which the urine becomes albuminous, and other signs appear, which have been so well pointed out by the extraordinary clinical research of Dr. Bright and his followers. A strong feeling had been growing up among practical men as to a close connexion between this disease and scrofula. The author's statements respecting the artificial production of the disease in animals bore upon this subject; but further research was necessary before the exact nature of the connexion could be determined.

Dr. Bright eulogized the paper, as showing great industry and perseverance. It was a paper of the greatest interest, and to himself more especially so. He could not vouch for the accuracy of all the conclusions come to by the author, but they bore the appearance of the greatest probability—they appeared like truth. Should future observers confirm the correctness of Dr. Johnson's investigations, a most important vacuum in regard to the disease under discussion would be filled up, and a more rational line of treatment would doubtless be the result.

Dr. Copland regarded the paper as one of the greatest interest; the chief point was new and striking, but there were particular statements in it which it was desirable to notice. It was stated by the author, that the disease had its origin in mal-assimilation of the food by the organs of digestion, resulting from a morbid state of the blood. Now, this was by no means a novel doctrine, for, in a treatise which he (Dr. Copland) had published three years since, he had expressly stated this to be his opinion. He should be glad to know, in respect to the presence of fat in the epithelial cells of the kidney, whether Dr. Johnson had any other than microscopical evidence of the correctness of his statement;—had any chemical examination been made? Granting that the disease did arise from the presence of fat in the epithelial cells, how could we account for the large quantity of albumen found in the urine, the absence of salts in that secretion, and their presence in the blood? The treatment advocated in the paper was that which was usually pursued.

Dr. Snow said, that he felt a difficulty in admitting that the fat which Dr. Johnson had discovered in the minute tubuli of the kidney, could, by its mechanical pressure, be the cause of the escape of albumen, and other constituents of the blood, into the urine; for, in the latter stages of the disease, when the amount of fat must be greatest, the quantity of albumen was diminished, and in many cases disappeared altogether. If there was any great amount of albumen, or blood, in the urine, in the advanced stage of Bright's disease, it was when, supervening on the chronic disorder, there was an acute attack, resembling the acute disease of the kidney from exposure to cold, or that which follows scarlet fever, in which acute diseases the author of the paper admitted that fat was not present. Whilst he (Dr. Snow) admitted that the disease in question generally commenced gradually, and was the result of cachexia, he considered that it sometimes owed its origin to an acute attack. In a child which died with dropsy at the end of two or three months, after scarlet fever, he found the kidneys so much hypertrophied, that they weighed a pound, and were, to some extent, affected with granular degeneration. Now, the dropsy commenced in this child about twenty-one days after the commencement of the fever, and just in the same manner as it comes on in other children, in whom we know, from their perfect recovery, that there was no previous disease of the kidney. Dr. Christison had mentioned one or two cases of this disease, which seemed to owe their origin to scarlet fever; and Dr. Bright had related some which seemed to date their origin from an acute attack. If the patient should abstain from fatty and farinaceous and saccharine food, as recommended in the paper, the only sustenance left for him would be nitrogenous food, as albumen and fibrine; but, unfortunately, he was in constant danger of secondary diseases, as inflammations and coma, with convulsions, from the nitrogenous products of excretion, which the disabled kidneys could not duly separate from the blood.

Dr. Budd had, some time since, been made acquainted with the views advocated by Dr. Johnson, and had put to the test the plan of treatment recommended. He had placed three or four patients in King's College Hospital, laboring under dropsy and albuminous urine, on a diet consisting of lean meat, bread, and water, and abstaining from sugar, fat, and starch. He had also administered iodide of potassium and liquor potassæ. The result of the treatment, as far as it had gone, had been quite satisfactory; but of course, at present, it would be premature to draw any conclusion from it. It would be scarcely possible to over-estimate the value of the paper, which threw a flood of light on the pathology of the disease, and realized the sagacious suspicion of Dr. Prout with respect to the true nature of the disease. Considerable stress had been laid on the structural changes in this disease; but the question arose as to the origin of the fat formed in the epithelial cells. It had been considered, that in the fatty liver which prevailed in the advanced stages of phthisis, the fat was attributable to deficient aeration of the blood; but this opinion was not tenable, for depositions of fat in spinal organs was found, in all cases, attended with much fever and rapid wasting. He considered, that in these cases there was not a deposition of new fat, but that the fat, stored in the system, was taken into the circulation, and re-deposited in the various organs found affected. The fatty livers in the well-known Strasburgh geese resulted from the causes he had alluded to. This view was further strengthened by the fact, that patients laboring under albuminuria were usually fat.

Dr. Johnson said, in reply to Dr. Copland's question regarding the evidence of the presence of fat in the kidney, that a microscopical observer was scarcely likely to confound fat globules with globules of albumen. He could, however, offer a chemical test of the accuracy of his statements, as he had, with Dr. Miller, commenced an analysis of portions of fatty liver and fatty kidney, by digestion in ether, by which it was shown that the former contained rather more than, and the latter about, one-sixth portion of fat. The analysis was not yet complete, or he should have alluded to it in the paper. When complete, however, it was probable that the fat would be found in even greater proportion. With respect to the observations of Dr. Copland regarding the treatment of the disease, he (Dr. Johnson) thought one important principle had never before been mentioned, that of the necessity of abstaining from fat as an article of diet.

Mr. Toynbee had, during the last three or four years, injected and examined as many as eighty kidneys. He complimented the author of the paper on his investigation, but he regarded the inquiry as not yet complete. He agreed with Dr. Johnson, that it was not probable a microscopical observer would mistake albumen for fat. In the investigations which he had made, he had paid particular attention to the condition of the arterial, venous, and tubular vessels of this organ, and for this purpose had made minute injections. In this particular respect, Dr. Johnson's paper was deficient. He had observed in the advanced, and, indeed, even in the early stages of the disease, that the blood-vessels had become diseased; the plexus of vessels in the Malpighian corpuscles were enlarged to three or four times their natural size, as were also the tubuli. Dr. Johnson had also neglected to investigate the condition of the true parenchyma of the kidney, and had paid too much attention to the lining membrane and the epithelial cells, for the parenchyma consisted of corpuscles or cells which in this disease became much enlarged.

Mr. Simon spoke of the great difficulty, and, indeed, impossibility, of injecting the kidney in Bright's disease, in consequence of the impervious condition of the vessels. With respect to the treatment of the disease, he differed with Dr. Copland, that the treatment recommended in the paper was that which had been always pursued. Dr. Copland, in his Dictionary, had enumerated, among other modes of treatment which had been employed, and, among others, that of hydragogue cathartics—means which were quite forbidden by Doctor Johnson, to whom the profession were at least indebted for pointing out an explanation of the rationale of treatment.

Dr. Copland, in explanation, said, that in his own practice he endeavored to improve the function of the digestive and assimilative organs: he ordered cuppings, to a small extent in the loins, in the early stage of the disease, and exhibited chalybeates, with the view of improving the system generally. He had found much advantage from the tincture of muriate of iron, combined with tincture of lytta, or some other stimulant. He was at a loss to explain, from Dr. Johnson's theory, the occurrence of very acute cases of the disease, which could scarcely be considered to arise from a rapid deposition of fat in the kidney.

Dr. Budd said, that Mr. Busk, of the Dreadnought, had, six or seven weeks since, exhibited to him some drawings of morbid kidney, which tended to confirm the views of Dr. Johnson.

Dr. Todd remarked, that if Dr. Copland would weigh well the observations made, he would change the order of his treatment. Small blood-lettings, in the early stage, did harm, for it was only in the more advanced stages of the disease, when the accumulation of fat had taken place in the kidneys, and the organ became congested, that small blood-lettings did good.

Dr. C. J. B. Williams was not present when the paper was read, but he had gathered sufficient from the speakers to arrive at a knowledge of the main points of treatment advocated by Dr. Johnson, and that he deprecated any depletory measures, particularly in the earlier stages of the disease. Now he (Dr. Williams) knew no treatment so beneficial and successful as cautious blood-lettings in the early stages, particularly in the acute form of the disease. He had recorded twenty or thirty cases of albuminuria, altogether independent of scarlet fever, and coming on as an acute disease, in which, in the early stages, he had employed cupping on the loins, hydragogue cathartics, conjoined with medicines calculated to improve the general health, with the greatest success. He had found no improvement in the condition of the urine, until these means had been applied. The enlarged and congested state of the kidney, together with tenderness over the organ, demanded this plan of treatment. Depletion, on the contrary, was contra-indicated in chronic cases: he agreed with Mr. Toynbee as to the importance of attention, in the first instance, to the condition of the parenchyma of the organ, as he (Dr. Williams) considered that the deposit of fat was a secondary effect, and not the *fons et origo* of the disease. There were certain conditions of the kidneys, somewhat resembling Bright's disease, in which there was diminution of the urine with albumen, but the water of the urine was diminished in quantity as well as the natural constituents of the secretion; but here, instead of having a mottled kidney, you had simply enlargement and induration; the organ did not abound in fatty, but in granular matter; and the cells constituting the parenchyma were increased in the number, and contained granules. Dr. Quain had also discovered these granules in the tubuli.

Dr. Golding Bird, whilst according his fullest approbation to the ingenuity and industry displayed in the researches of Dr. Johnson, still felt compelled to withhold his acquiescence in the statement that the hypothesis now propounded was adequate to explain all the phenomena observed in the disease under consideration. The late hour of the evening prevented his alluding to more than one or two of the most important points, in which he felt inclined to regard Dr. Johnson's theory as insufficient. The great novelty of his views consisted in the parallelism he had drawn between fatty degeneration of the liver and the state of the kidney under discussion. Yet what was the fact? In fatty liver the secretion of bile went on tolerably well; at all events, remarkably so when the state of the gland itself is borne in mind, there being no evidence of the retention of bile in the blood, jaundice being by no means a necessary concomitant even of a very fat liver. Yet how different are the facts observed in the granular kidney which Dr. Johnson assumed to be in the state of fatty degeneration: here the two elements of urine are not properly excreted; on the contrary, albumen and certain elements of the blood appear in the secretion, whilst the patient is poisoned by the retained elements of urine. Hence, if Dr. Johnson's views be accepted, we must admit that fat deposited in the cells of the liver and tubes of the kidney produces very different results on the secreting powers of the organ. Further, the fact of diuresis, often copious in the latter stages of morbus Brightii, appeared to him to be quite opposed to the belief, that increasing pressure on the vascular plexuses, by deposited fat, was an active agent up to the last stages of the disease.

Dr. WATSON having passed a high eulogium on Dr. Johnson's paper, the Society adjourned.

3.—*Tetanus Naseentium, Neonatorum, or Infantum.*

[As we have invited the special attention of Southern physicians to this curious and fatal disease, we have concluded that we could not do our readers a greater service than to lay before them the following extract from one of the ablest essays in the English language. We allude to—"*A Treatise on Tetanus, being the Essay for which the Jacksonian Prize. for the year* 1834, *was awarded, by the Royal College of Surgeons, in London: by* T. B. CURLING, *Assistant Surgeon to the London Hospital, &c.*" We find it in "*Bell's Select Medical Library: Philadelphia*, 1837." Although twelve years have elapsed since this essay was written, we are not aware that any thing of importance has since been added to its masterly analysis of all that was then known on the subject of Tetanus.]

This is a variety of traumatic Tetanus, occurring, as its name implies, in children soon after birth; generally in the course of the first week, or before the ninth day, having scarcely ever been observed to supervene at a later period than a fortnight. The muscles most frequently affected are those of the lower jaw, hence the term Trismus is commonly applied to this disease; but as many of the other muscles almost invariably participate, the term Tetanus Nascentium is more appropriate. In a case reported by Dr. Furlonge, at page 111, it was observed that the face was strongly marked with a tetanic expression.

Tetanus Nascentium very rarely attacks children in this climate. Dr. Cullen, however, speaks of it as occurring in the Highlands of Scotland, and in Switzerland; and Dr. Clarke and Dr. Colles have met with it in Dublin. It is said to occur occasionally in Germany, at Paris, Vienna, and Brussels.* That the disease is seldom the cause of death in this country, is shown by a valuable table taken from the register, kept with great care at the Rusholme Road Cemetery in Manchester, and published by Mr. Robertson, to illustrate the mortality arising from various diseases at different periods of childhood. In four years from April, 1821, there were 2056 deaths under the age of ten years, of which number 146 died during the first month after birth, but not one

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* Dictionnaire des Sciences Médicales—Tétanos.

from Tetanus.* Dr. Francis Ramsbotham, who has been connected for several years with a very extensive lying-in charity, assures me that he has not met with a single case. Like other forms of Tetanus, it is more frequent in warm climates,† especially in the West Indies, where the disease is the cause of mortality to so great an extent, that one writer states that a tenth of all the children that are born die of it.‡ Mr. Maxwell calculates its depopulating influence at about twenty-five per cent., which he justly observes has scarcely a parallel within the bills of mortality. Dr. Hancock remarks, it is so frequent and fatal in the colonies of Essequebo and Demerara, that, at an average estimate, it kills one-half of the whole number of infants born there, and the fatality he reckons at least at ninety-nine in a hundred.§ Mons. Fourcroy calculates that, at St. Dominique, eighty out of every hundred of the negro children die of it before the ninth day.‖ Dr. Morrison, who practised for some years at Demerara, has never known one authenticated case of recovery. Dr. Valentine witnessed several cases in America, but had never seen one cured. A Spanish physician, of the name of Hyacinthus Andreas, at the end of the seventeenth century, published an account of it as met with in the island of Minorca, in which he states, that in twenty-six years' practice he had scarcely known six cases of recovery.¶ As was observed of traumatic Tetanus, negro children are more subject to this disease than the whites, which appears to be partly owing to the greater care taken with the latter after birth. Dazille affirms, that during thirty years' practice in different colonies, he had never seen or heard of a single white infant who had died "du mal de mâchoire." Campet states that the disease usually proves fatal on the second or third day; but that sometimes, when rapidly developed, it destroys in less than twenty-four hours.

Dr. Holland, in a summary of the diseases of the Icelanders, observes that, although Tetanus Nascentium occurs very rarely, if at all, on the mainland of Iceland, it is eminently disastrous in Heimaey, one of a group of islands consisting entirely of lava, situated on the southern coast. The population of Heimaey, which is the only one of these islands that is inhabited, amounts to about 200 souls, being almost entirely supported by migration from the main land, as scarcely a single instance has been known of a child surviving the period of infancy. From the symptoms described by Dr. Holland, no doubt can be entertained of the tetanic nature of the malady that proves so fatal. The following table, which includes a period of twenty-five years, shows the mortality consequent upon this disease in that island, and exhibits also the days upon which death happened:—

Children.		Days.	Children.		Days.
1	lived	2	18	lived	9
3		3	10		10
14		4	2		11
16		5	1		12
22		6	1		13
75		7	5		14
16		8	1		21

It will be seen, from this table, that the number of deaths on the seventh day greatly exceeded those on any other; and also, that they are more frequent on the fourteenth day, than on the days immediately preceding or succeeding it.

* Observations on the Mortality and Physical Management of Children. Lond., 1827.

† A German author (Akermanns), who wrote on the disease in the last century, states that it is endemic in Guinea and other parts of Africa.

‡ Rush's Medical Observations and Inquiries. Philadelphia.

§ Edinburgh Medical and Surgical Journal, vol. xxxv, p. 343.

‖ Les Enfans élevés dans l'ordre de la nature.

¶ Dr. Cleghorn's Observations on the Diseases in Minorca.

No methods of cure had been resorted to by the inhabitants. There is no vegetable food, and the natives live principally on the sea fowls called fulmars and puffins, (*procellaria glacialis et alca arctica, Linn.*) which are slightly salted and barrelled. The only cause to which Dr. Holland could attribute the origin of this malady was, the practice of giving to the infant a strong and oily animal food, almost immediately after birth, which, it is supposed, would create irritation in the bowels. It is the more probable that the disease has some connection with the diet of the natives, as it appears to have been much more frequent since their fishery was destroyed by the volcanic eruptions in 1783; and also from the circumstance, that in St. Kilda, the most remote of the Western Islands of Scotland, the inhabitants of which, in their diet and mode of life, much resemble the natives of Heimaey, the same disease exists.*

Tetanus Nascentium has been thought to arise from the irritation produced in the intestinal canal, by retained meconium; and Dr. Hillary noticed, in cases where purgatives had been employed, that a large quantity of unnatural matter was dislodged from the intestines. Dr. Morrison, however, observes, that it has often been witnessed upon occasions when retention of the meconium could not be considered a cause, and he is inclined to attribute it rather to the influence of cold, or of vitiated air. Dr. Hancock regards active purging, soon after birth, as one of the efficient causes of this disease; and he states that the nurses of Demerara usually give a new-born infant, of one or two days old, a dose befitting a child of two years. Dr. Joseph Clarke has published an account of this disease as occurring in Dublin, at which city it formerly proved fatal to a great many of the infants born in the Lying-in Hospital. At the conclusion of the year 1782, it appears that, of 17,650 infants born alive, 2944 had died within the first fortnight after birth, being nearly every sixth child, or about seventeen in the hundred. Two forms of this affection are described; one acute, carrying the infant off in from eight to thirty hours, during the paroxysms of which the face was observed to become livid: the other was more chronic in its character, the patient surviving from three to five days, and in some rare instances to seven, or even nine. The memory of the oldest person, it is stated, does not furnish an instance of one being cured. Dr. Clarke, believing that this great mortality originated in an impure atmosphere, suggested alterations for the more complete ventilation of the institution. Of 8033 infants born since the period at which these alterations were made, only 419 died of this disease, the rate of mortality being thereby reduced from seventeen per cent. to five or six.† In Jamaica, also, the occurrence of Tetanus Nascentium is attributed, by Mr. Maxwell, to a vitiated atmosphere, and in Germany it is said to be owing to close and unventilated chambers. It scarcely appears surprising that a vitiated atmosphere should be a predisposing cause of this disease at such a critical period, when its vivifying influence must be so essential for the preservation and support of the independent existence of the infant, especially when we reflect upon its effects in the production of the same and of other diseases at a more advanced age. Ledeschault states,‡ that compression of the fontanelles, and pouring cold water on the head in the ceremony of baptising, have been known to bring on the disease.

It has been stated, that this disease is only a variety of the traumatic form of Tetanus, and since it occurs invariably within a very limited period after birth, we are justified in assuming that the injury done to the umbilical cord in its division or separation, bears the same relation to Tetanus Nascentium as the primary wound to traumatic Tetanus. Dr. Colles, of Dublin, has attempted to show that the immediate cause of the affection is inflammation and ulceration of the umbilicus. In numerous dissections of children who had fallen

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* Travels in the Island of Iceland, during the summer of the year 1810. By Sir G. S. Mackenzie, Bart. Edinburgh, 1811.

† Transactions of the Royal Irish Academy, vol. iii.

‡ Dissertation sur le Tétanos. Paris, 1815.

victims to it, the following morbid appearances were observed: — Externally, in the vicinity of the umbilical vein, there were evident marks of superficial ulceration in some few instances, and in all, the edges of the vein were thickened. The peritoneum covering the umbilical vein was highly vascular, as if from inflammation: this extended sometimes up to the fissure of the liver; often, however, not for a greater length than one inch above the umbilicus. The peritoneum, in the course of the umbilical arteries, seemed still more inflamed, an appearance which extended often as far as the sides of the bladder. The inner surface of the umbilical vein was pale, and free from any marks of inflammation, yet its coats were very much thickened. On slitting open the umbilical arteries, coagulable lymph was found within their coats, and in all instances their coats were much thickened and hardened, even as far as the fundus of the bladder. The centre of the umbilicus was occupied by a soft yellow substance, resembling coagulable lymph.*

Although the appearances described by Dr. Colles, and considered as morbid, are stated to have been found in all the tetanic cases which he inspected, but were not discoverable in infants of the same age, who had died of other diseases, yet I am induced to regard them as little else than the ordinary results of the natural process towards the obliteration and closure of those vessels which, after birth, become useless. This view is corroborated by the investigations of Dr. Labatt, of the Dublin Lying-in Hospital, who has clearly shown, by several dissections, that the appearances described are not, as Dr. Colles represents, always present in infants who have died of Tetanus Nascentium, and that they are sometimes found in those who have died of other diseases.† And it is further confirmed by Dr. Thomson, in the examination of nearly forty children who died of this affection in Jamaica.‡

Tetanus Nascentium has, by Dr. J. W. Heustis, been attributed to the injury done to the umbilicus: he states, that amongst the negroes in the West Indies, the cord is generally cut with a dull pair of scissors, and the part suffered to remain undressed, to ulcerate, and to run into gangrene, purely from carelessness and inattention.|| Campet, a French writer of considerable experience, likewise attributes the origin of the disease to the improper treatment of the cord, and of the umbilicus, after its separation. Agreeably, however, to the view which has been taken of the state of the primary wound in traumatic Tetanus, I apprehend no greater importance can be attached to an inflamed or unhealthy aspect of the umbilical parts, than considering it as sympathetic of and indicating constitutional derangement, either from disorder in the alimentary canal, a vitiated state of the atmosphere, or other causes, a condition eminently favorable to the production of every form of this disease.

The pathological changes, remarked after death from Tetanus Infantum, appear to be equally inconstant with those observed in the other varieties of the disease. Dr. Goëlis, of Vienna, in the examination of children who have died of this disease in the Foundling Hospital of that city, frequently found an appearance of increased vascularity in the substance of and in the membranes enveloping the upper part of the spinal marrow.§ The same has been observed by Dr. Thompson, of Philadelphia; whereas, in the bodies examined by Doctor Thomson, of Jamaica, no morbid change in any part of the body could be detected.

The following are considered the predisposing causes of Tetanus Nascentium: — Unwholesome nutriment; irritation of the intestinal mucous membrane, from the retention of unhealthy secretions, or from acrid purgatives; an impure atmosphere; damp and cold. In the West Indies it is sometimes

* Dublin Hospital Reports, vol. i., p. 285.
† Edinburgh Medical and Surgical Journal, vol. xv., p. 216.
‡ Ibid., vol. xviii., p. 41.
|| Observations on Tetanus. Medical Repository, New York, vol. iii., p. 122.
§ Dictionnaire des Sciences Médicales — Tétanos.

found to prevail in certain districts, or on particular estates; whereas, in other parts of the same island it will scarcely be known for many years. This apparent anomaly is very probably owing to some difference in the economy of the estate or districts, the food, soil, ventilation, or salubrity of the situation.

The dreadful fatality of Tetanus Nascentium, which, in fact, appears hitherto to have baffled treatment in almost every instance, renders necessary the utmost attention to avoid all those circumstances which are supposed to favor its occurrence. And here a more cheering prospect is presented to the medical practitioner. The great success that resulted from the judicious measures adopted by Dr. Clarke, in respect to ventilation, at the Dublin Lying-in Hospital, has been already adverted to. Mr. Maxwell remarks, that by paying attention to the same important circumstance, and to the condition of the bowels and the navel, the disease was soon rarely to be met with. Dr. Lionel Chalmers, Dr. Hillary, and Dr. Cadwallader Evans* found, that, by attending to the state of the bowels, and by exhibiting purgatives, the disease became much less frequent. Dr. John Stewart employed spirits of turpentine, as an application to the umbilicus in the prevention of this affection, with great success.† Dr. Colles also recommends the use of the oil of turpentine, as a dressing, and states, that on the estate of a lady in Jamaica, since this practice has been adopted, together with plunging the infant into a cold bath daily for the first nine days, this disease, which had formerly carried off a very great proportion of the children of the negroes, is now scarcely to be met with. Dr. Hancock mentions, that in Demerara cold bathing is looked upon as a certain prophylactic, and he strongly recommends the exhibition of opium for the same purpose. In warm climates, where the greatest regard should be paid to cleanliness, the frequent use of a bath must, no doubt, be highly beneficial in promoting the due performance of all the important functions. I can, however, see no advantage to be gained by employing stimulating applications to the umbilicus, but I would rather resort to the mildest dressings, and, should there be any appearance of inflammatory action, to cold lotions or emollient applications.

In the treatment of this disease, the same principles should be acted upon as in the treatment of traumatic Tetanus. Having cleared out the alimentary canal with castor oil, or the hydrargyrum cum cretâ, small doses of laudanum may be given—one, two, or more drops, as may appear necessary, every hour, until rest is obtained, or the spasms are relaxed. Should any difficulty be experienced in the exhibition of remedies by the mouth, injections might be employed.

I have only met with one case of recovery, the particulars of which have been recorded. It occurred in the practice of Dr. Furlonge, at Antigua; and the following is an account of it, slightly abridged.

Case.—An infant, on the eighth day after birth, refused the breast, and, at the same time, a stiffness of the jaws was observed. The next morning a dose of calomel and jalap was given, and the infant was placed in a warm bath every three hours. On the following day, it was not only affected with trismus, but was universally tetanic, the characteristic expression of the countenance being well marked. Although the case was considered hopeless, Doctor Furlonge ordered one drop of laudenum every three hours, a warm bath four times in the course of the day, the jaws to be rubbed frequently with warm laudanum, the umbilicus to be dressed with turpentine and mercurial ointment, the nape of the neck and back to be blistered, and two grains of Dover's powder, with five grains of the sulphate of zinc, to be taken between each dose of the laudanum. Next day, the bowels had acted and the blister risen well, and it was remarked that the jaws were less tightly closed. The belly was now directed to be rubbed with warm laudanum, and a small quantity of camphor was added to the powders, and the blistered surface dressed with turpentine

* American Philosophical Transactions, vol. ii., p. 336.

† Dr. Chisholm's Essay on the Malignant Pestilential Fever.

and mercurial ointment. This treatment was continued for three weeks, during which time the child slept much, evidently from narcotic influence. It could not take the breast for a fortnight, but, in four weeks from the first invasion of Tetanus, it was free from every symptom of the disease, with the exception of considerable tenderness of the abdomen on pressure.*

Dr. Duncan, surgeon at Grenada, states, that he knew one instance of recovery simply by the use of the warm bath.†

With these cases I shall conclude a treatise, in the course of which it may be thought, that on some occasions the opinions of accredited and experienced authors have been opposed with a freedom ill-befitting a young writer; yet I would rather render myself liable to this reproach, than diminish the force with which those views, that are the result of conviction, are expressed, by an overweening and squeamish regard for authorities. I trust, however, that on no occasion have they been treated with disrespect. Some may think that the facts which justify my conclusions have not always been stated sufficiently in detail, and that the references are brief and scanty. Prolixity is a fault always more readily incurred than avoided; and, if these pages have not been swelled with the abundant materials on record, it must not be inferred that the data which they furnish have been the subject of less patient and attentive consideration. Imperfections and defects, I fear, are numerous, but they are, in some degree, unavoidable. Those who have more especially studied the diseases of the nervous system, best know the difficulties which beset the path of him who strives to improve our knowledge of their pathology and treatment. In no part of medicine is the field more open—in none have the investigators been more numerous, persevering, and skilful; yet no where has the progress of advancement been slower and less sure. The toil must still be shared by many laborers; and if, in the prosecution of a branch only of a class of the diseases of this intricate system, I have succeeded in dissipating some slight portion of the error and fallacy which invest it, or have aided in establishing any one principle of importance and utility, I shall have a sufficient apology for submitting the preceding observations to the notice of my professional brethren.

* Edinburgh Medical and Surgical Journal, 1830, p. 57.
† Ibid., vol. ii., p. 203.

Part Fourth.

MEDICAL INTELLIGENCE.

FOREIGN.

1.—*Abstract of Chossat's Researches on Inanition.* From the Annales des Sciences Nat., vol. 20, new series.

In a valuable paper presented to the Academie des Sciences by M. Charles Chossat, on *Inanition*, he gives some most important results, respecting various vital phenomena, which he has arrived at by a long series of careful experiments. In these experiments, animals of all classes were submitted to an entire privation of aliment, excepting, in some cases, water was given.

Mean duration of life.—This for warm-blooded animals was 9 days 16 hours; and for cold-blooded animals, 226 days.

Diurnal loss of weight.—Warm-blooded animals, 0.0420; cold-blooded animals, 0.0021.

Entire proportion of weight lost before death.—Warm-blooded animals, 0.397; cold-blooded animals, 0.404.

So that, in these two classes of animals, when deprived of food, the duration of life is in proportion of 1 : 23, while the diurnal loss of weight is as 20 : 1; whence it results, that the entire loss of weight is, in each case, nearly the same. In the cold-blooded animals, therefore, the degree of inanition attained before death is even somewhat greater than in the warm-blooded animals, the time only varying with variations in the energy of the nutritive functions of the classes.

A summary of the results obtained, shows that the loss of weight was divided among the constituents of the body, as exhibited in the following abstracts:—

1. Proportional loss:

Parts which lose more than the Mean, 0.400.

Fat	0.393	Liver	0.520
Blood	0.750	Heart	0.448
Spleen	0.714	Intestines	0.424
Pancreas	0.641	Muscles of locomotion,	0.423

Parts which lose less than the Mean, 0.400.

Stomach	0.397	Respiratory apparatus,	0.222
Œsophagus	0.342	Osseous system	0.167
Skin	0.333	Eyes	0.100
Kidneys	0.319	Nervous system	0.019

2. Entire absolute loss:

Blood	7.86
Muscular system (to wit: muscles of locomotion, 66.32; heart, 1.87; muscles of the digestive apparatus, 6.44) .	74.63
Different organs (to wit: abdominal glandular system, 7.46; pulmonary apparatus, 0.86; skin, 5.64; other parts, 1.91)	15.87
Osseous system	5.34
Fat	38.47
Entire absolute loss . . .	142.17

These tables show us that, though the proportional loss sustained by the muscular system is about the mean of that of the other tissues, more than one-half of the entire loss is sustained by that system; and that, if the power of locomotion has been bestowed on animals principally with a view to their alimentation, when the materials of alimentation can no longer be drawn from external sources, it is still by the locomotive system that they are principally supplied.

In treating of the effects of inanition on animal heat, M. Chossat presents some most remarkable and interesting results.

Diurnal oscillations of temperature.—It has generally been supposed, that, in individuals among warm-blooded animals, the temperature of the body was nearly constant. Accurate observation, however, has shown that they are liable to variations of temperature, depending upon various surrounding conditions. These variations, however, are transient and irregular. There is, however, a variation which takes place with great regularity, with diurnal intervals, and may be termed the *diurnal oscillation.* This periodical oscillation consists in a depression of temperature during the night, and a rise during the day; and careful observation shows that it has no relation to the changes in the temperature of the air, but is entirely independent, and sometimes even in opposition to atmospheric changes: neither is it at all dependent on seasons.

This oscillation of temperature being observed in the normal state of alimentation, it remained to inquire if it depended upon the circumstance, that it is during the day that the supply of nutrient materials are furnished to the system, which might be supposed to increase the temperature, by augmenting the functional activity of the system generally. To ascertain if the oscillation depended on this circumstance, M. Chossat extended his observations to animals in a state of inanition, in which case this influence could not operate. These experiments, so far from showing that the food taken was the cause of the oscillation, gave as a conclusive result, that *during inanition the diurnal oscillation is much increased;* so that, when the oscillation, in cases of normal alimentation, is equal to 1.33 deg. Fahrenheit, the oscillation during inanition was increased to 5.90 deg. Fahrenheit.

The diurnal oscillation thus proved to exist independently of all appreciable connection with surrounding physical influences, may perhaps be hereafter ascertained to have close relation to the condition of periodicity in disease.

In the gradual progress of inanition, this oscillation seems to be the element which determines the period of death; and the progress is as follows:

1. The animal temperature of mid-day diminishes regularly, but slowly.
2. The temperature of the body at mid-night likewise diminishes, much more rapidly.
3. It results, that the diurnal oscillation goes on increasing as the state of inanition becomes greater.
4. It results, that the oscillation finally arrives at a point at which the refrigeration becomes so great that the diurnal reaction cannot take place, when the animal dies.

The hour of the day at which death takes place from inanition seems to be determined likewise by this oscillation. Thus, in dividing the day into two equal periods of twelve hours—one from noon till midnight, the other from midnight to noon—the first would represent the period of decline of temperature, from the maximum at noon, to the minimum at midnight; the second would represent the period of increase, from the minimum at midnight, to the maximum at noon. The result of M. Chossat's observation is, that in the great majority of cases death took place between noon and midnight—or, in other words, the period of maximum mortality coincided with that in which the diurnal decline of temperature takes place.

2.—*Exhumation and removal of the Remains of Bichat.*—On the 16th of November (1845), at eight o'clock in the morning, the Commission of the Medical Congress of France, charged with the duty of exhuming, and transferring the remains of Xavier Bichat to the Eastern Cemetery, where the municipal Council had given a grant in perpetuity of a spot destined to receive these remains, proceeded, after obtaining the necessary authorization from the Minister of the Interior, the prefects of the Seine and of the Police, to the old cemetery of St. Catharine.

This Commission was headed by Roux, and several members of Bichat's family were present.

Dr. Devilliers conducted the members of the Commission to an enclosure formed of a wooden lattice, one metre in breadth and two in length, supported at one end by the eastern wall of the Cemetery of St. Catharine. There they found a sepulchral stone, placed in an upright position, on which was the following inscription:—"*A Xavier Bichat*, *par les Membres de la Société d'instruction Médicale*." A crown of *immortelles* hung upon the wall, and pots of flowers witnessed that the modest tomb had not ceased to be the object of a pious care.

The stone and the latticed fence were removed, and an excavation made to the depth of five feet and seven inches. There the remains of a coffin—a skeleton, was found in a fine state of preservation, the bones retaining their relative position. The head was wanting, and excavations to the extent of near half a yard was made in each direction, in search of it, but in vain. M. Roux then interposed, and stated that, by circumstances useless to be recalled on the present occasion, he had come into the possession of Bichat's head three years after his death. He then presented the head, pointing out the following peculiarities:—1st, a fracture in the occipital bone, which he himself had made in the autopsy of Bichat; 2d, the obliteration of the alveoli of the first left upper large molar, and of the first right upper molar, which Bichat had caused to be removed after having suffered much with them, as he states in his *Anatomie Generale*, article "*Dents*;" 3d, the complete agreement of the condyles with those of the atlas found with the skeleton; all circumstances which establish beyond a doubt that both the head and the skeleton are those of Bichat.

M. Malgaigne deposited the bones, as discovered, in an oaken coffin, in their anatomical order, and by M. Roux the head was restored, after a separation of forty years. The whole was then covered with bran, and enveloped in the shroud; then the cover, with a plate of lead, bearing the date of Bichat's death, and date of his exhumation by the Medical Congress of France, was firmly screwed down, and the coffin was conveyed in a hearse to the Metropolitan church of Notre Dame.—*Extract from the proces verbal d'exhumation des restes de Bichat*—*Gaz. des Hopitaux.*

After the funeral service, a procession of upwards of 2000 persons followed the remains to the cemetery of Pére la Chaise.

3.—*Academy of Sciences, session of November* 17*th*, 1845—*Elie de Beaumont presiding*. Brachet, of Lyons, communicated a memoir *on the Ganglionic Nervous System.*—The author commences by establishing that positive science ought to proceed by analysis, and not by synthesis, if it wishes to avoid the risk of bewildering itself in the regions of conjecture. Applying this principle to the subject of which he treats, M. Brachet retraces the history of the functions of the ganglionic nervous system; he reminds us that Winslow suspected the functions of the great sympathetic. Buffon also anticipated the discovery of the two lives. Bichat carried out this idea, and made it the basis of the great division of the vital phenomena into two classes—the acts of organic life, and the acts of the cerebral life. Legallois attacked this doctrine: his experiments led him to regard the medulla spinalis as the source whence the ganglionic nerves themselves draw their principle of action.

First in 1821, and afterwards in 1830, M. Brachet published his works upon the functions of the ganglionic system of nerves, works which were crowned by the Academy of Sciences. He revived the doctrine of Bichat, though with modifications. Thus, Bichat had two orders of vital functions—the functions of assimilation, and those of relation. Not having analyzed the acts of each function, he had not observed that many functions require, for their performance, the participation of both orders of nervous acts, the cerebral and the ganglionic. A nice analysis led M. Brachet to admit an order of mixed functions.

The author developes the idea, that in the lower animals all the functions result from the operation of the ganglionic system of nerves. These animals merely vegetate. They reproduce by cuttings. This reproductive power still persists, in a diminished degree, in the cold-blooded vertebrata, reptiles and fishes, because the brain is in them but little developed. It disappears, however, altogether in birds and mammifers: moreover, as sensation and intelligence, the peculiar attributes of the brain, increase, the reproductive nutritive powers diminish. As we ascend in the scale of animals, the preponderance of the brain increases; and as we descend, the vegetative life acquires the preponderance. There are *two different and inverse forces.*

The following is a summary of M. Brachet's conclusions:

1st. The ganglionic nervous system exists alone, or nearly so, in the lower animals.

2d. In proportion as the organization becomes more complicated, the encephalon receives accessions of successive portions, which have for function an influence over the new organs, the life of relation.

3d. The cerebral nervous system is added anteriorly to constitute the brain, and the ganglionic nervous system remains in the ventral region; as may be seen especially in the arachnides, in which the ventral or intestinal ganglion is so different from the cerebral ganglion.

4th. In the higher classes the functions of the two systems become more and more marked and distinct, as is shown by direct experiments and by teratological deviations, as well as by morbid phenomena.—*Gaz. des Hopitaux.*

4.—*Arsenic detected in bodies embalmed, by Gannal's process.* (An abstract of papers in the Journal de Chimie Medicale, December, p. 645, &c.)

At a period when the worst of crimes, poisoning, has invaded all classes of society, it has become a matter of great importance to examine the opinion entertained by some physicians, that arsenious acid was one of the constituents of the liquor used in Gannal's process for embalming.

If such be the case, the employment of this process would necessarily throw doubt over medico-legal investigation. The best citizens, in attempting to protect the remains of their friends from decay, might, by the employment of this process, if arsenic be one of the ingredients, render themselves liable to the imputation of poisoning, in consequence of the discovery of arsenic in the remains at any future time: on the other hand, the villain who would poison a relative or a friend, for a heritage or money, could easily be protected from the law, by using materials for embalming the body, with the pretence of affection, and, in case of investigation, all that would be necessary for him to do, in order to shield himself, would be to prove that there was arsenic employed in the process of embalming.

This matter is settled now beyond the possibility of a doubt, by the investigation lately instituted at Paris by M. Morin, Professor of Chemistry of the School of Medicine, and reported in the Journal de Chimie Medicale, December, 1845, p. 645, et seq.

"Some weeks since, Dr. Lecoupeur, concessionary of Gannal's patent, embalmed the body of a young woman, at the general hospital, in the presence of Dr. Melays, principal dissector. Two weeks after the injection of the body, by the carotids, we asked the prosector for a portion of muscle from this body, which he took, in our presence, from the lumbar region. The muscle afforded no sign of putridity."

On examination, it was found to yield a large quantity of arsenic. M. Morin remarks—

"Science furnishes means to detect the poison in any part of the system to which it may be carried, either by imbibition or absorption; but it affords no means of ascertaining whether the arsenic detected has been injected, or taken in by ingestion"

In confirmation of the above, we add the following abstract of a report addressed by MM. Avenel and Girardin to M. Salveton, attorney-general to the Royal Court at Rouen, and published in the Journal de Chimie Medicale for December, p. 648.

"On the 5th of April, by requisition of the mayor of Rouen, MM. Avenel et Couronné were directed to proceed to exhume the body of Louis Bruma (embalmed by Gannal's process, and buried at the Monumental Cemetery, December 27th, 1843), in order to ascertain its state of preservation. This was done in the presence of Dr. Lecoupeur, concessionary of Gannal's patent. The body was found to be in a remarkable state of preservation considering the unfavorable conditions of its place of burial.

"The burial clothes having been bloody, numerous worms had been produced, which had remained dead upon the skin without attacking it.

"This circumstance recalled to the recollection of Dr. Avenel a communication of Professor Morin, upon the existence of arsenic in the anatomical preparations preserved by Gannal's process."

Portions were taken from different parts of the body, and when submitted to chemical examination, yielded a considerable quantity of arsenic, and "confirmed the opinion of M. Morin, that the liquid used in Gannal's process contains a great quantity of some preparation of arsenic."

M. Lecoupeur, who was present at the examination, was much confused, and endeavored to convince the gentlemen that the presence of the arsenic was due to some error; but his explanations were deemed unsatisfactory. Their conclusion was, that

"Governments ought to interdict the employment of poisonous substances in processes for the preservation of bodies, as it may serve to conceal crime, and interfere with the course of justice."

5.—*Examination of Green Urine: by* H. BRACONNET.

About twenty years ago Dr. Castara, of Lunéville, sent me, for examination, some urine which held in suspension a deep blue matter, which was regarded as Prussian blue.

The examination that I made of this substance showed that it possessed properties so peculiar, that I determined to give it the name of Cyanourine. I ascertained that this blue substance was attended by an excess of alkali, and that, without having the property of saturating dilute acids, it dissolved readily in them, producing transparent, brownish solutions, which a greater excess of acid caused to change to a bright red.* I was persuaded that this substance

* Ann. de Chimie et de Physique, second series, vol. xxix., p. 252.

was not very rare in certain kinds of urine, where it should sometimes be found: it does not appear, however, that since I announced its discovery, any new researches have been made respecting it. I have recently received, also from M. Castara, some urine passed in the morning by one of his patients. This had a very different appearance from that of which I have just spoken. There was no sediment, and it was perfectly transparent, of a fine emerald green color. Its odor was that of recent urine, and, like it, it reddened blue tinsel paper. A slight excess of potassa deprived it of its green color, which did not reappear on the addition of dilute acid, when it offered only the normal yellow of urine.

Lime-water produced a white precipitate of phosphate of lime, while the supernatant fluid retained its fine color, which disappeared by the addition of an excess of lime-water.

A small quantity of ammonia gave with this urine a white precipitate, and the liquid retained its green color; but when the ammonia is in excess, the green color gives place to a citron yellow: the green color is not restored by the saturation of the alkali by an acid.

The same result is obtained by allowing the green urine under consideration to stand by itself for several days; it changes gradually, from below upwards, to a citron color, producing a slight white sediment, barely tinged with blue. Sulphuric, chloro-hydric, nitric, phosphoric and oxalic acid, diluted, change the fine green to a dirty green color, which, after a few days, becomes red.

A solution of alum produces no change in the green color in this urine at first, even by adding a little ammonia to precipitate a portion of the alum, which appears white, the liquor remaining green; but at the end of twenty-four hours it assumes a reddish color, due to the reaction of the alum. This urine concentrated by evaporation at a gentle heat, loses, in a great measure, its green color, and leaves a residue, which becomes brownish red, in consequence of the concentration of the free acid of the urine, and its reaction on the coloring matter. This brownish-red residue, treated with a little magnesia, deposits a light bluish sediment.

Although the coloring matter peculiar to this urine is in small quantity, I conclude that it is identical with that which I, long since, made known under the name of cyanourine, since, like that, it becomes blue with alkalies and red with acid. I ought, however, to state, that urine, in its natural condition, contains a substance but little known, which has likewise the property of becoming red with dilute acids after a contact of greater or less duration. It is almost useless to state, that the green color of the urine just examined is due to a mixture of the yellow of the urine and the blue of the cyanourine.

This urine, moreover, yielded the principal elements of normal urine, such as urea and uric acid, whilst this latter did not exist in sensible quanties in the blue urine that I examined twenty years ago.

—*Journal de Chimie Medicale, Nov.* 1845, p. 569.

6.—*Epoch of the elimination of certain Metallic Substances introduced into the Animal Economy.*—MM. Milon and Laveran, in a note addressed to the Institute, have given the results of their numerous observations on cases in which their patients have taken tartar emetic, once, or at most twice, in the common dose of 1½ grains, but in some cases as high as 4½ grains. They observed—1st, that the antimony was always found in the urine; 2d, that the elimination of the metal was slow in many cases: they were thus led to watch its passage in the urine, not only many days after its ingestion, but even many days after it had ceased to be found in the urine; 3d, that they have seen the antimony reappear in the urine, *follow a true intermittence* in its elimination, and remain in the economy for periods much beyond what we could have imagined.

The fact of the intermittence fixed our entire attention. We are not without hope of showing relations between this peculiar progress of elimination of a metal, as shown by chemical analysis, and the parallel intermittent course of many phenomena, which are common in pathology, and yet very obscure.

—*Journal de Chimie Medicale, Dec.* 1845, p. 656.

7.—*On the Atomic Theory and Electric Conduction.* By G. P. T. HILL, Esq., Filey.—Having lately had the pleasure of perusing Liebig on "Organic Chemistry," as published in your journal, my attention was forcibly drawn to some remarks of Dr. Faraday, in which, assuming the truth of the atomic theory, he would represent the space between the ultimate atoms as the true medium of electric conduction.

The following objections to this hypothesis I should be obliged by your publication in *The Lancet*, if you consider them worthy a space in so valuable a journal:—

I. Water is a conductor of electricity, but if a compound salt, or a portion of sulphuric acid, &c., be added to it, its conducting power is materially improved, though at the same time its density is also increased. If space be essential to conduction, we ought to expect a corresponding diminution of this power; but the reverse happening, it is clear that this faculty does not depend on the existence of space between the atoms of a body.

II. A rod of metal is a conductor, a similar rod of glass a non-conductor. But space is in both these substances continuous throughout the mass, therefore in the one case it exhibits properties entirely different from the other. Neither can its conducting or non-conducting power be ascribed, in the one case, to its union with a metal, or in the other, to its union with glass, for it is inconceivable that there can exist any difference in the space between the atoms of glass and that between those of metal. The conduction by bodies is therefore a peculiar force solely resident in themselves, which force is not possessed by all.

III. Some bodies become conductors when heated. This cannot be attributed to the increase of space between their atoms, but may be easily accounted for by considering the close connection that subsists between the particles of heat and those of electricity, and the one may thus be said to render a substance susceptible of the other.

IV. Potassium is a conductor. Its oxide is a non-conductor. The latter has also a greater specific gravity than the former. This affords no argument for the necessity of space for conduction, for we may surely ascribe its non-conducting power to the change of properties induced by the union of the metal with oxygen. This view is, of course, applicable to the compounds of sodium, &c., with oxygen.—*London Lancet.*

8.—*On the Question whether Alkaline Carbonates exist in the Blood.* By Baron LIEBIG.

The food of the carnivora contains only alkaline phosphates; that no alkaline carbonates, therefore, can be found in their blood, scarcely requires any special proof. But the case is altogether different with respect to the graminivora, since their food contains a number of alkaline compounds with vegetable acids, which pass into their circulation, and are subsequently separated from the blood through the kidneys, and appear finally in their urine as alkaline carbonates. Now, if these alkaline carbonates exist as constituents of the blood of this class of animals, and if the elimination of carbon in the respiratory process is to be referred to their agency, it is obvious that we must be able to detect and to demonstrate their presence in the blood.

The following experiment was made, to determine the question whether alkaline carbonates are present in the blood of graminivorous animals or not, and it must, I think, be deemed conclusive.

A mixture of between four and five pounds of the blood of an ox was boiled with twice its volume of water, and the coagulated mass formed subjected to powerful pressure. The fluid obtained was about the weight of the blood employed in the process; it had a powerfully alkaline reaction. Now, it is evident, that this fluid must necessarily contain in solution any alkaline carbonates which existed in the blood.

On evaporation in a retort, (consequently not exposed to contact with the atmosphere), until it occupied about forty cubic centimetres, it became a thick, greenish-brown liquid, of the consistence of syrup, and had still a strong alkaline reaction. I took half this concentrated fluid, and placed it in a graduated tube, in contact with carbonic acid gas, and allowed the mixture to stand for twenty-four hours, when I found the fluid had absorbed three times its own volume of the carbonic acid. Now, it is evident, that if the capacity of this fluid for absorbing three times its own volume of carbonic acid gas depended on the presence of carbonate of soda, and consequently, the formation of bicarbonate, by the addition of the acid, the other half of the liquid obtained from the blood must evolve carbonic acid gas when acted on by a stronger acid, to the extent of about two-thirds that of the gas absorbed by the first half.

But this liquid, when brought into contact with hydro-chloric acid in a bell jar over mercury, mixes with the acid, without evolving the slighest perceptible trace of carbonic acid.

This experiment confirms the conclusions of Enderlin's analysis of the ashes of the blood of the graminivora—namely, that this blood contains no perceptible amount of alkaline carbonates. A subsequent and more minute examination has shown that the alkaline reaction of the fluid, obtained from the blood in the manner described, is owing to the presence of phosphate of soda. The residue was examined for urea and for sugar, but we failed to detect the presence of either of those substances.—*London Lancet.*

9.—*Observations on some points in the Chemistry of the Urine.* By J. W. Griffith, M. D.

I. It has usually been considered that the amorphous deposit ordinarily occurring in the urine is the lithate of ammonia, mixed with variable portions of urate of soda and lime, and sometimes magnesia. The cause of the deposit assuming the amorphous form has always been, and still remains, in our opinion, a difficulty, and we think receives no explanation from the mere fact of the lithate of ammonia being mixed with lithate of soda and lime. If so, why does it ever assume the crystalline form when mixed with the soda?—and the spheres of lithate not uncommonly occurring in urine are decided crystals, and possess the optical characters of such. We think it a question whether the crystalline salt obtained by M. Heintz, and containing from 12·73 to 14·79 per cent. of soda, is the same as the amorphous urinary compound, the inorganic constituents of which are exceedingly variable in quantity and amount (2·98—8·02 per cent.)

II. We differ from those who, with Liebig, consider that soda has anything to do with the solubility of uric acid in urine. The fact of urate of ammonia being deposited from the urine, either by exposure to cold or the air-pump, is sufficient to show that it exists therein in combination with ammonia, and not soda; and how can this occur from a mixture of phosphate of soda, uric acid, and hippuric acid (Liebig's artificial urine?) In our opinion, uric acid most probably exists in the blood in combination with soda; and although, under ordinary circumstances, as in the case with urea, we cannot detect it, (being probably excreted as rapidly as it is formed), still, in certain morbid conditions, where it is apparently formed in excess, it is secreted from the blood in this form, as we see in and around the joints of gouty patients: here the compound is not exposed to the salts capable of decomposing it, as in the urine. At the moment of its secretion from the kidney, it is probably decomposed by muriate of ammonia. If a deficiency of this is formed, a quantity of the urate of soda will escape decomposition, and appear in the urine. Whether tribasic phosphate pre-exists in the urine is very problematical, as we know of no process by which either the soda or its carbonate, or such compounds as yield this on incineration, can be removed from the alkaline phosphate prior to the combustion of the ash; and supposing with Loowig, that the phosphate is the ordinary one, the atom of water it contains could be replaced by an atom of soda, as long since shown by Graham.

III. Regarding the presence of ammonia in urine, the chloride of platinum test applied previously to evaporation (and, of course, it cannot be applied after) is entirely fallacious. Admitting that the lithic acid exists as bi-lithate of ammonia, 1000 grains of urine contain, on an average, one grain of lithic acid: this would combine with 0·1892 grains of ammonia, which would form 2·341 grains of ammonio-chloride of platinum. Since one part of ammonio-chloride of platinum dissolves in 150 parts of cold water, 1000 grains of urine (admitting urine to be as good a solvent as water) might contain more than twice as much ammonia, and yet the chloride of platinum would yield no precipitate.

—*Chemical Gazette.*

AMERICAN MEDICAL INTELLIGENCE.

A QUESTION OF ORIGINALITY SETTLED.

To the Editors of the New Orleans Medical Journal:

[Under this title we have received the following communication from our esteemed fellow citizen, Dr. Dowler. We cheerfully comply with his request to give it a place in our Journal, and have only to regret that it was not received in time to be inserted in the *first part* of this number. As none of Dr. Dowler's curious and novel researches on "*febrile caloricity*" have been published in this Journal, his references may not be accessible to many of our readers; but as an attempt has been made to deprive him of whatever credit may be due for their originality, we willingly allow him the use of our pages for the purpose of asserting his claim, which we think has been fairly established in this communication. In scientific discoveries, as in every thing else, our countrymen *know their rights*, and *dare maintain them* against all rivals. From our personal acquaintance with Dr. Dowler, we are satisfied that his observations have been made with the utmost care and exactness; and whether they lead to any useful results or not, we think him at least entitled to the merit of originality.—Eds.]

It is confidently believed, that there is not a man of common sense and right feeling, in or out of the profession, who will take the trouble to inform himself, which he may do in five minutes by reading this communication, but must conclude unhesitatingly, that my reviewer in the Northern Journal of Medicine, at Edinburgh, and the editor of The Half-Yearly Abstract of the Medical Sciences, at London, have fallen into a most gross error in claiming, gratuitously, for Dr. John Davy, of England, *priority of discovery* upon the subjects treated of in my numerous papers concerning Febrile Caloricity, both before and after death,—an error which, even were it unintentional, is not, for that reason, the less injurious to me, and must be unpleasing even to the learned and high-minded gentleman himself, in whose special favor it has been committed. This author's able work, (*Researches Physiological and Anatomical*—2 vols., London, 1839,) now before me, I never saw, until March 28th, 1846. It enables me to say (since it is the work referred to by the editors), that there is not the slightest foundation for the double statements under consideration, made in November, in Edinburgh, and soon after in London. There is not a single fact or expression directly or remotely authorizing such statements. Dr. Davy's experiments and results are dissimilar, and his speculations and language are directly opposed to mine.

The Edinburgh reviewer begins, and ends, with a claim for Dr. Davy,—otherwise, his remarks and extended quotations are not only just but extremely liberal. I regret the poor return for his courtesies that I may seem to make, in asserting my rights, against his errors.

The writer, who wields an able pen, after some introductory remarks, says: "As far as we remember—for our time, at present, does not permit us to search for authorities—*Dr. John Davy was the first to publish cases which seemed to point* to the necessity for a more diligent investigation of this subject. Dr. Davy's paper is contained in the third volume of the Edinburgh Medico-Chirurgical Transactions.* The facts stated in that paper are sufficiently remarkable, namely, that in a number of instances he found the temperature of the body, after death, to be considerably beyond the temperature admitted to belong in general to the most active stage of febrile disease. One case, in particular, he mentions, in which, some time after death, the temperature was as high as 113 deg. Fahrenheit.

"Dr. Davy's paper was published before the Asiatic cholera reached Europe. During the prevalence of that epidemic, it was a matter of common observation, that the coldness and shrunk state of the surface, characteristic of the disease, gave way after death, and that the warmth and plumpness of the surface returned.

"We have no intention of speculating on these statements [Dr. Dowler's], *made, as we see, by Davy first, and now extended* by Dowler. Dr. Davy does not mention where his observations were made†; it was plainly, however, in a warm climate, like those of Dr. Dowler. The subject is very worthy of investigation in our more temperate climates; nor, till observations in these shall have been made in sufficient numbers, can we pronounce the subject ripe for speculation. Dr. Dowler's first paper contains many valuable suggestions on the precautions to be used, and the fallacies to be avoided, in making observations on the temperature of the body, both before and after death."

The learned reviewer says, "It has long been known to practical men, that a much longer time than usual sometimes intervenes before the dead body acquires the temperature of the surrounding air; and it is not an unfrequent remark of those who have been much engaged in post-mortem examinations, that traces of the living temperature occasionally are discovered, even as long as twenty-four hours after death. It sometimes happens, that even the relations of the deceased take notice of the unusually long retention of warmth in the body, and send for the medical attendant some hours after the event, to ascertain if

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* My reviewer will find this paper, so "*romantically*" *misquoted*, in the first volume of Dr. Davy's Researches, p. 228.

† This is an error; see the sequel—Professor LEE's letter.

resuscitation be not possible." Has not the writer, also, heard the story of Columbus and the egg?*

"The Abstract" finds my *results strange*—the Northern journal, EXTRAORDINARY, as the following quotation will show:—"The papers before us, on FEBRILE CALORICITY, BEFORE AND AFTER DEATH, for the use of which we are indebted to the distinguished professor of Materia Medica in the University, bear so many marks of sterling honesty of purpose, notwithstanding some occasional flights of romance, that we cannot help putting considerable faith in the author's statements, *extraordinary as they are*." Who calls that "*strange*," or "*extraordinary*," which has been well known "before the Asiatic cholera"?

As a specimen of the *Logica Edimburgensis*, the following sentence is "*strange*," nay, "*extraordinary*." In speaking of certain long-known facts concerning post-mortem heat, it is said—"They appear to have hardly *suggested even a passing idea of the possibility* of this temperature of the dead body being, not the lingering remains of the living temperature, but a new development of heat, independent of life." That which is now proved, but which *heretofore* had not been even *suggested in idea as a possibility*, must be a discovery. Gunpowder and printing—steam power and magnetic telegraphs, did not come down quite so suddenly as this, without some *avants couriers, or forerunners*, at least, in thought. Did not the writer forget, on this occasion, Dr. Davy's claim, of nearly twenty years' standing—spread upon the published records of the learned societies, and republished in his works in London—works extensively reviewed and quoted in both hemispheres? Yet "*these strange*, these *extraordinary results of Dr. Dowler*," were "*first discovered*"—it is Dr. RANKING who speaks—"*by Dr. Davy;*" "*made*"—and it is now Dr. SELLER who speaks—"*made, as we see, by Davy first*, and *now extended* by Dowler." Now, as my claims to priority were directly set forth in the papers quoted, I fearlessly challenge the author of the History of Literature, and the author of the Curiosities of Literature, to furnish a parallel instance of—I will not say, mendacity, but error.

Our British brethren are not very indulgent towards us. The Edinburgh Review asks, "Who reads an American book?—What does the world owe to American physicians and surgeons?" They blame us because we are deficient in *originality*, and when we have the good luck to stike on a vein a little novel, they straightway claim it for themselves. Dr. Franklin's great discovery of the identity of lightning and electricity, far from pleasing them, only excited their ridicule: the members of the Royal Society refused to allow his papers, on the subject, to appear among their Transactions. When kings approved—when philosophers on the Continent praised—when Professor Richmann, of St. Petersburg, in following Franklin's experiment, drew down upon his martyred head the lightning of Heaven, killing him instantly, the Royal

* Several times in this city I have been called on with this view, the day following the death, and, after, the physicians had given the usual certificates. I recall to mind a case, nearly twenty-four hours after a French physician had written the cause of the death as *l'Apoplexie foudroyante*—a strong expression, but, in some cases, almost literally true.

Society could hold out no more,—it voted a gold medal to Benjamin Franklin, to whom Turgot, so happily, applied the sublime words—

"*Eripuit cœlo fulmen, sceptrumque tyrannis.*"
(He snatched the thunder from heaven, and the sceptre from tyrants.)

Where is the American who would not be pleased, nay grateful, to see his works republished, with or without annotations, in Great Britain? Did not one of the most eminent authors and medical reviewers that ever flourished in England, whose work has been circulated the whole length and breadth of our land, hold the following language towards us, no longer ago than October last, in his Journal, just before he descended to the tomb, regretted not only at home, but,

"By strangers honored, and by strangers mourned"?

"It is a very smart thing," he remarks, "to appropriate *another man's labor*. We really wish these gentlemen [Americans] would leave us alone; we can readily spare their *fraudulent courtesies*; the only thing we fear is to be *appropriated or absorbed*;" &c. Hence, it is evident, that we love the proud Islanders, if "not wisely," at least "too well," since we love them more than they are willing—which is a great misfortune to the rejected lover. But their great poet, long ago, discovered that the course of true love never yet did run smoothly. It may be, that the sun never ceases to shine upon the British territories; the crown of Britain may glitter more than any other; the nobility may excel in the blazonry of armorial bearings, and in the dazzling splendors of heraldric designs; but intellectual ability is not the peculiar property of any nation whatever. Nature's book is open to all, and, in the sciences, at least, all are Republicans, and ever will be, until God himself shall see fit to make "a royal road to knowledge," for the exclusive benefit of the few who imagine themselves superior to, and wiser than, other people.

I must repeat, once more, that Dr. Davy, in the work alluded to by my reviewers, has not made any experiments essentially similar to mine. I decline making quotations, in evidence. Dr. Lee, in the subjoined letter, has completely exhausted the subject. The only two cases (not *one case*, as asserted in the Edinburgh Journal) of temperature after death, which Dr. Davy himself regarded as important, were not made in such a manner as to be of value. The order of his experiments, their duration, the comparison of different regions at progressive periods, illustrative of the increase or decrease of temperature, though elements of the most *essential* kind, do not enter into his calculations at all. In no case did he ascertain the temperature *before death*, nor the regional *increments and decrements after*, so that his facts neither prove nor disprove his or my conclusions.

Some American journals have admitted—none have questioned my claims to originality. Some of the most learned physicians of the age have, without solicitation, sent me their declarations to the same effect, with permission to use their names in the journals or papers, should I think it expedient. The following extract from a letter written by Professor Charles A. Lee, a well-known writer, and the learned editor of the New York Journal of Medicine, is alone sufficient to settle this question forever:—

"NEW YORK, MARCH 14TH, 1846.

"*To Dr. Bennet Dowler:*

"My dear Sir: As to your claims to *originality* in researches relating to post-mortem caloricity, the facts, I take it, are these: In 1828, John Davy made a series of experiments at the British Military Hospital at Malta, in twenty different subjects, of which he has given a detailed account in his "Researches," (London, 1839). 1*st Case*.—Examined three hours after death: temperature of the air, 86 deg.; temperature under the left ventricle of the heart, 113 deg., under the liver, 112 deg.; (no other observations made). *Case* 2.—Examined six hours after death: temperature of the air, 86 deg.; of the left ventricle, 108 deg.; under lobulus Spigelii, 107 deg.: fifteen minutes after, temperature of the centre of the right lung, 105 deg.; (no other observations). *Case* 3.—Examined four and a half hours after death: temperature of the room, 80 deg.; under the heart, 97 deg.; under the liver, 101 deg. *Case* 4.—Disease, *dysentery.* Examined four and a half hours after death: temperature of the air, 82 deg.; temperature under the liver, 103 deg. 5 min.; under left ventricle, 103 deg.; (no other observations). *Case* 5.—Examined fourteen hours after death: temperature of the air, 69 deg.; under lob. Spigelii, 88 deg.; (no other observations). *Case* 6.—Examined twelve hours after death: temperature of the air, 68 deg.; temperature under the liver, 94 deg.; under the heart, 93 deg.; (no other observations). *Case* 7.—Examined three hours after death: temperature of the air, 66 deg.; temperature under lob. Spigelii, 96 deg.; in the substance of the right lobe, 98 deg.: five minutes after, in one of the ventricles of the brain, temperature 90 deg.; (no other observations). *Case* 8.—Examined two hours after death: temperature, 64 deg.; temperature under the liver, 86 deg.; under right ventricle, 88 deg.; (no other observations). *Case* 9.—Examined five and a half hours after death: temperature of the air, from 57 to 64 deg.; temperature of the centre of the liver, 96 deg.; brain, 82 deg.; (no other observations). *Case* 10.—Examined seventeen and a half hours after death: temperature of the room, 58 to 60 deg.; temperature of the centre of the cerebrum, 70 deg.: half an hour after, under the right ventricle of the heart, 82 deg.

"These are the first ten cases reported by Dr. Davy. He maintains that the extraordinary temperature was generated *before death*, and 'probably in the same way as the ordinary degree of animal heat experienced in health, or the extraordinary degree witnessed in febrile diseases. *A priori*, the effect of the heat-generating process, whatever it may be, can hardly be limited. In many birds, it raises the temperature of the body to 110 deg., when in perfect health, and in man to 101 deg., at least in the tropics, without deranging the health; and it is easy to conceive, that by increased activity or energy, it may exalt the temperature to the common febrile height, or to a height greatly exceeding that. *But, destitute of life, there does not appear to be in the body any source of heat, any power of generating it, that we are aware of.* Putrefaction had not taken place in these bodies: I believe I may say, it had hardly obscurely commenced. Even if it had, and had made progress, and were it even at its greatest height of activity, it is doubtful if it would be equal to the effect in question.'* (*Researches*, vol. i., p. 238.) No experiments were made by Dr. Davy, to ascertain the temperature in these cases *before* death.

"In 1838, Dr. Davy made observations in ten other cases at Chatham (England), on the bodies of soldiers: In these he extended his experiments to the brain, heart, liver, thigh, foot, spinal canal, abdomen, &c. In the first case, the observations were made 28 hours after death; in the second, 16 hours after;

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* Animal caloric, so far from accelerating putrefaction, retards it. If the atmospheric heat be favorable, that is, sufficiently elevated, decomposition begins soon after the dissipation of the animal heat, at which time the body and the surrounding media nearly coincide in temperature.—B. DOWLER.

in the third, 18 hours; in the fourth, 29 hours; in the fifth, 12 hours; in the sixth, 14 hours; in the seventh, 5 hours; in the eighth, $4\frac{1}{2}$ hours; in the ninth, 18 hours; in the tenth, 16 hours after death. Temperature of the air from 13 deg. below zero, to 72 deg. above. *In no case was the temperature noticed above 97 deg.* The *only inference* Davy draws from them is, that they may aid us in determining questions of medical jurisprudence, as, *e. g.*, how long a person has been dead; though he thinks that 'much judgment and nice discrimination may be requisite on the part of the medical man.' I should judge so, since there is no uniformity in the results of his observations. You will see from the above, that *you are entitled to the sole credit of proving*—1st, *that greater heat often exists in external than in internal regions;* 2d, *that the temperature increases in many regions for several hours after death;* 3d, *that the temperature rises and falls in different parts of the body, while the centre cools, contrary to the laws of refrigeration in dead or inert matter;* 4th, *that these results are not owing to the play of chemical affinities, or animal decomposition, which always commences in the internal parts of the body.* That, in the above, and perhaps other particulars, *you have made important discoveries*, I have no doubt whatever, and believe the *credit of originality* will eventually be awarded you, not only by your countrymen, but by the *whole medical world.* That no such results were previously *known, or even surmised*, I may refer to a review of Davy's 'Physiological Researches,' in the British and Foreign Medical Review for July, 1841, in which the observations of Davy are quoted, and his explanations adopted.

"I remain, your friend and obedient servant, "CHARLES A. LEE."

A distinguished author, teacher, and the president of a learned society, in a letter during the last year, holds the following language:—"It may gratify you to know, that it is the intention of my friend and colleague, Professor ****, to notice your labors prominently in the next edition of his Physiology; and that, in a recent letter to Dr. ***, the distinguished [European] physiologist, he made most respectful mention of your observations. So, you see, my dear sir, that you are likely to become extensively and advantageously known, as you ought to be, for your well-conceived experiments."

Another well known author of sundry medical books of great value, expresses similar opinions. A learned stranger, a professor and author, before my latter papers on Caloricity had been published, wrote—"I hope you will go on with your most interesting investigations into every department of CALORICITY, whether febrile or non-febrile, post-mortem or ante-mortem. It is a field in which you have already gathered unfading laurels, and which, I trust, is destined to be cultivated by you with still more brilliant success."

The charge of vanity cannot apply to the making public such testimony, from such disinterested quarters. It is the strongest evidence, and made by strangers of the highest character, and greatest knowledge of the subject upon which they testify, and from the purest motive, that of "rendering unto Cæsar the things that are Cæsar's."

One of the old Roman philosophers justified the speaking in his own praise, by saying—"*Every freeman had a right to speak what he thought of himself, as well as of others:*" whereupon Dr. Franklin remarks, that "most people dislike vanity in others, whatever share they may have of it themselves; but I give it fair quarter wherever I meet with it, being persuaded, that it is often productive of good to the possessor, and to others who are within his sphere of action; and therefore, in many cases, it would not be altogether absurd, if a man were to thank God for his vanity among the other comforts of life." I do not, however, avail myself of these authorities on the present occasion. This is not a question of vanity, or humility, but of history—of originality. Concede this latter; blot out the expressions of eulogy, as meaning no more than the words in a challenge ending with—"*your most obedient and humble servant*," and I shall be content. I waive, wholly, the nature or quality of these researches. I do not say that my correspondents and reviewers (whose good opinions I will ever remember with gratitude) have, or have not, seized upon the most salient and useful points; but I do say, that the attempt to deprive me of whatever credit may be attached to them, be it more or less, is as unwarrantable as it is unfounded. Who will blame me for saying thus much? Does any one desire to be held up to the world as a literary thief? I am not the originator of this controversy. Is not Dr. Ranking's able *Abstract* in the hands of every medical man in our country?—a book which proclaims "that Dr. Davy was the *first to notice*, that the temperature of the human body, after death, instead of gradually falling, actually rises, and gains even a considerably higher amount than it had in the highest state of fever;" (p. 238)—a doctrine which this author vehemently denies! This is a good example of what Malvolio, in The Twelfth Night, says on greatness:

> "Be not afraid of greatness:
> Some are born great—
> Some achieve greatness—
> *And some have greatness thrust upon them.*"

Dr. Ranking asserts, that my "observations on the temperature of the body were made *immediately* before and after death." Here are two material errors, which are plainly contradicted by the very cases quoted in the Abstract. The first case gives 104 degrees as the maximum temperature during the malady, and 113 as the highest, from ten minutes to three hours after death: in the second case, the observations commenced at one hour and ten minutes after death, and continued to the end of the third hour, and did not close until the twenty-third hour: in the fourth case, the temperature is given at two days, and, also, at one day, before death: so that the whole six cases quoted in the Abstract directly contradict its editor. The truth is, that among more than two hundred cases of temperature, not one is restricted, so far as I recollect, to the periods "*immediately before* and *immediately after death;*" for the principal object was, as far as circumstances permitted, to cover the whole ground, from the invasion of the malady (chiefly yellow fever), to convalescence or death, and from death to decomposition.

My papers, so far as yet published, after a temporary neglect, were fully reviewed, criticised, and pronounced upon by the American medical press, as the following references will show:—

Bibliography.—For papers, reviews, &c., *Western Journal of Medicine and Surgery*, June and October, 1844, Louisville; Dr. Bell's *Bulletin of Medical Science*, for August, September and October, 1845, Philadelphia; *Medical Examiner*, for June, August, September and October, 1845, Philadelphia; *New Orleans Medical Journal*, July, 1845; *Boston Medical and Surgical Journal*, for June and August, 1845; *New York Journal of Medicine*, September, 1845, and January, 1846;

Northern Journal of Medicine, Edinburgh, for November, 1845; *Half-Yearly Abstract of the Medical Sciences*, for December, 1845, London.

Accept, Messrs. Editors, the esteem of your obliged and humble servant, BENNET DOWLER.

New Orleans, April 4, 1846.

2.—LITHOTRIPSY.—We find, in the February number of the *Southern Medical and Surgical Journal*, an interesting paper, entitled, "An Account of the Operations of Lithotrity and Lithotripsy, with a successful Case. By PAUL F. EVE, M.D., Professor of Surgery in the Medical College of Georgia."

Dr. Eve gives a brief account of the origin of these operations, a description of the instruments used by MM. Civiale, Le Roy, Heurteloup, and Jacobson, and a historical notice of all the operations for crushing stone in the bladder, that have been performed in our own country. He then reports a case operated on by himself in November last, which he says is "the first case of the kind in which it is believed this method has been successfully resorted to in the South-west."

The report is entitled—"CASE.—*Calculus measuring one inch and an eighth in diameter, and composed of phosphate of lime—destroyed in eighteen sittings by Heurteloup's instrument, and causing neither pain, nor the loss of one drop of blood.*"

The subject was a man aged 34 years, who went to Professor Eve to have the operation of lithotomy performed. After a careful examination, Professor E. advised *lithotripsy*; which was performed on the 17th November last, in the presence of the medical class. The patient was seated on the edge of a table, and the stone being seized, was crushed with a noise audible to many in the room. The patient immediately voided some debris, and one or two fragments the size of buck-shot. "The operation did not occupy five minutes." The patient then "walked home to the Infirmary, took a warm hip-bath, and passed a comfortable night." The next day he passed other portions of the stone—took diluents, moderate diet, and the warm hip-bath twice. On the 19th, not so well; "feels some soreness along the urethra; has some fullness about the head, and pulse 84." The baths, and a little magnesia, relieved him, and he went before the class again in the evening. Some mucilage was injected into the bladder, and two fragments were crushed by the instrument. From this to the 5th of December, "the instrument was employed for a few minutes nearly every day." "On the 5th, I made a careful and minute exploration of the bladder by all the ordinary processes, without finding a particle remaining, the patient insisting he was entirely free of all symptoms."

Professor Eve thus closes:—

"It would be wrong to produce the impression that lithotripsy is a very simple or easy operation, or that it can be adapted to all cases of stone. It certainly requires great care and prudence, a good condition of the urethra and bladder, some tact in manipulation, and well-regulated perseverance. But during the whole treatment of this case, the patient was up and about, even in the streets; he never complained once of the operation; the only uneasiness he experienced was from fragments lodging in the urethra or neck of the bladder; he was never confined one moment to bed, and never passed a single drop of blood.

"Mr. Layton writes from Blakely, Early county, and dated 17th of December—'I got home the ninth day after I left Augusta, and had to travel through very cold and rainy weather. I feel no symptoms of stone since I left you, and am in hopes I never shall.' *

"Having thus been exposed, under trying circumstances, and without any return of the old affection, I think the case may with safety be pronounced cured."

REMARKS.—We regret not being able, from want of space, to give the entire report of this interesting case.—EDS.

3.—*Case of Ligature of the Left Subclavian Artery, within the Scalenus Muscle, for Aneurism.* By J. KEARNY RODGERS, M.D., Surgeon to the New York Hospital, &c.

In the March number of the New York Journal of Medicine, we find a minute detail of this difficult and dangerous operation. The subject was a man aged 42 years, who was admitted into the New York hospital September 13th, 1845, with aneurism of the left subclavian artery, of about four weeks standing. A judicious course of treatment was tried by Dr. Rodgers until the 14th of October, when he decided on applying the ligature, "and the operation was performed in the theatre of the hospital at 1 P.M. of that day, in the presence of Drs. Mott and Stevens, consulting surgeons, of Drs. Cheesman, Post, Hoffman, Buck, and Watson, surgeons, and an assemblage of about three hundred physicians and students."

"The patient was laid on a low bed, with his head and shoulders raised, and his face turned to the right side. The light from the dome shone directly on the part to be operated on.

"An incision was made, three inches and a half in length, on the inner edge of the mastoid, terminating at the sternum, and dividing the integuments and platysma myoides. This was met by another, extending along the sternal extremity of the clavicle, about two and a half inches. This last incision divided a plexus of varicose veins passing in the integuments, covering the clavicle to the subclavian. Free bleeding taking place from their cut and patulous extremities, it became necessary to check it by ligature.

"The flap of integuments and platysma myoides was now dissected up, and the lower end of the mastoid laid bare; a director was passed under this muscle, and the sternal portion and half of the clavicular divided by the bistoury. This muscle was now turned up, and the sterno-hyoideus muscle, the omohyoideus, and the deep-seated jugular vein were seen covered by the fascia.

"On turning up the mastoid, a portion of the aneurismal sac, strongly pulsating, was brought into view, overlapping about half the width of the scalenus, forming now the outer part of the track through which I was to pass, showing fearfully one of the dangers of the operation, which, from my previous examinations of the part, I had, of course, anticipated.

"The fascia being divided by the handle of the scalpel and the fingers, I passed in contact with the deep jugular on its outer side to the inner edge of the scalenus anticus, intending, for the purpose of avoiding as much as possible all danger to the thoracic duct, to reach this muscle fully half an inch above the rib, rather than at its insertion. I now felt distinctly the phrenic nerve running down on the anterior surface of the scalenus, and was confident that

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" * This report is confirmed by another letter, dated 26th of December, and written by Dr. T. M. Standifer, a practitioner of Medicine, of Blakely, who visited Mr. L. at my request."

I should be able to avoid any injury to it. Having attained the inner edge of the scalenus, by pressing downwards with the finger, I soon discovered the rib, and after some little search, easily found the subclavian artery. By pressing it against the rib, all pulsation ceased in the tumour, and by removing the finger, pulsation returned.

"I now felt that great care was necessary to detach the artery, and avoid danger to the pleura and thoracic duct. In accomplishing this part of the operation, I at first tried Sir Philip Crampton's instrument, but ascertaining that I could better carry the ligature around the artery, and bring up its end, by the invention of Drs. Parrish, Hewson, and Hartshorne, of Philadelphia (long since given to the profession by them, and lately claimed by Mr. L'Estrange of Dublin), I accordingly adopted that instrument.

"This part of the operation, it will be imagined, was not very readily accomplished. The great depth of the vessel (nearly the length of my fore-finger), and narrowness of the wound, prevented a very easy management of instruments. The point was introduced under the artery, and soon directed upwards so as to avoid injury to the pleura. The needle carrying the ligature was now detached from the shaft of the instrument, and drawn upwards so as to include the artery. I readily tied the ligature, and tightened it with the fore-fingers in the bottom of the wound. All pulsation immediately ceased in the aneurism and the arteries of the extremity."

We will not follow the details of the case: suffice it to say, the patient did very well, and gave promise of a favorable issue until the fifteenth day, when secondary hœmorrhage appeared, and recurred at intervals until 5 P.M. of the fifteenth day after the operation, when he died.

"*Post-mortem Examination, eighteen hours after death.*—The wound was filled with coagula and sponge, which had been introduced for the purpose of making pressure. The blood is already in a state of partial decomposition. The dissection was carefully performed, exposing the different layers of muscles. The lower incisions made at the operation were found to include three-fourths of the mastoid, leaving a small portion of the clavicular portion undivided. Below this, the aneurismal sac and the scalenus anticus formed the outer and posterior wall of the wound. The inner wall was formed of condensed cellular tissue covering the carotid artery, jugular vein, thoracic duct, and the edges of the thyroid muscle. At the bottom was the subclavian artery, completely divided by the ligature, which was found free in the coagula. The cellular tissue of all the parts around the wound was condensed by adhesive inflammation, rendering the dissection exceedingly tedious and difficult. The jugular vein, which skirted the inner wall of the wound, was obliterated, and filled with fibrinous coagula. Opposite the track of the ligature, the vein was contracted to a cord, and impervious as far as its junction with the subclavian. The vena innominata and subclavian were normal. The pleura at the bottom of the wound presented a large irregular lacerated opening, communicating from the wound with the left pleural cavity, which was filled with coagulated blood. This formed one large uniform coagulum, and had every appearance of being of rapid and recent formation; the membrane around these was thickened. On exposing and tracing the subclavian artery, it was found that the ligature had been applied about one and a quarter inches from its origin at the aorta, and immediately at the root of the vertebral, on its cardiac side. The artery had been completely divided by the ligature, which, as mentioned above, was found loose in the wound. The stump of the subclavian, between the aorta and ligature, presented the appearance of a round solid cord, about an inch and a quarter long, and impervious to liquids or air. The external coat of the stump was thickened and adherent near the ligature to the surrounding tissues, by adhesive inflammation. On laying open the vessel longitudinally, it was found that a firm fibrinous coagulum occupied the vessel, and was adherent firmly to its inner coat for three quarters of an inch: near the aorta, the coagulum was softer. The coats of the vessel were moderately thickened, and presenting a small patch of atheromatous deposit about the third of an inch from the tied end. Around this deposit the adhesion seemed as perfect as at any other part. Beyond, the ligature presented a different appearance. No plug other than a soft coagulum of blood occupied its cavity, and it presented much less evidence of adhesive inflammatory process in its coats. The vertebral was given off immediately at the point of ligature, and was open, containing a thin blood coagulum, like the one in the subclavian. These were drawn out with ease, and evidently had formed during the last moments of life. About one-third of an inch from the vertebral came off the thyroid axis, and nearly opposite the vertebral was the internal mammary. These vessels were all patulous and healthy. About half an inch from the thyroid axis commenced the dilatation of the artery, to form the aneurismal sac. This tumour was about the size of a small orange, and had involved in its growth part of the scalenus anticus, the cervical nerves going to form the cervical plexus, the surrounding cellular tissue, and the glands. The aneurism was completely blocked up with coagula, and the axillary artery which emerged from its distal side was plugged with a fibrinous clot exactly similar to the one in the stump of the subclavian, though perhaps not so perfect. It appeared sufficiently so, however, to obliterate entirely the calibre of the vessel. The plug extended some distance down the axillary artery. The thoracic duct, which had been injected with wax from the abdomen, was found uninjured. The aorta was thickened, and its coats irregular, from a considerable deposit of atheromatous matter in its tissues. The heart was somewhat larger than natural, but apparently sound. The other organs were not examined, as the friends insisted on an early removal of the body for burial.

"The hœmorrhage, in this case, came from the distal end of the artery, and the very free and direct anastomosis of the internal carotid at the base of the brain with the vertebral, induce me to think that it was the latter vessel which transmitted the blood. Some, indeed, may have come through the thyroid axis, but I consider the former mode more direct.

"Should this operation be repeated, I would suggest the securing of the vertebral, and if possible the thyroid axis, by ligature. The difficulties are indeed thus increased, but not insurmountable."

Dr. Rodgers says, that, "in examining anatomical and surgical authorities, I found the opinion prevalent among almost all British authors, that the operation on the left side was *impracticable*." He mentions Colles, Harrison, Flood, Guthrie, and Quain, as coinciding in this opinion. To these he might have added Sir Astley Cooper, and our countryman Mott, who discourages any further attempt upon this artery at the point indicated. Dr. Mott, in his late edition of Velpeau, says—"We saw the first, and perhaps the only attempt that ever was made to tie this artery. This was by my illustrious and revered preceptor, Sir Astley Cooper. After working indefatigably, with all his eminent skill and superlative tact, for an hour and a half, he abandoned the operation as hopeless." Dr. Rodgers mentions all the attendant dangers, but is satisfied of the *feasibility* of the operation, and says he would undertake it again, "with some slight variations, should another case ever present itself."

REMARKS.—That ligature of the left subclavian artery is both a difficult and dangerous operation, cannot for a moment be questioned; but as to its *practicability*, we learn that it has been performed *five times* by surgeons now living in this city. Four of the cases terminated fatally, and one recovered. The two last operations were performed by Dr. A. Mercier; one for traumatic aneurism of axillary artery, in 1844; the other, for true aneurism of the subclavian itself, in 1845, originating something like Dr. Rodgers' case, from carrying a basket on the shoulder. Dr. Mercier's first case died on the sixteenth day, from secondary hœmorrhage and pleuritis. This operation was performed below the clavicle. His second case was operated on above the clavicle: the patient recovered perfectly, and is living in the city at this time, in the enjoyment of good health.

4.—*Sulphate and Ferro-cyanate of Quinine.*—In the April number of the Southern Medical and Surgical Journal, we find an excellent paper on these medicines, by Dr. H. V. Wooten, of Lowndesboro', Ala. Dr. W. first gives his experience with the Sulphate in *large doses*, which corresponds entirely with the latest writers and greatest admirers of this wonderful medicine. We say *wonderful*, for it is evident that the great mass of the Profession have *yet to learn the full extent of its great and varied powers*. Dr. W. has found it of unquestionable utility in every kind of fever that has fallen under his observation, whether intermittent or continued, as well as a variety of inflammatory diseases, especially *winter epidemics*. He says —

"I am not prepared to assert, that the quinia exerts any curative effect over pure inflammation: I feel confident, however, that, in proper forms, it does not exercise any such injurious influence in that disease as has been ascribed to it. But in all *fevers* there is a prominent disorder of the nervous system, which, it seems to me, requires a remedy. This disorder may be primary or secondary. The languor, apparent exhaustion, and the irregularity with which the vital powers are exerted, and their actions distributed, all betray its existence, and it is too often the case that these important matters are overlooked in the treatment. We exhaust our resources upon the structural disease, real or supposed, whilst we neglect to attend to the regulation of that system by whose regular actions alone these structures can perform healthy functions, even after we have cured them. All fevers are not inflammations, nor are all inflammations fevers. Under this view of fever and inflammation, I frequently administer the quinia to act upon the nervous system, whilst, at the same time, I deplete the vascular system, generally or locally, to lessen inflammation, or relieve visceral engorgements, all with the happiest harmony of effect."

He calls the remedy a "*nervous alterative*;" and as the nervous system is implicated in so many affections, there is no telling to what extent quinine may be indicated. He illustrates his views with some striking *cases*, but we wish particularly to call the attention of our readers to his remarks on the *ferro-cyanate of quinia*. Dr. W. was induced to try the effects of this remedy, under the hope of avoiding the *uneasiness and distress* so often produced by the sulphate. He soon found his most sanguine expectations realised, as he shows by several peculiar cases. He thus sums up his observations on this medicine:

"I have, altogether, used the ferro-cyanate of quinia in about fifty cases, presenting a great variety of symptoms and diseased action, but in every case it was addressed particularly to some disease, or disorder, of the nervous system, upon principles which I have attempted to set forth above, and in no case have I been wholly disappointed in its effects. It should not be understood that I have cured all these various cases by this medicine alone. It was only applied to one general case of morbid actions, which frequently gave it a merely subordinate position, whilst again, in other cases, it acted the part of an indispensable remedy. *When pure*, I have found it to act uniformly without those unpleasant effects which generally arise from the use of the sulphate, whilst it is just as certain, and more powerful, as an anti-periodic remedy. It lessens the frequency of the pulse, and gives tone and regularity to its action. It is more of a sedative to nervous irritation than the sulphate, and I believe that it will be found to act more uniformly as a diaphoretic than any other medicine. Being at first unacquainted with its effects, I gave it in very small doses, but experience soon led me to use it in doses of from two to five grains, according to the promptness of effect desired.

"In ordinary cases of intermittent fever, I cannot see that its effects are superior to those of the sulphate, (except that it gives no uneasiness), and as it costs about double the price, I continue to use the sulphate in most cases of that disease. But in cases in which there is febrile excitement or inflammation, where the use of quinia is indicated, I use the ferro-cyanate altogether, as I find it more certain and decided in its good effects than the sulphate, and not liable to produce any of the disagreeable disturbances of that salt. And I may add, that I use it with full confidence in all cases where I wish to exert a sedative and alterative, or regulating power upon the nervous system.

"For near two years, I have been on the look out, amongst the medical journals, to find something on the use of this article, and have seen nothing at all, until, very recently, I met with a single case, reported by Dr. Fenner, of New Orleans, of intermittent fever, treated with it in five-grain doses, and in which its effects correspond with those which I have always obtained from it. This case is all that I have seen of its use elsewhere, except that Dr. F. merely observes in his note, that it is pretty extensively used in that city, but gives no particulars. I think it a most valuable accession to our medical resources, and one that ought to be known, and used by the profession generally; and, entertaining this opinion, I have offered these few facts and thoughts of mine to the profession, hoping that they may induce some one to give a more thorough investigation to the subject, for common benefit. I have put them together during leisure moments, when my mind was frequently engaged, and my body greatly fatigued with different matters, and, of course, I claim for them nothing like perfection, but simply an attentive reading, and a candid experimental examination.

"I will remark, in conclusion, that the ferro-cyanate of quinia, from its high price, is particularly liable to adulteration, and those who use it should be very sure that it is pure, or they may condemn it unjustly, as I was about doing while using my second ounce, which was found to contain a large portion of the sulphate."

We are pleased to find, that a brief observation of our own in regard to the happy effects of the *ferro-cyanate*, in a case which could not tolerate the *sulphate* of quinine, had met Dr. Wooten's eye; and that our remarks are corroborated by his valuable testimony. Our own experience with the remedy is comparatively limited, but we have no doubt it possesses all the virtues claimed for it by Dr. W., and therefore recommend its more general use. As it is sometimes hastily and carelessly prepared, we will give the following remarks on its *form* and *tests*, from "Dunglison's New Remedies":—

"It forms needle-shaped, confused chrystals, of a greenish yellow color, and very bitter taste, recalling that of the hydrocyanic acid. It dissolves readily in spirits of wine; not so in water; and is decomposed by hot water." F.

5.—*Report upon the Hemostatic Virtues of the Brocchieri Water and Ergotine.* By the Editors of the Southern Journal of Medicine and Pharmacy.

The first of these hemostatic substances has been lately brought before the notice of the people of this country, by the editor of the *Courier des Etats Unis*, who had just then witnessed experiments made with it in Paris. His statements were of such a marvellous character as to cast great doubt upon their accuracy; and although much of it is removed from our minds, there is still a portion left with reference to the regeneration of arterial tissue. To satisfy ourselves on this last point, we shall institute other experiments as soon as the water can be obtained.

In order that those of our readers who have not seen the statements and discussion concerning this water, may ascertain how the question stood at the time our attention was called to it, we refer them to the following notice taken from one of the New York journals:

"At a meeting of some of the most distinguished physicians and surgeons of this place, among whom were Doctors Berger, Trudeau, Washington, Hosack and Wilkes (Dr. Mott had been invited to assist, but could not make it convenient to attend), a sheep was placed on a table: Dr. Barrabino (surgeon aboard the United States frigate North Carolina) made an incision into the neck, and opened the carotid artery, not by a small incision, but by a long transverse cut that almost divided the artery. The blood gushed out with violence. A tent of wool, taken from the back of the animal, was placed and retained upon the wound, then moistened with the water. In a few minutes the hœmorrhage was arrested; ten minutes afterwards it had ceased entirely. At the expiration of twenty minutes the tent was removed, when the wound was found free from blood, and the artery firmly closed. After the lapse of twenty-five minutes, the animal was loosened, and it ran about."

Other physicians of New York appear to have tried it, and with equal success. Professor Mott, of the same place, comes out against it, as will be seen by the following extract from his Clinique, of the 10th of January:

"I knew M. Brocchieri when I was in Paris: he is an uneducated man, and a perfect charlatan. When his discovery was made known in Paris, it created some stir; and I made several experiments with it, in connection with several other gentlemen, one of whom was engaged in the preparation of the water. The subject of the experiments were strong and healthy sheep, upon whose carotid arteries we operated, and we found that its power to stop hemorrhage was next to nothing, and where the bleeding was arrested, it was principally from the pressure made by the large quantities of lint, with which the wound was filled. Therefore, I say, as the result of my experience, that the styptic powers of this preparation are not to be relied upon, for a moment: that it is infinitely less useful than an infusion of rhatany, or tannin, and that it can never take the place of needles and ligatures.

"The other qualities that have been ascribed to it, of curing disease, and arresting hæmoptysis, are equally non-existant.

"Dr. M. having occasion to remove a tumour from the cheek of a female, during his lecture, applied some of the nostrum, but without the least effect."

It is true, we have heard that Professor Mott thinks that he has given his opinion too hastily, but not having seen any authentic announcement to that effect, we must look upon him as still denying to it any virtue whatever.

The hemostatic properties of *ergotine* were alluded to in the first number of this journal (p. 92), as well as a detail given of some experiments by M. Bonjean. With these evidences before us, we determined to examine the question for ourselves, in the presence of Professors Geddings, Dickson, Frost, Doctors Jervey, Wragg, DeSaussure, Gaillard, Cain, Lee, Ravenel, the French Consul, and other gentlemen, all of whom were present at the last two experiments. Knowing that certain animals would not bleed to death, if their carotid artery was cut, it became necessary to ascertain whether a sheep (it being the animal with which we experimented), was of that nature, and for this purpose three experiments were instituted, viz.:—

Exp. 1.—A large and healthy sheep was placed upon the table, one of the carotid arteries laid bare, and then cut obliquely with a pair of scissors, (the artery was not completely severed). The animal was allowed to bleed, without any application being made to the wound; in seven minutes it was dead.

Exp. 2.—The artery of another sheep was laid bare, and cut in a similar manner to the last. Immediately upon the cut being made, a roll of scraped lint was saturated with ice water, and applied to the wound; above this, pulverized ice was placed, and lint upon this, and finally more ice, which was renewed from time to time. The animal continued to bleed, notwithstanding the application, and in fifteen minutes expired.

Exp. 3.—The artery of the third sheep was cut in the same manner as the last, and tar-water applied, but without any apparent effect; in ten minutes the animal was dead.

These experiments satisfied us that a sheep would bleed to death with his carotid cut, and that no application of water, even in the form of ice, could arrest the hœmorrhage. With this conviction on our mind, we were fully prepared to form just conclusions concerning the two following experiments:

Exp. 4.—A very large and vigorous sheep was placed on a table, and its left carotid laid bare. In cutting down to the artery, various small vessels were severed, from which resulted a loss of about four ounces of blood. The director was passed under the artery, and after all were satisfied that it was the artery, a large oblique incision was made with a pair of scissors, without severing it completely. The blood gushed out with violence, and in a large stream: a small roll of lint was saturated with the *Brocchieri water*, and applied; and above this, another roll of lint, upon which the water was allowed to fall in a gentle stream, in such a manner as to come as near as possible to the wound in the artery. The lint was retained by a little pressure. The blood continued to gush out in abundance, for about two or three minutes, (this portion of the operation was not timed), and ceased to flow entirely after the expiration of about five or six minutes. In twenty minutes from the time that the water was applied, the lint was removed, without its having adhered to the wound, which was what would be called a dry wound, there being only a little clotted blood around the margin.

After ten minutes more had elapsed, the sheep was taken from the table and unloosed, when it ran about with as much apparent strength as before the operation, although it had lost more than a pint of blood. Grass was thrown before the animal, which he at once ate. The appearance of the artery just after the operation was not ascertained, as we wished to test fairly whether or not the hœmorrhage would return; and we are sorry to say, that owing to the neglect of the person who afterwards killed the animal, we have been deprived of all opportunity of making it; but, as we hope to institute other experiments, this point will yet be looked to.

Exp. 5.—A sheep, smaller than the last, was operated upon, and his carotid artery laid bare. An oblique incision was made into the artery: the blood flowed out in a manner that might have been expected. A roll of lint saturated with a solution of *pure ergotine* (one part of ergotine to twelve of water), was applied to the wound, and a little pressure exercised. The violence of the hœmorrhage ceased in about two minutes, and all bleeding was arrested a few minutes later. In twenty minutes the lint was removed, and the sheep let loose, when he appeared as well as before the experiment, eating a few minutes after.

Want of material prevented us from trying the comparative value of *Brocchieri water* and a solution of *pure ergotine:* at present, we have no right to conclude that one possesses more hemostatic virtue than the other: one thing is certain—that both of them arrest hœmorrhage in a most marked manner, without either of them being *styptic* or *cauterising* in their action. Both must operate by a peculiar action upon the blood, or upon the walls of the artery. In the case of the Brocchieri water nothing decisive is yet known, although it is stated that the calibre of the artery is restored to its natural integrity; of this we have some doubt, that must be removed either by the evidence of undoubted authority, or that of our senses.

If the Brocchieri water has the peculiar action upon the blood that it appears to have, very little doubt can be entertained of its proving efficacious in many diseases, by internal administration; still, much remains for experiment, as to what its action may be on altered blood. The composition of this water is unknown. It is colorless, of very slight acid reaction: very little taste, and this not astringent. Its odor is aromatic, and the only idea that we are as yet capable of forming of its nature is, that it is water containing the volative principle of some plant over which it has been distilled.

Ergotine has been long known to have undoubted efficacy in internal hœmorrhage, although it has not been administered until latterly in its pure state, which ought hereafter to be the only form of prescription, as the ergot varies too much in strength to be relied upon. Besides, the ergotine is free from all the poisonous effects of ergot. The following is M. Bonjean's evidence of its value, when administered internally, in a letter to the Royal Academy of Sciences at Paris; (Comptes Rendus, 1843, p. 134):—"When you have tried the ergotine, you will be struck with the immediate effect that it produces in cases of the most alarming uterine hœmorrhage. Hæmatemesis of long standing is also readily cured by the use of this medicine, relapses rarely occurring, particularly if precaution be taken to continue its administration for some little time after the symptoms have ceased. To be convinced that the ergotine is also the *obsterical* principle of the ergot, treat the pulverized ergot with æther, until every thing that this liquid can dissolve is extracted. By this means the oil and resin, which contains all that is hurtful, are extracted. A powder remains which is not in the least unctious, but feeling like dry sand, without a disagreeable taste, possessing no toxic quality, and exciting vigorously the uterine contractions, in a dose from six to eight grains, in all those cases of uterine inactivity where the administration of ergot is thought advisable."

Dr. Ebers gives farther evidence of its utility in uterine hœmorrhage, (Lond. Med. Gaz., November, 1845, p. 1240):—"I have proved the efficacy of this extract in numerous cases, and I would remark, in the first place, that I have used it, and always with the most perfect success, in those forms of uterine hœmorrhage which are attendant upon carcinoma of the uterus, and which so greatly exhaust the strength of these unfortunate patients, and rapidly lead to their death. It is well known, that all the styptic remedies hitherto recommended in hœmorrhages of this kind either possess very little efficacy, or else they annoy the patients in various ways, and even render their condition worse than it was before: this is the case with mineral acid, opium, and especially the preparations of iron: well known to us, also, is the inefficacy of all external styptics, which, moreover, are often quite inapplicable in cases of medullary and fungoid diseases of the uterus, in which they greatly increase the sufferings of the patient. Latterly, I have treated these hœmorrhages exclusively with ergotine; and in order to satisfy myself of the eficacy of this remedy, I have only given it in doses of two grains every two hours. In almost all the cases, twelve doses were sufficient to arrest the hœmorrhage; only for a time, to be sure, yet for a period of several days, even weeks, and then the progressing destruction of the uterus has (so far as my experience goes) entirely precluded the possibility of any remedy succeeding in checking it.

"A second series of observations which I have made, for the purpose of trying the efficacy of ergotine, is in cases of atonic uterine hœmorrhage occurring at different periods of life, both in youth, and especially at the critical period of later years, in which females are so frequently threatened with affections of the uterus, particularly those of an organic nature. Of the uterine hœmorrhages which occur in the earlier periods of life, the following case, among many others, afforded me good opportunities of observing the effects of ergotine.,

"The patient which I had under my observation was that of a servant girl of a strong constitution, who, without any obvious cause, lost a large quantity of blood during a menstrual period, whereby she was reduced from a state of robust health to that of a complete exhaustion, rendering it necessary for her to give up her situation. A dozen two-grain doses of ergotine entirely restored this patient to health.

"It is stated that the infusion and decoction of ergot have been already used with advantage, and strongly recommended, by Olivier, Prescott, and others, in cases of this description: I have myself repeatedly made use of these forms of administration, but I have never, until lately, observed such decided and certain effects of this remedy."

With the above evidence of the utility of the two substances mentioned in this article, we recommend them to the profession for farther trial, especially the ergotine, the manner of preparing which is no secret, and may be found in this journal, January, 1846, p. 104.—*Southern Journal of Med. and Pharmacy.*

Remarks.—This celebrated *quack nostrum* (*Eau Brocchieri*) was brought to the notice of the Medico-Chirurgical Society of Louisiana, at its sitting in March, by a communication from one of its venders, accompanied by some bottles of the article, with the request that the society would examine and report upon its styptic powers. The society decided (we think very properly) that it would be setting a *bad precedent*, and respectfully declined the offer. Several of the members, however, determined to avail themselves of the earliest opportunity to test the virtues of the remedy, as it had attracted so much notice both in the medical journals and newspapers of the day. We have been kindly favored by Dr. A. Mercier, with the following account of his first experiments with the article, which we have translated from the French:

"*Messrs. Editors.*

"Gentlemen:—At a time when experiments with the '*Eau de Brocchieri*' seem to be the order of the day, permit me to communicate one or two facts which a happy chance threw in my way, and which seem to be of sufficient importance to demand a passing notice.

"Without alluding to all that has been said in relation to the styptic virtues of this water, I resolved to test it for myself; a few days since I was enabled, by good luck, to put the matter to the test of experiment. A French *cuisinier*, when washing a glass, broke it in his hands, and wounded the right index finger. In spite of all the means that were employed on the spot, such as cold water, salt and water, cob-web, the hœmorrhage continued for four hours. At the end of this time he came to claim my assistance, and on removing the dressings, I saw, upon the internal edge of the right index finger, about the middle of the first phalanx, a wound between five and six lines in length, (a piece of the integuments being completely detached) and penetrating beneath the cellulo-adipose tissue which constitutes the pulp of the finger. One of the small anastomotic branches of the index finger was opened, and poured out a constant stream of arterial blood. This I deemed a favorable case for the "*Eau de Brocchieri.*"

"I saturated some lint with this water, and applied it to the wound, recommending the patient to use slight compression. I occasionally poured a small quantity of the Brocchieri water over the lint as it lay on the wound. At the end of seventeen minutes I removed the compress of lint, and found the hœmorrhage as profuse as at first. I then applied two small compresses, one along the internal and the other on the external border of the index, confining both with a bandage. I then lightly cauterised the wound with the nitrate of silver. In five minutes, I removed the compresses; the hœmorrhage did not reappear; I then covered the wound with cob-web, confining it with a bandage. I saw the patient nineteen hours afterwards, and no hœmorrhage had reappeared.

"Shortly after this experiment was made, a colored man presented himself at my office, with a deep wound in the thenar eminence, about an inch and a half in length, in the direction of the axis of the thumb. He had, according to his account, lost about two pounds of blood, and the hœmorrhage was then considerable. In the midst of a mass of muscle which formed a hernia through the wound, I saw the blood spouting, per saltum, from three different points. The muscular branches distributed to the thenar eminence had evidently been opened. After carefully cleansing the wound, I saturated some charpie with the *Eau de Brocchieri*, applied it, and covered the whole with a compress, over which I poured, at short intervals, the *Eau de Brocchieri*. At the end of more than half an hour, in the presence of Dr. Beugnot, who, having heard of my good fortune, came to witness these experiments, I removed the dressings, and we found the hœmorrhage as profuse as before the application. Without pushing these experiments any further, we applied one or two sutures, drew the wound accurately together, and arrested, on the instant, the hœmorrhages. The bleeding did not return. The pain which these two patients experienced from the application of the *Eau de Brocchieri*—a pain incomparably greater than that from the application of strong salt and water, or any other styptic solution, together with its utter inefficacy in cases of hœmorrhage, have induced me to abandon any further trials with it, except, perhaps, in cases of hœmorrhage from *mucous* membranes, as from the nose, rectum, &c. &c., which are so common in this country. MERCIER.

New Orleans, April 9th, 1846."

6.—COMPARATIVE MORTALITY OF AMERICAN CITIES.

We subjoin such extracts as we have been able to glean from the American journals, in relation to the annual mortality of our large cities. We had hoped, by this time, to have ascertained the mortality of Philadelphia, Baltimore, Cincinnati and St. Louis, but have been disappointed. The facts, as far as ascertained, both as to total amounts, and some of the most fatal diseases, such as consumption, fevers, and inflammations, admit of a comparison most favorable to our own city. We refer such as feel any curiosity on the subject, to our remarks on the "Mortality of New Orleans," in our last number. We will simply state, again, that our mortality was 2783, which, taking the population at 150,000, would be about 1 in 54, or within a fraction of 1·85 per cent.

Mortality in Mobile in 1845.

We are indebted to a professional friend, for "*A table shewing the mortality of the city of Mobile for the year* 1845, *compiled from the Sexton's book*," from which we learn that the total number of deaths was

442:—of which 320 were whites, and 122 were colored. Estimating the population of the city at 15,000, which we presume is about correct, the mortality would be 1 in about 34; or 2·94 per cent.

We have selected from the table the same list of principal diseases as was taken from the New Orleans bill of mortality, and by reference to our last number, the reader may draw a comparison. In the Mobile table we found the same disease under different names, and have combined them; for instance, pneumonia and inflammation of the lungs; delirium tremens, mania à potu and intemperance; enteritis and inflammation of the bowels, &c.

The following is a list of the principal diseases, and the number of deaths from each, viz:—consumption, 57; enteritis, 19; remittent fever, 3; congestive fever, 9; typhus fever, 2; scarlet fever, 1; pneumonia, 19; delirium tremens, 21; convulsions, 23; tetanus, 2; trismus nascentium, 1; dysentery, 11; diarrhœa, 11; still-born, 13; old age, 8.

Deaths in New York in 1845.

Whole number of deaths in the city of New York, during the past year, 1845, 10,983: of whom there were—white males, 5,554; white females, 4,892; black males, 261; black females, 276. Of these there were, of the age of five years and under, 5,865; from five to ten, 410; from ten to twenty, 389; from twenty to thirty, 1,161; from thirty to forty, 1,131; from forty to fifty, 760; from fifty to sixty, 417; from sixty to seventy, 343; from seventy to eighty, 206; from eighty to ninety, 111; from ninety to one hundred, 21; one hundred and over, 3; unknown, 106. The principal causes of death are as follows: consumption, 1,659; convulsions, 721; fevers, 501; cholera infantum, 523; apoplexy, 252; congestion of the brain, 186; violent death, 165; old age, 113.

—N. Y. Journal of Medicine.

Remarks.—Taking the population of New York, 372,000, (which is several hundred more than was given by the census of 1845,) the mortality as above was 1 in about 33·89, or 2·95 + per cent.—Editors.

Mortality in Charleston for four years.

1842.—Deaths, 553, (1 in 54 1-5): 204 of these were under 5 years, and 24 over 80 years. Of the deaths, 80 were by consumption; 56 by dropsy; 46 by fevers, 35 by teething; 3 by tenanus. During this year the thermometer ranged from 28 deg. to 90⅔ deg; average, 66⅔ deg. There wese 111 rainy days, during which time there fell 38 inches of rain.

1843.—Deaths, 685, (1 in 43⅔): 216 of these were under 5 years, and 35 over 80 years. Of the deaths, 75 were by consumption; 61 by dropsy; 92 by fevers; 33 by teething; 4 by tetanus; 35 by inflammation; 45 by old age. During the year, the thermometer ranged from 26 deg. to 90⅔ deg.; average, 65 deg. There were 92 rainy days, during which time there fell 40⅓ inches of rain.

1844.—Deaths, 543, (1 in 55 1-6): 205 of these were under 5 years, and 23 over 80 years. Of the deaths, 75 were by consumption; 50 by dropsy; 97 by fevers; 20 by teething; 5 by tetanus; 29 by inflammation; 27 by old age. During the year the thermometer ranged from 24 deg. to 90⅔ deg.; average, 64⅔ deg. There were 94 rainy days, during which time there fell 33¼ inches of rain.

1845.—Deaths, 563, (1 in 53 1-5): 146 under 5 years, and 35 over 80 years. Of the deaths, 106 were by consumption; 45 by dropsy; 40 by fevers; 20 by teething; 13 by tetanus; 41 by inflammation; 48 by old age. During this year the thermometer ranged from 12 deg. to 92 8-10 deg.; average, 63¾. There were 85 rainy days, during which time there fell 45 2-10 inches of rain.

—Southern Jour. of Med. and Pharmacy.

7.—*Tetanus Cured by the use of Strychnine.* By Dr. Fell.

"Dear Doctor:—As I am not aware that Styrchnia has been used in tetanus, I send you the following, from my Case-Book, hoping your readers will try it, and report their cases.—I remain, truly yours, &c., J. Weldon Fell, M.D.,
"*To the Editor of the Reporter.*" "68 *Sullivan-street.*

—

1*st Case.*—On the first day of August last, I was called to see W. E——, of Hudson county, N. J., *æt.* about seven years. I found him lying in a kind of stupor, but upon his father suddenly touching him, he was thrown into a violent spasm,—his jaw set,—his body bent backwards, resting upon his head and heels, constituting a well-marked case of opisthotonos. So rigidly were the muscles contracted, that he could be raised by the head. The face was very much distorted during the spasm, which lasted about two minutes, when the muscles became somewhat relaxed; but his jaws were still set, and he complained of pain in the masseter and temporal muscles, with some febrile excitement.

A physician being called, gave it as his opinion, that it was a case of measles, (this disease being prevalent in the neighborhood), and prescribed accordingly. Next day the patient complained of cramps in his extremities,—jaws very stiff, with some slight spasms: the doctor now called it worms, and gave spigelia; this brought away one small worm. The difficulty of deglutition increased, the jaws were set, the spasms being more severe, and the mother, fearing lock-jaw, applied a large blister to the spine, and another to the knee. The child growing worse, I was sent for, and found him as I have stated,—jaws set, extremities stiff, difficulty in swallowing, distress about the præcordial region, &c.

Believing this to be a case of Traumatic tetanus, and acting upon a suggestion of Professor Mott's, I prescribed the following:—

℞ Strychniæ, gr. j.
Ext. Hyoscyami, dr. ss.
M. ft. pil. No. xiv.:
One to be given every two hours.

At the same time, I ordered ung. tart. ant. to be applied to the spine, and the knee to be put in a warm bath. After the fifth dose of the strychnine, the muscles began to twitch; after the sixth, this was very apparent. The pills were given every two hours for two days, their peculiar effects being apparent after each dose. On the third day, they were given every two hours; the fourth, every four hours, &c., making the intervals an hour or two longer every day, until he took them but three times a-day. He had no spasms after the third day; the muscles became gradually relaxed until the sixteenth day, when I ordered the pills to be discontinued, as he appeared entirely recovered.

Oct. 16*th.*—The boy has been perfectly well since he discontinued the pills, and is playing about as usual.

Case 2.—*Oct.* 4*th*, nine o'clock, P. M., was called to see Miss M——, *æt.* 25: found her in a spasm (the left side alone being affected), jaws, left arm, and leg stiff,—pain in the left side,—extremities cold,—pulse weak, &c. Ordered some wine and water, as soon as she could take it. Heard the following from her mother:—"She has been suffering for some months from the toe-nail growing into the flesh, upon the great toe of the left foot." A few days previously, Dr. Vanderpool removed a part of the nail. A few days after, she went out riding, very imprudently, and, directly after her return, the toe commenced swelling and to be very painful, the pain extending up the leg, side, arm, and soon affecting the masseter and temporal muscles of the left side. Dr. V. and myself were sent for, and, upon examination, we found the toe and foot swollen, and painful to the touch,—the lymphatic glands of the groin much enlarged,—the muscles of the left side contracted,—uneasiness about the præcordial region,—masseter and temporal muscles of the left side contracted, difficulty of swallowing, jaws stiff, &c. Believing this, also, to be a case of Traumatic tetanus, I suggested to Dr. V. the use of strychnia; he preferred, however, waiting until morning. Dr. V. made a free incision upon the toe, and prescribed—

℞ Tinc. Assafœtid. f. oz. j.
" Opii, f. dr. iij.—M.:
A tea-spoon full every two hours.

Oct. 5th.—As the patient was much worse, we determined to give the following:—

℞ Strychnia, gr. i;
Ext. Juglandis, dr. ss.
Ft. pil. xvi.:
One to be given every two hours.

6th.—The friends became alarmed during the night, on account of the patient having spasms; but, upon inquiry, we found them to be the effect of the strychnine.

7th.—The patient much relieved: has no difficulty except the twitching after each pill. Ordered the pill every three hours.

8th.—Still better. Ordered the pill every six hours.

9th.—Still better: has had no spasm since the 7th. Ordered half a pill every six hours, and, as she was weak, we ordered wine whey, &c.

10th.—Discontinued the strychnine, as the tetanic symptoms have all disappeared.

16th.—Saw Dr. Vanderpool to-day, and he says, Miss M. has had no return of the tetanic symptoms.

The peculiarity of the above case consists in the contraction being confined exclusively to the left side, making it a case of pleurothotonos.

—*N. Y. Medical and Surgical Reporter.*

8.—*Violent Chorea St. Viti—Cured by Strychnine.* Attendance of Dr. John H. Griscom, in New York hospital.

The subject of the following history presented the most violent case of Saint Vitus' Dance we had ever seen. It will be recollected by many students, and others who witnessed it, as having been characterized by the peculiar jactitation of the extremities, particularly the lower, when walking, from which it was called the "Polka case."

Eliza Holstappen, aged 19, born in Germany, single. Entered July 24, 1845. Is of large frame, and robust appearance. Has had amenorrhœa four months, but otherwise has enjoyed good health, until, about three weeks since, her friends noticed a twitching of the muscles. This increased, until there was involuntary motion of all her limbs. Upon admission, she was unable to remain in bed, so that she was obliged to be kept on the floor. Her bowels being opened, she was put upon Fowler's Solution, gtt. iv. ter in die. This was increased to every two hours by the fourth day, but her motions became more and more frequent and strong, so that she could not be restrained on the mattress, and tore her clothes from her body. Her nights were sleepless, and she constantly screamed, although perfectly sensible.

On the 2d day of August, she was put upon carb. ferri, which was continued for three days, the patient being, at the same time, freely purged with croton and castor oil. This did not produce much benefit. As soon as evening came on, her motions became more and more convulsive, and her screams became loud and incessant. For several nights in succession, she was obliged to be tied hand and foot to the bedstead, perfectly naked, as no covering could be kept on her. During the day, she was more pacific.

On the 12th, we began the use of pil. strychnine, gr. 1-16, ter in die. The effect of this was almost immediate, and very marked. It was continued four days, in the above quantity, with evident improvement, her nights being more quiet, and some sleep obtained.

On the 16th, the pill was increased to 1-12 gr. This night she slept for an hour or more together, in a chair.

17th.—Last night she slept in bed quietly for several hours, and this morning *was able to sew.* She walks about, although her motions are still violent. Has been on the use of the medicine just one week.

19th.—The two last nights the patient has slept perfectly well during the whole night, without any noise; walks now tolerably straight, and visits the other wards. Her appetite is very great. During the whole of the attack her mind has been entirely free from any delusion. She still continues the strychnine, half grain ter in die, with progressive improvement. During the last two days she has occasionally complained of headache.

Sept. 1st.—Our patient rapidly improved under this treatment, continued until within a few days, when, she being apparently *well*, it was stopped, and no symptom of a relapse appearing, she was to-day *discharged cured.*

The pathology of Chorea is among the mysteries of the science. The arsenical and ferruginous preparations, and drastic purgatives, which have, either one or the other, generally succeeded in relieving the symptoms, having in this case entirely failed, the determination to try the strychnine was made on the supposition of the condition of the nerves in this disease being analogous to that in paralysis. In the latter case, there is a total loss of power over the muscles; in the other, a partial loss only. If the rapid and felicitous result of the use of strychnine, in this case, should lead to its further administration in Chorea, some light may perhaps be thrown on the pathology of the disease.

—*N. Y. Medical and Surgical Reporter.*

9.—NEW MEDICAL COLLEGES.

It seems that the number of Medical Colleges in our country is constantly increasing. We have just been apprised of the incorporation of the Franklin Medical College of Philadelphia, and the Medical College of Memphis, Tennessee. The former is completely organised, and by reference to our advertisements, it will be seen, that the First Annual Course of Lectures will be opened on the 12th of October next. From an "Announcement" with which we have been favored, we learn, that "The Franklin Medical College was established by the Legislature of Pennsylvania, under the opinion that increased accommodation was demanded for the great concourse of students now resorting to Philadelphia." This institution is vested with privileges as ample as any other in the State, and its students will have free access to hospitals, libraries, museums, &c.

In regard to the Tennessee school, we extract the following notice from the Memphis Eagle, with which we have been favored. We care not how many medical schools are organised, if the professors will only *do their duty*, and withhold their sanction from all incompetent students. If the example of the U. S. Army and Navy Medical Boards were followed, a *diploma* would be a passport to favor.

"MEDICAL COLLEGE OF MEMPHIS.

"*Memphis, Tenn., March* 16, 1846.

"This day, the Trustees of the Medical College of Memphis convened in the city of Memphis, agreeably to previous notice, and elected H. H. Means President of the Board, R. H. Patillo, Secretary, and John Martin, Treasurer.

"After organizing, and adopting a code of By-Laws, the Board proceeded to fill the following chairs:

"1st. Anatomy—J. M. Bybee, M.D.
"2d. Surgery—D. J. M. Doyle, M.D.
"3d. Chemistry and Pharmacy—A. Hopton, M.D.
"4th. Theory and Practice—G. R. Grant, M.D.

"The Board resolved to postpone filling the other chairs at present, to wit:

"5th. Institutes, and Medical Jurisprudence.

"6th. Materia Medica.

"7th. Obstetrics, &c.

"*Resolved*, That the city papers be requested to publish these proceedings; and that the Board adjourn, to meet the fourth Friday in April next.

"*By order of the Board:* R. H. PATTILLO, *Sec'y.*"

"TO THE MEDICAL PUBLIC.

"The Trustees of the Medical College of Memphis, being desirous to fill the chairs of this recently-chartered Institution in a manner best calculated to insure its success, invite applications from the members of the Medical Profession, for the three following vacant chairs, viz.:

"Institutes and Medical Jurisprudence.

"Materia Medica and Therapeutics.

"Obstetrics and Diseases of Women and Children.

"Communications (post-paid) must be forwarded to the Secretary of the Board of Trustees prior to the 3d day of July next, at which time the appointments will be made.

"The names of the successful candidates only will be made public.

"*By order of the Board:* "R. H. PATILLO, *Sec'y*, B.T.

"*Memphis, March* 19*th*, 1846."

10.—PRIZE ESSAY OF THE LOUISIANA MEDICO-CHIRURGICAL SOCIETY.

We are requested to announce to the profession, that at a recent meeting of this Society, it was resolved to offer a gold medal, of the value of one hundred dollars, for the best Essay *on Strictures of the Urethra, with their treatment.* This prize is offered to the competition of the profession in all countries; but the essays must be written in the English or French language. The communications must be accompanied with a letter and corresponding mottoes, to the President of the Louisiana Medico-Chirurgical Society, New Orleans, La., and should be received by the 1st day of February, 1847.

The medical press throughout the country is respectfully requested to give publicity to this notice.

Here is the offer of a splendid prize, and we doubt not it will call forth the competition of great talent. The prize will be awarded at the anniversary meeting of the Society, on the first Wednesday of April, 1847.

11.—PRIZE ESSAY OF THE ALABAMA MEDICAL SOCIETY.

The State Medical Society of Alabama offered, last season, a silver cup, as a prize for the best Essay on the *Pathology and Treatment of Congestive Fever.* We learn that, at the annual meeting of the society, held at Selma, in December last, this prize was awarded to Dr. A. G. Mabry, of Selma. We take great pleasure in publishing the subjoined communication, by which it will be perceived that a similar prize is now offered for "*the best Medical History of Alabama.*" This is the way to excite professional emulation, and, we doubt not, it will bring out the best talent of the State. Every medical society should offer one or more prizes every year. Southern physicians have as much talent as is to be found in the ranks of the profession any where, but being scattered over a vast extent of country, and not feeling the stimulus of association, and of emulation, they are, in a great degree, too negligent of the progressive improvement of medical science. A brighter day is dawning upon us.

"*Editors New Orleans Medical and Surgical Journal.*

"Gentlemen:—At a meeting of the Alabama Medical Society, held on the 12th February last, it was resolved, to offer a silver cup as a premium for the best Medical History of the State of Alabama: the Essay, with the name of the writer, to be deposited with the Secretary of the Society, on or before the first Monday in December next.

"It was, also, further resolved, that the secretary be, and he is hereby instructed, to send the above resolution to the Editors of the New Orleans Medical and Surgical Journal, and the newspapers of this county, for publication.

A. G. Mabry,
Secretary Alabama Medical Society.

"*Selma, Dallas County, Ala., March 1st*, 1846."

NEW ORLEANS, MAY 1, 1846.

HEALTH OF THE CITY.

Our city can still boast of almost uninterrupted good health. Neither pestilence nor famine has visited our borders. Although the variations of temperature, about the commencement of spring, have been frequent and sudden, yet these have had but a trifling influence on the health of our population. A sporadic case of scarlatina, or rubeola, has been met with, particularly in the upper faubourgs of the city; yet these are now few and far between.

With these exceptions, no particular type of disease has prevailed: accidental cases of pneumonia—of cholera morbus—of dysentery, and of trismus nascentium, with a few others, incident to the season, have been witnessed in different sections of the city; such are, however, common to all latitudes, and therefore deserve but a passing notice. A few cases of variola were reported some weeks since; the cases were, however, too rare to cause much alarm, or to excite public attention. We have had unusually heavy rains, and it remains to be seen what influence this shall exert upon the health of our city. The spring has been quite backward; now, however, we can say, with the poet—

> "*Solvitur acris hyems gratâ vice Veris et Favoni;*
> *Trahuntque siccas machinæ carinas:*
> *Ac neque jam stabulis gaudet pecus, aut arator igni;*
> *Nec prata canis albicant pruinis.*"

HEALTH OF THE COUNTRY.

Under this head, we shall not be able to give such full and satisfactory information as is desirable, owing to the inattention of many of our correspondents: their silence, however, leads us to suppose, that the different points from which we have heretofore received communications in relation to the "existing state of health," remain exempt from disease of every kind. We would much rather speak from authority on this subject: and we again reiterate the request, that whether sickness should or should not prevail, it is equally important to be informed of the fact.

St. Mary's, April 17, 1846.—"This section of the country is now generally healthy. During the month of March cases of pleuritis and pneumonia continued to occur, and were probably more numerous than usual, owing to the inclement weather. We had excessive rains in March, and during the early part of this month; on Friday evening, April 3d, a violent storm, with rain, and, in some parts of this parish, with hail. Last week the Teche was higher, it is said, than at any time since '28.

"The measles are prevailing in some parts of this and the adjoining parish, St. Martin, but the cases are mild, and require little attention."

Montgomery, Ala., April 16, 1846.—"Our place may still be considered healthy, though the number of cases of disease has increased somewhat since my last. For a week or two past, we have had some cases of dysentery; and within the last few days, I have seen two cases of remittent fever. We have had occasionally, through the winter, cases of erysipelas, and for several weeks past the number has been somewhat increased. The cases of pneumonia are also more numerous and violent than during the fall and winter. I had recently a case of the latter disease, in which, when I was called, on the seventh day, I found the entire left lung hepatized. The febrile symptoms—which at first were of a violent character—yielded, in a short time, to quinine; the pulse, respiration, temperature of skin, and, indeed, the appearance of the patient generally, so nearly approaching the healthy standard, that without the information derived from auscultation and percussion, I should at once have discharged him as convalescent, and yet, for several days after (I am afraid to state the number), the whole lung remained in the condition above stated. By, at any time, omitting a few doses of the quinine, the febrile phenomena would increase, but would yield readily again to its renewed administration. How clearly does this case illustrate the influence of quinine over the action of the arterial system—may I not say, febrile phenomena generally? The amount of local disease (at first very considerable) remained absolutely the same for *several* days after the sympathetic fever resulting from it was held subdued. The length of time the disease had continued, before the case was attended to—admitting of an approach to organization in the coagulable lymph effused in the pulmonary parenchyma, and consequently rendering it less amenable to the action of the absorbents—is probably somewhat explanatory, for, as a general rule, the local disease commences to yield in a short time—seldom exceeding twenty-four hours—after the action of the quinine is manifested in the pulse. This is the first case of pneumonia in which, while the local disease remained unabated, I have remarked so decided an improvement in the respiration of the patient.

"My friend, Dr. Berney, has related to me the particulars of a case of pneumonia, which he treated recently, being the first of the kind for the cure of which he depended on quinine. Although the case was a violent one, with hepatization of one lung, it yielded promptly and readily to the treatment. That he might have no doubts in regard to the effects of the quinine, he relied upon it exclusively."

Galveston, April 16, 1846.—"I have the pleasure again to inform you, that the health of the inhabitants of our little city continues to be unusually good, notwithstanding the weather has been exceedingly unpleasant, and the vicissitudes frequent and sudden. For the last fifteen or twenty days, we have had repeated and violent storms, attended with heavy rains.

"The measles still prevails to a very moderate extent; and recently a number have complained of catarrh and sore throat. The rumor of small pox was not altogether without foundation; about the 10th of last month a decided case of it was reported. All necessary precautions were immediately taken to prevent its spreading, which appears to have had the desired effect."

INUNDATION OF THE BACK PART OF THE CITY.

We have to record a remarkable occurrence, which has been known to take place upon several occasions, at long intervals, within the recollection of some of our citizens; we allude to the inundation of the back part of the city, together with the entire swamp between this and Lake Pontchartrain, from the swelling of the lake, and the great fall of rain. It has long been known, that the customary equinoctial gales sometimes occur before, and sometimes after the equinox. On Saturday, the 4th of April, the wind blew violently from the east during the whole day, overwhelming our city with clouds of dust; at the same time, the heavens were overcast with lowering clouds. About 8 o'clock at night it commenced raining, and an immense quantity of water fell before morning. The wind and rain both continued through the day (Sunday), and it was now observed that the water was accumulating rapidly in the swamp, back of the city. It rained all Sunday night, and the wind continued to blow violently. On Monday morning the rear of the city presented the appearance of a vast sheet of water, and it was ascertained that the lake was rising at the rate of about an inch an hour, and had already encroached upon the city as far as the State House, on Canal street. This was the extreme limit; and although all the ground back of that parallel was not under water, yet, upon personal observation, we found that a very large number of residences in that region were completely surrounded by the flood. We examined it from Esplanade street up to the New Basin. We found the Old Canal full, and running over about the shot tower: laborers were there, raising additional levée. From this up to the New Canal, the water encroached upon the city according to the inequality of the ground. Canal and Common streets were inundated up to Burgundy street. At the gate of the New Shell Road an immense sheet of water could be seen, which a gentleman informed us extended nearly up to Carrollton. During the morning, the wind still blew strongly from the east, but it abated towards evening, and the waters began to retreat. On the following day, I found the water had fallen only three or four inches; still raining; wind still from the east, but now very mild; rained all the evening. The newspapers report the waters of the Gulf, at the mouth of the river, to be higher than it has been known for many years.

On Wednesday, the 8th, the sun shone out, and the day was fair, calm and beautiful. The waters in the rear of the city had retreated considerably, being now only about as far as Basin street on Common. A large portion of the rear of the city is still under water. The river is getting pretty high, but not yet up to the culverts leading into the cross streets.

April 9th.—The waters have subsided beyond the Charity Hospital on Common street. We found the following communication on the subject in the Commercial Bulletin of this morning:—

"*To the Editor of the Commercial Bulletin:*

"Sir:—In reply to your inquiry, this morning, as to the present height of the water in the rear of the city, I can say, that that height has not been within ten inches of the high water or inundation of 1831, nor within four inches of that of 1837.

"The water in the rear of that portion of the First Municipality between Toulouse street and Esplanade street, forming a part of the drained section of the city, has not filled the gutters above Claiborne street, and with ordinary care on the part of the municipal authority, would not have covered any portion of that section, although that surface is from one, to one and a half feet, below the surface of the ground in the rear of the portion of the city between the Old and New Canal, and the Metairie Ridge.

"The engine now being erected by the Draining Company, at the Bayou St. John, will, when completed, drain that portion of the city between Toulouse and Julia streets, from the river to the Metairie Ridge. It is now ready to commence work, and only awaits the completion of the levees along the New Canal and the Old Canal, which will probably be effected in three months, to prevent the possibility of a flood over the portion of the city referred to.

"The engine and works connected with it have been planned with a view to the draining of the city, both as it relates to surface and under-drainage, by which means the filth of the city may be carried off through the agency of an element, which, now considered a nuisance, would thus be found an instrument of health and general convenience.

"The great desideratum is to get rid of impure and unnecessary water, and to introduce and have the use and control of that which is pure and necessary. The impure and unnecessary, or surplus water, must first be got rid of, before that which is pure and necessary can be properly introduced. In illustration of this fact, it has been observed by a London physician, "where the drains and sewers are, there the fever is not, and where the drains and sewers are not, there the fever is;" and he added, that by inquiring of the commissioners where there were drains and sewers, and where there were not, or by examining plans indicating those facts, he could at once point out the locality in which the fever prevailed.

"This is the first time in the memory of man, that a flood like the present has occurred at this period of the year. The inundations generally take place in the fall, when gales prevail, say in August and September.

"The Draining Company is now constructing and carrying on its works, and when those works are completed, the rear of the city will be amply protected from any overflow or inundation.

"The water has already fallen six or eight inches, and is rapidly going off, having done little or no damage, except to a few vegetable gardens in the vicinity of the city.

"The Draining Company, through the exertions of its stockholders, and the enterprise of Mr. Richard Hagan, has its works in such forwardness that they will soon put it in the power of the property holders in the rear of the Gas Works and the Hospital to dig and construct good cellars under their houses for their accommodation.

"In conclusion, I am of opinion that, by proper drainage, the city of New Orleans will be rendered the most healthy city in the world, with the greatest conveniences for trade and commerce. "H. J. Ranny.

"*New Orleans, April* 7, 1846."

We learn from the city papers, that the water was over the Old Shell Road and the Ponchartrain Railroad, up to the city, and over the New Shell Road, up to the half-way house.

We have thought it proper to make this extended notice of the inundation, with the view of seeing what effect it may have on the future state of health. Two occurrences of a like nature happened, the first in 1831, and the second in 1837. In 1831, the inundation took place in August; yet, though the swamp in the rear of the city was highly offensive from the effluvia of putrid fish, there did not occur a single case of yellow fever. The year, indeed, was a remarkably healthy one. In 1832, however, we had a severe epidemic, both of yellow fever and cholera. In 1837, the inundation occurred in October, during the prevalence of a violent epidemic of yellow fever. The fever, this year, greatly abated before the usual period, and, by many, this effect was attributed to the gale and inundation. Our own opinion is, that the disease abated, for *cease* it did not, for want of subjects. We are now studying closely the *remote cause* of yellow fever, and we deem it proper to note every thing that may have a bearing on the subject.

We have been informed, and on good authority, that the inundation of 1816, from the crevasse at Carrollton, was, like the inundation from the lake in 1831, followed by no serious sickness. This event occurred in the early part of May, yet the city escaped the visitation of yellow fever; but in 1817, a violent epidemic occurred.

PHYSICO-MEDICAL SOCIETY OF NEW ORLEANS.—DISCUSSION ON TRISMUS NASCENTIUM.

We are indebted to the kindness of a friend and member of this Society, for the following report of an interesting discussion on a disease which is attracting much attention among the physicians of the South at the present time. In a previous number, we invited particular attention to this disease, on account of the great mortality which we discovered to arise from it, in our city. We hope that the various and extensive notices of it, we give in this number, will not weary the reader, as we deem it best, when any special disease is under consideration, to concentrate as much valuable matter as we can obtain in relation to it, for the convenience of reference. We find that New Orleans is not the *only* sufferer from this fatal affection; and we take this occasion to renew our invitation for farther facts in relation to it.

The medical societies of New Orleans embrace the ablest members of the profession, and we should be highly pleased to be furnished regularly with reports of their discussions. The Medico-Chirurgical Society have the subject of *Trismus* under consideration at this time, and we hope to give its discussion in our next number.

Session of the Physico-Medical Society, April 4th. — The subject of Trismus Nascentium was brought before the notice of the Society, for their consideration, by Dr. Harrison, under the following circumstances:

" During the medical conversations concerning the state of the weather and the diseases of the city, the President took occasion to welcome back to the meetings of the society, Dr. Axson, who had been absent from the city, in Texas, for several months, and requested that he would state to the society such cases of interest as had been noticed by him during his absence. The Doctor, after having returned his acknowledgments for the notice taken of his return, related, at length, an interesting account of a disease,* Trismus Nascentium, fatal to nearly all the negro children born on the plantation of a gentleman in Texas, which gave rise to a very interesting conversation, which was entered into with spirit by Drs. Hort, Harrison and Lindsay. The latter gentleman stated, that an occurrence similar to the one related by Dr. Axson had fallen under his notice in this State, and that he had directed that the women should be removed from the plantations previous to labor, and that their removal had, in every case, been attended with beneficial results. The conversation on this subject was ended by Dr. Harrison proposing the subject under consideration for debate at the next meeting of the society."—*Extract from the Minutes of February* 19.

The ensuing report of this discussion is necessarily general, it being a matter of some labor to note down all the desultory remarks that fall from gentlemen during a medical conversation. It is, however, believed to be faithful in its outlines, and to furnish a correct notion of the spirit in which these conversations are conducted.

The minutes having been read, of which we have given an extract, somewhat explanatory of the circumstance which led to the choice of the question—

Dr. Harrison rose and said, that he was not prepared to discuss the question, as he had no personal knowledge of the disorder, having never seen a case, yet, as the facts which had been given to the society by Dr. Axson were curious, and as analagous ones had been also witnessed by Dr. Lindsay, he suggested the question, in the hope that something might be elicited from the enlightened experience of some of his colleagues, which might serve as a solution to those facts, and tend to throw some light on the pathology and treatment of this disorder. He had recently received two letters on this subject, from practitioners in the South, and would content himself, for the present, with reading them to the society; which having been read,

Dr. Hort arose, merely to observe, that a step, in his opinion, had

* Dr. Axson observed, that while spending some time at a friend's plantation, he was enquired of in regard to Trismus, and its proper treatment, and was told that only four or six children had been raised there, all the others born on the place having died of Trismus. The plantation was respectable for its force, numbering some thirty active hands, who had comfortable cabins, and were well fed and clothed. The majority of the laborers were Africans. There was nothing peculiar in the location of the plantation. It had prairie opening, seven miles from the Gulf. On an adjoining place, distant about a mile and a half, and owned by a brother of the proprietor of the first place, this disease was rare, and many young and vigorous children were found. The same midwife officiated on both places. She was an old black woman, with all the requisite pretensions to such an office. Seven miles farther east, and across the Brasos river, and some four miles nearer the sea-board, the same fatality from this disease ravaged the plantation of Mrs. Wharton. In the locality of both these infected places there was nothing peculiar to mark them from adjacent settlements, while their administration was characterised by all the care and attentions which enlightened and humane dispositions could suggest.

been made to ascertain the nature and cause of the disorder. We learn from one of these letters, that a most severe lesion, starting from the umbilicus and envolving the whole peritoneum, with its invested viscera, existed, and doubtless was the pathological condition in these cases; yet, how does this accord with the history of the cases cited by Drs. Axson and Lindsay, in which the disease seemed to confine itself to particular localities, and where, in the instances of Dr. L., removed during the last month of gestation, to a *pine* region, prevented the manifestation of the phenomena and their fatal issue? Here, surely, was something peculiar and interesting, as seeming to indicate a special local agency in the development of this disorder—something altogether external to the body of the child, and which, in his opinion, required investigation. He trusted the society would not let the occasion pass, without endeavoring to improve it by a thorough sifting and examination.

Dr. Cenas said, the disease was one he encountered frequently in practice; that it certainly was frightful in its ravages, almost every case dying that was seized, still he had witnessed recoveries, and instanced a child of a medical gentleman of the city, which, in the father's judgment, received positive relief from large doses of tinct. fœtida. He concurred in the opinion, that black children were more obnoxious to it, and that its attacks coincided more frequently with the period of separation of the cord. He was inclined to regard umbilical irritation, communicating itself to the continuous tissues, lining the walls of the abdomen, as the most constant source of the phenomena; yet, he had observed atmospheric influences give rise to the same train of symptoms. He had resorted to every mode of treatment, with no encouraging success; and not long ago saw a case recover, where smart purging had been resorted to, and where the nurse persisted in applying a large and warm charcoal poultice to the whole surface of the abdomen, and renewing them for several days.

Dr. Carpenter, while concurring in the observation made by Dr. Cenas, in regard to its greater relative frequency in the persons of black infants, felt disposed to trace the chief agency in its production to external causes. These, operating upon peculiar susceptibilities, gave rise to an attack, and it would not be unusual, if in a disease of such severity, casual complications, such as those mentioned, denoting structural lesions, should happen. This all might be without those lesions being essential to it, or composing its anatomical character. His experience warranted him in saying, that the treatment was vague and unsatisfactory. Yet he had seen recoveries, and mentioned a recent one in the practice of Dr. Stone, where the body had been well smeared with sweet oil by the mother, and which the doctor sanctioned, as a gratification to its attendants. He, of course, was disinclined to impute any efficacy to this mode of using the oil.

Dr. Wedderburn arose, simply to ask a question. In hospital practice, where patients are brought into wards well filled with every variety of disease, it is common for the medical attendant to encounter erysipelas, ensuing on the most ordinary operations. Now, can it be possible that this occurrence may happen to infants and give rise to Trismus, thus furnishing the pathological changes of tissue which have been noticed? He instances this very possible analogy, from the history given by Drs.

Axson and Lindsay, of the endemic prevalence of the disorder in certain confined localities. Another consideration seems to lend some show of probability to his views, and this was the escape in all cases where the parturient woman had been removed to an elevated piney region. In such localities, we know a purer atmosphere is breathed, and they are proverbial for health, while negro quarters, generally, are "cabined and confined."

Dr. Stone arose and said, that as far as his observation went, he believed every child was born with a cord; that this cord separated at a given period, and in various modes, and that the vast majority of children lived, were healthful, and never had Trismus. What is Trismus? What is Tetanus? were questions he could not answer. He knew the symptoms which manifested their presence, but their essentiality he knew not. There was something intangible at the bottom, which he could not explain, could not detect, yet which he was content to refer to a peculiar organisation. Whatever it was, and by whatever designations it was recognised, he was disposed to refer all the phenomena characterising these diseases to it, as a necessary pre-requisite—without which, the disease could not happen, and which, being present, various and opposite causes were adequate to its development. He could not acknowledge any injury done to the cord, or any irritation seated there, or any other painful wounds, as necessarily producing either Trismus or Tetanus. He was daily meeting such cases, and with only occasional occurrences of either disorder. Nor was it what is called nervous temperaments that the disease was likely to assail. Such temperaments were generally resistant of morbid influences. They repelled them as long as the system was predominant, but once let it be affected by injuries—once let its nervous influences be abated, and they would surely succumb to prevailing external forces, whatever these might be. For example, he has seen, during epidemic seasons, this organisation resisting the influences which produce these seasons, until an accident, a broken leg for instance, has modified this power of resistance. They then yielded, and he could almost tell the hour when the black vomit would set in and the case die off. These were not the temperaments obnoxious to Tetanus. It was rather the dull phlegmatic organisation, where the organic forces were lazily exerted, that Tetanus was most likely to assail. Where this temperament existed, he had seen the slightest injuries give rise to the disorder, and in parts where no nervous filaments were known to exist. He had seen, of late, several cases of Trismus, and alluded to the one mentioned by Dr. Carpenter. He knew sweet oil to be innocuous, and yielded to its nurse to have its body smeared with it. His treatment had been mainly the use of purgatives. The child recovered.

Dr. Harrison arose and said, that in offering the question to the notice of the society, he was mainly influenced by a desire to obtain information from respectable and experienced sources; that he had never seen or treated a case, but, from its endemic ravages in places widely differing in latitudes, and other physical conditions, it certainly was a curious and interesting affection, and merited attention, from its great mortality, as well as from its singular historical features. He held in his hand some curious statistics. From these he learned, that in the West Indies it has prevailed with frightful destruction. In some of these islands, it has killed as high as fifty and seventy *per cent.* of the negro infants born on them. Again, in England, the disease is rare, while in Scotland, and across the channel in Ireland, it is rife, and always accompanied with a large mortality. Again, we find it scarcely existing on the main land of Iceland, but in a small island adjacent, such has been its fatality, that not a child has been raised there. These are highly valuable historic facts, which forbid the idea of its having any connection with umbilical injury. Indeed, the several writers who note these endemics, assign multifarious and opposite causes to account for their origin. Some ascribe it to too much purging, while others to a bound condition of the bowels, and not purging enough. In Iceland, the kind and quality of food was charged as mainly instrumental in its production; while in Germany, a writer was to be found who regarded compression of the fontanelles as its immediate agent. He (Dr. H.) went on to say, that recently he had referred to an excellent authority on the nature of this disorder, ("Curling's Essay,") and from him he derived this singular fact, that most of the cases terminated before the fifteenth day, and more frequently on the seventh and fourteenth. His opinions coincide with those of Dr. Stone, that we know nothing of its essential nature, and that a peculiar organisation was a necessary antecedent to its happening. He thought the whole matter worthy of enquiry, and by reference to the monthly mortuary records of this city, it would be seen that it is no stranger among us; and he hoped that the members of the society would note the disease, and endeavor to establish its essential character and its rational management.

After some other desultory conversation, the society adjourned.

A. F. A.

Abdomino-Pelvic Supporter.—We invite attention to Dr. Gans' advertisement of this convenient apparatus, on our cover. We have examined it carefully, and believe that, by keeping the weight of the intestines off an engorged and prolapsed uterus, it is calculated to give great relief to the painful dragging sensations of the patient, and to aid the employment of more efficacious remedies. We have known one or two women to have experienced considerable relief from wearing a similar apparatus.

SEMI-ANNUAL ELECTION OF PHYSICIANS AND SURGEONS TO THE NEW ORLEANS CHARITY HOSPITAL.

At an election held by the Board of Administrators of the Institution, on Monday evening, the 13th of April, the following gentlemen were chosen, to serve until November next, viz:—

Visiting Surgeons—Dr. T. Hunt, and A. Mercier.

Visiting Physicians—Drs. W. Rushton, A. Martin, Y. R. Le Monnier, R. M. Graham, A. Hester, Pecquet, C. McCormick, U. S. A., and E. D. Fenner.

We hope these gentlemen will have the kindness to furnish us every thing of interest they may meet with during their service at the hospital.

HOSPITAL REPORT.

NEW ORLEANS CHARITY HOSPITAL.

Monthly Report for February and March.

MAIN BUILDING.

February—Admitted: Males, 356; Females, 75. Total, 431.
Discharged: Males, 330; Females, 70. Total, 400.
Died: Males, 46; Females, 9. Total, 55.
Remaining on the 1st of March, 378.

INSANE DEPARTMENT.

Admitted: Males, 17; Females, 6. Total, 23.
Discharged: Males, 15; Females, 3. Total 18.
Died: Males, 1; Females, 0. Total, 1.
Remaining on the 1st of March, 66.

MAIN BUILDING.

March—Admitted: Males, 382; Females, 97. Total, 479.
Discharged: Males, 384; Females, 87. Total 471.
Died: Males, 38; Females, 5. Total 43.
Remaining on the 1st of April, 335.

INSANE DEPARTMENT.

Admitted: Males, 40; Females, 10. Total, 50.
Discharged: Males, 33; Females, 7. Total, 40.
Died: Males, 2; Females, 0. Total, 2.
Remaining on the 1st of April, 74.

MEDICAL WARDS.

We have no reports from the late attending physicians. In our occasional visits to the Hospital, we saw some interesting cases; among others, one of *hepatic abcess* of the right lobe. The tumour was so low in the right side as to render the diagnosis difficult for some time. It was finally determined to open it by means of *caustic*, which proved a tedious process. A vast quantity of matter was discharged for many days, proving a heavy drain upon the constitution of the patient, a stout young man. At length a new abscess formed in the left lobe of the liver, pointing to the epigastric centre, and he sunk before this was discharged. We did not witness the autopsy. A full report of the case from Dr. Turpin, the attending physician, would have been very acceptable.

SURGICAL WARDS.

Of the two cases in the wards of Dr. Stone, mentioned in our last number, viz., one of *traumatic tetanus*, and one of *empyema*, the former recovered entirely; the latter, after giving fair promise of recovery, was siezed with increased pneumonia, and died. The *autopsy* revealed fracture of the eleventh rib, from a fall before entering the hospital—large collection of pus within the right plura, also under the dorsal muscles—extensive pleuro-pneumonia on the left side, with effusion.

Dr. Stone has performed no capital operation here since our last report. A dislocation of the shoulder, of nearly a month standing, was very skilfully reduced after venesection and nauseating doses of tartar emetic.

The operation of tying the external iliac artery, for femoral aneurism, has been performed by Dr. Wedderburn, who will report the case in our next number.

MEDICAL COLLEGE OF LOUISIANA.

The Lectures in this Institution closed at the end of the second week in March. The number of matriculants was 103. The degree of Doctor of Medicine was conferred on the following named gentlemen:—

NAMES.	THESIS.
FROM LOUISIANA:	
M. F. Bonzano	
R. D. Barker	Conception.
Hiram M. Emerson	Malignant Tumours.
Charles McManus	Essence of Disease.
John B. Vandegriff	Dysentery.
MISSISSIPPI:	
Winfred D. Gibson	Croup.
A. F. Scott	Congestive Fever.
Joel K. Stephens	Modus Operandi of Medicines.
G. T. M. McGehee	Hysteria.
Alonzo Lancaster	Dyspepsia.
D. A. J. Lee	Dothinenteritis.
Henry Field	Malignant Intermittent Fever.
Nelson K. Leslie	Pneumonia.
John M. Butt	Rubeola.
ALABAMA:	
John B. Read	Pneumonia.
Charles A. Oliver	Bilious Fever.
MISSOURI:	
Thomas Cody	Auscultation and Percussion.
CUBA:	
Francisco V. Labarria	Pneumonia.
Antonio M. Hidalgo	Periodicity of Diseases.

Ellis' Medical Formulary.—*Correction.*—The Publishers of this Work respectfully request those persons who have the seventh edition, to correct a typographical error for the "*Medicated Hydrocyanate of Potassium,*" at page 83, wherein the symbol for an ounce is used in place of that for a drachm. The following is the correct prescription, and corresponds with the proportions directed in all the previous editions of this Work:

℞. Potassii hydrocyanici medicati, ʒ i.
Aquæ destillatæ, Oj.
Sacchari purificati, ℥ iss.

Fiat solutio.—Dose, a table-spoonful, night and morning.

ABSTRACT OF A METEOROLOGICAL JOURNAL FOR 1846.

BY D. T. LILLIE, AT THE CITY OF NEW ORLEANS.

Latitude, 29 deg. 57 min.; Longitude, 9 deg. 07 min. west of Greenwich.

WEEKLY. — 1846.	THERMOMETER. Max.	THERMOMETER. Min.	THERMOMETER. Range.	BAROMETER. Max.	BAROMETER. Min.	BAROMETER. Range.	COURSE OF WIND.	FORCE OF WIND, Ratio 1 to 10.	Rainy Days.	Quantity of Rain. — Inches.
February 28	68.5	40.0	28.5	30.44	29.82	.62	S.E.	3	2	1.949
March - 7	70.0	46.0	24.0	30.48	29.72	.76	E.	2½	1	0.720
" - 14	71.7	51.5	20.2	30.18	29.86	.32	E.	2½	2	1.605
" - 21	74.0	51.5	22.5	30.31	30.09	.22	S.E.	2½	2	1.325
" - 28	73.0	50.5	23.5	30.10	29.80	.30	N.W.	3½	2	4.750
April - - 4	72.7	54.0	18.7	30.15	29.98	.17	N.E.	3	3	7.340
" - 11	79.0	56.0	23.0	30.25	30.00	.25	S.S.E.	3½	3	1.395
" - 18	74.7	46.0	28.7	30.20	30.00	.20	E.	3½	3	0.892

REMARKS.—The Thermometer used for these observations is not attached to the Barometer, but is a self-registering one, and is placed in a fair exposure. Regular hours of observation, 8 A. M., 2 P. M. and 8 P. M.

The Barometer is located at an elevation of 19 feet above the level of the ocean, and is suspended clear of the wall of the building.

The Rain Guage is graduated to the thousandth part of an inch, and the receiver is elevated 40 feet from the ground.

On the first week of April, there fell, during 48 consecutive hours, 7.060 inches of Rain, which is the largest quantity which has fallen during the same space of time, in this city, for many years.

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