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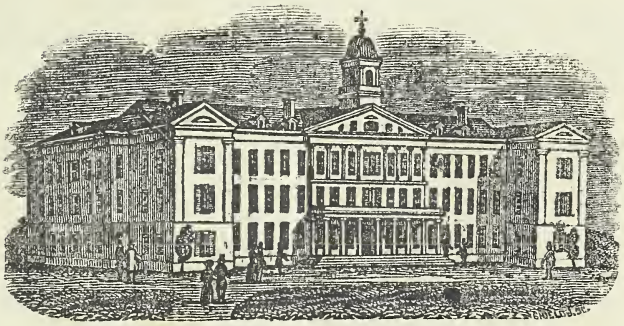
JANUARY, 1854.

A. HESTER, M. D.,

EDITOR AND PROPRIETOR.

SUMMUM BONUM MEDICINÆ, SANITAS.—*Galen*



N. O. Charity Hospital.

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TO READERS AND CORRESPONDENTS.

Our correspondents will place us under obligations, by communicating to the Journal the sanitary condition of their respective regions of country; and describe the cause, symptoms and treatment of any endemic and epidemic disease that may come under their observation. Short and practical papers are preferred.

Correspondents who may desire their papers to appear in the Journal, should forward them to the Editor at least *one month* previous to publication.

All communications should be addressed to the Editor of the New Orleans Medical and Surgical Journal, and they will receive prompt attention.

Papers for publication have been received from Dr. W. P. Reese, of Selma, Ala.; Dr. T. A. Cook, Washington La.; Dr. J. S. Davis, Salem,Miss.; Dr. E. McAllister, Port Gibson, Miss.; Dr. J. B. Hacker, Plaquemine, La.; Dr. J.U. Ball, St. Francisville, La.; Dr. E. F. P. Alexander, Neutonia, Miss.; Dr. J. C. Nott, of Mobile; Dr. W. J. Tuck, of Memphis, Tenn., Dr. J. Garril, of Apalachicola, Fa.

Since our last, we have received the following books, circulars, pamphlets, etc., etc., for review:

A practical treatise on the diseases of children. By Francis Condie, M. D. Secretary of the College of Physicians, Member of the American Philosophical Society, etc. Fourth edition, revised and augmented. Philadelphia, Blanchard & Lea, 1853. (From the publishers.)

Chemistry and Metallurgy as applied to the study and practice of Dental Surgery. By A. Snowden Piggot, M. D., late Professor of Anacomy and Physiology in the Washington University of Baltimore; with numerous illustrations. Philadelphia, Lindsay & Blakiston, 1854. (From the publishers.)

A Text-Book of Anatomy, and guide of dissections, for the use of students of medicine and dental surgery. By Washington R. Handy, M. D., Prof. of Anatomy and Physiology in the Baltimore College of Dental Surgery; late Professor of Anatomy and Operative Surgery in the Washington University, Baltimore. With 264 illustrations. Philadelphia, Lindsay & Blakiston, 1854. (From the publishers.)

Lectures on Surgical Pathology, delivered at the Royal College of Surgeons of England, by James Paget, F. R. S., lately Professor of Anatomy and Surgery to the College; Assistant Surgeon and Lecturer on Phy-

siology at St. Bartholomew's Hospital. Hypertrophy, Atrophy, Repair, Inflammation, Mortification, Specific Diseases and Tumors. Philadelphia, Lindsay & Blakiston, 1854. (From the publishers.)

On the Etiology, Pathology and Treatment of Fibro-Bronchitis and Rheumatic Pneumonia. By Thomas H. Buckler, M. D., formerly Physician to the Baltimore Almshouse Infirmary. Philadelphia, Blanchard & Lea, 1853. (From the publishers.)

Medical Communications of the Massachusetts Medical Society. Vol. 5. Boston. (From Edward James.)

Report of the Standing Committee on Surgery, read before the Kentucky State Medical Society, October, 1853. By Joshua B. Flint, Professor of Surgery in the Kentucky School of Medicine. (From the author.)

Hospital Hygiene, Illustrated. By John H. Griscom, M. D. From the Transactions of the New York Academy of Medicine. New York, 1853. (From the author.)

Essay on the Sudden Coma of Typhus and Typhoid Fevers and Typhoid Pneumonia. With Illustrative Cases. By J. Lewis Smith, M. D. New York, 1853. (From the author.)

All our Exchanges regularly.

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THE NEW-ORLEANS

MEDICAL AND SURGICAL JOURNAL.

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Part First.

ORIGINAL COMMUNICATIONS.

I.—ON THE TREATMENT OF PNEUMONIA.

BY S. AMES, M.D., MONTGOMERY, ALA.

It is now a good many years since certain defects in the ordinary treatment of pneumonia, by means chiefly of mercury, emetic tartar, and blood-letting first attracted my attention. The dissections which I then made, showed that some of my patients, dying between the sixth and tenth days, had a smaller aggregate of disease of the lungs than others who recovered had manifested, at any time in the attack, by physical signs. In connexion with this fact, of little consequence in itself, two other things were noticed: *First*, that the fatal cases, having less disease of the lungs, were attended with certain complications and a new set of symptoms, which seemed very materially to influence the progress and result of the attack; and, *secondly*, that these complications had a certain relation to the treatment. The complications were an ileo-colitis, with its attendant symptoms; a dry and red tongue, tympanitic abdomen, and diarrhœic or dysenteric stools; sometimes succeeded shortly after its advent, sometimes accompanied from the beginning by an affection of the liver and brain, giving rise to jaundice, delirium and coma. The relation of these conditions of disease to the treatment was supposed to be evinced by several circumstances. It was observed that those cases in which the treatment was begun early

resulted less favorably, as a general rule, other things being nearly equal, than those in which the treatment was begun later, and consequently was less protracted; that in the former the complications were more common, and, when not fatal, the attack was more obstinate and followed by a slower convalescence; so often indeed were these accidents presented in this apparent connexion, as to induce a good deal of doubt about the propriety of beginning the treatment early in the attack, the temptation being rather to defer it until the approach of one of those critical days on which this disease naturally inclines to terminate favorably. And, lastly, it was observed that serious affections of the gastro-intestinal mucous membrane never occurred in the beginning of an attack, nor indeed at any time in the progress of it, before any treatment was begun. Later experience has satisfied me that, however common may be the evidences of some degree of irritation in some part of the digestive canal, an active inflammation is naturally an exceedingly rare complication of pneumonia.

Thus, the facts seemed very obviously to lead to the inference that these complications were produced by the deleterious agency of the remedies, or some one of them employed in the treatment. At the same time the nature of these new conditions of disease, in connection with the well known toxicological properties of the medicines, while it served to confirm the former inference, pointed to the mercury and antimony as the only agents concerned in producing them. Further observations, however, seemed to be required, in order to determine the relative importance of these agents in bringing about these results; whether one only was concerned, or their joint action was required. Without entering into the details of this part of the inquiry, further, than to state that the method employed was that of occasionally leaving out of the treatment one or other remedy, it will suffice for the object in view to give the positive results obtained after some years appropriated to the investigation. It seems that while either mercury or antimony are capable of superinducing these forms of disease when administered in pneumonia, the accidents arising from the one are less frequent and somewhat different from those arising from the other; the latter, however, being equally formidable when they do occur. An ileitis, or gastro-enteritis is most common, and is the usual result of poisoning by tartar emetic. Mercury, on the other hand, may induce an inflammatory state of the intestinal mucous membrane, less frequently, if ever, involving that of the stomach, and more frequently that of the larger bowels, and occasionally, it may be, about the same time the liver and brain. When the two medicines are given together

the resulting complications are apt to involve all the structures mentioned, and, it may be added, are more likely to occur.

It is, perhaps, unnecessary to say that these complications were proved to be of the most formidable nature, always aggravating the pulmonary disease, and rendering it less amenable to treatment, they not unfrequently led to a fatal termination, when death most probably would not have occurred from the pulmonary disease alone.

Finding that the relation of cause and effect between these medicines, and the complications existed, there still remained a further inquiry respecting certain facts connected with these agents and their effects, which the mere existence of this relation did not account for. The facts are, that although the symptoms of these complications are little else than the expression of the usual toxicological effects of mercury and antimony on the gastro-intestinal mucous membrane, and on the functions of the liver, when given in small doses and long continued, they have, notwithstanding, been observed to occur much more frequently in pneumonia, in this climate than in some other climates; and, in this climate, much more frequently in pneumonia than in some other diseases. Now it is plain, that if the toxicological action of these two agents were alone concerned, this irregularity in their operation could not happen. There is then some other cause in operation besides the mere poisonous properties of the remedies. The immediate subject of inquiry is as to the nature of this cause? The properties of the agents and the nature of their effects lead us to seek it in the condition of the alimentary canal and of the liver. And here we are reminded, that the effects, very similar to those which take place in pneumonia, have been, perhaps, more frequently observed to take place in our endemic periodical fevers; and hence it may be inferred with great probability, that whatever may be the cause of the frequent development of the toxicological action of these medicines in the one, is also the cause of the same effects in the other. Now, it is well known that a state of irritation of the intestinal, or gastro-intestinal mucous membrane, and of functional excitement in the liver, are almost uniform conditions of our endemic disease. These conditions are indeed so general, that we may very properly conclude their production to be a law of the remote causes, whatever may be their nature, of the most important of our acute endemic diseases. Sometimes these conditions are openly manifested, while at others they are latent, existing rather as a tendency to take on inflammation in the membrane and functional excitement in the liver, when the former is acted on by irritative injestæ, whether of food or medicine, or the latter by its peculiar

stimulant—mercury. It is on account of the condition of these organs in our endemic diseases, that tartar emetic has never become established as a remedy in any of them except pneumonia, and in fact, also, that mercury has been for a great length of time slowly falling out of use, particularly in periodical fevers, when its deleterious effects are most obviously and most frequently displayed.

That pneumonia falls within the operation of this causative influence might be inferred from the similarity of the effects of these two irritant poisons in this and in other endemic fevers; and we might, I think, rest on this inference as on an assured fact, if there were no direct evidence to support it. The induction, however, is supported, nay verified, by some direct evidence in the primary symptoms of this disease. While it is true, that the gastro-intestinal irritation is not so often shown primarily by prominent signs, such as nausea, epigastric tenderness and diarrhœa in pneumonia as in periodical fevers, yet there is still sufficient evidence of its actual presence, though, as it were, latent, in the state of the tongue, and the uniform susceptibility of the bowels to the action of purgatives; and so too in regard to the evidences of over action of the liver, the signs of it are generally obscure in pneumonia, hardly at all manifested, unless vomiting or purging are provoked by medicines; but at times there are cases of pneumonia, in which the evidences of a bilious diathesis are very prominent, and in all, this diathesis is more or less manifest, either by the primary symptoms, or by the immediate action of emetic or cathartic medicines. But I need not dwell on this point, the facts being, no doubt, familiar enough to the reader. Assuming, then, from all that has been said in this connection; in the first place, that the production of a certain state of irritation in the digestive tube and liver, obvious or obscure is a law of the cause or causes of our acute endemic diseases; and in the next place, that pneumonia is no more exempt from the operation of this law than other endemic fevers, we are enabled to refer the more frequent occurrence of injurious effects from these medicines, and their greater activity, in a Southern climate, to the state of irritation so induced as their immediate antecedent, or cause, and thus to arrive at a satisfactory explanation of a number of well ascertained facts which otherwise seem inexplicable. The difficulty of tracing the frequency of these effects to its proper source hitherto, has consisted in two things; first, in the frequent obscurity of the signs indicating a proneness in the organs affected to take on inflammation, or a high degree of functional activity; and, secondly, in viewing the facts in regard to pneumonia in an aspect of isolation, and not in connection with similar facts common to several

varieties of endemic disease, and, as we conclude, referrable to the same cause.

We take up now the subject of *blood-letting*, which for the sake of perspecuity, as well as on account of its distinct nature and effects, required to be treated of separately. The objections that I have noticed in the course of my experience, to its employment to any great extent in pneumonia, have been presented in two aspects. In the first, a considerable mitigation of the symptoms has been obtained, which, lasting but a few hours, has been followed by a reaction in which the disease has passed beyond the point of severity it had previously attained; that is to say, the pulse became in the reaction more full, frequent, and sometimes harder; the respiration increased in frequency, while the restlessness and general feeling of malaise were aggravated. Along with these outward signs, there was commonly a more or less evident extension of the limits of the diseased parts. In this manner, the effect of one bleeding was to render the indications for another more urgent, and if repeated, it was again followed by temporary relief and an ultimate aggravation; results which I have known to follow repeated diurnal or semi-diurnal bleedings until the near approach of a fatal issue arrested the treatment. Such effects from bleeding, there is reason to believe, are not confined to any particular climate or locality. Of this, any one, I think, may satisfy himself, who will study carefully, in reference to this matter, the published clinics of the European hospitals, where he can hardly fail to find examples of this kind.

These results, however, have not there been attributed to losses of blood, but to other circumstances connected with the natural course of the disease. And here the question may be asked, how has it been ascertained that the latter opinion is not the correct one, or that the real error does not lie in attributing to bleeding what properly belongs to the disease itself? The answer is, and it will equally apply to what has been advanced in regard to the deleterious agency of other remedies, that this question has been submitted to that kind of test which logicians call the method of difference; that is, the circumstances of the disease being as nearly as possible the same, the supposed cause, namely, the bleeding, has been withdrawn from the treatment and the result noted. This test so certain ordinarly, is not so in this instance, unless greatly extended as regards time, and the number of cases observed. If the symptoms attributed to bleeding were uniform attendants in the cases bled, the case would be different, for then it would only be necessary to set aside the remedy in one, or at most a few cases and note what followed. But being only an occasional, though not an

unfrequent occurrence, it is impossible to say in one, or in a few cases, that the absence of the symptoms had any necessary connection with the suspension of the remedy. Hence arises a necessity, in order to arrive at any great accuracy in the conclusion, to apply the test, if successful in a few, to a great number of cases, and through a series of years. The test has been so applied; and the observations of many years have satisfied me, that sudden and violent changes for the worse, of the kind here spoken of, do not occur in the progress of pneumonia, unless bleeding constitutes an essential part of the treatment. Perhaps it would be more accurate, instead of stating the conclusion in such general terms, to say that under my observation they have not occurred in one hundred and thirty-two case, not bled.*

In the second of these aspects, no mitigation of the symptoms followed the bleeding, the immediate effect being to quicken the pulse and to enfeeble it; and, though there might be a subsidence of pain, the condition of the patient was in other respects altered for the worse. I take for illustration a single example of this kind of change from my note book now before me. A young man of good constitution, and before this attack in good health, was bled at 12 M,, on the fourth day of September, 1843, being the third day of his illness, to twelve ounces. While the blood was running the pulse rose to 120, becoming small and soft; no faintness; half an hour afterwards, pulse 132; at 6 P.M., pulse 125. The disease occupied the lower and middle lobes of the right lung; a part only of the diseased portion having passed into the second stage.

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* Some weeks after writing this and the preceding paragragh, I met by accident, and so far as I remember, for the first time, with an extract from Lænnec, which fully confirms my opinions in regard to the effects of bleeding in pneumonia. It is contained in Mr. Guthrie's tenth lecture, "On some of the more important points in Surgery," published in the June No. of the London Lancet, for this year (1853.) The most valuable remark of Lænnec, says Mr. Guthrie, on its use, (*tartar emetic*) is, "that by bleeding we almost always obtain a diminution of the fever, of the oppression, and of the blood expectoration, so as to lead the patients, and the attendants to believe that recovery is about to take place; after a few hours, however, the unfavorable symptoms return with fresh vigor; and the same scene is renewed often five or six times after as many venesections. On the other hand, I can state that I have never witnessed these renewed attacks under the use of tartar emetic."

The common opinion, however, in regard to the cause of these changes is, I believe, correctly stated in the text, and I have not therefore, thought it best to alter it.

The unfavorable effects of this remedy have not appeared to be governed by any specific circumstances that can be appreciated in individual instances, so as to enable one to determine before hand the probability of a good or bad effect from it, independently of those general rules which apply to all climates, and to all diseases in which the remedy is employed.

But if the condition on which such effects immediately depend are inappreciable, or undiscovered, the more remote causes are not perhaps altogether so. The general experience of physicians is, that the loss of much blood is not so well borne, nor its curative influence so favorably exerted in this as in Northern climates. If this be generally true, of which I think there cannot be much doubt, there seems to be two causes in operation which make it especially true of pneumonia. The first concerns that class of our population most liable to its attacks, namely, the blacks, whose nervous and muscular tone, or force, is more easily acted on by depressing influences than that of the whites; requiring more animal food to sustain it in health, and giving way more readily to the impressions of cold, or fatigue, and, in disease, to any kind of active depletion. The second concerns a peculiarity in the disease itself common to both races. The peculiarity consists in this, that the disease hardly ever lingers in the first stage, but presses on to the second with a degree of rapidity which, while it constitutes the rule with us, is the exception in colder latitudes. It is not uncommon, for example, to notice the rust colored sputæ, with a well marked dullness, bronchial respiration, and bronchophony in some part of the diseased structure, sometimes over a large part of it, within the first twenty-four hours. On one occasion, I saw the characteristic sputa at the very beginning of the attack, even before the cold stage which ushered it in had passed off. It is much more common than otherwise, to find the physical signs of the second stage fairly developed on the second day. And thus it happens, the greater number of first visits being made on the second day, that when the physician come to prescribe, he finds his patient in that stage in which the propriety of bleeding at all, is held by many to be more than doubtful, and by all to be much less effective for good.

Whatever may be the influence of these causes, in modifying the effects of bleeding in this climate, and among a slave population, certain it is, that any very obvious and permanent, as well as immediate benefit, is seldom obtained from it in this disease, while it is sometimes obviously and immediately injurious. Hence, it has come to be very sparingly employed by much the greater number of physicians of expe-

rience in this section of the country, and many dispense with it altogether; while among those who bleed most, not one, I suppose, could be found who ever entertains a thought of following out the *coup sur coup* plan, which Bouillard, it would seem, found so effective in France.

That either, or all of these potent remedies were frequently beneficial, I was not permitted to doubt. That they were often extremely deleterious in the ordinary way of using them, seemed to me to admit of as little doubt. The latter conclusion led to, and, I think, justified, (a matter, however, which I leave to the casuists in medical ethics) the institution of a series of observation in order to determine, first, the possibility of so employing them as to obviate their injurious effects, and afterward, failing in this, the practicability of finding efficient substitutes less liable to those contingencies, which, according to Dr. Boling,* referring, however, chiefly to antimony, are quite as formidable and fatal as the disease itself. It is not necessary to the object of this explanatory introduction to follow up in detail the successive steps, running through a number of years, by which slowly and even reluctantly, first one and then another of these old and familiar remedies were laid aside and others substituted. Suffice it to say, that after the conclusion was come to that a change of remedies was necessary, neither of the established ones was rejected suddenly or capriciously. The first steps in the experimental inquiry encouraging me to proceed, blood-letting and mercury came to be used only in certain circumstances, (the former, when the breathing was not only frequent but embarrassed by excessive pain, the latter, after the acuteness of the febrile symptoms had subsided, and the physical signs persisting, the disease threatened to become chronic,) and afterwards were dispensed with altogether. It is now about seven years since I have drawn blood in any mode, or prescribed mercury in any form or dose, in the treatment of this affection. Tartar emetic, by far the most valuable remedy of the three, was continued longer, because it was found on trial that its poisonous effects could generally, though not always, be

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*"Among the cases of pneumonia which we have treated with tartar emetic principally, we do not hesitate to say, that half as many deaths have occurred in consequence of gastro-enteritis—induced seemingly by the remedy—supervening during the progress of the disease, or at the moment of apparent convalescence, as from the primary disease itself."—*N. O. Medical and Surgical Journal, vol. 5, p. 291.*

avoided by giving it in large doses, repeated at long intervals, (two grains every third or fourth hour, dissolved in at least two ounces of water) and suspending it during eight or twelve hours at night. This, however, with the others was finally laid aside, giving place to a contra-stimulant more prompt and efficient in its action, and at the same time divested of all the deleterious qualities of the other.

The treatment of pneumonia, then, which was finally settled down on somewhat more than four years ago, and since, with some slight modifications, steadily pursued, consists in discarding the three principal remedies in common use, and substituting others in their stead, after the following manner:

On visiting, for the first time, a person of adult age having pneumonia, in the first or second stage, pleuro-pneumonia, or pneumo-bronchitis, I make the following prescriptions:

℞	Tinct. Aconitum Napellus (saturated)	gtt. xii.
	Quinine Sulph. vel. Ferro-Cyan.,	gr. xxxvi.
	Morphia Sulph.,	gr. i.
	M. ft. pil. xii.	
℞	Solution of Phosphorus,	gtt. xvi.
	Water,	℥ iv. M

Of the first, two pills are directed to be taken every third or fourth hour, usually every fourth, each dose being preceded one or two hours by a teaspoonful of the phosphorus mixture. If an anodyne be required in addition to that contained in the pills, a quarter of a grain of morphine is given at bedtime. If the disease is in the first stage, the beginning of the second, or, after the second stage is fully developed, if there be much pain, not yielding permanently to anodynes, a large blister is directed to be applied over the seat of the disease. Such is the outline; the details will be given in speaking of the remedies separately.

The preparation of Aconite used, is a saturated alcoholic tincture, made by percolating through a pound of the bruised root alcohol enough to make a pint of tincture. This obtains, if the root be of the right species, is unmixed, and not too dry or too long gathered, a stronger tincture than that of Dr. Fleming, of London; whose valuable paper on the therapeutic and toxicological effects of this drug first suggested to me its employment as a substitute for bleeding and antimony. The dose advised, two drops, may be considered a medium dose, when made up into pills according to the prescription, or a full dose if given in water. I speak here as well as in what follows of the tincture made from the best specimens of the root. But as our druggists hardly ever

get two successive parcels of equal strength, the dose requires sometimes to be raised to two and a half or three drops. This difference in activity is partly owing to a difference in the varieties, of which there are several, but also in part to the mode of cultivation, and the length of time it has been gathered, and the amount of exsiccation it has undergone. I am not familiar enough with the physical qualities of the several varieties of the A. Napellus, to furnish the means of deciding, by an examination, of a given specimen, what degree of effect is to be expected from its administration; the effect in kind, I believe, is the same, or nearly so, of every variety of this species. It is desirable, however, to obtain some guide in this respect, and though none may be afforded by the root itself, there is a mode of testing the activity of the tincture, which serves a very good purpose in selecting it for use. The best tincture, diluted in the proportion of an ounce of water in sixteen drops, taken into the mouth in a small quantity, produces a burning in the tongue and lips, with a feeling of tingling and numbness, and a loss of taste; the sensations lasting from two to eight hours. Diluted with twice this proportion of water the same effects follow, though less actively and durably. This test can be depended on only to a certain extent,—an article requiring, for instance, three drops for a full dose, could hardly be distinguished in this way from one requiring only two. One is able, however, to decide by it at once, between a good and a bad preparation. If a few drops of the tincture, diluted as first mentioned, produces no burning, no tingling, when applied to the tongue, the specimen should be rejected without hesitation; and so also of the weaker dilution, if a teaspoonful be taken into the mouth and retained there but a moment. Other things being the same, the root making a dark-colored tincture, is not so active as that which gives it nearly the hue of Madeira wine. A still more effective test may be found in a few tentative doses. If in a trial of this kind, the tincture has first been tasted, and found to produce the effects described, the experimental dose should not exceed, at first, two drops. No danger, certainly, is to be apprehended from a dose two or three times as great, but the effects of two drops are sometimes very disagreeable and even painful.

The curative influence of this medicine, though by no means dependent on doses sufficient to produce any poisonous effects, is, nevertheless, the more promptly exerted in proportion as the latter are developed within certain limits. It is desirable, therefore, to give enough, or to repeat the dose often enough to induce some nausea, or slight vomiting, particularly in the first stage of pneumonia. I believe, it is not needful to go farther, as a general rule, in order to get the best effects of the

remedy, without, at the same time, harassing the patient with its sickening influence. On some occasions, however, when the attack wears an unusually threatening aspect a more decisive impression may be required; and in such a case, it is better to repeat the dose more frequently, rather than enlarge it much. Under ordinary circumstances, then, let us say the development of the toxicological action of the remedy, to any great extent, is not desirable; for if the dose be too large or too often repeated, its effects may become exceedingly distressing to the patient, and alarming also to him and his friends. In such instances there occurs, more or less suddenly, a feeling of great prostration of strength and of sinking; coldness, palor, and profuse sweating of the skin; pretty constant, though not painful nausea; frequent efforts to vomit; purging; a slow, feeble, and thready pulse; and sighing respiration; to these symptoms there are added, a dryness, or rather the feeling of dryness, and constriction of the throat; burning, tingling and numbness in the mouth, and numbness in the skin of the hands and feet, and frequently over other parts of the surface. These symptoms I have witnessed, to the full extent described, in two instances, both adults, from a single dose of three drops, given in water. Hence, I have adopted the custom of beginning with a dose of two drops, in the pill form, and one and a half drops in water, increasing the latter to two drops in the second dose, and still more in the third, if required. The poisonous effects from the largest dose spoken of, even if very actively developed, are only to be dreaded on account of inconvenience to the patient; they always pass off in a few hours, never exceeding eight, without leaving behind anything injurious or unpleasant.

Children bear somewhat larger doses, in proportion to age, than adults. A child, six or eight months old, can generally take one-fourth of a drop without inconvenience, and one of twelve or eighteen months will frequently bear a third or half a drop; having reference in all that I have said, of the dose for adults and children, to its repetition, except in cases of unusual violence, at intervals of not less than three hours; but in no circumstances ought the intervals to be less than two hours; in children, I am not sure that it ought ever to be repeated so often. Ordinarily, its repetition once in four hours is sufficient.

The best effects of this remedy in pneumonia, like bleeding, is exerted in the first stage, or that of capillary repletion. After the second stage is completed, throughout the greater part of the inflamed structure, though not at all doubtful as a remedy, nor, indeed, any the less efficient or certain in its curative action than before, the latter is usually less promptly exhibited as regards both the rational and the physical

signs. As a substitute for bleeding, it seems to possess several other advantages. While it reduces the force and frequency of the pulse with greater certainty, though somewhat less speedily, its action in this respect may be kept up for any length of time required, without fear of present or subsequent injury from it of any kind; if suspended, there is no tendency to any violent reaction in the circulation, nor, indeed, to any speedy febrile reaction at all, the pulse coming up to the natural standard, after having been brought belows it, very slowly. Hence, if it be thought desirable for any reason to suspend its administration during the night, no fears need be entertained of finding the pulse materially accelerated the following morning. It may be added, in concluding this part of the subject, that, convalescence is never retarded through the influence of this remedy, and that unlike bleeding, it is safe, as well as efficient, in all circumstances of the acute disease, if used with but ordinary caution. If any serious harm results from it, the fault, I will venture to say, will lie with the physician and not with the remedy.

Phosphorus, it seems, from the accounts given of it in the books, is not a new remedy in inflammatory affections. It is said to have been employed in several diseases of this class, and among the rest, in pneumonia, but precisely under what circumstances, and with what success I have not been able to learn. It is generally acknowleged to be a remedial agent of great power, and available in the treatment of a great variety of morbid affections. It appears to have gone out of use on account of the dangerous and even fatal consequences resulting from its employment in the large doses usually recommended. It is proper to say, that I am indebted to the representations of its value by my friend, Dr. James Berney of this city, for my employment of this remedy in diseases of the lungs.

The solution referred to in the prescription is a saturated solution in anhydrous alcohol, diluted by nine additional parts of alcohol. This diluted solution is preferred for several reasons: It is not liable to undergo waste or change from exposure to the air like the stronger solution, which gives out a vapour of phosphorous every time the vial containing it is opened, this vapour, combining with oxygen of the atmosphere, forms hypo-phosphoric acid, a part of which being absorbed into the solution, may, to some extent, alter its medicinal as well as its chemical qualities. The weaker solution mixes better with water, and the dose for children as well as adults is more easily regulated with it. This last consideration, it will be seen presently, is of great importance. The dose recommended is the smallest that I am in the habit of using,

and is the one I prefer after many trials of larger ones. Authors recommend the oily solutions to be given in doses of from two to ten drops. Each drop containing about the one-hundred-and-fiftieth of a grain, the dose is found to vary from the seventy-fifth to the fifteenth of a grain. In these doses, small as they may seem, it is spoken of in many instances, as a dangerous and uncontrolable remedy, and cautions against mischief from it are everywhere numerous, urgent, and impressive. Dr. Chapman, referring to doses of a sixteenth of a grain, says—"Whatever may have been the degree of its utility, this appears to be fully balanced by the hazardous nature of the medicine, and the positive mischief which is acknowledged to result from it. Even in the very small doses of the prescription above, though always safe, and generally free from any unpleasant consequences, it occasionally produces some very sensible effects on the head and stomach. Given in what I suppose to be the minimum dose of authors, the seventy-fifth of a grain, these effects, according to my experience, are not only frequently produced, but with such additional activity as to require the suspension of its administration, and the dose on renewing it to be greatly reduced. The same thing is occasionally true of doses of one-fourth the quantity. Dr. John McLester, Dr. Taylor, Dr. Hinkle and Dr. Oliver, all of whom have witnessed its effects, in doses varying from half a drop to two drops of the saturated solution, (from five to twenty drops of the diluted solution,) concur with me in the opinion, that the medicine cannot be continued in the smallest quantity just mentioned for any great length of time without inducing considerable disturbance of the stomach, shown by nausea or vomiting, burning heat and a feeling of oppression at the epigastrium; and that in the larger quantity, though a single dose, or perhaps a few doses, may be given with impunity, it cannot be continued for any great length of time with ordinary, or at least with a proper exercise of prudence. If therefore we suppose, as is probable, that anhydrous alcohol will hold in solution as much phosphorus as the æthereal or other oils, it is seen that the minimum dose of authors is much too large for ordinary use in diseases that require a frequent repetition of remedies, and that much watchfulness is required to render it even safe. So far in regard to its activity merely; but in estimating the proper dose, several other things require to be taken into consideration, having reference to certain peculiarities in its operation: First, the eccentricity of its action as a poison; thus while it is said on good authority to have been given, at times, in doses of several grains without doing serious mischief; at other times less than one-tenth of a grain (six milligrammes) has been known to prove

fatal.* Secondly, its effects are cumulative; that is to say, a dose which singly is not large enough to produce any sensible effect, may become very troublesome, or even dangerous after several repetitions, at intervals of three or four hours; this quality was develo ed in one instance by repeating it, in a dose of two drops of the strong alcoholic solution, three times, at intervals of twenty-four hours. Thirdly, unlike most other therapeutic agents, its medicinal and its toxicological action are, in a certain degree of the development of the latter, antagonistic; so that in proportion as its toxicological powers are brought into active exercise, so are its medicinal virtues diminished, and thus it is found that its curative effect is not in the ratio of the quantity administered, except within much narrower limits than are prescribed for other poisonous remedies. It may be concluded, therefore, from all that has been said, that if it be desirable, as in this instance it certainly is, to obtain the curative, without danger of developing the poisonous properties of this agent, the first object can be very surely attained by giving it in nearly the doses recommended, small and even minute as they certainly appear, when the activity of the poison is not fully appreciated, while the evil can be hardly avoided by giving it in much larger ones.

In dwelling so long on this subject of the proper medicinal dose of phosphorus, I have been influenced by the double conviction, in the first place, of its great value as a therapeutic agent in other diseases, as well as in the one now under consideration; and, in the next place, of the facility of doing mischief, by a careless or improper use of it. There is another reason: Both the nature of the remedy, as such, and the limits of safety in giving it, have hitherto been greatly misunderstood, and consequently greatly misrepresented; a matter which I shall have occasion to refer to again presently.

If the medicinal qualities of aconite adapt it more especially to the first stage of pneumonia, so, it may be said, those of phosphorus recommend it more particularly in the second and third stages. Given alone in the first stage, it is occasionally effectual in arresting the further progress of the attack, but cannot be depended on for this purpose

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* *Cazenave.* But this author thinks that in all cases in which such large doses have been given without harm, the article had undergone some change in its chemical state, which rendered it inert. "Si l'on a pu dire qu'il a été administre avec innociuité à la dose de 3, 4, 5 et 6 decigrammes, on doit croire que, dans ces cas, il y avait décomposition et change dans son êtat chemique." My own experience of its effects most certainly leads to the same conclusion.

with nearly the same confidence as aconite alone. More frequently it does not prevent the second stage from forming, and decided signs of amendment are commonly deferred to the fifth or sixth day. While either phosphorus or aconite, within my experience, is more efficient in either the first or second stage than any other single remedy, it is nevertheless desirable to obtain their combined action, as being more efficient than either separately. In regard to the value of phosphorus in the third stage, I can speak only from a very limited observation, and that chiefly in cases under the treatment of others; but so far as this goes, the result has been highly favorable.

In concluding what I have had to say of phosphorus, I wish to add a few words concerning its therapeutic qualities and what I conceive to be the mode of its operation. In doing so, I hope to place this article in class with those articles of the Materia Medica with whose qualities it is most closely assimilated, and where, consequently, it properly belongs.

Phosphorus certainly acts as an expectorant, with great promptness and efficiency in pneumonia and bronchitis, in some forms of asthma, and in the bronchitis of asthmatic subjects. It is also an effective remedy in irritation about the neck of the bladder; in chronic or subacute inflammation of the inner membrane of the urethra and bladder; and also, as a diuretic in dropsy. Its action on the lungs seems, from its effects, to be directed especially to the minute bronchial tubes, and the air cells; and in inflammation, more especially to the capillary vessels than to the heart. In all this we recognize a kind of speciality in its operation, which likens it to a number of medicines, such as mercury, cantharides, digitalis, and others, which seem in the same manner to act on certain organs, parts of organs, or secretions, in preference to others. But we may, I think, proceed a step further, and inquire into the mode of its curative action.

It is well known that under certain circumstances the remedies for inflammation are required to be of a stimulating nature, not merely locally, but generally, and may be required to be of the most active and diffusible kind. It may be added, that in one point of view nearly all the most efficient remedies for inflammation are stimulants. The pathology of inflammation explains this seeming paradox. The phenomena of inflammation are now known to be derived from an engorgement, or "repletion in excess" of the capillary vessels carrying blood; the repletion being itself dependent on a deficiency in the organic, contractile force, which in health propels the blood, in part at least, through these vessels. This force is to a considerable extent

regulated (not imparted) by the nervous influence carried along the nerves distributed on the vessels; consequently the organic force, under influences derived from the nervous system may be impaired, or aggravated, or possibly otherwise disturbed by causes acting either generally through the nervous centres, or locally on the nervous filaments themselves. Now, in this view of the proximate cause of inflammation, (engorgement of the capillaries) its immediate antecedent being a deficiency of contractile force in the capillaries, remedies for inflammation ought to be stimulants, at least in their local action on the part inflamed. This is indeed true of nearly all the so called contra-stimulants, and would be true of all remedies for inflammation, if the organic force of the capillary vessels were the only force concerned in circulating the blood. Taking, however, the contractile power of the heart into consideration, we have to add to the list some remedies, active and efficient ones too in some circumstances, which are in no sense stimulant. These act exclusively to reduce the injecting force of the heart; and these alone, therefore, in a pathological classification of remedies for inflammation, can be ranked as pure sedatives. The list of this class is small; blood-letting being its best representative, and veratrum viride, and digitalis, probably next in rank. Leaving these out of consideration, because the received views of the pathology of inflammation being admitted, their operation must be indirect, and merely adjunctive to the others, we may, for the purpose of better understanding their relation to this branch of pathology, divide the others into three classes.

The first that I shall mention belong to the diffusible stimulants. Their operation is on the nervous system generally, increasing by this means the energy of the action of the heart, and of the secretions, and at the same time augmenting the power of voluntary muscular contraction. They have no especial local action on the capillary vessels; or if any, they tend rather to relax them. They are applicable only in those states of the system, where local inflammation co-exists with a depression of vital power, a deficient action of the heart, and of the nervous force of the capillaries; the organic force of the latter remaining normal, at least in its capacity to act. They are injurious in all cases of inflammation where the injecting force of the heart is equal to the propulsion of the blood through the capillaries in their normal state; the vis a tergo imparted by them in such cases increasing the repletion, while the agents themselves exert no compensating effect on the local organic force of the capillaries. In this respect they differ from

the class next to be spoken of. The best representative of this class is alcohol.

Those of the second class are also medicines which stimulate the nervous system generally, and through it the heart's action also, but moderately, but at the same time have an especial action on the organic force of the capillary vessels. Thus the local nervous power, stimulated through the nervous centres, and the stimulus of these medicines to the organic force of the capillaries co-operate in doubly compensating for the disadvantage of the slight additional injecting force of the heart imparted by them. This class includes a great number of individuals, and may be subdivided into several groups, as, first, tonics, quasi stimulants, such as mercury, iodine, colchicum, and others; secondly, narcotics; thirdly, the pure tonics; and lastly, some of the astringents.

The third and last class consists of such medicines as combine the properties of a sedative to the heart's action, and of a stimulant to the contractile force of the capillaries. These properties make them, as they have proved to be in practice, especially applicable to and efficient in acute inflammation and fevers; though applicable in all cases whether chronic or acute, in which the vital power, and the force of the heart's action are equal to, or above the standard of health. In this class may be placed in the order of what I conceive to be their relative value in acute inflammatory affections generally; first *aconite;* secondly, *antimony;* thirdly, *phosphorus;* fourthly, *quinine*. *Aconite* takes precedence of all others, because, so far as my experience goes, besides its greater efficiency, its application does not require to be limited by any peculiarities in its operation, nor by the character of the organ affected. It is proper to add, in connection with this last remark, that my experience in its use is limited to inflammation of the brain and its meninges, of the throat, of the lungs and pleura, peritoneum, intestinal mucous membrane, whether attended with dysentery or diarrhœa, rheumatism, chronic and acute, rheumatic gout, erysipelas, acute cornitis and conjunctivitis. *Antimony* comes next in order, because so far as any thing is known of phosphorus, the former has the advantage of being available in a greater variety of inflammatory affections, although it is believed to be much less efficient in those to which both have been successfully applied. *Phosphorus* is put in this class solely because of my own experience, and that of a few others, of its immediate sedative, or contra-stimulant influence on the general circulation, when given in a dose large enough to produce any sensible influence of any kind on the action of the heart, but still not large enough to excite inflammation or

a high state of irritation of the stomach and bowels. Its sedative or contra-stimulant, is its medicinal or therapeutic effect. Its poisonous effect is the reverse of this, namely, highly stimulant by reason of the local inflammation it excites. In this way is brought about the antagonism between its effects in large and small doses. There is a point at which it ceases to be medicinal or sedative, and becomes poisonous or stimulant. Thus it is not possible to produce by it the extreme depression which follows large doses of aconite; for when the dose is enlarged for this purpose beyond a certain point, a new and opposite action is immediately set up, by which the power is lost or merged in the local inflammation and its concomitant influence on the nervous system and the general circulation. In pneumonia it is not often that the quantity in which I usually prescribe it exerts any immediate influence in reducing the frequency or force of the pulse. Its action is more slowly developed in this respect than either of the other remedies included in this division, but at the same time its remedial influence is more certainly and uniformly obtained than that of antimony or quinine and hardly less certainly than that of aconite.*.

The reader is aware, no doubt, that the properties here ascribed to phosphorus do not accord with those ascribed to it by the profession generally. It is said by all those who have published an opinion about it, so far as I know, to have no other therapeutic qualities than those of a diffusible stimulant of the most active kind; and I suppose the impressions of nearly all others who have given any attention to the matter, accord with what has been published about it. Perhaps the only exceptions are to be found among a few professional gentlemen near me, who, guided alone by their own experience, concur with me on this point. And here, in alluding to this discrepancy, I wish to say, that I am fully sensible of the responsibility of uttering as a new medical fact, that which is opposed to the standard authorities in medicine, and to the established opinions of the great mass of the profession.

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*Two young gentlemen, my personal as well as professional friends, have been recently engaged in some experiments to test the effects of phosphorus on persons in health, they themselves being the subjects of the experiments. These gentlemen (Dr. Pollard and Dr. Oliver) found that a single dose of two drops of the saturated alcoholic solution invariably reduced the force and frequency of the pulse. The changes in frequency ranged, in the number of pulsations to the minute, between eight and twelve beats. A change was perceptible within about twenty minutes, which reached its maximum in from an hour to an hour and a half.

Certain, however, of the correctness of my own observations, to say nothing of those of my professional friends just alluded to, whose capacity to observe and truthfulness do not, to my conception, admit of a shadow of doubt, I have no hesitation in stating the result of those observations, being satisfied that a more widely extended experience can but the more certainly correct the common error. This common error, as I have unhesitatingly assumed it to be, is a remarkable one, and deserves, I think, something more than a passing allusion to it. It is remarkable in this, that relating to a matter of fact, viz., the medical properties of a remedy; one of pure observation, or at least, one which could truly be derived only from observation, it had its origin in a community of scientific observers, obtained universal credence among them, and has held its place there through several generations unquestioned; in this time a great deal has been written about it, and of course a great deal observed, such is the inference, and thus it has all the time been liable to instantaneous correction, without having been corrected. But is the mistake really one of observation as it appears to be? Let us see if the medical history of phosphorus does not afford some explanation less discreditable to medical experience, and to medical authorities?

And first as to the way it was introduced. No one, it seems to me, can read much of what has been written on phosphorus without coming to the conclusion, that its medical action had not been studied with the caution required by the nature of the agent, nor with the care due to the successful introduction into practice of any new remedy. On the contrary, it appears to have been rather suddenly introduced, soon after its discovery, not on account of any observations, accidental or otherwise, of its medicinal virtues, but rather from a priori considerations connected with its ascertained poisonous properties, and its inflammable nature as a chemical agent. As a poison it is a stimulant; as a chemical agent it is highly inflammable. The reader will remember that the principle of phlogistion, (to inflame) existing in dead matter, and that of inflammation in living matter, were held to be identical. Now as phosphorus contained more of this principle than any other substance in nature, and observing its effects as a poison, it was natural enough to suppose it to be an active stimulant under all circumstances, capable of imparting its *phlogistion* to the living body, and thus to excite an active *phlogosis*, or inflammation, local as well as general; it was in fact inferred, a priori, both from its combustible or inflammable nature, and from the symptoms induced by poisoning with it, to be a powerful *phlogistic* agent. Such, in brief, seems to have

been the reasoning; and the conclusion thus obtained could not fail to be confirmed by the experience obtained by the mode in which it was administered; and thus what is known of it has come down to us with a prestige of authority which in appearance is hardly questionable, and still confirmed, no doubt, by later and later experience to this time. In truth it could not be otherwise, so long as physicians acted under the weight of subsequent authority, which supports the original opinion of its properties. Being prescribed, or recommended, chiefly, perhaps only, in those states of disease which required or was supposed to require active diffusible stimulants, the quantities necessary for exciting this condition were given. The consequences were deplorable; but at the same time, the fault seems to have been, not in observing wrongly, but in binding down the observations to a hasty and altogether untenable hypothesis. Latterly, since the article last went out of use, the opinions of the older authors appear to have been blindly copied by their successors; or if their truth has been tested, it has been under the same influences, with the same object, and in the same doses. On the whole, therefore, I think it may be safely concluded that the true properties of phosphorus have never been put to the test of unbiassed observation, by any considerable number of physicians.

In view of the dark episode in medical history exhibited in the use of this medicine, we are not permitted, therefore, to wonder that the hopes excited by the hypothetical notions entertained of its qualities were never realized, nor that its use was speedily abandoned after a first trial; nor for a moment to regret. that, taken up again and again with renewed energy and hope, but on the same insufficient and deceptive grounds, it was again and again abandoned as an unruly and dangerous remedy. The whole of the brief history of its use and abandonment is indeed highly instructive; furnishing, as it does, an example of a pure a priori and rational practice carried out more speedily and palpably than any other on record to its legitimate results; first unmeasured injury to human health and life; then the abandonment of the remedy (or treatment), and finally the explosion of the hypothesis which afforded the deductive authority for its employment.

Quinine constitutes a very important part of the treatment of pneumonia in this climate. If it be sometimes inefficient, it is also at times indispensable. In an affection simply inflammatory, as I suppose pneumonia usually is in cold climates, and often is in this, though never a tonic or stimulant in any dose, its sedative influence is too feeble to be available for much good; and if such cases could always be distinguished, quinine might very well be dispensed with in them. On the

other hand, when a malarious taint is an obvious complication, or when the pulmonic disease seems, as it were, engrafted on an intermittent fever, no other remedy can be so confidently depended on. But this taint may exist, while the signs of it are so masked as to be detected with great difficulty. The excess of fibrine in the blood, the violence of the local inflammation, and the exalted state of the innervation, may overcome the tendency of the cause of periodical fever to manifest itself in the usual way. To this it may be added, that in this latitude, whenever intermittent fever is endemic, there is more or less of periodical disease in every season of the year, assuming most frequently the form of fever, but often of other forms of periodical disease. Residents of such localities are, in fact, all the time subjected to the influences out of which proceed a class of diseases, *a quinquina*, as the French have it, which are amenable to treatment by the bark and its preparations. Hence, it becomes a safe rule to begin the treatment of pneumonia in malarious districts (so called) by making quinine a component part of it. My custom therefore is, to give it in the manner above specified, until its peculiar effects on the head become very well marked, and if the signs of amendment are not then satisfactory, or such as may be properly attributed to the quinine, to discontinue it, and give the aconite in water.

Blisters are also important adjuncts in the treatment of pneumonia. Systematic writers, drawing their experience from places north of 36 degrees of north latitude, in this country, and in Europe, are almost unanimous in proscribing blisters before the violence of the inflammation is subdued by bleeding, or has worn itself out. Here, however, in pneumonia, as well as in other inflammatory diseases, this rule is reversed. Blisters are found to be most beneficial applied in the first stage of pneumonia, and the earlier the better. So applied, they never seem to produce any general irritation, are prompt in relieving pain, and, there is reason to believe, assist in resolving the local inflammation.

Morphine (or opium) besides its occasional use as an anodyne merely, is introduced in the plan of treatment in order to effect two special objects. One of these is to prevent any irritation of the bowels, or to remove it if present. We have seen that the tendency of the disease is to take on this kind of complication, which, when it occurs, always aggravates the danger of an attack, as well by its unfavorable reaction on the primary affection, as by the presence of disease in two vital organs instead of one. The predisposition to an enteric complication is sometimes so strong as to be developed into active disease

from very slight causes ; even quinine, slightly irritative as it is, may produce this effect. Hence the advantage of combining the opiate with it, and of so timing the doses of the opiate as that its quieting effect on the bowels may be continuous. One of the effects of this mode of administering the opiate is, to secure a constipated state of the bowels, which I think is always desirable. The rule being to keep the bowels quiet, cathartic or even aperient medicines, form no part of the treatment. If a diarrhœa happen to complicate the attack from the beginning, the treatment for the primary affection is usually found sufficient to remove it very soon ; otherwise I interpose a few doses of the acetate of lead and opium.

The other object is to prevent inflammation of the pleura. This complication, at least in an active state, is by no means common in the beginning of an attack, although a predisposition to it is very common. Thus, while my notes show but one instance of a pleuritis marked by physical signs in sixty-eight cases, the predisposition to it, evinced, by acute pain, was evident in nearly the whole number. It is not, therefore, as I conclude, at the beginning, that a pleuritis occurs in pneumonia, but some time afterwards in the course of an attack. So strong, however, is the predisposition, and so frequently is it developed under the usual treatment, that very few dissections are made after death from pneumonia, which do not exhibit the evidences of it. This is, indeed, so much the case, that Andral proposed to prefix the word *pleuro* to pneumonia, for its common name. I find also that other authors come very nearly to the conclusion of Andral, that pleuritis is an essential part, or an invariable complication of pneumonia. Nevertheless, the fact is that the physical signs of pleurisy are very seldom exhibited in the beginning of an attack. A pleuritic pain, however, is seldom absent. This pain is, for the most part, in the first stage, (perhaps always in the absence of the physical signs of pleurisy) a pleurodynia merely. This may depend on a repletion of the capillaries, not yet amounting to inflammation, or at least not active enough to produce the products of inflammation, but enough so to excite a painful irritation in the nerves ; or, which I think more probable, the relation may be reversed ; the capillary repletion, and the consequent development of an active inflammation, being dependent on a precedent irritation of the nerves. However this may be, the presence of acute pain is a sign of the predisposition, and the severity of the one is ordinarily the measure of the strength of the other ; while on the other hand, the removal of the pain may be depended on, as a general rule, as the evidence of the removal of the predisposition also. That is to say, speak-

ing only from my own experience, whatever will remove the pleurodynia permanently, its cause going with it, will also remove the disposition to pleuritis Hence, I have been disposed to look on the pain of pneumonia as a most important object to be cared for, independently of the suffering from it, and to regard opiates as a most important adjunct in the treatment; if for this reason alone, that it is more efficient in removing the pleurodynia of pneumonia than any thing else in common use.

By the judicious use of opiates, then, we expect with confidence to ward off two sources of difficulty in the treatment, and of danger in the result. Occasionally, however, the additional aid of a blister may be required; when this is the case, the blister should be applied, not over the seat of the pain, for this may be in the side not otherwise affected,* but over the seat of the inflammation of the pulmonary parenchyma.

Of the advantages of opiates in alleviating the feeling of restlessness and malaise, in mitigating a violent and harassing cough, in quieting an oppressed and hurried breathing, and in procuring timely sleep, in this disease, I need not speak, inasmuch as the name of opium is associated in the minds of physicians with all such kindly, and, to a certain extent, curative influences. The fears so often expressed by systematic writers in regard to the use of opiates before the acute febrile excitement is subdued, and the apprehension of their aggravating the inflammation by checking expectoration, seem to me altogether unfounded; except, indeed, as regards the latter, under the following circumstances:

It sometimes happens that, late in the attack, usually after the seventh day, a copious expectoration sets in, which is evidently a secretion from the larger bronchial tubes; at least it resembles such a secretion, and not that which previously took place, being white, frothy, and only slightly glutinous; it is discharged in considerable quantities by an almost incessant cough, which is accompanied by a loud and coarse mucous rale, and a feeling of suffocation; at the same time the face is pale and anxious, and the pulse small and quick. I have for some years looked upon this state of things as denoting a favorable crisis, and experience has taught me that it is not altogether safe to interfere with it by opiates at or near the beginning of it. After a few hours the expectoration may be safely stopped, and then is not likely to return in excess, which it will do after the effect of the anodyne has worn off, if checked too soon. In all other circumstances, opium is, within

* See note to case 50.

my experience, a safe, highly useful and pleasant adjunct, the more valuable and efficient when the inflammation is most acute.

The results of the treatment here recommended are shown in the annexed table, which is a record of all the cases of pneumonia treated by me during the last four years, including also all that were treated by Dr. John McLester while associated with me in practice in 1849 and 1850. In noting the cases, the rule was followed of excluding all cases of disease of the lungs in which the diagnosis of pneumonia could not be clearly made out, as well as those in which the pulmonic inflammation was not the primary and predominant disease. For example: Those cases which we occasionally meet with among children, in which the predominant disease being bronchitis, some inflammation of the pulmonary parenchyma may be suspected from the rapidity of the breathing, and some peculiarities in the expression of the countenance and in the character of the cough; but the physical signs of this affection are absent, and there is no visible expectoration to guide us in the diagnosis; somewhat similar cases occurring among adults, in which, as in children, we may suspect the existence of pneumonia, chiefly from the frequency of breathing, but there are neither the physical signs nor the characteristic sputa to justify it; the pneumonia, if present, being probably limited to a small part of the deep seated structure of the lung, or to small points scattered through it. Two cases of this kind, cases primarily of acute bronchitis, have come under my observation, one of which proved fatal; no opportunity having been presented for a post mortem examination. And lastly, cases of typhoid fever, in the course of which pneumonia has supervened. In these cases I have been in the habit of treating the pneumonia in the same way as when idiopathic, and it has been common to see this complication yield happily to the treatment, while the primary affection held on its course.

It is to be observed, also, in further explanation of the statistics, that the beginning of the attack is fixed at the time of the first febrile movement, accompanied by cough and pain, or a well marked soreness in the chest; and the termination at the time when all treatment was discontinued. When the treatment, or any part of it, was continued on the day of the last visit, this day is included in the estimate of the duration of the attack, whatever may have been the extent of improvement in the disease; when, on the other hand, the last visit has been one of observation and inquiry merely, it has not been included in the estimate of time. M. Louis dates the termination of an attack from the time the patient begins to take food; three days, at least, according to

STATISTICS OF PNEUMONIA, AND THE RESULTS OF TREATMENT.

Name.	Number.	Age.	Male.	Female.	White.	Black.	Date of Attack.	Date of First Visit.	Discharged.	Deaths	Days of Treatment.	Duration of Disease.	Seat of Disease.	First Stage.	Second Stage.	Remarks.
1849.																
Westcott,	1	4m's	1			1	Apr. 25	April 29	May 2		4	8	Upper lobe of left lung, anteriorly.		1	
Pitkins,	2	5y's	2		1		May 7	May 7	" 9		3	3	Right lung, lower lobe.	1		
Larkin,	3	3		1		2	June 7	June 10	June 15		5	8	Left lung, lower lobe,		2	
Amason,	4	26	3			3	Aug. 5	Aug. 8	Aug. 8		3	4	Right lung, lower and middle lobes.	2		
Freeman,	5	4	4			4	Sept. 2	Sept. 3	Sept. 7		5	6	Right lung, lower lobe.		3	
Wood,	6	22		2	2		Oct. 24	Oct. 25	Nov. 10		17	18	Double, clavicular and sub-clav. region of both lungs.		4	Pleuritis in right clavicular and sub-clavicular region, the loudest friction sound being under the clavicle. Parturition six weeks before the attack, pale, sallow and anasarcous. Bellows murmur in right side of heart, which persisted after recovery. Had been subject for several years to occasional attacks of rheumatism.
Graham,	7	36	5			5	" 31	Nov. 1	" 12		12	13	Right lungs, lower and middle lobes.		5	Delirium tremens after fourth day.
Vandeveer,	8	3	6			6	Nov. 1	" 3	" 9		6	8	Right lung, lower lobe.		6	
Wood,	9	22		3	3		Dec. 16	Dec. 16	Dec. 25		10	10	Left lung, entire.	3		
Stewart,	10	3	7			7	" 25	" 29	Jan'ry 2		5	8	Left lung, upper lobe, posteriorly.		7	
1850.																
Whiting,	11	58	8			8	Jan. 1	Jan. 2	" 10		10	11	Right lung, upper lobe.		8	Had been drinking to excess some days before the attack.
Larkin,	12	3		4		9	" 3	" 3	" 5		3	3	Right lung, upper lobe, posteriorly.	4		
Allen, Alfred,	13	15	9			10	" 15	" 23	" 25		3	11	Left lung, entire.		9	
Allen, Anthony,	14	14	10			11	" 19	" 23	" 25		3	7	Right lung, middle and lower lobe.		10	
Westcott,	15	20		5	4		" 25	" 26	" 28		3	4	Left lung, clavicular and sub-clav. region.		11	
Hobbie,	16	55		6	5		Feb. 5	Feb. 6	Feb. 16		10	11	Right lung, middle and lower lobes.		12	
Westcott,	17	20		7	6		" 9	" 12	" 15		4	7	Right lung, sub-clavicular region.		13	
Chisholm,	18	28		8	7		" 10	" 10	" 20		10	10	Right lung, lower lobe.		14	
Taylor, Tom,	19	48	11			12	" 11	" 13	" 22		10	12	Right lung, entire.		15	Preceded by cholera seven days—succeeded by hepatitis and jaundice.
Taylor, Rose,	20	20		9		13	"						Right lung, lower and middle lobe.		16	
Taylor, Jack,	21	35	12			14	"						Right lung lower lobe.		17	
Figh,	22	40	13			15	" 13	" 15	" 18		4	6	Right lung, clavicular and sub-clav. region.		18	
Freeman,	23	50	14			16	" 14	" 16	" 20		5	7	Right lung, lower lobe.		19	
Allen,	24	45	5			17	March 5	March 11	March 17		7	13	Right lung, upper and middle lobes.		20	
Hayne,	25	40	16			18	" 12	" 17	" 20		3	8	Double, lower lobe of left and lower, and middle of right.		21	
Laprade,	26	54		10	8		" 14	" 16	" 20		5	7	Right lung, upper lobe.		22	
Fair,	27	4	17			19	" 16	" 19	" 25		6	9	Right lung, lower lobe.	5		
Gilmer,	28	26	18			20	" 18	" 20	" 24		4	6	Right lung, lower lobe.		23	
Whittaker,	29	33		11		21	" 25	" 27	April 6		10	12	Double, lower lobe of both lungs.		24	
Thornigton.	30	30		12		22	April, 1	April 2	" 7		6	7	Right lung, lower lobe.		25	
Taylor,	31	41	19			23	" 11	" 13	" 15		3	5	Right lung, lower lobe.		26	
Pitkin,	32	2		13	9		" 8	" 10	" 12		2	4	Left lung, lower lobe.	6		
Pryor,	33	60	20			24	Oct. 28	Oct. 29	Nov. 3		6	7	Right lung, lower lobe.		27	
Freeman,	34	29		11		25	Nov. 25	Nov. 26	Dec. 4		8	9	Right lung, lower and middle lobes.		28	Four months pregnant, miscarried 30th November, 6th day.
1851.																
Bardwell,*	35	32	21			26	Jan'ry 9	Jan'ry 14	Jan'ry 16	1	3	8	Double, lower and middle lobes of right and lower lobe of left.		29	
Wilkinson,	36	26	22			27	" 27	" 28	Feb. 5		9	10	Right lung, lower lobe.		30	Treated the first five days by a steam doctor.
Randell,	37	22	23			28	April. 2	April 3	April 7		4	5	Right lung, lower lobe.		31	
Harris,	38	6	24			29	Sept. 16	Sept. 17	Sept. 23		7	8	Right lung, upper lobe, posteriorly.		32	
Eckels,	39	47		15	10		Oct. 20	Oct. 22	Oct. 30		7	10	Right lung, upper lobe.		33	Preceded three days by severe cephalalgia.
Pitkin,	40	10		16	11		Nov. 14	Nov. 16	Nov. 21		5	7	Left lung, lower lobe.		34	
Ogbourne,	41	27	25			30	Dec. 10	Dec. 11	Dec. 17		6	7	Right lung, entire.		35	Rotten egg sputa—highest range of pulse
Lewis,	42	20	26			31	" 20	" 21	" 30		10	11	Right lung, clavicular and sub-clav. regions.		36	130, resp. 65†
1852.																
Harwell,	43	26	27			32	Jan'ry 8	Jan'ry 9	Jan'ry 18		9	10	Left lung, lower lobe.		37	
Hall, Henry,	44	31	28			33	" 15	" 16	" 22		6	7	Left lung, lower lobe.		38	
Hall, Samuel,	45	17	29			34	" 15	" 16	" 22		6	7	Left lung, lower lobe.		39	
Hall, Phœbe,	46	26		17		35	" 19	" 19	" 28		10	10	Double, sub-clav. of right and lower lobe of left.	7		Six months pregnant, miscarried on the fourth day—highest range of pulse 136, of resp. 74.
Hall, Amy,	47	49		18		36	" 20	" 21	" 28		8	9	Double, upper lobe of right and lower of left.		40	Bronchitis of both lungs—highest range
Hall, Charles,	48	32	30			37	" 21	" 22	" 27		5	6	Right lung, lower lobe.		41	of pulse 126, of resp. 64.
Westcoll,	49	2	31			38	" 31	Feb. 3	Feb. 7		5	8	Left lung, entire.		42	
Shaver,	50	9		19	12		Feb. 24	" 25	March 2		7	8	Left lung, upper lobe.		43	Very acute pleurodynia on right side, no pain on left—highest range of pulse 130, of resp. 52.
Chisholm,	51	3		20		39	March 8	March 10	" 16		6	8	Left lung, lower lobe.		44	Highest range of pulse 160, of resp. 70.
Jones,	52	7		21		40	" 20	" 25	April 4		10	15	Double, lower lobe of both lungs and middle lobe of right.		45	Highest range of pulse 146, of resp. 84 —bronchitis of both lungs.
Randolph,	53	14	32		13		April. 12	April 13	" 20		8	9	Right lung, lower lobe.	8	46	Highest range of pulse 120, of resp. 40.
Goode,	54	4	33			41	" 14	" 17	" 24		8	11	Right lung, lower lobe.		47	" " " 152, " 56.
Westcott,	55	7	34		14		May 8	May 9	May 14		6	7	Left lung, lower lobe.			" " " 132, " 54.
Farley,	56	3	35			42	" 27	" 29	June 18		21	23	Right lung, lower and middle lobes.		48	" " " 140, " 64.
Ogbourne,	57	16	36			43	Aug. 4	Aug. 9	Aug. 12		4	9	Left lung, entire.		49	" " " 132, " 65.
Noble,	58	8m's	37		15		Oct. 5	Oct. 6	Oct. 12		6	7	Left lung, upper third of lower lobe.	9		
Sayne,	59	12		22	16		" 23	" 26	Nov. 2		7	10	Right lung, lower lobe.		50	" " " 124, " 66.
Freeman,	60	27	38			44	Nov. 4	Nov. 6	" 14		9	11	Left lung, lower lobe.		51	" " " 108, " 66.
Updegraff,	61	17	39		17		Dec. 15	Dec. 16	Dec. 20		4	5	Left lung, lower lobe.		52	" " " 130, " 48. —rotten egg sputa changed to rust-colored on the day of disease.†
Hassell,	62	24	40			45	" 17	" 18	" 21		4	5	Right lung, upper and middle lobes.			Highest range of pulse 96, of resp. 32.
Jones,	63	36	41			46	" 26	" 27	Jan'ry 4		9	10	Left lung, lower lobe.	10	53	" " " 100, " 32.
1853.																
Coxe,	64	46	42			47	Jan'ry 4	Jan'ry 7	" 13		8	10	Right lung, lower lobe.		54	" " " 120, " 38.
Randolph,	65	22	43		18		" 23	" 24	" 25		2	3	Left lung, lower lobe.	11		" " " 94, " 32.
Figh,	66	16	44			48	April 17	April 20	April 25		6	9	Left lung, whole of upper lobe.		55	" " " 104, " 48.
Coxe,	67	12	45		19		" 21	" 23	" 27		4	6	Double, whole of right lung and lower lobe of left.	12		" " " 144 " 56.
McKane,*	68	1	46			49	May 27	May 28	June 3	2	7	8	Double, whole of left lung, a lobular portion of upper, and upper third of lower lobe of right lung.		56	" " " 180, " 106.

* Some circumstances attending these two fatal cases required to be noticed, in order to render the table a true expose of the effects of the treatment.

The first, (case 35) was treated by my associate, Dr John McLester. I did not see the man, and consequently know but little more of the circumstances connected with the attack than is contained in the brief statement in the table, viz: that the patient had double pneumonia, and had been five days under treatment on the Thompsonian plan; that is, by active stimulants. To this I am able to add, that Dr. McLester's first visit was made sometime in the night of the fifth day, and that the patient died early in the morning of the seventh day, having been under treatment by Dr. McLester not more than thirty-six hours.

Of the second, (case 68) the following is a brief history. The child, one year old, playing on the edge of a terrace, ten feet high, fell over the edge and rolled to the bottom. Being in the charge of very young black children, no information could be obtained of the immediate effects of the fall. The next day he was attacked with convulsions, followed by high fever and a cough. It was in the afternoon of the next day, the second of his illness, that I first saw him. He was then comatose, with an ardent fever, cough, and hurried respiration, and the physical signs of pneumonia, as noted above. The head symptoms continued with little variation until the seventh day, but the pulmonic symptoms had, in the meantime, very much improved, the pulse having fallen from 180 to 124, and the respiration from 70 to 40 in the minute. There was at this time considerable remission in the head symptoms also; but in the afternoon of this day he grew stupid again, and by night, having become profoundly comatose, he was seized with convulsions, which continued, with short intervals, until he died. No post mortem examination was made. From the time the convulsions returned, the affection of the lungs, indicated by the physical signs, remained nearly stationary; the pulse, however, ran up to 180, and the respiration as stated to 106.

† So named from its resemblance in color, consistence and smell to rotten egg. This kind of sputa begun to change to the usual rust color, and to decrease in quantity on the third day of the disease; on the fourth day the change was completed. The matter was discharged in considerable quantity at a time, I suppose about half an ounce, and very frequently. The breath partook of the fetor of the matter expectorated.

M. Grisolle, after the cessation of fever. M. Bouillaud, on the other hand, dates the termination from the time the fever had been decidedly mitigated, and other signs of convalescence had appeared, although the characteristic sputa, as well as some fever, remained—" à l'époque où les signes caracteristique de la pneumonie et le mouvement fébrile ont presque entièrement disparrée." I have followed neither. While I have put the termination further off than M. Bouillaud, waiting until the fever and the characteristic sputa had, not almost, but altogether disappeared, and the tendency to health was so well marked as to give unequivocal assurance of the safety of the patient, I have not, with M. Louis, supposed the disease to exist on account of the remaining dulness and crepitation, after all other signs had ceased, along with the fever; for after this, the dulness and crepitus ought, in my opinion, to be viewed as evidence, not of present, but of recent disease—the crepitus being in fact a sign only of convalescence.

The stage of the disease is represented as it was exhibited at the first visit.

II.—VESICO-VAGINAL FISTULA—ULCERATION OF THE OS TINCÆ—LEUCORRHŒA—CURE.

BY H. J. HOLMES, M. D., MISS.

Mrs. V. aged 26, of sanguine nervous temperament, the mother of two children, visited my infirmary on the 20th April last, for vesico-vaginal fistula and uterine disease. After a careful examination of the uterus and bladder, I detected a large ulcer seatedaround the os tincæ, embracing both labia of the uterus and uterine canal with leucorrhœa and engorgement—with the bladder, a transverse fistula three-fourths of an inch in length, occupying the bas fond and a portion of the urethra through which the urine had escaped for eleven years past.

A partial history and its treatment will comprise what I have to say of this case. At the age of fourteen this lady was married to a very worthy and respectable planter of the State of Louisiana, who proved to be one of the most indulgent and devoted husbands, as the sequel will show. Eighteen months after marriage she was confined with her first child, which proved to be a very tedious and protracted labor, continuing near three days and nights, during which time she was attended by Dr. ——, of the parish of ——, who it was said by the husband and lady to have shown but little skill in the management of the case; which finally resulted in the birth of a still-born infant. Extensive inflammation supervened, from which immense sloughing ensued of the walls of the vagina—finally healed by several cicatrices and closing the entrance to the vagina, which barely admitted a small finger at any time afterwards. Her periods did not return for two years; then made their appearance at intervals of two and three months for the space of four years—during which time they were very painful and scanty—and each time attended with very severe hysterical paroxysms, which continued from one to twenty-four hours. At or about the expiration of five years from the birth of her first child, an effort was made to cohabit, and although an entrance could not be effected, yet conception occurred. Having passed nearly through gestation, she was placed under the care of Dr. P., of I., who made several efforts to enlarge the entrance by frequent incisions through these hard cicatrices, but with partial success only; labor came on and continued four days, during which time frequent incisions were made through the ostincæ; in the meantime the efforts of the womb being very great, and with the use of instruments, delivery was at last effected of a fine healthy daughter. From this she recovered very slowly, but perhaps as well as could be expected under the circumstances.

The great injury she sustained in her first delivery, rendered her

exceedingly nervous and susceptible to these paroxysms, and from the slightest cause she could be thrown into one of them, from which, to all appearance, she could scarcely revive; and to guard against which, every means were resorted to by her attentive physician and indulgent husband. This case had been submitted to the care of several eminent surgeons as well as many other eminent practitioners, none of whom had been able to afford relief—and in this helpless and deplorable condition the case was presented to my notice. Being cognizant of this fact, I required three days to form and express an opinion. Perhaps while here, I had better give a more accurate description of the uterus and vagina, than that which I have already mentioned.

1st. A large ulcer seated in the neck around the os tincæ and extending into the canal.

2d. A very considerable engorgement of the neck and body.

3d. Neck hard and exceedingly tender upon touch.

4th. Leucorrhœa of a straw color adhering to the ulcer and uterine canal.

5th. Vagina sufficiently large and capacious; about three inches and a half in depth, from large old cicatrices, commencing about one inch within the vagina, half to three quarters of an inch apart and running backward and upward, meeting at a point where the neck of the uterus passed through the walls of the vagina, and had the appearance of being puckered or drawn together by a strong band.

6th. The perineum, labia majora, nympha, clitoris and urethra, all natural in appearance and in situ.

A very important matter with me at this time was to ascertain whether or not the womb could be probed, canal dilated and cavity reached; with this view I set to work with my metallic probes, varying in size, and on the third day I ascertained that I could pass the probes very well a distance of five inches with less pain than I had expected. All doubt having been removed in my being able to treat the womb thoroughly, I ventured to promise to cure both the uterine disease and the vesico-vaginal fistula. This opinion, though cautiously given, was viewed by herself and friends with a good deal of doubt.

TREATMENT.—My whole time for the first three months was devoted to the treatment of the womb and her general health.

1st. By drawing blood regularly from the neck of the uterus for two weeks.

2d. By dilating the uterine canal, which I found to be seven inches in length.

3d. Applying the nitrate of silver to the ulcerated surface; within the canal and uterine cavity with the *Porte Caustique*, and with the

solid nitrate of silver in a quill to the ulceration around the os tincæ and neck of the womb. This was repeated every fifth or sixth day, according to circumstances. The pain attending the use of the caustic was very severe, often ending in a severe paroxysm; to relieve which morphine and brandy were given internally and by enemata, warm fomentations to the abdomen, warm bricks to the feet, and sinapisms to the extremities. At the expiration of twenty-four or thirty-six hours, she would become quite comfortable, perhaps sitting up or walking about. These means improved the appearance of the ulcer, reduced the engorgements and improved her general health very decidedly. The severe paroxysms to which she had been accustomed having ceased for one month, together with the improvement in her general health, I concluded to embrace this favorable opportunity to operate for the vesico-vaginal fistula, and with her consent, invited my friends, Drs. J. J. Pugh and Wm. H. Thomson, to assist me. At the hour of 11, A. M., on the first day of August, in a large and spacious room, I proceeded to operate by placing her in a bed upon her knees and elbows before a large window. The lever speculum was introduced and held by Dr. Thomson, at the same time separating the right glutei muscles with fingers extending to the labia majora; the left side was attended to by Dr. Pugh; then by pulling the nates upward and outward and lifting the perenium, stretching the sphincter and raising up the recto-vaginal septum, the vesico-vaginal fistula was brought fairly to view. A slightly curved tenaculum served to hold it steady, while I inserted a sharp pointed bistoury one eighth of an inch from its margin, and then by a circular sweep I endeavored to remove this much all around. But the difficulty of carrying it on a straight line was often interfered with by the knife cutting through the margin into the fistula; however, by renewing my hold with the tenaculum, I succeeded after a time in removing a sufficient slip to answer my purpose. But little hemorrhage occurred during the operation, and that little removed often by one of my assistants, which enabled me to operate faster than I had expected. Having suceeeded in this part of the operation to my satisfaction, I next passed a spear-pointed needle around with a silk ligature passing it a quarter of an inch anterior to the margin; then withdrawing the needle and threading it again, I passed it through the upper margin at the same distance. This I effected much more easily than I expected by holding a piece of sponge attached to a small wire, which held it firm and solid. The needle was again withdrawn, leaving the thread, as intended; a second and third was carried, making three sutures in all. A small silver wire was then attached to each silk ligature, and carried in succession through the punctures I had made with the assist-

ance of a blunt hook, which was made to answer the purpose of the crescent shaped fork. The distal ends of the wire were withdrawn from the vagina sufficient for us to pass their ends through a small leaden bar, one and a quarter inch in length and one sixteenth of an inch in diameter, previously perforated with an awl; upon the ends of each I pressed a small shot, which had also been previously flattened and perforated, and upon which the ends of the wire were turned over so as to form a knob; the proximal ends of the wires were then pulled so as to bring the bar of lead above the fistula; another bar of lead of the same size and length was also perforated, and the ends of the wires passed through and pushed up with a blunt hook; by this means the raw edges of the fistula were brought directly in contact and secured by perforated shots being compressed on each wire, and the ends of the wire cut close to each shot with a pair of bone forceps. The operation being completed, a large dose of morphine was given, and the lady put to bed with a catheter two and a half inches in length in the urethra and bladder.

She complained more of the position in which she had been placed than the pain attending the operation. The first night and second day was passed quite comfortably; on the third morning I removed the catheter and washed away the slight secretion which had occurred up to that time. Every morning up to the tenth day this was attended to regularly, at which time I removed the bars of lead by clipping the ends of the wires without difficulty, and found the fistula healed perfectly. The catheter was worn for several days afterwards, when it was removed, and an effort made to pass the urine into a chamber. On my return to her room, I at once discovered her countenance radiant with joy, and the first word that greeted me was—"that there was now some pleasure in living, doctor; I find that I can retain my urine for any length of time and pass it at pleasure into a chamber, a thing I have not been able to do for eleven years past." It is here proper to remark, that I kept this lady in a horizontal position, on a light diet, and bowels confined, so as to give the parts an opportunity of healing by the first intention.

One month having passed since the operation, and finding that she had entire control over her urine, I commenced again the use of the caustic to the external surface of the neck and uterine cavity, at intervals of six and eight days for two months; at the expiration of which time there being no ulceration, leucorrhœa or engorgement, I discharged the case as cured.

It is due however to remark, that this lady has been a confirmed

dyspeptic, and the least imprudence on her part in eating, especially a piece of ham, a peach, or a fried fritter, she might expect with a degree of certainty that it would be followed by a severe spell of vomiting, and finally end in one of her worst paroxysms.

These paroxysms, she has often remarked, seemed to be a family disease; three or four married sisters are alike subject to them, and can be produced from any slight cause.

In conclusion, I must respectfully add, that I am mainly indebted to Dr. J. Marion Sims, of Montgomery, Ala., for a pamphlet on the treatment of vesico-vaginal fistula. In my operation I was guided principally by his remarks upon this disease; and to him all praise should be given for the great light he has thrown upon this hitherto obscure and intractable disease.

Oct. 28, 1853.

III.—THEORY OF MOLECULAR FORCES—EXPLANATORY OF THE GASEOUS, LIQUID AND SOLID CONDITIONS OF MATTER:

Read before the American Association for the Advancement of Science, Cleveland, Aug. 2, 1853.

BY J. L. RIDDELL, M. D.,
Prof. Chem. Med. Dep. Univer. La., New Orleans.

The following is a condensed abstract of the paper presented to the Association under the above title:

1. No system of philosophy can be true, which does not recognise the necessary continuity and equivalency of cause and effect.

2. All hypotheses are false which preclude rational explanation.

3. Fundamentally involving few and simple data, the problem of nature is one of infinite complexity.

4. The problem of nature has its geometrical projection in space, in the forms of material organization, aggregation and position.

5. The phenomena of nature must occur in a rational manner.

6. Natural laws must rest on a physical basis.

7. Matter and motion must be indestructible.

8. Beyond its passivity to motion or rest, matter cannot possess inherent qualities.

9. Force and power are strictly equivalent to, and identical with, momentum, which is *matter moving*.

10. Such attractions between individual molecules or bodies, as vary in intensity reciprocally as the square of the intervening distance, must be generally borrowed from the omni-directive impulses, which pursue their endless rectilineal paths, in the different coextensive systems of refined media, with which boundless space has ever been furnished.

11. The crude hypothesis, that the more simple molecules, like those of oxygen, water, etc., are usually revolving on axes with intense rapidity, enables us to form a conception of the possible cause, of what may be called polaric attraction and polaric repulsion. Similar, but more gross attractions and repulsions, arise from rotating solid disks in the air; when, if free to move, the disks arrange themselves in relation to each other, with coincident axes; each rotating disk acquiring an equatorial ring of repulsion, and centrally transverse thereto, two opposite poles of attraction. The experiment with the disks is easily made, and the origin of the polaric forces which manifest themselves, plain and easy to be fully comprehended. Thence by parity of reason, some notion may be formed of the probable mechanism, by which polaric molecular forces are exercised, crystalization and chemical action occasioned, and solidity or molecular fixedness produced and maintained; the passive agency of media more refined than air, being considered as instrumental.

12. The different media, as air, water and solids, transmit at all times, by the mutual impact of molecules, lines of molecular momenta in all possible directions, with a velocity normal to each case; say, in air near 1140 feet per second.

13. Molecular momentum is partly identical with heat.

14. We have a complex and partial apprehension of molecular momentum, in our sense of temperature.

15. We appreciate momentary wave-like disturbances of molecular momentum, recurring at appreciably short regular intervals as the tone of sound.

16. Repulsion among gasseous particles arises from their mutual impact, occurring in the transmission of molecular momentum.

17. The force of collisionary molecular repulsion in a gas, varies of necessity, in reference to a constant transverse plane as

$$\frac{1}{X^3}$$

X standing for the variable intermolecular distances.

18. The molecular forces in fluids are best expressed algebraically,

by reference to a plane regarded as constant in size, and transverse to the direction of the forces under consideration.

19. If between two molecules or particles of a fluid, at variable distance (X) from each other, the force of attraction vary as

$$\frac{1}{X^2}$$

between the molecules on opposite sidesof the constant plane, the attrac tion varies as

$$\frac{1}{X^4}$$

20. Owing *possibly*, in part to the variable amount of refined material envelope, appropriated at different mutual distances by each molecule; and in part *probably*, to the influence of polaric repulsion, which as explained, (11), would of necessity extend further from the molecular centres than polaric attraction; owing perhaps to these causes, the molecular attraction in fluids appears by observation, to vary, say within the exponential limits 1 to 4,

$$\frac{1}{X} \quad \text{to} \quad \frac{1}{X^4}$$

according to a continuously varying law; which law, by assuming z to be some direct and continuous function of X, may be thus expressed,

$$\frac{1}{X^z}$$

21. The equation theoretically expressive of the molecular equilibrium, existing among the particles of fluids, (liquids and gases) in reference to a constant plane, must have the following form:

$$\frac{1}{X^z} + \mathrm{P} - \frac{r}{X^3} = 0$$

Here X = the variable intermolecular distance.

z = a direct continuous function of X.

r = rate, or co-efficient of collisionary repulsion, dependent for its value on the temperature.

P = extrinsic pressure, as for example the weight of pressure o the atmosphere.

Assuming P = 0, r at any constant value as unity; and for convenience, assuming $z = X^*$, the equation

$$\frac{1}{X^z} + P - \frac{r}{X^3} = 0, \text{ becomes } \frac{1}{X^x} - \frac{1}{X^3} = 0;$$

then as X varies from $X = 0$ to $X = \infty$, there will occur, among others, the several maximum and minimum values of

$$\frac{1}{X^x} - \frac{1}{X^3}$$

corresponding in a general and remarkable manner to what is experimentally demonstrable, respecting all the natural phases of balance or preponderance of the molecular forces, in liquids and gases.

Thus when

$X = 0$,	$\frac{1}{X^x} - \frac{1}{X^3}$	$=$ max. $= -\infty$,	Repulsion.	A.
$X = 1$,	" "	$=$ min. $=$ 0,	Balance,	B.
$X = 1.87$	" "	$=$ max. $=$ + Finite.	Attraction.	C.
$X = 3$,	" "	$=$ min. $=$ 0,	Balance.	D.
$X = 3.95$	" "	max. $=$ — Finite.	Repulsion.	E.

A. The molecular relations implied by value A, $X = 0$, are imaginary and impossible, since two or more molecules of the same order, cannot be supposed to occupy simultaneously the same space. From this, however, we can form some notion of the insurmountable barrier to indefinite compression in liquids.

B. With the value of $X = 1$, we have a molecular position of stable equilibrium, such as exists in the liquid condition of matter; exemplified by water, mercury, alcohol, etc.

C. In the case of the value of $X =$ near 1.87, we have the intermolecular distance, at which the greatest amount of separative force would be required, in the operation of causing a rupture of the continuity of a liquid mass. This is also the natural transformation point, between the liquid and gaseous condition of matter; whenever such

* The result is essentially the same, as to the succession of maximum and minimum values, with or without this assumption; but the special finite value of X, at which the maxima and minima occur, would be somewhat different.

transformation is caused by increase or decrease of collisionary repulsion.

Taking the varying intermolecular distance (X) as an abscissa, and the corresponding molecular forces lineally expressed as ordinates, so as to project the curves of attraction and repulsion, it will be seen tha these curves cross each other, normally, at $X = 1$, and $X = 3$; now by increasing the amount of repulsion, as by the application of heat to a liquid, these crossing points will approach each other, so that at $X = 1.87$, the curves will be tangent to each other. The condition of the liquid will now be one of unstable equilibrium, for by the slightest in crease, the force of repulsion predominates, and the liquid begins to assume the condition of a gas.

Since the limit of height to which liquids will ascend in capillary tubes depends upon the maximum limit of cohesive force subsisting between the uppermost row of liquid particles, and the particles below, it follows that the intermolecular distance between the upper and the subjacent row must correspond to $X = 1.87$.

D. The condition $X = 3$, marks the normal limit between the same substance as a liquid and as a gas.

E. The condition $X = 3.95$, is that wherein the repulsive force of a compressed gaseous body is a maximum; being the maximum limit of pressure required for condensing a gas to the liquid form. At this point, the repulsive force is nearly three and a half times greater than the force of attraction. At greater distances, such as we find exemplified in the molecular constitution of the atmosphere, the diminished force of repulsion alone manifests itself, while attraction, observing a more rapid law of decrease

$$\frac{1}{X^4}$$

becomes wholly inappreciable.

SUMMARY.

Solids. The molecules constituting solids, are held in a fixed relation to each other by polaric force. (11.)

Liquids. In liquids, the molecules are in effect held equidistant from each other, (X), by virtue of a balance between the force of molecular attraction, which in reference to an invariable plane, (18), varies as

$$\frac{1}{X^4}$$

(19), usually conjoined with a subordinate influence, external pressure, acting *centripetally*; and impingent or collisionary repulsion, which varies with the temperature and also varies as

$$\frac{1}{X^3}$$

(17,) conjoined with polaric repulsion, (11) acting *centrifugally*. Considering the polaric repulsion as added to the molecular attraction, then between the limits $z = 1$ and $z = 4$, the resultant attraction varies as

$$\frac{1}{X^z}$$

z standing for some direct and continuous function of **X**. (20.)

Gases. In gases, the molecules occupy also equal and equidistant spaces. External pressure mainly, and molecular attraction, varying as

$$\frac{1}{X^4}$$

subordinately and for the most part inappreciably, act as the *centri petal* forces; while impingent molecular repulsion, varying with the temperature and as

$$\frac{1}{X^3}$$

acts *centrifugally*.

IV.—OBSERVATIONS ON THE NATURE AND TREATMENT OF YELLOW FEVER.

(*Translated from the French of* ———, BY THE EDITOR.)

In some of our former numbers we published portions of an excellent work, written by an able and learned French Naval Surgeon, on Yellow Fever. The work (very rare) from which we made the translation, has been misplaced, and the author's name forgotten; this will not make the facts and observations, which he has collected from personal observation in the Gulf—at Havana, Vera Cruz, etc., the less valuable and interesting to our readers, especially in view of what

transpired the past season. We continue these translations from time to time, without any very scrupulous regard to connection or date. The facts will prove useful, we hope, to many who read the Journal, and feel anxious to learn the peculiarities of the disease in other regions than our own. At the time the author gathered the materials out of which his work was made, he was full Surgeon on board the French frigate "Herminie."

PRECAUTIONARY MEASURES.

1st. To avoid exposure to the sun and rain; placing one's self in a current of air, sleeping during the night in a place exposed to the dew; the guard should converse, promenade and sing, rather than remain sitting, rather than give themselves rest, from inaction and silence, to sleep which they are unable to resist.

2d. For the night guard, woolen clothing should be substituted for the light dressing of the day.

3d. If men become wet from a shower of rain, they should as soon as possible change their linen, keep in motion, and give themselves up to constant exercise, which prevents chilliness and its dangereus effects.

4th. It is important, when men with wet clothing undress themselves, that they should leave their effects in the battery and descend undressed into the forecastle, in order to avoid depositing there the elements of humidity. For the same purpose, it is proper to place there after each squall, stoves lit up with dry fuel, and cut into small pieces, as the heat of the furnace is inadequate to dessicate this part of the frigate.

5th. Lastly, the influence of the *moral* on the health of a ship's crew, being a fact incontestible, it hence becomes highly necessary to engage marines in some kind of occupation, and when they have no duties to perform, light amusements serve to enliven the spirits and divert the mind. The various exercises on board, but never too protracted—the dance in the evening, gaiety, etc., within certain limits, become excellent preservatives against epidemic diseases.

I owe my acknowledgments to the officers, for the pains which they took to enforce the strict observance of these measures, as ordered by the Commandant; thus during the winter season the crew had no communication with Havana, so that in fact no excess was committed; the introduction of fruits on board the vessel, and strong spirituous liquors, were rendered impossible, in consequence of the vigorous surveillance; finally, from 10 in the morning up to 3 of the evening, all labor was suspended.

SECOND TABLE—VERA CRUZ.

Grade and employment of the men on board.	*No. on board.*	*No. of sick on board.*	*Proportion of sick in the hundred.*	OBSERVATIONS.
Chirurgeons.	4	4	100.00	In this table I have separated from each other the topmen, assistant cooks, deck sailors and rowers; in this I have been guided by the lists on board, but I should observe that during the epidemic this distinction was not practicable; every man in good health mounted the top, rowed in the small boats, in a word, obeyed all the demands of the ship's service; hence we must not calculate rigidly from cyphers placed opposite the employments.
Superintendent of Infir'y	12	10	83.33	
Domestics	11	9	81.81	
Topmen	52	42	80.76	
2d mates & deck sailors	259	202	77.83	
Cooks and assis't cooks	9	7	77.77	
Rowers	113	81	71.63	
Bakers	3	2	66.66	
Officers and Eléves	14	9	64.28	
Caliers	11	7	63.65	
Cambusiers	8	5	62.50	
Blacksmiths	2	1	50.00	
Masters	7	3	42.85	
Total	505	382	75.64	

We perceive by glancing over the above table, that the proportion of the sick varied greatly, according to the nature of the employment on board. The highest cyphers, firstly, undoubtedly appertained to the surgeons; secondly, to the superintendents of the infirmary; and lastly, to the domestics, all of whom were in attendance upon the sick officers.

Here is, without doubt, a series of facts favorable to the admission of contagion; but I will remark that contagion is not requisite to explain this result; the surgeons, first on the list, and in the second place, all those who paid attention to the sick, experience physical fatigue far superior to that of all the rest of the crew, and were on this account alone much more disposed to contract a disease, to the causes of which all were exposed.

It is thus that among the commanding officers, the only one who died was the capitaine d'armes, whose duties, always laborious, were ten times more so during the epidemic. Lastly, a strong proof that fatigue contributes greatly to the development of Yellow Fever, is the comparatively small number of the caliers, of the cambusiers, and of the first in command; during the months of August and September all labor was suspended on board, and the provisions of two thirds of the equip-

age were distributed by the superintendents of the infirmary, instead of the victualler.

On comparing this second table with the first, (published in a former number) we discover that the effective force of the equipage was much greater at Vera Cruz than it was at Havana; this is owing to the fact, that a supplement of forty sailors were sent to us from the Antilles, to supply the place of those who had died, and of such as had been compelled to return to France for the recovery of their health. We should likewise remark, that the total number of Yellow Fever cases surpassed the number of men on board; the changes which took place on board of the frigate in part explain this result, but it was due chiefly to relapses, as I shall hereafter prove.

Symptoms.—When the Yellow Fever broke out on the Herminie, at Havana, almost all the sick were sent to the Maison de Santé of M Bélot; there I watched attentively the progress of the disease. I wrote down, at the bedside of the patients, all the clinical observations. I watched carefully both the immediate and consecutive effects of the therapeutic agents; finally, I myself made a post mortem inspection of the majority of those who succumbed, and I assisted in those performed by MM. Bélot, Legrana and Chaumel. I give these particulars in order to justify the facts which I shall cite, because I have been induced to record in this memoir many remarkable cases, presented by subjects who did not belong to the crew of the frigate.

To develop the symptoms of Yellow Fever, I shall adopt the classification of M. Bélot, and this will afford an opportunity to appreciate the relative frequency, and consequently, real value of the four types which this physician has established; I shall only change their numerical order, for the purpose of reasoning, to include the most common form, namely, that which requires the fullest explanation.

First Type—*Acute Gastritis.*

This type of Yellow Fever usually declares itself after a copious repast, an excess in eating fruits, or drinking strong liquors; when it takes place, the stomach is always under the influence of violent excitation, and it reacts suddenly upon the entire organism. This is the type which is characterized by the absence of all precursory symptoms. An individual enjoying good health eats with appetite; suddenly experiences weight in the stomach; a sense of nausea is felt and is promptly succeeded by vomiting of alimentary matters; the vomiting continues after the stomach is entirely relieved of its embarrassment. As soon

as the nausea has made its appearance, the epigastric pain has progressively augmented, then comes cephalalgia, lumber pains, and suffering in the limbs; the countenance is *vultueuse*, the ears of a deep red color, the conjunctiva injected, the skin becoming hot and entirely dry, the pulse quick, accelerated, frequent, and sometimes remarkably hard; in some cases it is tense and seems small; but the cœliac artery always produces distinct pulsation at the epigastric centre.

The hand, when placed flat upon the epigastrium, detects them very distinctly. The tongue promptly sympathises with the stomach; it is loaded with a thick mucous coat, its borders become red, it soon assumes a dry appearance, and it is particularly in this form of the disease that it becomes unequal, and as rough as the surface of a lime. The anxiety of the patient rapidly increases. Such is the state of the patient during the first hours of the invasion of this type, which runs a rapid course; happily, to compensate for such a risk, it is also that over which we can triumph with the greatest ease, when we begin the treatment at an early period. Let us suppose that nothing is done, and let us trace the development of the disease. The vomiting continues, and the sensibility of the gastric mucous membrane is so much increased that the contractions of the stomach, already very painful, become excited by the slightest cause. The liquid rejected by the vomiting deserves the particular attention of the physician; it assumes successively different colors; sometimes whitish mucosities with a yellow bilious tinge, again the brown red of pure blood, and finally the deep black of the genuine *vomito-negro*. Then the urinary secretion is suppressed or diminished in quantity; in this latter case it presents alterations in the product secreted; thus the urine becomes troubled, highly colored, and often, when a cure takes place, it assumes a black color, inducing us to believe that a true humoral crisis is effected by the kidneys. If the patient does not yet succumb, and the disease becomes more and more aggravated, the vomitings succeed each other rapidly, and are renewed by a single spoonful of any kind of liquid; the countenance is altered, the eyes sunken; the icterus, which has already commenced, rapidly spreads over the whole surface of the body; the pulse is weak, becomes unequal; the extremities become cold, and the patient is hurried into the agonies of death without exhibiting, most frequently, the slightest disturbance of the intellectual faculties.

The symptoms of the first type present a striking analogy to those of the second; besides, the disease first invading the stomach, very often attacks the small intestines, insomuch that the two types then become confounded. That which distinguishes more particularly the first

is, the suddenness of the invasion, and the great rapidity of its progress; it however demands the same treatment as the second form.

Second Observation.—*First Type of M. Bélot.* Heinrich Radmacher, a youngster attached to the Julius Edwards, entered the Maison de Santé of M. Belot on the evening of the 6th September, and died the 9th. The youth, aged fifteen years, was indisposed from the evening until the morning; he had vomited frequently; had experienced violent pain of the head and loins; and, instead of entering the hospital immediately, he had taken some medicine, the nature of which I was unable to ascertain.

6*th September*, 18—. Face injected; eyes brilliant; conjunctiva red; acute cephalalgia; general pains, particularly in the lumbar region; skin dry and hot; pulse full, hard and frequent; repeated vomiting of bilious matters; constipation; suppression of urine; tongue red and dry; great thirst; peculiar odor of the breath; pulsations at the epigastrium extremely violent. Bleeding in both feet, ad deliquim animi. Discharge of urine during the syncope; no stool; no nausea. As soon as consciousness returned, eight cups to the epigastrium, four ad nucham; purgative lavement; sinapisms to the feet; proscription of all food and drink.

7*th Sept.* He passed a tranquil night; he vomited but once. At the morning visit the pulse was less strong; the skin generally moist; the pain less intense; the tongue still red along its borders; the finger, when placed at the base of the tongue, excited nausea; however, the general condition of the patient was quite satisfactory. The evening lavement having produced only a small evacuation, it was repeated; three cups to the epigastrium; cold applications to the head. At the evening visit a fatal change was observed. The vomitings returned at twelve, and were renewed every quarter of an hour; at each attempt he rejected scarcely a spoonful of liquid; at first yellowish and bilious, but which, in the evening, assumed a brownish tinge; at the same time the face was altered; the eyes were sunken; the skin had a dirty aspect; the pulse was small; the skin cold on the extremities. Sinapisms to the feet; blisters to the legs; compound camphorated julep.

8*th Sept.* The vomiting continued, and the black matter was now perfectly distinct; life seemed on the point of being extinguished; the pulse at the radius was scarcely perceptible, and the cœliac artery still beats with energy. The blisters had produced only a slight rubifacient effect; we re-applied them; we continued the potion. But the evils progressively increased, and the patient expired between 8 and 9 o'clock at night.

Autopsy. In the cranium and chest nothing. Abdomen—The peritoneum was healthy; so were the liver, the kidneys, the bladder, and the spleen. The stomach contained nearly one third of a pound of black vomit; the mucous membrane was softened down to a pulpy consistency; it was detached by a slight pressure of the finger, and presented a black color exactly similar to the grumous matter which floats in the liquids contained in the stomach. Near the piloric extremity we found true gangrenous spots, the largest of which was from six to eight lines in diameter, the smallest as large as a grain of millet. Under these spots, which involved the whole thickness of the mucous membrane, were seen the sub-mucous cellular and muscular tissues, which were very highly injected. This injection was continued into the duodenum and as far as the commencement of the small intestine; but we could neither discover black vomit nor gangrenous spots in these intestines. The end of the small intestine and the beginning of the large, were in their normal state.

A case identical in every respect occurred in the person of a boy named Aulion, (Louis François) aged fifteen years, a youngster on board the French brig Leopold, who entered the 11th of September and died the 15th. The symptoms, including the black vomit, and the pathological alterations found after death, were all exactly similar. The only difference was, that the cause of the disease in this case was not arrested for a single moment; the violence of the symptoms continually increaeed up to the hour of death.

THIRD OBSERVATION.—*Same Type.*

Lefèvre, a domestic, aged 31 years, of a good constitution, was for fourteen days in attendance upon a sick officer, when, on the 5th of September, after dinner, he was seized suddenly, without any premonitory symptoms, with copious vomiting, at first of undigested alimentary substances, afterwards of bilious matter, acrid and bitter. After the vomiting, which occurred five or six times in the course of an hour, Lefèvre was conveyed to the Maison de Santé.

September 5. Nausea; acute epigastric pain; tongue now whitish in the middle and surrounded with a red circle; pulse strong and frequent; skin burning; face and conjunctiva highly sujected; ears of a deep red color; thirst; cœliac pulsations; cephalalgia and lumbar pains. Copious bleeding from the teeth, followed by syncope and a free evacuation from the bowels.

When the patient recovered himself, he said that he was entirely relieved; he had no pain either in the head, the abdomen, or the kidneys

nausea no longer existed ; the thirst itself had abated ; yet, as a measure of prudence, twelve scarified cups were applied ; eight to the epigastrium, four ad nucham; sinapisms were applied to the legs; drinks prohibited.

Sept. 6. Spent a good night; perspiration abundant. At seven in the morning the skin was cool ; the pulse natural ; the bitterness and acidity of the mouth had disappeared ; in a word, convalescence was established. A severe regimen, a few lavements, and repose, completed the cure. Lefèvre went out the 16th September.

The 25th he returned to his affairs at Havana, performed some duties in the city, fatigued himself, and was suddenly seized with colic, with a slight cephalalgia and pain in the limbs. Dieted for two days ; some emollient lavements, and two sinapised pediluvia, sufficed to dissipate all the symptoms of this relapse. Since this time his health has been good.

At Havana, in 1837, out of three hundred and twenty-three cases of Yellow Fever, which I witnessed, I saw only fourteen of this number, which could be referred to this first type.

At Sacrificios, in 1838, it was still more rare ; in four men only did the disease make its first appearance by vomiting, without premonitory symptoms. In three of them the skin remained cool and moist ; the cephalalgia was so slight, and the epigastric pain, when the alimentary vomiting had ceased, yielded so promptly to a single application of cups, that I have not included these men in the list of Yellow Fever cases. I have then only had but one case of the first type.

Fourth Type. *Meningo-Cephalo-Gastritis.*

I have never seen, either in the Island of Cuba, or in Mexico, an example of this form of the disease. I do not doubt its existence, since M. Bélot has reported many observations of this kind ; but the experience of two consecutive epidemics authorises me to affirm, that this type is at least the rarest of all. We should not, however, allow the consequence of this opinion to carry us too far. I do not pretend to say that the encephalon and its envelop always escape morbid reflection. In the majority of men whom I have lost, the delirium manifested itself two or three hours before death, sometimes sooner, and fully testified the part which the brain or the arachnoid played in the latter stages of the disease.

To express myself more clearly, I will assert that I have never seen the Yellow Fever progress in the order indicated in the beginning of

this paragraph ; the encephalon first becomes affected in the development of this affection.

THIRD TYPE.—*Colo-entero-Gastritis.*

This type is infinitely more rare than the second ; at Havana, the only observation which can be presented is the following :

FOURTH OBSERNATION.—*Third Type.*

M. F———, aged 55 years, of a biloso-sanguine constitution, with much *embonpoint*, entered the 23d of August, and died the 26th. During the nights of the 22d and 23d, very painful colics ; great disengagement of gas by the mouth ; five liquid stools.

August 23d—Morning. General pain in the abdomen, sense of stricture, mouth clammy, tongue foul and covered with a yellowish coat; breath inodorous; pains in all the limbs ; in the lumbar spine ; cephalalgia ; pulse frequent, small, hard and contracted ; no pulsations in the cœliac vessels ; skin dry and hot ; urine turbid, thick, producing a sense of smarting in its passage through the urethra. Sixteen scarified cups to the abdomen, to the back-head, and to the dorsal spine ; so little blood followed the scarifications of the abdomen, that the operation was repeated; we obtained nearly a pound of blood. The skin continued hot ; the pulse did not fall ; three liquid stools of a greenish yellow color and of an infectious odor.

Forty leeches to the anus ; vapor bath ; a free discharge of blood. Better; a little moisture on the skin ; cephalalgia dissipated ; pulse still remained frequent, continued state of somnolency, sinapisms to the legs.

August 24. Night disturbed ; sleep interrupted by reveries ; stools three, of yellowish brown color, and of an extremely offensive odor. Pain in the course of the transverse colon and in the region of the cœcum ; skin dry and hot ; pulse frequent ; tongue of a yellowish white color ; dull pain in the kidneys ; a sensation of uneasiness in the limbs ; cephalalgia ; oppression. Ten cups in the direction of the transverse colon ; apply 30 leeches, ten by ten, to the anus, at intervals of two or three hours. Cold applications to the head and abdomen ; a potion with nitre and camphor. Slight amelioration.

August 25. Disturbed night ; somnolency ; skin hot and dry ; heaviness of the head ; one stool almost black ; pulse quick and frequent, 95 pulsations ; persistence of the other symptoms. Forty leeches to the anus, at two applications ; continued the cold applications ; the same potion with the addition of one grain of emetic. At noon same

state, sinapisms volants to be applied successively, to the feet, to the thighs and over the abdomen.

August 26. Delirium through the night; no stool; in the morning tongue dry and yellowish; the delirium continued at intervals; pulse small, contracted and frequent; one hundred and ten pulsations per minute; skin hot and dry; the patient replies correctly to the questions addressed him; he then emerges from his somnolent condition, but to relapse again into the same state, as soon as we cease to interrogate him. Blisters to the legs; nitre and camphor; two ounces of the Ol. Ricini.

At two o'clock of the morning, cold sweat; pulse growing feeble; jactitation; slight icteric tinge of the sclerotica. Twenty grains of Protoc. Hydrarg. He succumbed at 11 o'clock.

Autopsy. Icterus well marked. The abdomen only could be opened. The stomach and duodenum sound; small intestine slightly injected at its superior three fourths; the redness augments in proportion as we approach the cœcum, and it was at this point that we found the greatest lesions; the cœcum and the colon contained a large quantity of brownish yellow matter, in the midst of which were suspended floculi of a perfectly black color, similar to coffee grounds; the intestine when washed presented a vivid color, with some brown striæ along the mucous membrane. The liver had the yellow color indicated. The kidneys and the bladder were in their normal state.

At Sacrificios, in about twenty men, the Yellow Fever broke out with dysenteric diarrhœa. The better to comprehend the progress of these particular cases, I shall quote the following example, furnished by a man named Simon. As the sailor had had the Yellow Fever on two occasions at Sacrificios, I avail myself of this opportunity to report both cases, in order that we may be able to compare with each other the two series of symptoms, and thus be convinced of the identity of the disease in both these two cases.

Fifth Observation.

Simon, a sailor, belonging to company No. 123, a rower, 26 years of age, of a bilioso-sanguine temperament.

Second Type.

April 11, 1838. During the night and day. At three in the evening face and conjunctiva much injected; violent cephalalgia; the patient could scarcely open his eyes; acute pains in the loins and in all his limbs. Tongue white in the centre, red along its borders; character-

istic odor of the breath ; skin very hot and dry ; pulse hard, full and frequent ; epigastric pulsations. Venesection, six cups to epigastrium three ad nucham ; purgative lavement ; sinapisms to the feet in the evening. Three pounds of blood were drawn by the lancet without producing either syncope, vomiting, catharsis, or a discharge of urine. The patient had passed no urine since the day previous.

April 12. No sleep during the course of the night ; skin dry and hot ; pulse not so hard ; pulsation in the cœliac arteries very active ; the cephalalgia and lumbar pain had diminished but little ; the face was constantly red ; one bilious stool during the night ; urine scanty and highly colored. Eight cups to the epigastrium, four ad nucham ; two emollient lavements.

April 13. No sleep ; great jactitation through the night ; the skin was much hotter and more dry than on last evening ; pulse hard and vibrating ; the pulsations of the cœliac artery were perceptible at some distance ; there exists at the scrobiculis-cordis a well marked depression, in the centre of which the recti muscles were distinctly seen; the cephalalgia and lumbago had increased in violence ; the patient was unable to keep one position a single moment. Eighteen ounces of blood from the arm ; fifteen leeches to the anus ; emollient lavement ; blisters to the legs ; cold applications to the head and to the epigastrium. This second bleeding was followed by syncope, by a bilious stool, by nausea, and a free flow of perspiration ; the patient urinated half a glassful ; prompt relief.

At noon amelioration continued.

April 14. One hour's sleep in the night ; the skin was soft, much cooler ; the pulse had fallen ; the cephalalgia not so intense ; the pain of the kidneys still continued to annoy the patient ; the urine was more abundant, but always highly colored ; two copious liquid stools during the night ; the epigastric pulsations almost entirely disappeared ; the blisters drew well. Diet; emol. lave. suppurative dressing for the blistered part ; cold application to head and abdomen.

April 15. Skin still a little hot, pulse quick and frequent, yet he did not complain of any pain ; the tongue loaded with a thick adhesive coat ; odor of the breath disagreeable. Diet ; cold water dressing for blister ; emollient lavement.

April 16. Skin hot ; pulse small, contracted and frequent ; eyes sunken ; pulsation at epigastrium very distinct ; urine more abundant, not so high colored ; prostration. Diet ; ptisan of barley, two spoons-

ful; a potion of nitre and camphor, one spoonful every hour; cold water.

April 18. Sensible improvement; from this day the patient progressively advanced in improvement.

Third Type.

August 16, 1838. Violent colicy pains in the night; a sensation or obstruction in the course of the transverse colon; pressure painful; from eight to ten liquid stools, with griping; tongue white, moist, and red at the point; pulse but little developed; skin hot, but moist with perspiration; no nausea; some wandering pains in the limbs. Ptisan of rice with four drops of laudanum; demi-lavement of starch. Three o'clock in the evening; he had also had five stools from twelve to three o'clock; then the face red, injection of the conjunctiva; acute cephalalgia above the orbits; pain in the epigastrium, in the lumbar spine, in the joints of the lower extremities; skin dry and burning hot; pulse hard, full and vibrating; energetic pulsations of the cœliac trunk—the tongue white and moist; characteristic odor; obvious depression at the epigastrium; the urine suppressed since the morning. Syncopal bleeding, eight cups to the epigastrium, four ad nuch., demi-lavement again; in the evening sinapisms to the feet. The loss of thirty-five ounces of blood produced complete syncope, vomiting twice, one operation of the bowels, a free discharge of very turbid urine, and an abundant perspiration. The relief was instantaneous.

August 17. The fever had greatly diminished; the pains were less acute; both pulse and skin better; no stool. Diet, acidulated gargarism; emollient lavement.

August 23. Convalescence declared itself on the 24th; a little fever and some agitation without any pain. I interrogated the patient and I learned that he had not been to stool for many days; pressure over the epigastrium produced pain; a few cups entirely dissipated it, an emollient failing to operate, an injection of salt and water produced a consistent and copious stool, and since then the case has been uninterrupted. The patient's exit is dated the 8th of September. The 3d of October he had the scurvy, but convalescence was protracted and it was not until the 25th of May that he resumed his labor.

Reflections. If we compare carefully the symptoms of the 11th of April with those of the 16th of August, as developed at three o'clock in the evening, we shall easily recognize their identity. The progress of the disease was suddenly arrested in the second case by the synco-

pal bleeding ; in the first case, syncope did not take place, and the evils were prolonged. But this is only a difference in the course, and not in the nature of the disease. The observations of the 16th of August are only distinguished from those of the 11th of April, by the diarrhœa which ushered in the attack. But this diarrhœa ceased spontaneously, so to speak, in the course of ten or twelve hours, and it was only when it had been arrested that the pathognomonic signs of yellow fever manifested themselves in reality. Does the symptom, which constitutes the beginning of the vomito, authorize the creation of a type ? I believe not, for the treatment is absolutely the same as in the second type ; only that it is not necessary, in consequence of the frequent stools, to prescribe a purgative lavement ; moreover, remark on this subject, that I have always seen the yellow fever yield more readily when it had commenced by a diarrhœa, just as often as it happened that convalescence follows closely copious and repeated alvine dejections.

I could cite the cases of Bernard, Boules, Callet, Deroff, Rersalvo, Croisier, Degall, the quartermaster, &c., but they are exactly similar to that of Simon ; the disease in all commenced with a diarrhœa of some hours' duration, which in all cases ceased of itself. Again, being enlightened by clinical observation, when a sailor came up, complaining of diarrhœa, I was content, for twenty-four hours, to make him drink rice water. If the diarrhœa was only a *prodrome* of yellow fever, the latter manifested itself in the course of the day, and I had not interrupted a favorable critical effort ; but if, on the contrary, it was a prelude to a dysentery, I had not lost much by deferring for a single day the treatment of this affection.

It now remains for me to demonstrate that the case of Simon, and similar ones, ought to be referred to the second type. Thus, in three hundred and twenty-three cases, at Havana, and in three hundred and eighty-two at Sacrificios, I have really seen but a single example of the third type. This form of the disease is then still more rare than *acute gastritis*, and in fact there remains only the second type, which includes in itself all the cases of yellow fever, with the fewest possible number of exceptions.

It is well understood that these conclusions are only applicable to the facts which I have observed, and which I now give ; for I cannot too often repeat, the yellow fever may, according to localities and certain circumstances, modify its power of development ; in a word, the history of an epidemic is very far from being the veritable history of all other epidemics of the same form.

It is evident, moreover, that in the yellow fever, as well as in many other diseases, nature sports with our methodical classifications, and sometimes presents to us, united in a single individual, the greater part of the arbitrary divisions which we have admitted. Under this head the following observation seems to me interesting, because the morbid lesion assails successively different organs of the body.

OBSERVATION.

Coubart, a fourrier of the 122d company, aged twenty years, of a nervoso-sanguine constitution, entered the 15th of September, 1837, died the 16th November following. At his entry into the Maison de Santé, this man appeared but slightly affected. Although he presented all the symptoms of the yellow fever, as they were not intense, and as the skin was moist, he was not bled; cups, leeches to the anus, purgative lavements, and a rigid diet, soon triumphed over the disease. Still convalescence was not fully established or confirmed.

Coubart sometimes experienced a little malaise of short duration; it is true that he had, on the internal faci of the *preputium*, venereal ulcers, which had produced phymosis, and after operating on it, a very high degree of inflammation had determined a fever for some days. However, he finally reached a satisfactory state, and he was on the point of returning to his duties, when on the eighth day of November he relapsed.

November 8. Febrile movement, heaviness of the head, wandering pain during the night; at the morning visit, abundant bilious vomitings; pain in the right hypochondrium; cephalalgia, a sense of uneasiness in the lumbar region, pulse frequent and well developed, skin dry and hot; thirst, tongue loaded with a whitish coat. Absolute diet; twelve cups to the abdomen; four ad nucham; two emollient lavements.

9*th*. Aggravation of symptoms; vomitings more frequent; injection of the conjunctiva, which presents a slight yellow tinge. A potion of camphor and nitre; purgative lavement; in the evening hot pediluvia. Syncope during the footbath.

10*th*. Restless night; frequent vomitings; six stools during the night; six cups to the abdomen; twenty leeches to the anus; two lavements of starch; cold applications to abdomen.

11*th*. In the morning some perspiration, pulse more regular; the vomiting was arrested; one operation on the bowels; the icterus, which since the 9th continued to increase, was complete. In the eve-

ning the patient expressed much anxiety in relation to his condition. The pupil was dilated ; urine natural. Diet—half cup of tea, to be taken in the course of the day; suspended the potion. Evening, two blisters to the legs.

12*th.* Delirium during the night ; great anxiety ; a little more calm in the morning. Pain in the epigastric and hypochondriac regions. We repeated the application of the leeches and continued the cold fomentations. He has neither been purged nor vomited ; urine flowed freely ; the blisters suppurated finely.

13*th.* Same state. Besides, the tongue was surrounded by a bright rose colored circle ; in the centre, some reddish points begin to develop themselves, which predicted a buccal hemorrhage. The patient complained that his sight was a little obscured ; he required some time to recognize those persons who approached his bedside; he spoke of death, which he seemed to fear greatly. Same prescription ; two other blisters to the thighs.

14*th.* Acute pain in the direction of the transverse colon ; dryness of the skin ; pulse frequent and contracted ; a number of black fetid stools ; the red circle bordering the tongue disappeared ; extreme prostration; the patient had some slight wandering of the mind, yet he replied correctly to the questions addressed him. A suppurative dressing of the blistered surface. An anodyne draught ; emollient lavements. Evening, blister to the neck.

15*th.* The state of the patient was aggravated, the urine flowed less freely ; frequent stools of dark matters mixed with blood ; a number of discharges of almost pure blood, until the close of the day. He expired in the night.

Autopsy. Icterus, arachnoiditis, encephalic structure sound and consistent ; nothing in the thorax.

Abdomen. Stomach and small intestine so much contracted, that the latter is very little larger than a writing quill ; walls thickened, mucous membrane of a reddish gray color, doubled on itself, and forming longitudinal folds in the stomach, but circular and oblique in the intestine, these folds were of a black color, and evidently softened. The large intestine was distended and dark on its exterior surface ; its walls were attenuated ; the whole of its calibre, from the cœcum to the rectum, was filled with an immense quantity of black, viscous and decomposed blood, exhaling an odor of putrefaction. The parenchyma of the liver was of a dull yellow ; the biliary vesicle contained but little bile ; the bladder was full of urine ; the kidneys sound.

In this observation, the progress of the disease appears to me to offer much interest. First, it is the stomach which suffers; afterwards the liver and small intestine; then the tongue assumes an hemorrhagic appearance, but it fails to take place, and the patient succumbs to a true intestinal hemorrhage. The symptoms, during life, correspond precisely with the lesions after death; we find traces of the disease in the stomach and small intestine; but the greatest lesions exist in the large intestine. The abnormal condition of the liver corresponds with the pain of the hypochondrium; the arachnitis with the delirium. This single example presents, when united, in the short space of eight days, most of the symptoms belonging to the fourth type.

We may remark, to anticipate, the identity which exists between the coagulated, black, viscous blood which the large intestine contains, and that which we find to constitute the intermuscular effusions.

Second Type.—*Entero-hepato-gastric.*

This form of Yellow Fever, the most common of all, differs from the first type, not only from its seat, which is much the most extensive, but moreover, from the existence of precursory symptoms.

It seems that in the first there had been a sudden poisoning, so quick and overwhelming is the attack; in the second, a gradual introduction of a *tonic* agent, which progressively modifies the organism, until, accumulated in sufficient quantities, it unexpectedly develops the disease.

Prodromes. The prodromes of Yellow Fever are general or pathognomonic; it is unnecessary for me to treat of the first. The second are, a peculiar sensation of uneasiness in the lumbar region, a cephalalgia most frequently supra-orbital, slight dimness of sight, which is almost never wanting; an odor *sui generis*, exhaled by the mouth, and with which the tongue seems highly impregnated; this last symptom is almost infallible; every individual who presents it, whatever may be, in other respects, his apparent health, is threatened *du vomito.*

Symptoms. After one or more days, this general malaise is dissipated, or rather each of the symptoms is exasperated, and the Yellow Fever declares itself. The actual invasion is often marked by a chill, to which succeeds a burning heat; at other times the heat is primitive. There is a feeling of heaviness, and sometimes of acute pains in the joints, which make it impossible to maintain the erect position. The lumbar region suffers from similar pains. The cephalalgia is oppressive; the face is animated, and so red as to remind us of eruptive fe-

vers; the conjunctivæ are strongly injected, the tearful eye can scarcely bear the impression of light. The tongue grows red and soon becomes dry; the breath of the patient is characteristic; the skin dry and burning hot, or covered with a slight degree of moisture; the epigastric pain is very severe and generally extends to the right hypochondrium; the pulsations of the cœliac artery are very distinct; it is rare that any nausea occurs during the first day; the thirst is intense; the urine suppressed, or at least diminished in quantity; constipation often exists; the pulse is strong, full, hard and frequent; a sense of anxiety is stamped upon the patient's countenance.

These symptoms continue for a period of time which varies from one to four days; but if the disease is abandoned to its own course, and sometimes in spite of treatment, from the second day of their appearance these phenomena assume a new character of gravity. Then the tongue is covered with a coat whose consistence and color present a thousand varieties; nausea and then vomiting supervene; the face is discolored and assumes an air of dejection; icterus is developed; it begins in the conjunctiva, and finally spreads over the entire surface of the body; the circulation is reduced; the respiration becomes embarrassed; the gums, the lips, the tongue and the nose discharge black liquid blood; the parotids swell; petechial eruptions, and sometimes gangrenous spots appear on the body or limbs. Death soon releases the sufferer.

But this simple enumeration of symptoms is by no means sufficient; many of them are of the utmost importance, and consequently demand special attention.

A. *Icterus.* Icterus constitutes a phenomenon too prominent for authors to have forgotten to mention it; on the contrary, they have dwelt at length on this point; this symptom has likewise given its name to the disease which I am describing. Yet, we repeat, it is a sign which is very often wanting, and I shall have the same remark to record constantly in reference to the black vomit, so that Yellow Fever exists without yellowness, and without black vomit. This proves, to my mind, that the disease to which we have given these two names is not a local affection, as these nomenclatures would have us believe; if the morbid changes were confined to the stomach and to the liver, the effects produced should always be the same, and the black vomit as well as the icterus should never be absent. If, on the contrary, the lesion which constitutes the Yellow Fever was generalized, and distributed at the same time, to one or more apparatuses, we might readily con-

ceive that some of its phenomena of outward expression were not fully developed, without, on that account, their absence giving room to the smallest rational presumption against the identity of the disease.

When, in spite of the gravity of the Yellow Fever, the icterus is not present during life, it is almost always established after death; the exceptions to this general rule have been extremely rare indeed, both at Havana and at Sacrificios.

The time at which the yellowness makes its appearance in the course of the disease, is, according to the observations of M. Bélot, a prognostic sign of great certainty. When icterus establishes itself towards the third or the fourth day, death is almost inevitable; towards the fifth or the sixth, the chances are rather against than in favor of a case; lastly, when it is not developed until after the seventh day from the invasion, the patient is almost sure to be saved. I have been enabled to verify the justness of this remark, and I will cite, among many examples, the two following:

1st. The steersman Lohier entered the 8th of September, and died on the 17th; after the second day a considerable icterus covered the entire body.

2d. A man named Victor, a sailor on the packet boat of Bordeaux, No 4. This man being seriously attacked, manifested moreover great disquietude; the eighth day of his disease, when the yellowness commenced, the icteric tinge was very manifest, and convalescence was soon established.

Is the icterus owing to an effusion of blood—to an ecchymosis, or is it purely bilious in its character?

On the one hand, when the yellowness develops itself during life, we are almost certain to find, after death, the color of the liver altered; the biliary vesicle is often distended with a great quantity of black and thick bile, as though this fluid had ceased for a long time to flow into the duodenum; there may be then a re-absortion and a transportation of bile into the torrent of the circulation; or perhaps in consequence of the interruption of the functions of the liver, the materials which are furnished for the secretory action of this organ are retained in the blood.

On the other hand, the blood is altered in the Yellow Fever, and becomes everywhere infiltrated with great facility. It is then probable that the yellowness borrows its characteristic qualities from this two-fold etiology.

B. *Black Vomit.* At Havana and at Vera Crux, I have witnessed but a small number of cases of black vomit. The frigate Iphigene,

which was anchored near us at Sacrificios, and which has had much greater losses than we to deplore, presented in more than forty deaths, only three or four cases of black vomit.

To what cause is the rarity of this symptom to be attributed? I believe that it is easy to indicate it. The marines of ships of war promptly come forward, on the slightest indisposition, to claim the assistance of the physician ; the disease is combatted as soon as declared, and if a treatment as energetic as that which constitutes the method of M. Bélot does not always rescue the patient from death, it at least suffices, when timely adopted, to prevent a rapid sanguino-congestion upon the digestive tube, and thus to spare the patient the horrors of black vomit. The material which constitutes this fluid being blood, the result of a profound lesion of the mucous membrane, whose secretions are mingled with the altered blood, escape either by exudation or rupture from its vessels ; it is necessary for this to be accomplished, either that the inflammation should be acute, or that a poison of great activity should be brought in contact with the walls of the stomach or the digestive tube. But, since the miasms which produce Yellow Fever possess a medium of transmission in the torrent of the circulation, it is evident that by diminishing the influx of the blood towards the organs of digestion, we remove at once the two primary causes of black vomit.

It often happens that in an autopsy we find the black matter in great quantities in the stomach and intestines, when in consequence of the absence of all nausea and any peculiar appearance of the tongue, we would not have suspected its existence ; it is at the close of the disease, at a moment in which the effusion takes place, when the interior organic dissolution precedes the complete abolition of the vital forces ; the sensibility of the stomach is exhausted, its contractility destroyed, and its sympathies extinguished. I have often remarked this at Havana, and I am persuaded that in the majority of patients who died at Vera Cruz, the black matter existed in the stomach. Unfortunately, I have not been able to satisfy myself of the fact; we know that in the midst of an epidemic which attacked so many individuals at the same time, it was impossible to make autopsies ; besides, on board, where seclusion was impracticable, a necropsy performed in public would have produced on the *morale* of the crew the most pernicious effects.

C. *Exhalation of blood from the mucous membrane.* When the Yellow Fever, instead of improving under the influence of therapeutic means, continues to grow worse, it may happen that the mucous membrane of the nasal passages and of the mouth shall suffer black and liquid

blood to escape from their vessels. The fluidity and deep color of the blood deserve to be taken into consideration, and to recall to mind the exhalation which takes place in the stomach; the hemorrhage is rarely so great as to become a cause of uneasiness ; sometimes, however, it tends to hasten death. When this hemorrhage is abundant, a short time after the invasion of the Yellow Fever, it may be regarded as a salutary crisis ; it is not so when it occurs at a later period of the disease ; yet, a man named Rivoal presents a striking exception in this particular. On the 13th of November, 1837, the sixth day of the disease, this sailor complained of a spitting of blood; an examination displayed a tongue surrounded by a red border, but smooth on the surface ; the central part exhibited inequalities, owing to the papilæ, which were erect, conical, of a red brown, and which permitted a small drop of blood to exude ; the lips were cracked, as from the action of cold, and these small solutions of confinuity give rise to the discharge of a dark liquid blood. This hemorrhage continued four days, caused the patient to lose from 20 to 30 ounces of blood, and was for lowed by complete recovery.

V.—REMARKS ON THE NATURE AND TREATMENT OF TYPHOID FEVER.

Dr. A. Hester:

The following thoughts are respectfully submitted to your consideration, if perchance they should contain any thing worthy of it. Notwithstanding it is my lot to be cast far from the seat of scientific research, yet (thanks to the medical press) have the genial rays of the sun of science penetrated the dark vista, and I have read with the most profound pleasure many of the articles contained in your Journal. There is one subject in which I have taken the greatest interest—Typhoid Fever. Can this disease be cut short in its forming stage ?—can it be arrested, or must it be allowed to pursue a regular course to its terminuation, as small-pox, scarletina, measles, etc. ? Although this important question has been ably and extensively discussed, there are some points which seem to have been almost entirely overlooked. I believe it is an admitted fact, that diseases, of whatever name or order, are influenced by, and partake, to some extent, of the nature of the most prevalent diseases of the particular locality in which they occur ; and

more especially is this the case when that influence is of miasmatic origin.

Pneumonia, for instance, being purely an inflammatory disease, under ordinary circumstances requires no other than a depletive course of treatment to arrest it, whereas when it assumes a typhoid form, which is frequently the case when this disease is prevalent, we must use tonics and stimulants, and if complicated with the pathological element of periodicity, we find it indispensable, in order to counteract this influence, to use quinine, though it may evidently act as a stimulant, and be positively contra-indicated by every other symptom. No reasonable man will say that we use it here for its sedative effect, or because we believe it to exert any specific influence over purely inflammatory diseases. We know that two specific, distinct diseases may, and do frequently complicate each other, (gonorrhœa and syphilis, for instance) and in the treatment of such cases we must use the specific remedies applicable to each.

Now I ask, may not this view of the subject serve greatly to facilitate the investigation of this vexed question—the therapeutical value of the sulphate of quinine in the treatment of Typhoid Fever. Why is it that this war in the profession is so much more rife here in the South than amongst our Northern brethren? I think the answer is plain—simply because this morbific cause, called miasma, is more generally diffused through our Southern country than is the case in the North. Far be it from me to advocate the theory that quinine is worthless, nay even poisonous, (as some have done) in Typhoid Fever; it would be injustice, when I feel quite sure that to it I am indebted for the life of many friends, who would have been swept off by this disease but for the prudent use of this drug. On the other hand, I am well assured that great men are liable to err on the opposite extreme. There can be no doubt but that Typhoid Fever, as it occurs in the South, is more frequently than otherwise modified to some extent by this miasmatic influence, which being the case, necessarily points to the use of the antidote to this poison, and he who obstinately refuses to administer it, under such circumstances, because he believes the disease, when uncomplicated, is not controllable by it, is either an empiric or legally culpable. Cases have occurred under my own observation, in which I have removed the tendency to periodicity, and yet the typhoid symptoms still continued.

Why is it that the strongest advocates of the abortive treatment frequently fail in their expectations? Is it not because they have a case of purely, specific, uncomplicated Typhoid disease? Why is it that

many of us have so often most happily succeeded in cutting short a disease which presented almost every symptom of Typhoid Fever? Is it not because we have mistaken cases of bilious fever for typhoid, the distinction between which is not always so apparent upon a superficial examination, more particularly if there is a decided inclination to diagnose a case of typhoid.

I am aware that I may be here met by the theory of the convertibility of all fevers, and the unity of cause in their production, which being the fact, would of course lead us to the use of the same remedy in their treatment. A few interrogatories to the advocates of this theory may serve to substantiate the views already advanced. If typhoid and intermittent fevers are of common origin, produced by the same cause, why is it in some localities, where Typhoid Fever is prevalent, intermittents and remittents are almost unknown. Is it not probable that there would be at least some comparison in the relative frequency of their occurrence? Until the fact is established that Typhoid Fever never exists except in malarious districts, or in those who have been exposed to this influence, it must ever remain a distinct disease, produced by a distinct cause, to say nothing of the pathognomonic lesion of Peyer's glands, and the great weight of authority in favor of this distinction. (See Bartlett on Fevers.)

To my mind it is evident, that the great reason why many have fallen into this error is, the modifying influence miasmatic poison is capable of exerting over the progress of this disease, and the difficulty with which it is sometimes distinguished from bilious fever, particularly in the incipient stage. Again, it is very evident that intermittent and remittent fevers may assume what is usually termed a typhoid type, if not properly treated; but this is far from being a case of true Typhoid Fever; such cases might be cited as evidence of the convertibility of the two diseases, but this argument loses its force when we remember that they occur only in miasmatic districts, whereas Typhoid Fever is not confined to such localities.

The above remarks are founded entirely on my own experience. I have observed this disease where Typhoid Fever was of more frequent occurrence than all others combined, and where intermittents scarcely ever appeared. I am now observing it in a miasmatic district, where typhoid is of very rare occurrence, and intermittent and remittent fevers prevail extensively. I have used quinine in typhoid cases, as recommended by Dr. Fenner and others, and I have never seen any deleteri-

ous effects from it ; on the contrary, it often reduces the frequency of the pulse, and produces a moisture of the skln, which is always a desirable object, though I cannot say that I have ever succeeded in cutting short the disease.

The veratrum viride in my hands has been of inestimable value.

Very respectfully,

L. Y. GREEN, M. D.

Mississippi, November 23, 1853.

VI.—PATHOLOGY AND TREATMENT OF APOPLEXY.

A Letter from Dr. Cornelius S. Baker, of Bucks County, Pa., addressed to and answered by Samuel A. Cartwright, M. D., New Orleans.

Dear Sir—When I last wrote to you, I supposed that in all probability our correspondence was terminated, since my ends were satisfactorily accomplished, and beyond my expectations a series of valuable papers from your pen have been obtained, suggested in part, perhaps, by my interrogations. But the paper published in the Boston Journal for October 6, on Apoplexy, &c., has, by an irresistible impulse, driven me again to my pen, so that it "is not I," but the spirit of the essay, that is the occasion. I know of no other medical practitioner who would venture the assertion that "for 25 years I have been in the habit of curing Apoplexy almost as readily as intermittent fever," and know truly that it would never have dropped from your pen without due consideration! It is *this* that surprises me.

I have long regarded the theory and distinctions of the books as fallacious, and their conclusions as liable to lead to erroneous practice—at the same time I distrusted my own views and waited for further observations. In the first place, *it is not true*, that the flushed face, turgid veins, and other marks of cerebral congestion, are the first indications of an apoplectic paroxysm ; on the contrary, they are the *second* stage of the paroxysm, and only a result of the preceding, at least so I have regarded them. The first stage is characterized by a death-like pallor and a shrunken countenance ; vacant swimming of the eye within its orbit, and an idiotic or demented expression of the countenance. These phenomena are well attested in the cases that have fallen under my notice, in which an opportunity for observation was afforded. This

first stage is only of momentary duration, yet it is as well defined and as characteristic as that which follows, and which, as I stated before I regard as only a consequence or result of the former. The first may indicate a suspension, interruption, or obliteration of mental power in the brain or nervous system ; or just that condition, if more local, which we would denominate Palsy, which I am strongly inclined to believe to be one and the same, so far as both may be regarded as diseases of function, with the former ; the one being *local* and the other *general*. Of course, both Palsy and Apoplexy may, and often do exist, in consequence of organic lesions, alterations of structure, effusions, morbid growths, &c. ; with these we now have nothing to do, since it will scarcely be questioned but that they as often, and perhaps more commonly exist as mere functional diseases, and as such not susceptible of being demonstrated by the scalpel.

If the preceding views are correct, then a reason is afforded for the sudden effusion of the countenance, congestion of the brain, &c., which so uniformly attend an attack of this disease. The brain, no longer able to defend itself, or to resist the action of the heart, and removed from the influence of atmospheric pressure, by its bony encasement, creates a vacuum and invites the flow of blood to itself as it is propelled from the heart. How far some of the cases that show organic lesions are a mere result of the foregoing condition, is an interesting enquiry.

There are yet two other phenomena to be noticed, constantly attending upon Apoplexy and other spasmodic diseases, which very materially enhance their danger, and in no inconsiderable degree embarrass the treatment, viz : the spasmodic closure of the larynx and convulsive action of the respiratory muscles, and a copious excretion of tough, viscid phlegm from the mucous tissues, expressed, in a word, by an increased mucosity. Account for the phenomena as we may, the conditions uniformly exist ; they may, and possibly are, like the congestion, *mere* results, and if for the laryngismus, &c., we accept Marshall and Hall's "Excitor-Motory" theory, we are still in the dark as to the sudden filling up of the trachea and bronchia with such a superabundant amount of mucus. In the earlier part of my practice this condition was forced upon my attention while in attendance upon protracted cases of hemiphlegia. Patients who had hitherto been free from all suspicion of pulmonic disease, will very soon after an attack of paralysis begin to excrete and throw off large quantities of tough phlegm from the bronchial surface, very much resembling the increased mucosity incident upon the relaxation of the mucous tissue consequent upon sen-

ility; and in one instance that now occurs to my mind, the discharge was so great, and its debilitating influence so apparent, as in other circumstances to suggest the idea of "hasty-consumption." This condition, so closely analogous to that observable in Apoplexy and other spasmodic diseases, has led me to infer a closer connexion between the mucous and nervous systems than is generally recognized, and to regard this copious excretion of mucus as a result from the loss or suspension of nervous power in the brain. I am fully aware that the term "loss of nervous power" is in itself an opprobrium; in our present state of knowledge it is an ultimate fact, and as such we employ it.

The existence of the above conditions, in embarrassing the respiratory act, and in retaining carbonic acid within the lungs, not only of themselves are adequate to the production of asphyxia, but are probably efficient agents in preventing or retarding any recuperative effort on the part of the brain itself, and thus perpetuates the condition on which all the others are dependent, and thereby very materially lessens the prospect of recovery.

Such, in a few words, have long been my views of the nature and conditions of phenomena observable in Apoplexy; but to adopt a corresponding treatment, without being reasonably certain as to their correctness, would savor more of the effrontery of the charlatan than the caution of an enquirer after truth. Fortunately for my own comfort, in a diffuse country practice, the story is generally told before our agency can be expected, for either weal or woe; at least it has so happened to me.

In a recent case of paralysis, involving only the hand and forearm, but attended with vertigo, dimness of sight, and other symptoms involving the brain, I had recourse to the lancet, but with no benefit whatever. I have (according to custom) employed it in paralysis, epilepsy, and other spasmodic affections, with like results, save in one case, in which I am satisfied its effects were decidedly pernicious. From all this I have been led to infer, that the lancet was not "the all" in Apoplexy, nor the first. Emetics promise the most, and according to my view, more completely cover the *whole* ground than any one class of agents; but this "congestion," like the ghost of Macbeth, ever stares me in the face. I am pleased with the irritant emetic you have suggested, and have no doubt but it is the "very thing." The hot water is all of a piece with the foregoing; but surely there can be "no fear" of congestion, with all its train of ills "before your eyes," or you

would not have ventured the ordeal. Would "Granville's Lotion" be a substitute, in your estimation ?

The ordinary course of nature, in throwing off the influence of "concussion," has strongly impressed me in favor of emetics; often the very first indication of returning consciousness is denoted by a free emesis, and when the act is consummated, the patient often seems to wake as from a profound sleep. So uniformly has this occurred, that I have no hesitancy in such cases of availing myself of the benefit of a prompt emetic in bringing on reaction, in cases where they can be administered. I think Charles Bell says, the landlady who administered a glass of hot punch to a patient laboring under "concussion of the brain," is, rather than the surgeon who bleeds him, "the better surgeon of the two!" I never was quite satisfied with either, but would look to an *irritant*, rather than either a stimulant (in the true sense of the term) or a depletant.

There is yet another difficulty. Suppose a diseased condition of the arterial coats to exist, and of the existence of which we can never be certain, if upon the sudden invasion of Apoplexy and the consequent violent influx of blood upon the brain, a rupture of the arterial coats should occur—would blood-letting, in this passive conditi on of the brain have a greater tendency to lessen the amount extravasated, rather than an irritant emetic, by arousing it from its torpor, and thus better regulate the circulation? Or, is the one mode safe, and the latter hazardous? I doubt the efficacy of any treatment in the generality of such cases, yet the books seem to hold out the idea that an "apoplectic-clot" of a former period has been found, snugly stowed away in some vacant corner of the brain.

The foregoing, though greatly extended beyond what I at first designed, is but an introduction to the immediate object I had in view, and which might, after the manner of the ladies, about as well be embodied in the postscript, viz: to solicit from you, if practicable, *the paper published in the New Orleans Journal*, referred to in the essay under consideration.*

I know not that my suggestions are of any value; yet such as they are you will receive as an expression of my high regard for your services in our profession. Should you feel disposed to notice any of the queries that presented themselves in the course of writing, will you, in addition, inform me as to your *mode of insufflating* the lungs, as in the case of young Ely? I do not think that we have at the North any con-

* Dr. Baker refers to my paper on Apoplexy published in this Journal some two years ago.

venient apparatus, beyond a common bellows and stomach tube. I mentioned the case of Ely to Professor Mutter, whose first inquiry was, as to the mode and apparatus; from this I infer the correctness of my conclusion. If we are to reap such glorious fruits from Hæmatokinety, and I scarcely entertain a doubt on this subject, it becomes a matter of importance that a convenient instrument should be at our command, and more seems requisite than merely to force atmospheric air into the bronchial tubes; it seems to me important that the action of a suction pump should be conjoined, to remove the effete air that may already be locked up in the ultimate air cells of the pulmonic tissue. In cases of asphyxia, I should think it desirable to pass the nozzle of the tube within and beyond the glottis; this would not be difficult with an ordinary gum elastic stomach tube, yet it prevents the free return of the air thus forced in, and consequently it is objectionable, at least to allow its remaining for any considerable time.

I remain, my dear sir,

With great respect, yours, &c.,

CORNL. S. BAKER.

SAML. A. CARTWRIGHT, New Orleans.

THE ANSWER

To Dr. Baker's Inquiries, with some additional comments on the Pathology and Treatment of the Apoplectic forms of Disease.

Prefatory Remarks.

I deem it proper to observe that Dr. Baker's letter was not intended for publication. It is one among several very interesting letters I received from that gentleman, and I have ventured to publish it without having obtained his permission to do so. It is too valuable to remain private property; and when the late Dr. Hester called on me for its publication in the New Orleans Medical and Surgical Journal, I could not refuse without doing injustice to the profession, to whom it rightfully belongs. I am sure if Dr. Baker was as well satisfied as I am of the good it is calculated to do, he would not permit modesty or any personal consideration to bar the claims of science to it. I should not have ventured to have sent it to the press, without his permission, if, not only myself but others to whom I submitted it had not have regarded it as a production entitled to rank among the highest, and of which any physician, whether of city or country, might justly be proud. Soon after it was sent to the press, and before I had prepared some remarks upon it by way of reply, which I had promised to accompany it, the profession

and this city met with a deplorable loss in the unexpected and sudden death of Dr. Hester, the able editor of the New Orleans Medical and Surgical Journal. Supposing that the publication of the present number of the Journal would be postponed, I neglected to prepare the reply until the printers began to set up the letter, and now have to write as they print, with constant demands on my time from pressing professional business. This must be my apology for not being able to make what I have to say more worthy of the subject of which it treats. Fortunately any failure on my part will not impair the value of Dr. Baker's communication, as it opens several new doors to the investigation of an old subject of great practical importance, which increases, instead of diminishing in interest, with the lapse of ages. Should I fail to stumble on any thing convincing or useful by entering in thereat, the way will still be open for others better qualified to make explorations on those important points to which he has called attention—nothing doubting but that they will find most of the apoplectic forms of disease, now so formidable under the treatment recommended by Watson and other text books of the day, not only as obedient to medicine as intermittent fever, but a good portion of them, including convulsions in children, mere gastric irritations, bad colds, or intermittent and remittent fevers in disguise.

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Dear Sir:

The remark that "for twenty-five years I have been in the habit of curing Apoplexy almost as readily as intermittent fever," was made to Dr. Jackson of Boston, Professor of Pathological Anatomy in the Medical College of Harvard University, and I deemed it unnecessary to add, that Apoplexy from structural disease of any important organ, as the heart or brain, was excepted. But I must admit that, including the most of the cases in that numerous class of diseases attended with coma from functional derangement, the assertion was a bold one, as it announced the attainment of an object, supposed to be a great way off, but which the progress of the sciences has put within the reach of practitioners of very moderate attainments. I include under the term the various apoplectic forms of fever, convulsions and epileptic fits in children and adults, the different phases of asphyxia, sun stroke and congestive maladies, where the sentient system is more or less suddenly stricken with torpitude—mental or corporeal, or both—general or partial—which are not the effects of inflammation, mechanical violence, or the wasting of the vital energies from pre-existing disease. I would not be understood as saying, that all the various forms of apoplectic affections, embraced in this definition, when depending only on func-

tional derangement, are almost as readily cured as intermittent fever, but I contend that the great majority of them are, when promptly succored by science, not blinded by an erroneous theory, or trammelled by dogmas drawn from a hypothetical physiology. Some of them, I admit, are from necessity fatal, owing more, perhaps, to the shortness of time given to act, than to any thing in their nature. Asphyxia, for instance, from the inhalation of carbonic acid gas, apoplexia fortissima and that form of coup de soleil, which Dr. Dowler, in an unpublished work, calls *solar asphyxia*, often occurring in this warm latitude and extinguishing life in thirty minutes, and altogether different from the coup de soleil of more northern latitudes, which he calls *solar exhaustion*, are incurable from their falling into an incurable state before medical aid can be obtained. Whereas that form of coup de seleil, properly called *solar exhaustion*, is even more quickly cured by the proper remedies, than a common intermittent fever, and is almost as fatal as solar asphyxia if improperly treated. Laudanum, brandy, cold affusions and rest in a horizontal position, will quickly cure conp de soleil produced from mere solar exhaustion. It is, in fact, a mere fainting, the face being pale and the skin cold, or not above the natural temperature. In solar asphyxia, the face is flushed and the skin burning hot to the touch. Dr. Dowler by actual experiment, has found the temperature as high as 112 degrees of Fah., the sphinctres relaxed, and mind and body utterly insensible to impressions. In cases of this kind, post mortem examinations furnished no evidences sufficient to account for the symptoms, or fatal termination within the cranium ; but sanguineous infiltration was so abundant in the substance of the lungs, that instead of the usual crispy feel under the knife, they cut, says Dr. Dowler, like a mass of jelly. I mention these facts, not to anticipate Dr. Dowler's intended work, but to give interest to it, if I could, and to hasten the publication of his vast and highly important contributions to science. They demonstrate the role the brain plays in apoplexy, attended with general and complete insensibility to pain or impressions of any kind, is only secondary—that it is not properly a disease of the brain. The assumption that it is a disease of the brain, or nervous system, is founded on an erroneous hypothesis and is the main cause of the apoplectic forms of disease being less amenable to treatment than remittent or intermittent fevers. The latter diseases are about as often intractable, at present, as Apoplexy would be under a judicious practice founded on observation and experience, instead of those vain hypotheses drawn from a speculative physiology, which decoyed the medical

teachers of the last century from the pathway of true science marked out by Hippocrates. Most diseases have since been brought into the light of the inductive sciences, but those of the apoplectic kind still remain afar off in that dark hypothetical ground where the speculative philosophy of the last century placed them—the pathology of Apoplexy resting upon a presumed obstruction of a hypothetical sensorial power manufactured in the brain, and its treatment founded upon that hypothetical pathology as a law given to direct the physician what to do. It left him but two things to do—to bleed and purge or to stimulate. One to remove a supposed pressure on the brain, causing the imaginary nervous fluid or power to be obstructed in its passage through the nerves, and the other to stimulate the nervous system to convey it or to force a passage for it. It made but little odds in the rules of practice what the disease was, causing the coma, insensibility, or convulsions, the theory would keep the attention of the practitioner in spite of himself fixed upon the single point of adopting measures to remove an imaginary obstruction of an imaginary nervous fluid or sensorial influence, supposed to be manufactured in the brain and transmitted through the nerves. It may have been dentition, an overloaded stomach, the recession of a cutaneous eruption, or many other morbid states as easily remedied as an intermittent fever, or it may have been the paroxysm of an intermittent fever itself causing the convulsion or coma; yet whatever it may have been, it was all referred to a unity of cause requiring unity of treatment in diseases and pathological conditions of the system as different as possible. No wonder that the apoplectic forms of disease have proved to be more formidable and fatal under such a routine practice, directed by but one idea and that erroneous, than they were in ancient times. Even at the present day the closer the text books are followed the more fatal are such complaints; the best read doctors, fresh from the best schools, having the worst success. The announcement I made to Professor Jackson, that they may be almost as readily cured as an intermittent fever, will cease to startle the profession as soon as the inductive sciences have been brought to bear upon them, and every vestige of that hypothetical pathology discarded which has so long directed their treatment.

Since I received your letter I was called in consultation to a negro man with coma and convulsions, whom I found under the secundem artem treatment to relieve the brain of a supposed congestion, interrupting the transmission of a supposed sensorial power to the rest

of the system, on a pathology to that effect, though expressed in different words. Bleeding, purging, cold applications, blistering, etc., being the means tried, without success. I advised hot applications to the head, stomach and bowels, by cloths wrung out of hot water, and a mixture of quinine, camphor, capsicum, calomel and laudanum, to be forced in his mouth from time to time, until he swallowed a good dose. The attending physician thought such a treatment would kill the negro, and called in another counsellor. They followed the text books most faithfully, the patient growing worse and worse all the time, until the yoor negro was thought to be utterly hopeless. At length to satisfy all parties, and reconciling the conscience by the reflection that nothing could hurt a dying man, totally insensible to pain, it was concluded to apply the hot cloths and to give the hot medicine. At first it appeared to strangle him, but on being turned on his side, the throat and fauces became disembarrassed of much tough phlegm, which the hot medicine had loosened sufficiently to be ejected, together with it, from the mouth. More was put into his mouth from time to time, until he swallowed a good dose,—the power of deglutition, before lost, returning as the tough phlegm, obstructing the respiration, was thrown off by the secretory action so powerful an apophlegmatic excited; soon after which, to the amazement of the bystanders, he came to his senses and recovered. The account of what transpired I received from the master himself, who was astonished and delighted at the wonderful cure. It was not so wonderful, as the case was nothing more than an intermittent fever under the disguise of coma and couvulsions. The sexton's book lost the report of another case of apoplexy, or congestion, or inflammation of the brain, which would, no doubt, have been added to it. King James died with an intermittent fever. The physicians of the present day would not have let him die with a disease under that name; they would have fancied it something else. Intermittent fever still continues to kill a great many people in this enlightened age and to put out their eyes, but it does it under the disguise of some undefined congestion, inflammation or neuralgic amalgam. A gentleman, among the most distinguished men of the United States, was suddenly attacked in the night with a violent inflammation, as he supposed, of the eyes, attended with the most intolerable and excruciating pain. There was great intolerance to light, so much so that it seemed almost impossible to exclude the painful impression it made by trying to shut it out by darkening the room and covering the eyes. In addition to the pain in the head and eyes, there was a buzzing sensation in his ears, which he

compared to that produced by quinine. Indeed he thought his constitution had been greatly impaired by his having, in former times, taken "too much of that pernicious drug," and its effects still remaining in the system, was supposed to be the cause of the buzzing he then felt in his ears. The veins of his face and head were turgid, the arteries beat violently—the integuments of the head and face were burning hot to the touch. The pulse at the wrist weak and irregular, the extremities cold, and there was a weak, faint, sinking sensation at the epigastrium. He informed me, that the year before he had been attacked with a similar inflammation, which, after confining him in a room, hung with black, for more than three months in great torture, finally disappeared; leaving the sight of one eye permanently impaired without any perceptible disorganization or blemish in the organ itself. Although I loved this patient well, not only as a man, but for his noble achievements in his country's service, yet after contemplating his case a few moments in deep grief, I was suddenly filled with extreme pleasure when whispered to by inductive Science, that on that very day she would display her power and make him whole. According to her bidding I went to work. His head and the lids of his eyes were washed in chloroform, and diaper cloths, seven folds thick, wrung out of water hotter than the hands could bear (without cooling them with ice preparatory to the wringing,) were applied all around the whole head, including the eyes, and renewed every ten or fifteen minutes. At the same time two grains of opium and ten grains of the sulphate of quinine, with a tea spoonful of the wine of the seed of colchicum, were given at a dose and repeated every three hours. The relief afforded was almost instantaneous. It was morning, and before the sun set he was nearly entirely relieved of the pain, the buzzing in the ears had gone off, and with it the insupportable sinking sensation at the epigastrium. After having taken twelve grains of solid opium, sixty of quinine, and half an ounce of colchicum wine, I gave him a dose of castor oil to move the bowels and to procure sleep. After it operated he slept, and the quinine, colchicum and opium were occasionally given through the next day to prevent a relapse; after that, the opium was omitted and one or two doses a day of the quinine and colchicum until he regained his strength and appetite. The eyes were let alone—nothing was applied to them, leaving them to take care of themselves after having dislodged the misplaced intermittent fever which had fallen so suddenly upon them. They did take care of themselves, and in a few days not a vestige of the inflammation or turgescency could be seen, and the sight was improved

instead of having been weakened by the attack, and the patient got in better health than he had been in for a long time previously. His was about as much a case of inflammation of the eyes as the negro's was a congestion of the brain. The disease of both was essentially the same, but choosing different organs to manifest its effects; the organs themselves—the brain in the one and the eyes in the other—being neither at fault, but perfectly passive, having nothing to do with causing or curing it. Lest you might think it was the previous depletion, almost to death, which prepared the negro for the cure, I have given the eye case, where a similar quinine, opium and hot water treatment, without any depletion or preparation whatever, had the same marked and almost instantaneous effect. But was not such a treatment a hazardable experiment? It was no experiment at all.* I had often seen the same kind of treatment, in similar morbid states of the system, have the same happy effect in saving life, as well as eyes, from destruction. It would have been hazarding a fearful experiment to suppose that nature would be untrue to herself and to have trusted either patient to any treatment she had in such cases repeatedly pronounced against. Resin Bowie, brother of the inventor of the knife that bears his name, with a similar affection of his eyes, coming on suddenly in the night at his hotel in Philadelphia, was made so furious with the pain that he broke the bedstead to pieces. Dr. Chapman, I think, was sent for. Bleeding, leeching, cupping, purgatives, cold applications, blistering, a severe regimen, lotions and nitrate of silver to the eyes, all availed nothing—the sight of one eye was totally lost, and not long afterwards a second attack nearly entirely destroyed the sight of the other. His athletic frame and great muscular strength gave him the appearance of blind Sampson. The history of his, and other like cases, added to my previous experience, convinced me, that in such cases of misplaced intermittents, whether called inflammation, neuralgia, or any other name, antiphlogistics are not much more favorable to the eyes than Sampson of old found the Philistines to be. I found them to be Philistines in the case of Mr. Warren's eyes, as far back as November, 1826. On looking now at the notes of his case, I read that after copious and repeated bleedings from the arm, free purgation with mercurial and saline cathartics, cold applications, a liberal use of antimonials and local bloodletting, and finally boldly opening the temporal artery, that instead of having given the smallest relief to the pain in the eye chiefly affected,

* Hip. Dr. Flatibus. Sect. 7 & 12: "We relieve such diseases by applying hot applications externally."—At § 12. Apoplexies are specially named and referred to the same cause — § 7.

another pain, equally intolerable, sprung up at the end of the second day, in one of his ears. It was proceeded, as in the case above mentioned, by a sensation as if wind was breaking into his ears. He was promptly cured and his eyes saved by four grains of quinine (a large dose in those days,) every hour and three grains of opium every three hours. In September, the next year, I was called to a negro boy of Mr. R. L. Smith's, in an apoplectic fit. He was comatose, but not perfectly insensible. Loud calls, close to his ear, would rouse him for a second or two, when he would fall back into coma. Mustard and external stimulants to the skin were not felt at all. He had fallen into this condition while a dose of calomel and jalap was operating. That it was not faintness, was proved by the turgescency of the blood-vessels of the face and head and the burning heat of that part of his body, while the extremities were cold. His pulse was 160 in a minute,—precluding all idea of bleeding. I ascertained that four days previously, after bathing in Lake Concordia, he had a slight chill, succeeded by fever, not remarkable for any other thing than a great determination of blood to the head, for which evacuating medicines were used. It returned more violently the next day and the day after, and on the fourth day the apoplectic fit took the place of the paroxysm. Diagnosis—misplaced intermittent affecting ths brain. Treatment—the same as for the misplaced intermittent affecting Mr. Warren's eye the preceding year. Four grains of quinine every hour was, however, first tried. A few doses reduced the pulse to 130, but finding the extremities growing colder, instead of warmer, two and a half grains of opium and a dose of calomel were conjoined with it. The next day, under this treatment, the patient regained his senses. The quinine was continued at longer intervals, the pulse coming down all the time, approaching more nearly the healthy standard, and on the third day after the fit the patient was well.

During one day I prescribed for six cases of coma or profound insensibility of body and mind, among the grown negroes at Mr. P. M. Lapice's White Hall plantation, several cases of fits among the children, like epileptic fits ; three cases of congestive fever, and a number of cases of remittent and intermittent fever. A large sloo or shallow pond of water, near the houses, had dried up about the time of the occurrence of these cases. No better proof could be afforded of the vanity of nosological definitions and their inutility in a practical point of view. Here were upwards of 150 negroes, all living in the same, kind of houses, engaged in the same occupations, eating at the same table, drinking the same water, wearing the same kind of clothing, and breath-

ing the same atmosphere infected by the drying up of the pond, attacked nearly about the same time with some of the most common, as well as the most formidable diseases to be found in the Nosology. Theoretical classifiers had drawn very marked distinctions between them, placing some in the brain and nervous system, others in the sanguineous or hepatic system; some as fevers, others as congestions or inflammations of this organ or that, requiring special treatment to be directed against the organ which their fancy blamed with being the cause of the mischief. But Nature declared she knew no such distinctions, not only from the cases on that plantation, but elsewhere in the same neighborhood; where filthy pools of water had begun to dry up. Some fell down in the field apoplectic without any warning. Others took a chill and in a day or two the fit took the place of the chill; but in most cases the comatose affection occurred during the exacerbation of the fever; and the epilepsy in the children was started by any irritating cause, as that of teething or the operation of drastic medicine. Produced by unity of cause, they required unity of treatment. What was found to cure one would cure all, notwithstanding the difference in the symptoms. They were all cured by the same treatment, a little varied in kind, to suit the circumstances of each particular case. Thus when the air tubes were choked with mucus, rattling in the throat, preventing the free ingress of air, a little mustard and salt, or some apophlegmatic, as capsicum with assafœtida and ipecac, forced into the throat, caused its disgorgement, let in the air and restored the power of deglutition; after which the apoplectic cases were treated precisely as the reigning disease of the season, only more actively, with larger doses. The mortality was frightful throughout the country where the books were followed and the treatment, in the comatose form of the disease, was directed to the removal of some imaginary pressure on the brain stopping the transmission of some hypothetical fluid, power or influence, supposed to be manufactured in the brain and transmitted through the nerves. Whereas, when the specific remedy for the disease itself was freely given, no matter what the protean forms it had assumed, the cure was almost equally certain and speedy, in one form as in another. When the season changed and the cold weather set in, intermittents themselves often resisted the specific treatment, requiring bloodletting or evacuents to prepare the system for them; so did the apoplectic forms of disease then occurring. The stoppage of the insensible perspiration, by exposure to cold, gave the mucous surfaces more to do, and showed its effects by influenzas, pneumonias and comatose affections of the brain. What would cure one was generally found to be a good remedy

for all the diseases prevailing at the same time. The apoplectic forms nearly always required an apophlegmatic or pungent substances, combined with nauseating or emetic medicines, to disembarrass the throat and air tubes of that excessive secretion of phlegm, you have noticed in such cases, and to let in the air freely to the blood in the lungs. This being done, whatever would cure the prevailing disease, whether quinine, the lancet, purgatives, antimonials or sudorifics, would assuredly cure the apoplexy. Hence, when I penned the lines to Prof. Jackson, "that for 25 years I had been in the habit of curing apoplexy almost as readily as intermittent fever," you were right in supposing that I would not have made such a startling assertion without having some good grounds for it. From what I have said, you will perceive that I am no believer at all in the existence of such a thing as idiopathic apoplexy or a disease of the brain, interrupting a supposed nervous influence, fluid or power transmitted through the nervous system, and I carry the war into Africa by contending that the practice, founded upon that hypothesis, is the cause of the greater fatality of apoplectic and comatose affections—the last half century than they were previously—when treated chiefly by pungent emetic medicines, and like the reigning malady of the time being. It is admitted that a disease of the brain may be the primary cause of muscular rigidity or insensibility, so may a disease of the toe or the finger, a splinter under the nail for instance, or a tooth coming through. It is admitted that there may be effusions of blood or serum in the brain, but these are mere effects and not causes, as they are often found in other parts of the body in those who have died apoplectic, with no morbid appearances whatever in the brain. In 1792, Young America, in the person of Dr. Phillip Syng Physick, bearded the great British reformer, Prof. Cullen, in the city of Edinburgh, on this very point, by proving, "that in cases of apoplexy with effusion, the presence of the fluid cannot be considered as the cause of the apoplectic symptoms." (See his Inaugural Thesis.) But the American youth, announcing the truth, was not listened to, with all Europe against him, as it overturned the very foundation of that theory which decoyed the medical profession from that solid ground, observation and experience, that Hippocrates had enjoined on them never to leave under any pretence whatever. They left it, however, and followed a theory which, so far from showing them how to cure apoplectic affections, has led, not into a comedy, but into such a tragedy of errors from that time until this, that yourself and many members of the profession are startled at the announcement, that such affections are curable like intermittents and other diseases. But, unfortunately, it is to

be feared that much the greatest number are too deep in error, too much like Ephraim, joined to their idols, to be startled by anything—not even by the facts announced by the celebrated Marshall Hall, who proved conclusively, that so far from plethora and pressure on the brain, interrupting the manufacture or transmission of a nervous fluid or sensorial influence, being the essential cause of apoplexy, that the disease very often occurs from anæmia and inanition; dyspepsia, gout and rheumatism, or anything that vitiates or impoverishes the blood; excessive bleeding from the arm will produce it; so will Bright's disease of the kidneys; also fright, fear, joy, anger, etc. A great error lies in confounding mechanical apoplexy, produced by external violence, a depressed bone or a clot of blood acting as a foreign body on the brain, with the disease or rather those morbid phenomena called apoplexy, and applying the theory of the former to the latter. When the inapplicability of pressure as an essential cause of the phenomena classed as apoplexy is proved, they save their theory by a quibble on the word, restricting its meaning to those cases only where a depressed bone, clots of blood, or serous or sanguinous effusions are found on post mortem examinations, although the phenomena may have been the same where no such causes are found to exist. Dr. Physick and a great many others have found such causes to exist without the patient, during life, having been afflicted with any apoplectic symptoms whatever, as in the case of Mr. Webster, where a false membrane was found in the brain and other morbid appearances, sufficient to have given him apoplexy or to have made him an idiot, if the prevailing doctrine in regard to the brain and nerves be true. A great while ago, Dr. Elliott, now of Lake Providence, and myself, had a case of apoplexy supervening on a fall on the head, which did not deprive the patient in the first instance of the power of muscular motion, but not long afterwards he became apoplectic, and had strong convulsions. There was no fracture. I trepanned him, and removed a large clot of blood lying on the brain, and although the man had been insensible for some days, without any use of his limbs; the operation was no sooner over than he got up from the table and walked about, and was perfectly restored to health, except the wound. It seemed to be clear enough that the clot of blood produced the apoplexy, but it is not equally clear that it produced it by pressing on the brain. It may have acted simply as an irritant or foreign body on a tender organ, as a mote in the eye, affecting the general system, and not by virtue of any direct pressure. A tooth coming through a resisting gum often causes similar symptoms. A melon seed in the trachea produced even more alarming apoplectic symptoms in a little boy, whose case I reported in the

New England Journal of Medicine and Surgery, vol. 14, page 136, for the year 1826. It is there stated that "he fell to the ground apparently dead, being breathless and black in the face, as if suffocated." I cut into the larynx and took the seed away, and in a short time he was well. In this case the apoplectic symptoms were caused by a foreign body interrupting the respiratory process—producing a species of asphyxia. Asphyxia and apoplexy are essentially the same in their anatomical characters. Asphyxia was not understood until Kay explained it. The blood stagnates in the capillaries of the lungs, in consequence of its failing to undergo the change from venous to arterial, and the left heart ceases to act for want of blood to act upon. Cullen considered asphyxia from mephitic air, narcotic poisons, cold, concussion and certain passions of the mind, as a veritable apoplexy, and supposed it was occasioned by such agents "directly destroying the mobility of the nervous power." (Practice Physic, MCXV.) His hypothesis in regard to asphyxia has been disproved by Kay; yet the same hypothesis is virtually at the bottom of the theory and practice in apoplexy at the present day. Asphyxia is an apoplexy where the heart ceases to act for the want of blood; but anything, as Cullen has truly said, is calculated to produce it, "that produces a more difficult transmission of blood through the vessels of the lungs," as that "interrupts the free return of the venous blood from the vessels of the head." It differs from asphyxia only in degree. It differs from palsy by its affecting the whole of the powers of sense and motion, while palsy affects only a part. Epilepsy is nothing but apoplexy in an intermitting form. All these affections are essentially the same, differing only in degree. Whatever stops or retards the transmission of blood through the lungs produces this affection. When the causes, interrupting the passage of blood through the lungs are temporary, the disease assumes the form of epilepsy. The blood not being able to pass through the lungs, in due time, produces a fullness in the vessels returning the venous blood from the head. Hence, "the turgid veins and other marks of cerebral congestion" are secondary, not primary symptoms, as you truly say, "they are preceded by death-like palor and a shrunken countenance." As a natural consequence of the turgidity of the venous system, from the retardation or obstruction in the transmission of the blood through the lungs, the mucous surfaces pour out a more abundant secretion. This is properly only an effort of nature to relieve the turgid veins of a portion of the fluid which distends them. But in her attempt to relieve herself in this way, as far as the bronchial tubes are concerned, nature commits suicide by filling them with tough, viscid phlegm, making them

more impervious to the air, and preventing the transmutation of the venous into arterial blood. Hence, the necessity of removing, by emetics, the super-abundant mucosities, poured out into the air passages, in order that the air may be able to permeate them and vivify the venous blood and to facilitate its transmission through the lungs. The motive power of the blood being itself derived from the air, whatever removes the obstructions to its entrance into the air cells must, from necessity, facilitate the cure. In my essay on apoplexy, published some few years ago, I endeavored to point out the uses of the membrane lining the nasal fossæ, sphenoidal and ethmoidal cells, the frontal sinuses and the antrum maxillare. I proved that it is, properly speaking, an anatomical contrivance to relieve the brain of its surplus fluids. In children, the office performed by this membrane is more important than in the adult, owing to the larger comparative size of the head, requiring this safety valve for the excess of fluids determined to the brain. In apoplexy, irritating substances applied directly to this membrane, by exciting a copious excretion of mucous, is the quickest and most effectual method of relieving the plethora of the cerebral vessels, amounting in fact to the most direct local depletion that can possibly be adopted. The experience of two thousand years has proved the virtues of volatiles to the nose and errhines directly applied to the membrane. They not only produce a copious secretion of mucosity, but equalize the circulation and invigorate the languishing vital energies. About the close of the last century they were condemned on theoretical grounds. Although experience proved them to be very valuable in cephalalgia, and in affections of the eyes, ears and teeth, attended with a plethora of the veins returning the blood from the head—and unattended with any increased impulsion or corresponding fullness in the arteries. Increased determination of arterial blood to the head may cause phrenitis or inflammation, but not apoplexy. Anything diminishing the impulse of the blood to the head, as Abercrombie truly says, may cause the latter affection, which is the very opposite of inflammation, as it consists in a sluggish or impeded motion of the venous blood. The bloated and puffed appearance of the face in the apoplectic forms of disease, so well marked in puerperal convulsions, is owing to an exudation into the cellular tissue, caused by the slow motion of the blood in the engorged veins. This cellular infiltration is an effect not a cause of apoplexy, so are the extravasations, whether sanguinous or serous, often found in the brain, mere effects not causes. All the antecedents and concomitants of apoplexy point it out as a pathological condition of the system, produced by a diminution of the vital forces, from a retardation or impedi-

ment in the transmission of the blood through the lungs; in a word a partial asphyxia. The fullness of pulse, sometimes observed, is a deception arising from the slow breathing. In the milder cases, where the blood passes freely and in full volume through the lungs, only slower, the left heart struggles the harder to unload itself. It is a good symptom instead of a bad one. Instead of bleeding to reduce the vigor of the heart, that remedy is only useful in diminishing the amount of venous blood, enabling the balance to pass more freely through the lungs. Hence, when judiciously used it increases the force of the arterial circulation instead of diminishing it. To bleed until the force of the pulse is reduced, is fatal in apoplexy. The operation is mostly useful in those cases where the lungs are so overloaded with black blood as to prevent its transmission into the pulmonary veins and on to the left ventricle. Hence, in that species of asphyxia from retrocession of the measles, attended with lividity of the face, opening a vein will enable the blood to pass through the lungs, and to give the patient a pulse who previously had little or no pulse. I have repeatedly followed Sydenham, and saved life in the lividity occurring in measles after the patient was pulseless or nearly so by bloodletting. An emetic of mustard and ipecac should go with the bleeding, and a mercurial cathartic follow it. Also, sprinkling the face with cold water, and pouring a little on the extremities or rubbing them with ice. They are always cold in such cases, and cold is much more effectual in warming them than hot applications. But you are deterred from the use of emetics, volatile and acrid substances applied to the mucous membrane lining the mouth, nose and fauces, and especially substances calculated to cause sternutation, by "*this congestion*," which, "like the ghost of Macbeth stares you in the face." Suffolk's description of the body of Henry VI (Shakspeare's Henry VI, second part, act 3d,) will effectually exorcise that ghost. It paints that "*congestion*" to the life, and points to the cause of it. The cause of it is asphyxia, or a want of a proper oxigenation of the blood. Irritant emetics, by removing the mucosities obstructing the bronchial tubes, are the most effectual means of removing the congestion by facilitating the metamorphosis of venous into arterial blood, and by exciting secretory action in that extensive mucous lining of the nasal fossæ, sphenoidal and ethmoidal cells, directly depletes the engorged vessels of the brain. But there is still another depletory measure, the application of many folds of cloths, wrung out of hot water, applied all around the head. The hot applications around the head produce a copious sweat and tend to unload the blood vessels of the head, and to remove the cerebral congestion. They also stimulate the venous sys-

tem to push forward the sluggish congested blood towards the right auricle. Granville's lotion, you perceive, would not answer as a substitute. But is not all this a mere practice founded on a vague theory? So far from it, it is an old practice, which, after having proved its efficacy from the time of Hippocrates, has recently been displaced, and has gone out of use in consequence of a vague theory, introduced into medicine by Cullen, towards the close of the last century, referring apoplexy to a want of mobility in a hypothetical nervous fluid, supposed to be produced by some imaginary pressure on the brain. Sydenham, Fothergill, Pitcairne, Selle, and the most distinguished physicians, anterior to Cullen, used emetics and apophlegmattcs in the treatment of apoplectic affections. Van Helmont declares that they are the infallible secret of curing such affections. It was the common practice, anterior to Cullen, for physicians to use horse-radish, mustard seed, spirits of ammonia and acrid volatiles to stimulate the schneiderian membrane to secretion, and to cause sneezing, as methods of cure in apoplexy. Cullen condemned such substances purely on hypothetical grounds. Berselius was in the habit of rubbing the tongue and mouth with the volatile oil of amber to relieve the congested vessels of the head, by causing a copious flow of mucosities from the membrane lining the mouth, tongue, fauces and posterior nares. Paul, of Egineta, removed the obstructions in the bronchial tubes, preventing the ingress of air into the lungs, by a feather dipped in oil, made more stimulating by the addition of castor, opoponax and such substances, passed down the throat. When such means fail to promote the transmission of the blood through the lungs, I have recommended and practiced artificial insufflation. In an article I published, sometime ago, in the Boston Medical and Surgical Journal, on Chloroform, I recommended artificial respiration as the most certain curative measure for apoplexy or asphyxia, caused by the inhalation of that agent. It excited no attention at the time, but I am happy to perceive that the practice is beginning to be adopted, and has already saved many lives. In the case of Dr. Ely's child, and in those cases where I have used it in restoring suspended animation, no instrument was employed, the air being blown into the lungs by the mouth of the operator. Hunter's double bellows would no doubt act quicker and better. But in such cases we have no time to loose in looking up instruments. The air can be thrown into the lungs from mouth to mouth, without loss of time. In bringing dead alligators to life, I used the common fire bellows; I exposed their lungs to view and watched what was going on. The phenomena, I saw, told me plainly that the flesh derived its life directly from the blood, and that

the blood derived its life and motion directly from the air. The brain and nerves are only a finer species of flesh, consisting, like the muscles, of two elements—the one vascular and the other fibrous. In order to arrive at the correct theory and practice in apoplexy, it is necessary to begin by reading and believing, in its full literal sense, the 14th verse of the 17th chapter of Leviticus.

Very respectfully, your obedient serv't,

SAMUEL A. CARTWRIGHT.

To Dr. Cornelius S. Baker,
Richboro', Bucks County, Pa.

VII.—ON THE MOTIVE POWER OF THE BLOOD.

BY ALBERT WELLES ELY, M. D.

In the November number of this Journal, for 1852, it will be recollected I published a detailed exposition and refutation of Dr. Samuel A. Cartwright's strange and unsupported theory, to which he has given the sesquipedalian and somewhat uneuphonous name of "*Hæmatokinety.*" In the September number, for 1853, Dr. Cartwright replies to my last article, after taking nearly a year to reflect upon it. He begins by excusing himself for remaining so long in a brown study on the snbject, and for concocting new means of resuscitating poor asphyxiated "Hæmatokinety," by saying that he did not reply to my article "because all those well versed in Natural History would perceive that it needed no answer, his data being incorrect; and it was perfectly useless to get into a controversy with those who had not studied the subject.* Dr. Ely had got hold of an incorrect translation of Cuvier's works, and turned the same against the new doctrine with tremendous effect in the eyes of all those unacquainted with Comparative Anatomy, and who had never seen the Napoleon of Naturalists in any other than an awkward English dress."

This is indeed a very singular and summary way of disposing of all the facts and arguments contained in my last article, in which I proved, by facts, and by quotations from standard authors on Natural History,

* The whole of this controversy about "hœmatokinety," must have convinced many, that it is Dr. Cartwright who has "not studied the subject;" for if he had, properly, he would not have committed so many blunders in natural history.

that all of Dr. Cartwright's positions were without foundation, and overthrew every one of them by citations from Cuvier and his commentators; from Dr. T. W. Harris, an eminent naturalist of Cambridge; from the latest French authorities, such as H. Milne Edwards, M. Deschayes, Lanarck, De Behn, Strauss and others, from whom I gave extracts in French, except in the single case of Cuvier, whose *Règne Animale* was only accessible to me in an "English dress," published in London, by a body of eminent naturalists, in 1840. The etxract which I made from that work completely overthrows Dr. Cartwright's position, "that it is known to naturalists, that of the twelve thousand species of fishes, not one has any muscular organ answering to the left ventricle of mammals"—a position from which Dr. Cartwright very erroneously concluded that "Hæmatokinety" must be true. The extract in question was the following: "The blood is brought to the gills by the heart, which thus answers to the right ventricle of warm blooded animals; and from the gills it is sent to an arterial trunk lying immediately upon the under side of the back-bone, which trunk is the left or systemic ventricle of the heart, and sends the blood throughout the body of the fish." Cuvier's *Animal Kingdom*, p. 290, London edition, 1840.

Dr. Cartwright meets this quotation, so fatal to his theory, with the simple *assertion*, that it is "an incorrect translation of Cuvier's works." Such assertion would do very well, provided that he had accompanied it with some *proof* of its being an incorrect translation. But the proof is wanting, and the quotation will therefore have to stand, until Dr. Cartwright can rebut it with something better than bare assertion.

Dr. Cartwright quotes the following passage from Cuvier's *Histoire Naturelle des Poissons*, as though it contradicted the passage above, which I quoted from Cuvier's *Règne Animale*. Speaking of fishes, Cuvier says:

"Leur caractere propre consist en ce que leur circulation branchiale a seule à sa base un appareil musculaire, au ventricule droit des animaux dont nous venons de parler, et qu'il n'y a rien de semblable à base de système de la circulation de corps; c'est à dire que les analogues de l'oreillette et du ventricule droit leur manquent entièrement, et que les veines branchiales s'y changent aux artères sans être envelloppées des muscles."

In this passage Cuvier means simply to say, that the hearts of fishes are single, having only one auricle and one ventricle, and those on the *right* side only. Nobody denies this; nor does it conflict with the passage which I quoted above, from the *Règne Animale*, which also ad-

mits that the hearts of fishes are single ; but adds, that the "arterial trunk lying immediately upon the under side of the back-bone, performs the office of the left ventricle, which is wanting.

After a long narrative of his alligator experiments, Dr. Cartwright observes, "The burden of Dr. Ely's objections to the new doctrine was chiefly founded upon the doubts of Dr. Dowler and others, in regard to the experiments of 1852—doubts which have been entirely removed by the experiments of 1853. This passage is incorrect in every particular. I do not know that Dr. Dowler ever had any doubts on the subjeet. On the contrary, I believe that he has always regarded "Hæmatokinety" as the greatest and most miserable of all modern humbugs. I have had many conversations with that distinguished physiologist on the subject, and he has always, so far from expressing doubts, distinctly declared that all these experiments prove nothing in reference to Dr. Cartwright's "Hæmatokinety." Nor has he pursued his vivisections with any reference to this ridiculous and unsupported theory of "Hæmatokinety." He has made a better use of his time. He has instituted vivisections in reference to the functions of the nervous system, and has arrived at many very important results.

As to my having founded my objections to "Hæmatokinety" on "the doubts of Dr. Dowler and others, in regard to the experiments of 1852," I distinctly declare that I have founded nothing upon doubts regarding any subject. Doubts are not a proper basis for any thing. I neither deal in doubts nor assertions. Dr. Cartwright appears to have a very poor memory ; for in my last article, in this Journal for September, 1852, in speaking of the question regarding the tying a ligature about an alligator's trachea, my words were these : "We do not, however, attach any importance to the question, as regards its bearing upon Mrs. Willard's theory. Admitting that the ligation of the trachea will speedily kill the alligator, that fact would merely prove that the animal, like all others, cannot live without air ; and not, as Dr. Cartwright supposes, that air, inhaled into the lungs, causes the blood to circulate."

The sum and substance of the whole of Dr. Cartwright's argument derived from experiments on alligators is this : The blood ceased to move as soon as atmospheric air was excluded from the lungs, and while its supposed chief motive power was hard at work;" *ergo*, "the contraction and expansion of the heart cannot be the chief motive power which circulates the blood." We think it will be difficult for any one to discover the connection between his premise and his conclusion. Dr. Cartwright's logic is as miserable as his theory. We have before refuted this argument, and we will only add, that in the

above euthymeme, the premise is false. The blood does not cease to move as soon as atmospheric air is exhaled; a short space of time must elapse; and the blood finally ceases to move because, not being oxygenated, it ceases to stimulute the heart into action. So that the heart, and consequently the circulation, does not cease its action for want of air, but for want of a proper stimulus. The air only *fits* the blood for circulation; but Dr. Cartwright, by a sort of Irish blunder, has "got the cart before the horse," and makes the air circulate the blood.

Dr. Cartwright continues: "Instead, therefore, of entering into a controversy with him, I refer him to those whose doubts deceived him." I beg to inform Dr. C. that I was deceived by the doubts of no one; and that this appears to me like a very ingenious way, on the part of Dr. C., to try to crawl out of a controversy.

We will close this paper, by adding what we have recently written in the columns of the National Intelligencer of the 22d of November:

Dr. Cartwright, who has undertaken the difficult task of proving that the circulation of the blood in animals is not caused chiefly by the contractions of the heart, but by a certain power inherent in the atmospheric air we inhale, (which power he calls "Hæmatokinety," contends that this strange theory of the circulation is fully established by the *fact* that there are "twelve thousand species of fishes which have no heart or other muscular organ at the base of the systemic circulation to propel the blood." This *fact* of Dr. Cartwright, regarding the twelve thousand species of fishes, we do not at all admit; nor do we know that any distinguished naturalist admits it as a fact, although Dr. Cartwright affirms triumphantly that "it is proved by the twenty-four volumes of the Natural History of Fishes, written by Cuvier himself; and not only by him, but by every other distinquished writer on the subject." It would be exceedingly interesting to naturalists if Dr. Cartwright would cite the names ef those "distinguished writers" on the subject who assert that there are twelve thousand species of fishes that have no apparatus for circulating the arterial blood. For ourselves, we humbly confess that we do not know who they are; but as to Cuvier, whom he includes among the number, we can assert possitively that he does not maintain the *fact* as Dr. Cartwright calls it, that there are twelve thousand species of fishes that have no muscular apparatus for propelling the arterial blood throughout the body.

Dr. Cartwright must have read Cuvier's works very carelessly, or he would not have fallen into the error of supposing that the writings of that great naturalist afforded any support to the strange idea that the heart

of animals does not circulate the arterial blood. If he will take the trouble to read the following passage in Cuvier's *Leçons d'Anatomie Comparée*, he will readily discover that Cuvier lends no support to "haematokinity ;" on the contrary, that he maintains that the single heart alone of fishes is sufficient to propel the arterial blood throughout the body :

" Lorsqu'il n'y a qu'un seul coeur," says Cuvier, "il faut que celui des deux systèmes artériels qui en manque épreuve encore l'influence du coeur unique, et que le sang y conserve son mouvement, après s'être filtré au travers de toutes les subdivisions do systeme pourvu de ce coeur ; ou bien qu'il faut qu ce système artériel sans coeur agisse assez par lui-même sur le sang pour le pousser, par le contraction successive de toutes ses parties, dans toutes ses subdivisions, et de celles-ci dans les veines, ou bien enfin, que les deux actions s'entr'aident, et c'est cette derniere idée qui nous semble le vraie. Le sturgeon, par exemple, nous donne une preuve évidente de la continuation de l'action du coeur pulmonaire sur le système aortique. A peine les veines du poumon s'y sont elles réunies pour former l'aorte, que celle-ci s'enfonce dans un canal cartilagineux qui lui est fourni par le corps des vertèbres. Elle semble s'y dépouiller entièrement de ses tuniques, et le sang y coule dans un tuyau à parois absolument immobiles ; c'est des trous de ce tuyau, ou canal cartilagineux, que sortent les branches arterielles qui se rendent aux parties. Le sang ne peut évidemment entre dans ces branches, qù en vertu de l'impulsion qu'il a recue primitivement du coeur et des artères pulmonaires."—Cuvier, *Leçons d'Anatomie Comparée*, tome 6, pp. 354–5.

From this extract Cuvier's views of the circulation of the arterial blood in fishes are very evident. He maintains that although the hearts of fishes are what are called single—that is, having but one auricle and one ventricle—still this single heart does actually propel the arterial blood through the body. What, then, becomes of Dr. Cartwright's assertion that Cuvier and other distinguished naturalists admit that there are twelve thousand species of fishes that have no muscular apparatus, no heart, for propelling the arterial blood through the body ?

But lest Dr. Cartwright may still remain sceptical on the subject of Cuvier's opinions, we will give him another short extract from that great naturalist :

"Dans beaucoup d'autres poisons les parois de la grosse artère sont adherentes, en partie, dans le demicanal osseux qui contient cette artère. Il faut donc que *l'impulsion imprimée aux sang artériel par le coeur se conserve à travers les branches, jusqu'au moins dans les troncs principaux des artères du corps;* voilà pourquoi la base de l'artère pulmonaire es distincte du reste par sa dilitation et par ses fortes parois, en partie musculaire. C'est, pour ainsi dire, un second ventricule, placé audevant du premier, et dont l'action augmente plus ou moins l'impulsion imprimée au sang par celui-ci. On le remarque même dans les *Batraciens.*"—Cuvier, *Leçons d'Anatomie Comparée*, *p.* 355.

Dr. Cartwright calls Cuvier the greatest of modern naturalists, and seems to rely implicitly on all that he says. He will therefore be

obliged to admit that Cuvier completely falsifies the assertion, that fishes have no muscular apparatus for circulating the arterial blood. Dr. Cartwright ought to read Cuvier more carefully; but he is so much absorbed in his favorite theory of "*haematokinity*" that the truths of that great naturalist entirely escape him.

The great error that Dr. Cartwright has fallen into is that there must necessarily be always a left auricle and a left ventricle, similar and similarly situated to that of mammals, in order to the circulation of the arterial blood; but the above extracts from Cuvier clearly show that the left auricle and ventricle may be dispensed with, in fishes. The hearts of fishes, although called single, have in fact two ventricles, and one auricle, the second ventricle being placed after the first to give additional force to the blood. It is a distinctly muscular organ, and nearly as large as the first ventricle.

To Dr. Cartwright's assertion that fishes have no muscular organ for propelling the arterial blood, I replied in a paper in the New Orleans Medical Journal by quoting the following from the London translation of Cuvier's *Règne Animale*, not then having at hand the Paris edition. Cuvier says:

"The blood is brought to the gills by the heart, which thus answers to the right ventricle of warm-blooded animals; and from the gills it is sent to an arterial trunk lying immediately upon the under side of the backbone, *which trunk is the left or systemic ventricle of the heart, and sends the blood throughout the body of the fish.*"—Cuvier's *Animal Kingdom*, p. 290, London edition, 1840.

After pondering over this complete refutation of his assertion for nearly a year, Dr. Cartwright at last comes out in the New Orleans Medical and Surgical Journal for September, 1853, and asserts that the above extract is not a correct translation of Cuvier. He says:

"Dr. Ely had got hold of an incorrect translation of Cuvier's works, and turned the same against the new doctrine with tremendous effect in the eyes of all those unacquainted with Comparative Anatomy, and who had never seen the Napoleon of naturalists in any other than an awkward English dress."

We are greatly surprised at this broad assertion of Dr. Cartwright, which he accompanies with no proof whatever; and we are half inclined to think that it is Dr. Cartwright himself who has "never seen the Napoleon of naturalists in any other than an awkward English dress." It is quite certain that he has never read him very carefully, for if he had, he would have seen the following:

"Ce sang, après avoir respiré, se rend dans un tronc artèrial située sous l'epine du dos, et qui *faisant fonction du ventricule gauche*, l'envoie par toute le corps, d'où il revient au coeur par les veines."—Cuvier, *Règne Animale*, p. 4, Paris, 1840.

What will Dr. Cartwright say to this ? And what becomes of his pompous assertion that the London translation af Cuvier, made by a body of English *savans*, is only "the Napoleon of naturalist in an awkward English dress."

After so many blunders made by Dr. Cartwright in relation to Cuvier's works, we are almost disposed to believe that his knowledge of French is as imperfect as that of his Hebrew, which he has been in the habit of quoting in the Boston Medical Journal ; and which, we are credibly informed, was furnished him by a distinguished Hebrew scholar in New Orleans, the Doctor being not even familiar with the Hebrew alphabet. Of this, however, we may be mistaken. We would not judge him too harshly. But it is a little singular that being, as he pretends, so familiar with Cuvier's works, he should have entirely overlooked the passages which we have quoted, and the more so because one of the passages is in the very chapter which he quotes, and almost in juxtaposition with his own extract.

Dr. Cartwright, in his reply to my last paper in the New Orleans Medical and Surgical Journal says :

" The expression in the English translation of Cuvier, *'which trunk is the left, or systemic ventricle of the heart, and sends the blood throughout the body of the fish,'* was evidently intended by the translator to be understood in a metaphorical sense."

This is the first time we ever heard of fishes being described in metaphors. Who would ever think of looking for metaphors in a grave and elaborate anatomical description of fishes ? What would naturalists think of a man who should undertake a work on itchthyology, giving all his descriptions in metaphors ? Metaphors would be as foreign to the subjoct as quotations from Shakspeare or Hudibras. Dr. Cartwright must be driven to an extremity indeed to be obliged to attempt to invalidate my quotation from Cnvier on the circulation of fishes by asserting that it was metaphorical. Will not Dr. Cartwright favor the scientific world with a commentary on the anatomical metaphors of Cuvier ? Such a work would doubtless contribute to the improvement of the style of scientific writers, and to the severe exactness of scientific descriptions.

Dr. Cartwright is equally unfortunate in entomology. He has asserted that insects have no heart, or any thing corresponding to it ; that " the dorsal vessel is a heart only in a metaphorical sense," and that their circulation depends entirely upon " haematokinity." We have cited the most eminent authors, and completely refuted all of this misconception of his, regarding insects, but still he reiterates his assertions.

We must beg to be excused from examining his statements further on entomology, until he produces something like facts and arguments in support of his positions. Science is built up on observations and experiments; and we apprehend that Dr. Cartwright will have to resort to these before "haematokinity" will be able to make further progress, as all observations and experiments thus far made go directly against it.

Part Second.

EXCERPTA.

I.—*Opium in Irritable and Anæmic States af the Brain in Fever.*

BY HUMPHRY SANDWITH, M.D.

"The employment of opiates in cerebral affections," says Dr. Holland, "is a question of much interest and various difficuly;" and "there is a great scope for farther research on this subject, as on all that relates to disorders of the brain, and a strong presumption that opium is capable here of larger and more beneficial application than has yet been given to it." His subsequent remarks, in the same article, "On the Use of Opiates," embrace, but are not restricted to, its use in fever, may be consulted with advantage. Meanwhile, the profession owe Dr. Latham a large debt of gratitude for his masterly sketch of those irritable and anæmic states of the brain in fever, which demand the cautious use of this powerful narcotic. His brief but comprehensive paper on the subject, published twenty years ago, is still a beacon to guide us in the path, which his observant genius first irradiated.

The class of cases of purely irritable states of the brain is to be discriminated, as Dr. Latham shows, less by any *series* of symptoms flowing from the brain, than from the *single* symptom of a state of protracted wakefulness. Nor is the wakefulness pathognomonic *per se*, but to warrant the use of opium, it must occur in combination with an irritable state of the nervous system, induced either by depressing moral agencies, or by the physically exhausting one of alcohol. The fever may be mild, and "exhibit a *sort of contrast* with the existing affection of the brain; or it may correspond in severity with the sensorial disturbance up to a certain point, and then the symptoms referable to the brain outrun the febrile phenomena. In the latter case, though the vascular over-action may have been kept in check by general or local bleeding, still the sensorial disturbance progresses. "As other symptoms are relieved, the delirium is even aggravated." We are thus presented with two forms of irritable brain in fever,—the one being marked by simple wakefulness with no other cerebral symptom, and the other by wakefulness coupled with symptoms of high sensorial excitement. Both varieties are "incident only to those, whose habits and mode of living have been calculated to do an abiding injury to the nervous system, and who have been long actually suffering from such injury."

The same acute observer, however, recognizes another variety of sensorial disturbance in fever, which is obviously associated with anæmia. We shall quote his words:

"Again, I have seen the sensorial affections incident to fever, which require opium for their cure, manifest themselves in another form. There has been high vascular action from the first; and *large depletion* has been required to subdue it and to guard particular organs, and especially the brain, from injury. Under such treatment, all has gone on successfully, and the patient has reached the point of convalescence, with a soft pulse, a cleaning tongue, no pain, and refreshing sleep for two or three days; when suddenly (the tongue, the pulse, and all other circumstances continuing the same) some strangeness of manner has arisen, and then the wildest delirium, and then the unrestrained passage of the evacuations. I have known the transition from such a state of *convalescence* to such a state of peril, take place in a few hours; and I have known the patient again brought back to a state of convalescence in twenty-four hours by a moderate dose of opium. This is a rare form of disease, but one in which, when it does occur, opium is eminently indicated."

Now here was an anæmic condition of the brain, but not to so frightful an extent as in the case to which I beg leave now to call the reader's attention; and I may add, that its very extent suggested, not "a moderate dose of opium," but the liberal use of the remedy.

Case—Mrs. T., a rather delicate woman, about thirty years of age, fell into fever during the second week of November, 1848, when near the close of the third month of pregnancy. Her abode was in the vicinity of open and offensive ditches; and it was soon evident that the attack would prove severe. The case ran on nearly three weeks, with symptoms of grave and increasing disturbance of the sensorium, and other indications of low typhoid fever. By careful management, indeed, had she not been pregnant, the disease might probably have evinced no symptoms of more than usual danger. On the 2d day of December, the irritation of the great nervous centres palpably interfered with gestation, and abortion was the speedy consequence. The process, unhappily, was accompanied with a very profuse hemorrhage, which ceased indeed with the expulsion of the fetus and placenta, but which in twelve hours had brought the patient into imminent peril. Along with continued sensorial disturbance, there were the signs of incipient collapse, as manifested by a rapid and fluttering pulse, unequally diffused animal heat, and laborious respiration. Moderate stimulation by wine had been latterly allowed; but it now became necessary both to give ammonia, and augment the dose of alcohol. The extreme restlessness, subsultus, and other alarming symptoms demanded, however, a cordial, on which more reliance could be placed than even on the stimulus of brandy. Life, in short, was now in extreme peril. Calling to mind the marvellous examples of the virtues of opium in uterine hemorrhage *without fever*, as recorded by Dr. Stuart in the *Medico-Chirurgical Transactions*, as well as its power to sustain life in the fearful struggles of angina pectoris, and reasoning from the analogy of its virtues *in fever*, when the circulation is at the same time depressed by antimony, as also in delirium tremens, when excessive and habitual intoxication may be presumed to have exhausted the vital energies of the brain, I determined on an attempt to steady the heart, restore the capillary circulation, and calm the irritation of the nervous centres by a full dose of opium. Mr. Millin, the surgeon with whom I was attending in consultation, fully concurred with me in these views. We accordingly gave a draught containing a hundred drops of laudanum. The effect justified our most sanguine hopes. Sleep ensued, the circulation rallied; every symptom of cerebral irritation subsided, fever broke up, and convalescence was speedily established.

The present short practical paper contemplates chiefly that condition of the brain in fever, in which an anæmic state of its vessels warrants us in availing ourselves of the stimulant properties of opium, with a view to maintain the equilibrium of the cerebral circulation. In such cases the use of the drug is salutary, chiefly as it favors an amount of congestion essential to healthy sleep. Not that its effects on vascular structure are its merits; for it is fair

to argue with Dr. Holland, that as "narcotic substances have effects, locally applied, on nervous sensibility," so also "there can be little doubt, that in sleep it is the same singular influence, extended more widely over this part of organization, and reaching through the cerebral parts of it, the higher faculties of our being." But in anæmic conditions of the brain in fever, the use of opium seems to be salutary, we repeat, chiefly as it favors an amount of congestion essential to healthy sleep. "It is certain, that the state of sleep and coma frequently graduate into each other, in such way as to show that the proximate physical conditions are nearly the same in both; for "one degree of pressure seems essential to perfect and uniform sleep, while a greater degree, without other alteration of State, assumes more or less the character of disease." While such changes in the circulation of the head are obviously concerned in influencing the functions of consciousness and volition, it is equally manifest that an anæmic state of the vessels is precisely the one calculated to disarm the narcotic of its dangerous properties. But for that anæmic state, the cerebral capillaries might be goaded on by a full dose of opium into fatal coma. The necessity of a healthy amount of congestion and consequent pressure, however essential to the production of sleep, may be inferred also from considerations regarding the nutrition of the organ. "The sleep of animals," Dr. Carpenter tells us, "consists not in a state of diminished energy of the *nutritive* functions, but in the cessation of the *sensorial* activity, dependent upon a suspension of the functional power of certain parts of the nervous system, during which there is reason to believe that the nutritive and reparative operations of those organs go on with even augmented rapidity." The value of sleep in this view, and the importance of a healthy circulation of the materials for nutrition, considering the wasting effects of sensorial hyperactivity in fever, are self-evident.

On witnessing such striking displays of the remedial virtues of opium, as in the case related above, one is fain to break out with Sydenham "in praise of the great God, the giver of all good things, who has granted to the human race, as a comfort in their afflictions, no medicine of the value of opium, either in regard to the number of diseases that it can control, or its efficacy in extirpating them!" After all, most of its value depends on the descrimination with which it is prescribed. When injudiciously administered, as in sthenic cerebral excitement or in the improper arrest of diarrhœa in certain states of fever, it has been observed to produce phrenitis, epilepsy, and coma; so also, when indiscriminately prescribed in delirium tremens, its employment has occasionally been followed by apoplexy. Great and marvellous, therefore, as are the virtues of opium in a variety of diseases, and admirable as are its soothing qualities in several of the forms of cerebral disorder in fever itself, yet let no man venture to prescribe it for the latter (whether in large or small doses) in the dark or at random. It is a sharp-edged tool, and of such fearful potency, that, if it fulfills not a curative intention, it will probably destroy life. There is no instance, in the whole range of practical medicine, more imperatively demanding a sure diagnosis. In short, our warrant to prescribe it hinges on our ability to ascertain precisely that condition of the brain which alone will admit of its safe employment.

It was a rare sagacity which led Dr. Graves to employ tartar-emetic in combination with opium in those cases both of idiopathic fever and delirium tremens, in which the narcotic would probably stimulate to over-congestion of the brain, but for the depressing action of the mineral on the heart and capillary circulation. The complex practice finds its parallel in the analogous operation of opium in states of anæmia in fever.

Medical science is more advantaged, perhaps, by defining the circumstances to which our known remedies are applicable, than by exploring the resources of nature for others. Not only is great discrimination necessary in deciding on the cases which demand the use of opium, but also in regulating the doses

adapted to each variety. Little can be added to Dr. Latham's admirable directions. Our rule of conduct indeed is suggested by the degree of sensorial excitement. "Simple wakefulness may be gently lulled to sleep by a few drops of laudanum, but wild delirium requires to be mastered and (as it were) forced into repose by a much larger dose." Opium, however, goes much farther; and, therefore, a much less dose is required in quelling asthenic sensorial disorder, combined with fever, than when it exists alone. Five minims in the milder cases, and twenty in the graver, may be considered minimum and maximum doses in fever. Much, after all, must be left to our vigilance in watching its effects, and to our discretion in judging when to desist, and when and in what doses to repeat the remedy. There are yet other "cases where the indications for the employment of opium are *doubtful.*" We shall describe the variety of sensorial disturbance in fever in Dr. Latham's own words:

"Wild delirium, and long wakefulness, and a circulation weak and fluttering, seem to call for a considerable dose of opium. Yet, withal, there is a *certain jerk* in the pulse, so that we cannot help suspecting that the blood vessels have something to do with the sensorial excitement. Under such circumstances, I have certainly seen twenty minims of laudanum produce tranquil sleep, from which the patient has awoke quite a new man; but I have also seen the same quantity produce a fatal coma, from which he has never been roused."

Dr. Latham's recommendation, therefore, is in so dubious a case, and to avoid "striking a heavy blow in the dark," to administer a small dose at intervals of one hour or two," so as to stop short of actual mischief at the first glimpse of its approach, or be led by a plain earnest of benefit to push the remedy to its full and consummate effect." Judicious as is this advice, we cannot but think, that on the mode of treatment of cases, in which, on Dr. Latham's own showing, "the indications for the employment of opium are doubtful," any practice which aims at once to keep down vascular action and soothe the nervous system, is a real improvement. And such, we need scarcely add, is the complex method of Dr. Graves.

It only remains that we should revert to the *anæmic* type of sensorial disturbance in fever, in that milder variety of it which, as described by Dr. Latham, we placed in contrast with a much graver form, "a moderate dose of opium," he tells us, suffices to change a state of peril to one of convalescence. But his remarks do not contemplate so frightful a form of anæmia in fever as that of which we have given an example, and which might result equally from a large intestinal, nasal, or bronchial hemorrhage, as from uterine He speaks of heroic doses of opium only "in extreme cases of delirium tremens," while twenty minims of the tincture, he asserts, are quite sufficient for the purpose of subduing "the very same symptoms, carried to the greatest extremity, *when combined with fever.*" But while "a moderate dose" suffices to relieve the less urgent anæmic forms of the disease, the case related above as having occurred to my own observation, seems to sanction a bolder practice in the perilous cases of an extremely ex-sanguine condition of the brain in fever, coupled with the exhaustion resulting from protracted sensorial excitement.

(*Rankings Abstract.*)

II.—*The Blood—its Chemistry, Physiology, and Pathology.*

BY THOMAS WILLIAMS, M.D.

The fluids and solids in all biochemical inquiries should rightly be studied in conjunction. They interblend, subtantially and dynamically, with such intimacy, that to indicate a partitional boundary were to divorce what nature

has united. They reciprocally cede and accept. They mutually act and react. Fluidity and solidity are the first conditions of all organized matter. The aeriform state has nowhere a single illustration. Every gas must become a fluid as the fundamental term of union with the elements of the living body. The fluid is the active condition; the solid, the passive. The latter is sedentary; the former, locomotive. The former is the scene of rapid molecular transformation; the latter is distinguished for its stability; between these two extremes there exist manifold intermediate conditions. In the category of the nutritive fluids, unmixed fluidity is not known; solids are introduced. Fluids are not self-productive; they require either the direct or the catalytic agency of solids. The fluids even of the vegetable organism bear floating corpuscles. In the vegetable economy, it has hitherto been falsely supposed that the cells of the fixed tissues traversed by the fluids effect that change which, in the instance of the animal fluids, is accomplished by the self-borne corpuscles. The formative capacity of a fluid, in the abstract, is limited to a low standard. Albumen cannot mount into the condition of fibrine in virtue of any self-originated and self-sustained inherent molecular activity; the agency of a third body must be interposed. This is the "doctrine of the schools."

The blood is a wondrous epitome of fluids and solids. In complexity of composition it has no parallel. He who would resolve it into its components, must in himself unite the qualifications of the chemist and the microscopist, the anatomist and the physiologist. The chemistry of organized beings has now assumed the exalted rank of a rich and varied science. The microscope, at first a costly toy, has grown into its inseparable handmaid. Micro-chemistry is opening upon the mind like a vast panorama, opulent in wonders and boundless in limits. The microscope conducts the eye to the confines only of visible form; the formless eludes its scrutiny; it is, emphatically, an instrument fitted to define the geometric properties of bodies. Into the world of fluids it cannot penetrate. Fluidity is a condition of matter which it cannot appreciate. Of solids, it takes cognizance only of the state of quiescence; it does not reveal the "forces" impelling them to activity; it is an apparatus of inquiry into the statistics, not the dynamics, of matter. In micro-chemical inquiries, its peculiar value consists in empowering the eye to read and register the changes of form and color wrought by test-fluids upon solids: the knotted tangle of a multiplex compound is thus gracefully unwound; it is in this elegant manner that mind arrives at a knowledge of the chemical constitution of bodies—even those so minute in mathematical dimensions as to occupy the remote frontier-line which divides the fluid from the solid state, the extreme verge of morphological substance.

From the arena of zoochemical research, in these unsuperstitious days, all substanceless divinities must he exterminated. The era of the *anima mundi* has become historic. The "essence of life," *vis vitæ*, *vital principle* (Prichard); *nisus formativus* (Blumenbach); *organic force* (Muller): *organic agent* (Prout); *vis medicatrix naturæ* (Hoffman and Cullen) now number amid the effete jargon of an extinct nomenclature. The "illuminating orb" of a new mental life has arisen above the edge of the eastern horizon. "Abstract terms" are as luring as "golden calves; they demand the homage of believers. The "*principle* of motion," the "*principle* of gravitation," the "*principle* of attraction," are the relics of an entombed terminology. The "*life*" of the blood is an ontological expression which belongs to a former phase of science. No *divinæ particula auræ* swims in this fluid. Modern philosophy displays a tendency to refer all phenomena to the *laws* of matter—*directions* of action first implanted by the Deity. A "law," in its ontological sense, is a *deus ex machina;* it sounds as though endowed with an existence distinct from the body —as something superadded to the organism—like magnetism to iron, heat and light to a luminous body, *sic.* The word "*principle*" is characteristic of a less advanced state of science; it should *now* be used only as final letters of

the alphabet are employed by the algebraist—to denote an unknown element, which, when thus indicated, is more conveniently analyzed. It is customary, even now, to speak, of the "principle"—the agent of heat, light, electricity, magnetism, gravitation, etc.—as signalizing the severally undetermined causes of familiar phenomena. When these phenomena, in the progress of science, fall under a more just interpretation, they come to be referred to the primary properties of matter; they then may be deduced by demonstrative reasoning, like geometric theorems from the postulates on which they are founded. But in the science of physiology the term "law" has been employed in a less justifiable acceptation; a "principle," a "force" has been personified, and invested with a spontaneity of action. As an expression descriptive of the "conditions," the "circumstances" under which the actions and reactions of the material elements of the organism originate and proceed, it may be tolerated as a convenient phrase; the physiologist, however, idolatrously animates it with a creative and directive power, in virtue of which it acts upon matter removing its particles from the pale of physical and chemical laws, transforming them into organized tissue, endowing these tissues with novel properties, prompting them to action, opposing resistance to the injurious influences, defining the cessation of these acts of the organic body as synonymous with the departure of the "vital principle." A "law" ought not to be defined as altogether resident in the mind of the observer. The mind only links certain acts into a certain order of occcurrence; this *order of occurrence* is coeval with matter; it moves, "displays its forces," exerts its energy," only in a given direction. This *mode* of occurrence is the *law;* it is the final impress of the will of Him who made matter; it is an ultimate fact beyond which philosophy cannot penetrate. The task of the organic chemist, then, lies not in a search after an *ignis fatus*, a *vital principle;* he is required only to define the terms of events, the conditions of phenomena, as they occur in the living organism. *Words* must be used to indicate events. The exigencies of language will oblige the use of such expressions as vital force, affinity, property, &c. It will however be now understood within what latitude such expressions are to be limited.

Two new ideas have recently taken birth in science. The mutual convertibility of the "forces" is an accepted doctrine, and the transmutability of matter [Dumas, Faraday] is no longer held as an alchemic extravaganza. The facts of allotropism, established by the genius of Schönbein, and the late prophetic speculations of M. Dumas on the chemical electrical, and mathematical *progression* traceable through the properties of isomeric or conformable bodies, awaken in the mind of the modern chemist a reverence for the disentombed manes of alchemy. The transmutable bodies group themselves in nature in triads, or ternary series, thus: chlorine, bromine, and iodine; sulphur, selenium, and tellurium; calcium, strontium, and barium; lithium, sodium, and pottassium. The members of these triads, severally, are capable of replacing one another in chemical compounds. "When three bodies, having qualities precisely similar, though not identical, are arranged in succession of their chemical powers, there will be also a successive arrangement of mathematical powers, indicated by the respective atomic numbers of the substances, and amenable to every mathematical law. "That this symmetry of chemical with mathematical function points to the possibility of 'transmutation, is unquestionable, yet not transmutation in the sense of the old alchemical philosophy." Chemists see no manifestations of a tendency of being able to convert lead into silver, or silver into gold. These metals are not chemically *conformable;* one cannot take place of another by substitution; they do not form an isomeric group.

The preceding illustrations are drawn from inorganic bodies. Chemists have long believed that certain organic compounds display, in their properties, a close resemblance to metals. Of this kind are the three organic radicals—

C2 H2 O
C4 H5 O
C6 H7 O

which may be regarded as the three several oxides of an isomeric triad, bearing analogy to those already adverted to in the inorganic world. With reference to these radicals [emitting the oxygen] it is found, as in the case of the inorganic triads, that the sum of the atomic weights of the extreme bodies, divided by two, is equal to that of the intermediate body.

The discovery of allotropism has bereft these speculations of impossible extravagance. The allotropic modifications of which sulphur, phosphorus, oxygen, and carbon are susceptible, suggest the thought that the countless array of organic compounds, which now bewilder the chemist, may prove only modified forms of one unchanging radical. Like those of isomerism, the phenomena of allotropism inspire the zoochemist with new hopes.

Solidified albumen and fibrine are allotropic conditions of liquid albumen and fibrine. Physically the former differ remarkably from the latter, chemically they are identical. "There was a time when the doctrine, which supposed the convertibility of metals, was opposed to known analogies: it is now no longer opposed to them, but only some stages beyond their present development." [Faraday.] These discoveries have been accomplished, not by the magic touch of the philosopher's stone, but by the touch of genius.

Coincidently with these recondite speculations as to the transmutability of *matter*, have been projected novel thoughts as to the "mutual correlation and convertibility of '*forces.*'" [Grove.] Heat, light, electricity, magnetism, motion, etc., are severally inter-producible "forces." [Faraday.] "Vital action," cell-growth, nerve-force, muscular action, *are the* physical imponderable forces modified in manifestation by passage through an organic material substratum. [Carpenter.] Heat becomes vital force by passing *from* without *into* the egg. Such expressions imply the locomotion of an entity. Against the vague use of this hypothesis-involving language, the student of organic chemistry should jealously guard. He must be warned once more, lest he confound the *ego* with the *non ego*—the conditional with the absolute and unconditioned. All general ideas are born of abstraction. They should be permitted in science only as intellectual guides. The "forces" of matter are the "properties" of matter. The quality cannot be separated from the body. The word "property" in chemistry should be accepted in a phenomenal, not entological sense. The dynamics of matter cannot be studied apart from the statistics. Though, however, the student be warned against the idolatrous worship of all bodiless essences; though it be affirmed that the physical philosopher is in pursuit, not of fictions, but facts, not of subjective creations, but objective realities, it must be understood that the intellectual "aid to pursuit" to be drawn from an intelligently conceived theory is not to be contemned. A "theory" is not an illusive palace raised by fancy; it assists in two ways; it guides the mind in the marshalling of novel phenomena under known analogies—and it dictates method, suggests conditions, devises experiments. It ought not to trammel the freedom of research, nor circumscribe the range of just speculation. It is a mental light, an intellectual instrument of inquiry. It is the bright path projected by forethought into tracts of research yet unsurveyed by the eye of science, and uncatechized by experiment.

"Chance" may have imparted motion to the classic chandelier in the Cathedral of Pisa; but if chance also suggested to Galileo the laws of the pendulum, it must have belonged to that multitudinous order of casualties by which ideas are ordinarily propagated in fit and fertile minds. Theoretic provisions had qualified the mind of the philosopher to evolve an immortal principle out of an accidental event, to generalize a "law" on the basis of an accident. "Facts," uninterpreted by "theory," are inert, incoherent verities.

A *vital fluid* is different in no single particular from any cosmical compound

fluid. The constituent elements of an organic fluid, completely interblended in the living organism, manifest the same laws and the same properties with those distinctive of them while yet within the region of inorganic matter. In the body they acquire no *new* properties. The *old* are only modified, intensified, neutralized, coordinated with, or subordinated to, "conditions" of action known nowhere but in the living body. An organic substance may be composed of five different elements. Each element may be present in a great number of equivalents explains the complexity of organic compounds:—thus,

	Carbon. Atoms.	Hydrogen. Atoms.	Nitrogen. Atoms.	Sulphur. Atoms.	Oxygen. Atoms.	Total. Atoms.
Albumen,	216	169	27	2	68	482
Caseine,	288	288	36	2	90	544
Fibrine,	216	169	27	2	68	482

A "force" exerted in *one* direction—i. e., between *two* elements—unites them in the closest and strongest manner. The result is a compound which is gifted in the highest degree with the power to resist the causes of perturbation. To this compound if one more element be added, the uniting force is reduced to one-third—it is now exerted in three different directions; if a fourth constituent be added, each line of force will be four times less than the sum of the power by which the first two elements were joined. If this reasoning be prosecuted to the limit of 500 atoms, it admits of no doubt that the power will remain the same in *nature*, though multiplied in *directions* of action. The centre of force remains unchanged, though the radii therefrom may diverge to numerous points of an embracing circumference. Where, then, is the limit to be inscribed between vital and cosmic chemistry, between organic and non-organic affinity? Is it at 8, or 10, or 20, or 500 equivalents? Is it at the fifth element, or the fiftieth? Is it at carbonic oxide (C_2O_2=28) and cyanogen (C_2 N=26,) or at starch [C_{12} H_{10} O_{10}] and sugar [C_{12} H_{11} O_{11}?] Where between oxalic acid and those composite products containing sulphur and phosphorus, can the chemist mark the bound beyond which the physical force does not ascend? In the formation of chemical compounds, "organic principles," it is quite certain that the supposed line cannot be drawn which is said to divide physical from vital affinities. Thus, then, the instability, the ready decomposableness, of organic compounds is a property which flows from the multiplied directions in which the uniting power operates upon the constituent atoms. In an atom of sugar the attraction radiates in 36 different lines—in one of olive oil, in many hundreds. But this denotes merely a difference of *number* in the combining elements. Is there no difference of *kind*, of arrangement? Without adding or withdrawing any element, we may conceive the 36 simple atoms of which the atom of sugar consists to be arranged in a thousand different ways; with every alteration in the position of any single atom of the 36, the compound atom ceases to be an atom of sugar, since the properties belonging to it change with every alteration in the manner of the arrangement of its constituent atoms. It will be subsequently proved that whatever may be the groupings of elementary atoms, no *new* force will be required to explain their altered disposition. There is, however, in the study of organic structures, a point at which the evidences of a *new* power are encountered. It is in accordance with analogy to suppose that albumen, fibrine, gelatine, etc., owe their formation to the play of purely chemical affinities. The power which moulds a cell, configurates a fibre, shapes the tubularity of a vessel, transcends the conceptions of the pure chemist. It cannot be experimentally catechized, it cannot be imitated by any artificial arrangement. It is a force *sui generis*. The corpuscles of the blood assume the discal figure, not mechanically, in virtue of a rotary or elliptic motion like the planets in the orrery. In the latter case the *figure* may be predicated from a calculation of the attractive and centrifugal forces. The oblate spheroid is a *necessary* figure. In the attempt to explain the *form* of organized solids, all mathematical and

mechanical conceptions utterly fail. Wherefore the discoidal shape of the blood-cell? why the elliptical in the fish? *This is the vital force;* chemistry compounds the materials, vitality chisels them into form. Chemistry unites the elementary atoms into composite groups; vitality utilizes these compound groups, and no other, in the fabrication of the solids. The cells are the only parts of the fluids which, in this sense, exhibit the evidence of a vital force. The hematosine and globulin and cell-walls are chemical products. "Vitality" gives to these products a special arrangement; but no other "principles" could be shaped, even by the vital force, in corpuscles. It requires conditions of action as well as every other force. There is, therefore, some mysterious relation between the *shape* and the *substance*, between the *material* and the *form.* The cells of the blood, though the constructive principle were provided, can be formed nowhere but in the blood—no other locality offers the required conditions. Muscular fibre can only be formed out of previously prepared compounds, and *then* only in a particular place. Thus obviously governed by "conditions," the *vital* force, as much as any other force, becomes a legitimate object of investigation. Allied intimately to the chemical force, it discovers a mechanical and mathematical mode of operation. In this inquiry the difficulty has hitherto proved insurmountable, of ascribing effects to their true causes—of explaining the share which belongs to each "force" in the arrangements which constitute life. *Med. Chir. Review.*

III.—*On the Rational Treatment of Spasmodic Affections.*

We earnestly direct the attention of the reader to the following practical observations, based on physiology and pathology, on the therapeutics of spasmodic affections, from *Contributions to Rational Medicine*, by Dr. A. Wood, of Edinburg. (*Ed. N. O. Med. and Sur. Jour.*)

1st. The importance of great attention to a proper supply of nutriment and of air in all circumstances, where either hereditary tendency or other circumstances are likely to develop convulsive diseases. Trismus nascentium is epidemic in the West Indies, from the absurd way in which infants are there treated. The same disease was banished from the Dublin Lying-in Hospital by proper attention to ventilation and cleanliness.

Laryngismus stridulus is never so satisfactorily treated as by change of air. The mortality of tetanus in our naval stations in the West Indies has been very much reduced, mainly, according to Dr. Dickson, by improved hygiene. —(*Med. Chir. Trans.* vol. vii. p. 765.)

2d. Let us divest our minds of the idea that it is necessary to treat the fit in any of these diseases. It is only a part of a train of morbid phenomena, and though the part most striking to the bystander, ought not to make the same impression on the intelligent physician. No one now thinks of treating the fit either in epilepsy or chorea, and why should we think it necessary in tetanus or hydrophobia?

3d. Is it not worthy of consideration, whether the obstinate constipation in tetanus and lead poisoning may not be a spasm of the muscular coat of the intestines analogous to that of the voluntary muscles, and like it, not to be overcome by brute force, (that is purgatives?) Certain it is that in lead colic, the finger, introduced into the rectum, is held by the sphincter as by a vice, and pressed tightly upon by the gut, and that this pressure returns at each paroxysm. Certain it is also, that the retraction and hardnes of the abdomen,

associated by Merat with that internal constriction, is found also to exist in tetanus, though I am not aware that any one has ever explored the internal state of the bowel in that disease. It may be necessary to remove the morbid secretions in the bowels, though every intelligent physician will have to balance the amount of irritation produced by their presence with that caused by the drastic purgatives necessary for their removal. It is to treat symptoms, to attack it alone ; and when we succeed in overcoming the dieease, the bowels will spontaneously relieve themselves. Tralles found opium succeed in a case of ileus where purgatives had failed. I have seen the same. Lentillus has confirmed this ; and in a severe form of colic, Bonn became convinced by experience that it was the most powerful remedy.

4th. The most efficient and the most frequent agents in the production of these diseases are sedatives. Bloodletting is a most powerful sedative, and if carried to any extent in a healthy person, produces convulsions. Is bloodletting, then, a suitable remedy in these affections ? In chorea it was formerly practised, and is now abandoned from the injurious effects which it produced. In delirium tremens and hysteria, its use has also been given up. In epilepsy it is rarely used by intelligent physicians, except to meet the requirements of secondary affections; and if we still retain its employment in the more convulsive diseases, it is probably only because their rapid course and frequently fatal issue makes it very difficult for us to ascertain the effect of any treatment.

5th. In one class of convulsive diseases, bloodletting seems, in the present state of our knowledge, to be indispensable. Where urea exists in the blood and produces convulsions, it must either be expelled or counteracted. We scarcely know how to accomplish the latter indication, and therefore are driven back on the former ; but even while seeking by bloodletting to get rid of the urea which is mixed with that fluid, we must never forget that it "both acts on the nervous system as a narcotic poison and impoverishes the blood, inducing degeneration of the tissues," and that therefore, while we take blood to remove the poison, we must do our utmost to replace the nutriment which we are unavoidably compelled to abstract along with it.

6th. Our views on this subject would become more definite and precise, could we avoid imagining that spasm implies augmented strength. It is not easy for us, when seeing the violent agitation of the system which prevails, to divest our minds of the idea of great power being developed, but the same remarks apply to mania, in which disease, thanks to the exertions of Dr. W. A. F. Browne, general depletion, tartar emetic, brisk cathartics, and ice to the shaved scalp, are no longer in such vogue as they once were.

If such are the objections to the routine practice, what course ought to be pursued ? It is easier to point out error than to demonstrate truth. But I venture to suggest—

7th. That sedatives should be cautiously used. Chloroform and cold affusions have each proved fatal in delirium tremens.

8th. That every effort should be made to put into the system as much nourishment as it is capable of beneficially employing.

9th. But if there is any faith to be placed in antipathic treatment, it is to stimulants we must not trust.

10th. It is worth observing, that most of those chemical agents which produce convulsions, acting, as has been already said, in large doses as sedatives, do as small doses act as stimulants.

11th. We find also, that where the minor spasms, as cramp, have been excited by irritation of the peripheral distribution of one class of nerves, as those of the mucous lining of the bowels, they are often relieved by irritation of the peripheral distribution of another class of nerves, as by friction on the skin.

12th. The cerebral functions, more especially volition and sensation, being

much in abeyance, any stimuli, whether mental or physical, by which they can be excited, should be freely given. It was on this principle that Boerhaave prevented the recurrence of epileptic attacks, by directing a red hot iron to be applied to any who might be seized.

13th. The extreme sensitiveness to all irritations which exist whenever the spinal predominates over the cerebral system, suggests the propriety of enforcing the most absolute quiet, and preventing the access of all bodily and mental stimuli. In tetanus and hydrophobia, the creaking of a shoe, the slamming of a door, the sight or even thought of water, or the gentlest zephyr playing on the surface of the body, excites a fit. Hence Armstrong tells us, that in tetanus those patients recover best who get little active treatment, but are nursed, as it were, through the fit.

14th. And if sources of irritation ought to be prevented, those actually existing ought to be removed, but never by irritants which are more powerful than themselves. This surely gives sufficient latitude to the most devoted admirer of the gum lancet, the bolus, or the bag and pipe, while it suggests to him a little caution in these somewhat coarse, though most popular remedies.

15th. And lastly, if I have not succeeded in pointing out any one remedy, which stimulates the cerebral without exciting the spinal system, it is because few, if any such, are known. It is a subject of investigation well worth attention. And if no particular plan of treatment has been announced, which can be unhesitatingly recommended, I am perfectly satisfied if I have created any doubt as to the course which at present is too unhesitatingly followed. I think at least, something has been done to show how much more constitutional these affections are than is usually supposed, and thus open up new plans for prevention, if not for cure. And, if in acute mania, in puerperal insanity, in delirium tremens, in chorea, in laryngismus, and may I add in epilepsy also, empiricism or the influence of authority has induced us to abandon antiphlogistic treatment, and to adopt stimulant and soothing remedies, I do not despair of a sounder pathology soon prevailing in regard to this whole class of affections, and of the discovery yet being made, that they are much more amenable to treatment than has hitherto been supposed.

(*Ranking's Half Yearly Abstract Medical Sciences.*)

IV.—*Uterine Contractions excited by Terebinthenate Injections.*

Late French journals speak of turpentine injections as a valuable means of stimulating the uterus to increased contractions. It has been used in this way to bring about, with a medical view, premature delivery, and to hasten natural labor at full term, when the uterus failed to expel its contents. Over ergot it possesses several advantages ; it does not, like this medicine, excite vomiting, whilst it acts with more certainty and greater celerity. When the os is sufficiently dilated, and the head fairly presenting, we may with perfect safety administer turpentine enemas, combined with a suitable proportion of mucilage, with every assurance of hastening the expulsion of the fœtus. To produce artificial delivery, turpentine enemas should be preferred to the usual violent mechanical means resorted to in such cases.

V.—*Rennet as a remedy in Diabetes.*

Some experiments have recently been made by Dr. Gray and others, with rennet in the treatment of diabetes. Several cases are reported in a late No. of Ranking's Abstract, in which the free use of this article had produced the most marked beneficial effects. It is given in tea spoonful doses after each meal, until the flow of urine is diminished and the sugar entirely disappears in this fluid. In the meantime, a suitable course of diet is instituted, as auxiliary to the cure. From the testimony adduced, with the cases reported, we have every reason to entertain a favorable opinion of the remedy proposed.

VI.—*Occlusion of the Vagina.*

Dr. Storer read the following case before the Boston Society for Medical Improvement, sent to him by Dr. Brainard of Chicago, and published in the American Journal.

July 7, 1852, I visited Mrs. ———, a young married woman, aged 19 years, in the central part of Wisconsin, on account of an obstruction of the vagina.

On attempting to pass the finger into the vagina, it was arrested at its very orifice. Immediately behind the carunculæ myrtiformes there was a firm cicatrix, which entirely shut up the passage, there being a transverse band from side to side, both above and below which there was a slight depression scarcely half an inch deep. On introducing a catheter into the bladder, and the finger into the rectum, the septum seemed thin, and as thick as the natural septum of the bladder and vagina. The uterus was felt distended, filling up the cavity of the pelvis, and rising so as to be felt in front two inches above the pubis. She was constantly affected with severe expulsive pains.

This young woman, who was but 19 years of age, and presented appearances of not fully developed womanhood, was married on the 8th of October, 1851. About the 20th of November following, she was attacked with great pain, hemorrhage, profuse discharge of pus and mucus. There was constitutional disturbance of the severest kind, and her life was despaired of. There followed a discharge of shreds and sloughs, horribly offensive, which continued several weeks. This attack was taken for an abortion, but without good reason. It was simply a violent case of acute vaginitis, from too frequent sexual connection with the sexual organs imperfectly developed. She informed me, in answer to inquiries, that coition was very painful, and became more and more so.

Pains, indicating the return of the menstrual period, occurred February 1, 1852, and recurred twice at four weeks' interval without discharge. From that time the pains were continuous up to the time the operation was performed, the uterus gradually enlarging.

Operation July 8, 1852. The patient being placed in a suitable position, an incision was made from before backwards through the band before described extending the diameter of the vagina. The sides being separated by the finger, it was found that the mucous membrane of the rectum could be separated from that of the bladder with the point of the finger, without great violence. When a band of tissue resisted, it was divided with a blunt-pointed bistoury. The separation was freely made to the uterus.

Here, however, instead of finding the os uteri, nothing but a smooth round tumor presented itself. The finger was carried over its surface, and the surface denuded for a space at least two and a half inches in diameter. Finding no mouth, I determined to make a puncture at a point where a slight elevation was felt, and where it was presumed the os had been situated before it sloughed away. Accordingly, I made an incision in the form of two thirds of a circle an inch in diameter, raising up a piece attached towards the anterior part. There was a copious discharge of the menstrual blood. The wall of the uterus was thick, and gave the sensation of fibres in cutting. Passing the finger within, a slight depression was found opposite the little tubercle noticed externally, seeming to confirm the idea that this was a vestige of the neck.

After treatment, a tent, or pessary, one and a half inches in diameter, made of pieces of sponge strung together upon a stick, and covered with oiled silk, was passed into the artificial vagina. The point was sufficiently small to press into the opening of the uterus, and the whole long enough to press externally. The case was entrusted to the care of Dr. Robert W. Earll, to whom and to Dr. Axtell, I was indebted for assistance.

Not a bad symptom occurred; no swellings and but little inflammation. August 25 she was able to walk from her room without difficulty.

Four months after the operation, November 8, she was well, doing the work of the house and able to ride several miles. The cicatrization was perfect, at least that was Dr. Earll's opinion. The menses have occurred regularly without pain, excepting the first, August 20, which was painful. She is unwilling to dispense with the pessary, as she experiences a sensation of falling of the womb without it, even when lying. Sexual connection is not painful, and there is no tendency to contraction.

May 30, 1853, she continued in the same satisfactory condition.

VII.—*Sugar of Milk an article of Food in Consumption and other Pulmonary Diseases.*

BY JAMES TURNBULL, M. D.

Those who are familiar with Liebig's works are aware that he established several years ago the fact, that all the various substances used as food, belong to one of two classes; the azotised or plastic, which form the tissues of the body, and replace the worn out tissues; and the non-azotized or combustive, which furnish food for the lungs, supporting respiration and animal heat. Now it is a curious fact, which he also established, that though the fibrin, albumen, and casein, which constitute the chief of the first or azotized class of alimentary substances, exist in vegetables as well as animal food, animals have not the power of forming in their own bodies any of those azotized alimentary principles. They are primarily derived from the vegetable kingdom; and the digestive organs of animals have no power of producing them, but merely of assimilating what has been already formed by plants, or previously drawn from the vegetable kingdom, from some other animal. These views met with considerable opposition when they were first advanced; but their correctness is now generally admitted, and there is no essential difference in the chemical composition of fibrine procured from vegetables and that obtained from the flesh or the blood of an animal, or between vegetable and animal albumen or casein.

Of the other class of alimentary substances—the non-azotized—the chief use of which is to supply food for respiration and the support of animal heat,

the principal are starch, sugar, oil or fat, and alcoholic liquors. These unite with the oxygen absorbed at the lungs, and are the chief sources of the carbonic acid and watery vapor given off by these organs. They are in fact burnt by a process of slow combustion, which is the great source of the high temperature of animals.

It appeared to me, that as this function of the lungs must necessarily be more or less impeded in all pulmonary diseases and as cod-liver oil had been found so beneficial in that particular disease, consumption, advantage would be gained by selecting from this, the non-azotized, or combustive class of alimentary substances, such of them as would have the greatest tendency to unite readily with the oxigen absorbed at the lungs—and thus, in the disabled condition of these organs, to facilitate the performance of their functions.

I was thus led to inquire which of the non-azotized or combustive class of alimentary articles are most readily digested, and have the greatest affinity for oxygen. Sugar of milk is an article belonging to this class of aliments, which possesses these properties in a high degree, and is deserving of more attention than it has yet received as an article of food. I shall therefore state a few facts respecting it, which seem to me sufficiently interesting to be worthy being brought under the notice of the profession.

There are three principal varieties of sugar—cane sugar, milk sugar, and grape sugar. They are closely allied in composition, though they differ considerably in chemical properties. All kinds of milk contain sugar of milk; but it is worthy of notice that asses' milk, which has always had a greater reputation than any other kind, as an article of food in consumption and other pulmonary diseases, contains the largest proportion, relative to the caseous and oleaginous principles, of any kind of milk. Whey, which consists almost entirely of sugar of milk, has also been found a useful article of diet in consumptive cases.

When we inquire into the chemical properties of milk sugar, we also find that it has so strong an attraction for oxygen, that when dissolved with an alkali, it has the power of reducing more or less completely some of the metallic oxides. It is readily absorbed into the blood, which being an alkaline fluid containing oxide of iron, furnishes the necessary conditions for its oxygenation. Besides this, its composition is such, that it must be readily converted into carbonic acid and water. There is only one other point in relation to its fitness to supply material for respiration, which I shall at present notice. It is the fact, originally pointed out by Liebig, and now admitted by physiologists, that one of the great offices of the liver is the preparation of combustive material for the respiratory process. This is a point which has not been sufficiently kept in view by medical men; but it is one of great practical interest when we consider, that the function of the lungs and that of the liver are so intimately connected and mutually dependent, that derangement of the secreting function of the latter must necessarily interfere with the former, and may not improbably be one of the chief causes of a tubercular state of the blood. The liver prepares the combustive materials for respiration; and of this there are two sources, one being the worn out tissues of the body, the hydrocarbonaceous part of which forms bile, and being re-absorbed, is consumed at the lungs; the other the saccharine and fatty matters of the food, which are consumed in a similar way. It would seem, however, that the liver has not only the power of preparing the latter, but also of forming saccharine at least, if not oleaginous matters, from the blood. A defect in this power may be one of the great causes of tubercular diseases; and if we can, by giving a ready formed oil, which is stored up at certain times in the liver of the codfish, rectify to a great extent any defect in its action, so far at least as the oleaginous material for respiration is concerned, there is good reason to expect that still more may

be gained by giving, in a ready formed state, the other combustive material, the saccharine.

The facts I have brought forward have led me to use sugar of milk in the treatment of consumption; and as I have seen benefit from its use, I wish to recommend it as an article of food deserving of more attention in the treatment of this disease than it has yet received. I believe also that they embrace an important principle, applicable to the dietetic treatment of other diseases.

(*Assoc. Medical Journal, June* 24, 1853.)

VIII.—*Nympho-Maniacal Hysteria.*

M. Sandras defines hysteria "an habitual nervous condition, in which occur, at longer or shorter intervals, paroxysms characterised by a peculiar sensation of strangulation, by oppression of the respiration, more or less acute pain in the head, and by clonic convulsions in all or nearly all parts of the body." This rapid description, the only definition possible, omits a double phenomenon, which is very common in hysteria, and of which a patient in M. Grisolle's ward at La Pitié, presents a remarkable example. We refer to anæsthesia (absence of tactile sensibility) and analgesia (absense of sensibility to pain.) However rude the pressure exercised upon different portions of her body, she does not perceive it. A feather may be introduced into the nostrils or pharynx without exciting the slightest sensation. A pin may be passed through the fold of the skin without producing pain. This woman is therefore affected with anæsthesia and analgesia, but not in every point; a very circumscribed portion, the septum of the nasal fossæ is still sensible, and it is sufficient to pinch this between the fingers to occasion a convulsive paroxysm.

Every sensation may be decomposed into three elements, which are, the impression, the transmission of the impression, and the perception. The impression and its transmission are not indispensable, inconceivable as this appears at first. Do not those who have been subjected to amputations experience sensations referable to the limbs which they have lost—and are not sensations reduced to perceptions in hallucinations? In the particular case of which we are speaking, in hysterical anæsthesia and analgesia, which of the three elements of sensation are wanting, or are they all absent?

The patient of M. Grisolle is interesting in another point of view; and here again we have to signalize an omission in M. Sandras' article on hysteria. It does not mention the libidinous, erotic, or nympho-maniacal form of hysteria; a form which is nevertheless common. Not only is M. Sandras silent in regard to the erotic form of hysteria, but he denies that hysterical women "are more disposed than others to take an active part in sexual intercourse;" and he adds, that the contrary rather is true.

In M. Grisolle's patient the erotic form is very marked. This woman's first action, when her paroxysms come on, is to seize upon the nearest man, and she moves the pelvis in a voluptuous manner throughout the whole duration of the attack. At the visit, when she is surrounded by the medical staff, her desires are seen to become excited whenever she is spoken to. This is not nymphomania, for the hysterical paroxysms are perfectly characteristic. On that score it is sufficient to say that she is considered hysterical by such an observer as M. Grisolle. It will be easily understood, that under such circumstances no attempt has been made to ascertain the condition of the sensibility of the genital organs.

M. Grisolle saw, some years ago, a case of hysteria in a very distinguished

woman of fashion, thirty-five years of age, the widow of a man, who, having become the victim of satyriasis, subjected her to the rudest assaults. It cannot be doubted that the privation of sexual intercourse, succeeding such great abuse, was the cause of the hysteria in this case, and for this reason. This lady soon married a robust man, and under the influence of marriage the hysteria was dissipated.

Relatively to the treatment of this strange neurosis, the case we present is likely to modify the opinion of M. Sandras, who expresses himself in these terms: "I have never witnessed a case in which the enjoyment, or even the abuse, of venereal pleasures, have produced a diminution or cure of hysteria."

The seat, or rather the starting point of hysteria, is very obscure, and is the subject of much dispute; the majority of physicians and philosophers have placed it in the womb. Vain attempt at localization! Is it not true that hysteria has occurred in a woman without a womb, and also in men?

M. Grisolle has seen one example of hysteria in a woman in whom the uterus was absent. The anatomical fact was established by M. Chassaignac and himself. It was easy to be satisfied in regard to it by introducing a sound into the bladder and a finger into the rectum. On either side could be felt two small bodies supposed to be ovaries. The hymen was intact, and behind it, instead of a vagina, there existed an opening of scarcely more than a line in depth. The external genital organs were normal. The mammæ very large. Strangely enough, hysteria assumed the strongly characterized erotic form in this unfortunate (à cause de son extreme lubricité) was greatly sought after by the nurses of the hospital. Is it not clear, after this case alone, that the cause of hysteria is not in the uterus, neither in venereal appetite?

Where then does it reside? We say simply, without entering into considerations which would carry us too far, that it varies, probably, in different cases; but, that wherever it may be, the morbid action, even if eccentric, inevitably rebounds upon the nervous centres, and from these irradiate the influences which produce the multiplied neuropothic phenomena which constitute the fantastic symptomology of hysteria.

As to venereal desire, it arises in the central nervous system, in a point which we do not now seek to determine (apparently in the cerebellum), but which is affected, on the one hand, by the excitement of the imagination, and on the other, by physiological or morbid excitement of the external organs of generation.

Is hysteria diathesic, that is to say, connected with a general condition of the economy, or does it consist in an organopathy limited to the nervous system? This is still a problem unsolved by the sagacity of pathologists.

(*Gazette des Hôpitaux.*)

IX.—*Fistula in Ano, treated by Iodine Injections.*

BY M. BOINET.

At a meeting of the Institute, of August 1st, M. Boinet read a memoir, designed to demonstrate the efficacy of injections of iodine in the radical cure of fistula in ano, whatever their form, extent, or complications. Seven cases are detailed, which offer examples of almost every variety of fistula—complete, blind, or incomplete fistula, deep fistula, with loss of substance of the intestine, and fistula in tuberculous subjects. These observations tend to prove that iodine injections may be advantageously employed in all cases of fistula, but especially in those in which the method by incision is dangerous or ineffectual;

such, for example, as extend deeply, or occur in phthisical patients, or depend upon some alteration of the ischium, the coccyx, or sacrum.

The advantages of iodine injections over the ordinary method, consist, in obtaining a cure more easily, and in a shorter time, in avoiding pain and the danger of hemorrhage, and in permitting the patients to continue at their usual avocations.

The following are the conclusions of the memoir :

1. Iodine injections, properly administered, can cure radically all cases of fistula, whether complete or incomplete, simple or complicated.
2. They cure them more promptly than the method by incision commonly employed, and with less danger.
3. They produce no pain, and are easily practised.
4. They permit patients to follow their occupations, and relieve them from daily painful dressings.
5. They are applicable to all cases, and especially to those in which incision or excision are dangerous or impossible.
6. They do no harm, even if they are ineffectual, and do not prevent the subsequent use of the knife. It is therefore rational to employ them before having recourse to a cutting instrument (*Ib.*)

X.—*Structure and Function of the Spleen.*

There are still numerous opinions almost constantly being advanced on the structure and function of this complicated organ. Buk supposes that the colorless corpuscles of blood which are to be changed into the colored ones, are formed in it. This takes place by the passage out of the twigs of arteries ramifying on the Malpighian vesicles of an organizable lymph, which thus gets into the lymphatics. (He believes the Malpighian corpuscles to communicate with the lymphatics.) Here the first developed elements of the blood, the colorless corpuscles, are formed ; part being transferred to the lymphatic vessels, and part to the veins. Thus the venous blood contains an important component not found in the arterial.

Tigri, in a paper reprinted from an Italian medical journal, and written in order to vindicate his claim to priority over Assor and Kolliner, in their researches on the function of the spleen, re-asserts, as the results of his microscopic and other investigations on the spleen of men and animals, the following conclusions :

The spleen is an organ which nature has destined to preside over the material composition of the blood. It receives into its vessels blood loaded with solid matters for elimination ; these are the used up epithelial cells and red globules, which are assimilated in it, and reduced into new principles of nutriment.

1. The anatomical elements of the spleen are blood and lymphatic vessels, to which are united the Malpighian corpuscles, the fibrous structure, the microscopic web, and the splenic fluid.
2. That it is not credible that the vessels of the spleen, looking at their size, as compared with that of the organ, are destined only for its nutrition.
3. This is confirmed by observing that in other organs in which the blood has to undergo a modification, there exists two orders of different vessels, i. e., the pulmonary and bronchial arteries for the lung, the hepatic artery and portal vein, for the liver.
4. Similarly, these two orders of vessels must be recognized in the spleen ;

the first comprises the nutrient vessels; the second, those which carry into the venous system the blood loaded with eliminable materials.

5. The special conformation of the splenic venous canal of ruminants, visible from the point of their entrance into the organ, has reference not only to the form of the canal, but also to the structure of its walls.

6. To the form, which is cylindrical, but irregular, from hollows and projections, to which he gives the name of splenic productions.

7. To the structure, inasmuch as the parietes of the veins are formed by the red substance of the organ, together with a most subtle and transparent membrane, which divides it from immediate contact with the blood.

8. This membrane, organized like that of the capillaries, performs the office of a filter, and gives passage to the red globules of the blood, which are rendered inactive as well as the epithelial bodies.

9. This structure, so visible in the large venous trunks of the spleen of ruminants, is verified also in that of the horse, pig, and lastly in the human spleen.

10. The communication between the arteries and veins of the second category, by the intervention of a capillary system, is effected by channels so ample, as to permit the easy passage of bodies as large as one third of a millimetre.

11. The spleen pulp otherwise is not a dense liquid, but an assemblage of fusiform nucleated cells, involuted or folded on themselves, isolated nuclei, and red blood globules, which elements are contained in a most delicate areolar web.

12. The presence of the Malpighian bodies is undoubted.

13. The structure of the spleen presents no resemblance to the cavernous bodies.

14. The microscopic web, with its areolæ, is in direct communication with the venous cavity, by the porosities of the stratum limiting the isolated or confluent splenic productions.

15. The same web is in communication with the lymphatics.

16. The epithelial bodies, detached from the walls of the vascular system, and mixed with the circulating blood, are brought by the artery into the splenic tissue, in which there is every reason to believe that they are arrested.

17. The same happens to the worn out blood globules.

(British and Foreign Med. Chir. Review.)

XI.—*Diabetes Mellitus not Incurable.*

M. Schutzenberger, Professor of Clinical Medicine at the Faculty of Strasbourg, has just published a case, which would tend to show that diabetes mellitus may be in some degree arrested, and the patients restored to comparative health. After detailing the case at length, he comes to the following conclusions:

1. Glucosuria is not an incurable disease, and although the tendency to relapse is certain, it is possible, by perseverance, not only to remove the sugar from the urine, but also to bring the patients so far as to bear a varied diet, in which feculent substances may enter without causing a relapse.

2. The amount of glucose excreted by patients is sensibly proportionate to the quantity of feculent matter added to the food, and it is possible to discover

errors of diet by the increase of the glucose in the urine. The quantity of urine is equal to that of the fluid ingested, and the latter also proportionate to the amount of the feculent substances taken into the economy.

3. Diet forms the principal part of the treatment of glucosuria; the food ought chiefly to consist of milk, fatty substances, butter, oil, eggs and meat.

4. The entire banishment of feculents seems necessary to make the sugar disappear completely.

5. Small quantities of bread, viz: about three ounces per diem, are usually well borne, and do not excite a fresh secretion of sugar when once it has disappeared.

6. The powers of assimilation increase gradually, and it is possible, by means of chemical analysis, to ascertain the extent to which the peculiar diet and medicine ought to be carried.

7. The effect of the diet is powerfully assisted by the use of certain therapeutic agents, and especially by opium, in increasing doses, and by alkaline drinks. In this affection the tolerance of the opium is very great.

8. It is certain that the glucose is formed in the alimentary canal, and that the absorbents generally take it up, so that solid motions come to contain no sugar.

9. Purgatives may cause a diminution of glucose in the urine, as more or less of the sugar which would have passed into the urine is carried away by the liquid stools.

The author does not enter into any theoretical discussion touching glocosuria, as his object was merely to call attention to a series of interesting and instructive facts.

(*Lancet, August 6th*, 1853.)

XII.—*Ligature of the Femoral Vein.*

BY M. ROUX.

At a meeting of the Surgical Society of Paris, on the 27th of July, M. Roux asked leave to communicate an observation which had recently occurred in his practice.

A patient had been operated on twenty-eight days previously for a voluminous tumor, occupying the whole of the inguinal region, and descending into the scrotum.

The tumor was composed of various elements, but contained no cancer cells; it had relapsed five times.

It had been removed four times by Chelius; it first appeared eight years ago; the last operation was done ten months since. The constitution was not impaired.

After having dissected the tumor superficially, when it became necesssary to separate the deep seated portions, the surgeon avoided the artery, which it was necessary to dissect for a considerable extent, but it was not possible to avoid the crural vein, and this was opened.

I thought it best, said M. Roux, to place a ligature above and below the wound. The wound was above the junction of the saphena.

At the moment I applied the ligature, the limb became livid and cold—but, on the following day, the coloration was less, and the temperature was normal.

On the third and fourth days a considerable œdema supervened, invading the

whole of the limb quite up to the hip. A few days afterwards erysipelas came on, and passed through its regular stages; and then an abscess formed on the side of the foot. This was opened and readily healed up. At present the patient is in a very satisfactory condition.

The operation lasted two hours; the patient was under the influence of ether the whole of this period.

The wound has nearly cicatrized.

This fact, said M. Roux, in conclusion, appears to me most important. Perhaps I was the first who called the attention of surgeons to the danger of wounding the femoral vein above the saphena. A priori this should be considered a most serious lesion; nevertheless, in this case, perhaps unique, we see that the accidents were altogether insignificant.

(*Gazette des Hôpitaux.*)

XIII.—*Furunculus.*

M. Nelaton (Gazette des Hop.) observes, that the development of furuncles may be always arrested by keeping the part covered with a linen compress, which has been dipped in concentrated alcohol. This must be accurately applied to the part, and care taken to keep it constantly moist, so that evaporation may be constantly taking place from its surface.

Part Third.

REVIEWS AND NOTICES OF NEW WORKS.

I.—*A Treatise on operative Ophthalmic Surgery.* By H. Haynes Walton, Fellow of the Royal College of Surgeons, England, Surgeon to the Central London Opthalmic Hospital, etc., etc., etc.

In his preface, Mr. Walton says: "As an untried writer, I cannot be otherwise than anxious for the success of this work. I have spared no pains in my attempt to make it of practical value, and I have laid under contribution all that has come within my reach or my knowledge, whether English or Foreign on the subject of Ophthalmic Pathology and Surgery."

In addition to the vast amount of book-knowledge to which Mr. Walton has free access, he also brings to the task the results of his own personal experience and observation, which must have been very great, from his holding for a long time the post of Surgeon to the *Central London Ophthalmic Hospital.* Thus much then for the auspices under which this work was elaborated by the author.

The first chapter is devoted to the consideration of the "*Use of Chloroform in Ophthalmic Surgery.*" He pronounces Chloroform one of the great improvements in the Surgery of our times; and rejoices that its benefits have been extended to operations on the eye. By its tranquilizing effects upon the patient's mind it prepares him for those

delicate operations on the eye, which not only require a steady hand in the operator, but likewise, demand on the part of the patient the most perfect repose of the parts. In operations of this sort upon children, the use of Chloroform is especially indispensable,—for without it the struggles of the child may frustrate all the foresight and dexterity of the most consummate operator.

Mr. W. awards the honor of the first discovery of an Anæsthetic to this country, to the "*New World,*" as he expresses it. Of the glad tidings, he says:

"This discovery, fraught with so much interest to the suffering part of mankind, spread with unusual rapidity over the world, and in an incredibly short period, ether has been inhaled in the most remote quarters of the globe. He then goes on to give a short sketch of this great discovery, mentions the parties who justly claim the credit, those also, who have pointed out the precautions which are required in its administration, and particularly singles out Mr. Snow, who stands pre-eminent for his zeal and successful efforts in this important question.

In the *Second Chapter*, he describes the various instruments required in operating on the eye, furnishing plates, both full and accurate; but we refer to the text for particulars under this head.

The *Third Chapter* discusses the *injuries from mechanical or chemical* agents. Among these are most common and serious burns and scalds, which end in deformity and permanent injury to the parts on account of contractions which are apt to result from the thinness and looseness of the structures concerned in the parts adjacent to and surrounding the eye.

It requires therefore particular care and constant attention to arrest and limit as far as possible the suppurations which are so liable to follow burns and scalds of the lids. For this purpose, the author speaks in high terms of the application of cotton wool; it shuts out the air and absorbs the morbid secretions. This application is preferable to cold water dressings, as it can be easily removed and as often reapplied.

If the *cutis vera* be deeply involved, no treatment that can be instituted will obviate the resulting contraction and deformity.

If the injury be superficial, sprinkling the part with some simple powder, or penciling it with the solution of the nitrate of silver, or covering the whole with gold beater's skin or gum, should be preferred to collodion which is too painful to be borne. For *ecchymosis* about the eyes he discards leeches, denounces punctures, and only in

particular cases advises cold or warm sedative lotions, as the feelings of the patient may require.

In speaking of the different states of the constitution as influencing and modifying the products of inflammatory action, Mr. Walton quotes the late Mr. Dalrymple "*on the rapid organization of lymph in cachexia,*" and the latter gentleman tried to ascertain if effusions of the organizable materials of the blood become vitalized by the production of new vessels more readily and sooner in cachexia, than in robust states of the constitution. After a number of careful observations on this point, he thus sums up his convictions :

"The conclusion he arrives at is, that in those who are enfeebled and depressed, effusions from the capillary vessels are more speedily and completely organized, with vessels capable of being permeated by minute injections, than in the more vigorous and plethoric, in whom inflammation is more acute in the outset, and passes through more speedy and determined stages. The greater tendency to the effusion and organization of fibrine on the surface of the iris in syphilitic cases, than in those of idiopathic iritis is noticed ; and the remark is made, that there will be no difficulty in admitting that the specific cases occur, at least in London, in far greater proportion, in enfeebled constitutions, and in those debilitated by excess and irregular habits, or the mal-administration of mercury for the primary disease.

A well-marked case of syphilitic iritis, peculiarly valuable and well worthy of perusal, is given in support of this view, and further evidence is adduced of the rapid organization of lymph in other instances of disease where life has been nearly extinct."

Mercury in diseases of the eye, is considered by the author to be highly beneficial; at the same time he acknowledges that, in these, as in many other diseases, it has been much abused. This salt does not always, says he, control inflammatory affections of the eye, for he has seen irritis attack persons who were laboring under the effects of mercury, for syphilitic diseases. In one case, at St. Bartholemew's Hospital, the iritis notwithstanding the patient was thoroughly salivated, proceeded so far in spite of treatment that the eye was totally lost.

We have seen iritis develope itself in syphilitic subjects whilst fully under the influence of mercury ; in such cases, the iodide of potash the mecurials being suspended, acted favorably, aided by counter irritation and revulsives.

Mr. Walton tells us that in his practice he is in the habit of using mercury in very small doses. He gives the *hydrargium cum creta,* in two and three grain doses, combined with hyosciamus or conium, as the state of the bowels may require. This combination is effectual, mild in its action, and answers all the purposes of an alterative and antiphlogistic. He regards salivation as a poisonous influence on the

system, and thinks all the benefit desirable may be accomplished short of ptyolism. Our author positively condemns salivation as necessary to cure syphilitic iritis; it is rarely necessary to push the mercury to this extent in this affection.

Mr. W. advises the artificial dilatation of the pupil whenever there is a tendency of the iris to adhere to the capsule of the lens. To effect this, he prefers the sulphate of atropa to the belladonna, in the proportion of two or three grains to the ounce of water, of which a drop or two applied three times a day, on the conjunctiva of the outer part of the lower lid, will produce all the effects of dilatation. An ointment, made of one grain of the atropa to a drachm of lard, may be applied to the lid with excellent effect.

This work condemns, except in extreme cases, the evacuation by puncture of the aqueous humor in inflammation of the eye ball, as recommended and practised by Mr. Wardrop. Both Mr. Lawrence and Dr. Jacob reject the practice, whilst Mr. Tyrrell is a strong advocate for the operation, particularly when the size of the chamber was sensibly augmented, and when the globe was very tense and tender, associated with much ophthalmia or sclerotitis.

The operation is thus described:

"Should chloroform not be used, considerable difficulty will be found in exposing and steadying the globe. When the patient is insensible, the operation is very simple. The humor should be let out with the smallest possible puncture, with a needle, one rather broader than the straight solution needle, or with the point of a cataract or iris knife. The operator stands behind the patient, who may be sitting or lying down, and proceeds as if about to operate for cataract, by raising the lid with the forefinger, fixing the globe with the point of that, and of the middle finger, while an assistant depresses the lower lid. The globe thus steadied, the point of the instrument is introduced close to the edge of the cornea, and when fairly through the cornea, turned on its axis to make the wound gape, when the aqueous humor will flow out."

The accumulation of pus in the anterior chamber of the eye, should not be evacuated by puncture, except in rare instances; with active purgation and counter-irritation behind the ears, the matter may be removed by absorption, and the sight restored. Mr. W. is not a warm advocate for blisters either *behind* the *ears* or to the *temples*; but we must acknowledge that we have seen Dr. Stone of this city apply them with excellent effect in chronic ophthalmia in our great Charity Hospital.

Wounds of the eye, produced by blows, sometimes cause effusion of blood in the chambers; but such effused blood may be readily absorbed by the use of cathartic medicines and cold lotions to the organ.

Mr. W. never ventures to evacuate the blood by puncturing the cornea; he has never been disappointed in seeing it absorbed.

The crystalline lens may be displaced—thrown forward by blows on the eye or head, and if the capsule be unbroken, it should be at once removed by operation, as the chances for its absorption are exceedingly small indeed.

Hernia of the iris produced by its protrusion through a wound of the cornea, may be reduced sometimes by dropping a solution of the *sulphate of atropa* on the conjunctiva. Our author mentions an instance of this kind. He moreover hints of the practicability of enlarging the corneal wound, the patient being chloroformised the while, and thus relieve the strangulated or strictured iris and restore it to its natural position. It would require great skill and expertness on the part of the surgeon, and absolute repose of the patient, to execute this delicate little operation.

A common eye-probe may be used to produce a prolapsed iris, when not too closely grasped by the cornea.

The application of caustic to the protruding iris is recommended by good authority; and we have seen it used in the Charity Hospital of this city with good effect in similar cases.

Some practical observations then follow on the injuries to the eye by chemical and mechanical agents; except in certain cases, but little can be done to neutralize the destructive effects of chemical agents when accidentally brought in contact with the eye; for the most we must be content to combat the resulting inflammation or ulceration. Our author advises a thorough exploration of the eye, and particularly the duplications of the conjunctiva, in cases where we have reason to suspect the presence of any foreign substance—such as sand—small pieces of iron, &c. When we are unable to detect them with the eye, and yet, from the sensation of the patient, suspect their presence, a stream of pure water must be driven by a small syringe over the globe of the eye. In this way, we once relieved a female whose eye was charged with a number of small irregular pieces of glass, which accidentally got into the eye by having her head thrust violently by a brutal husband through a pane of window glass.

Foreign bodies sometimes are projected with force and are driven into the cornea or sclerotica, giving rise to much pain—sometimes inflammation and other troublesome symptoms. These should be removed as early as practicable, when all pain &c. will disappear. Several examples are given by the author, and among these are the following:

"The following case of the impaction of metallic particles occurred in the practice of my friend Mr. Browne, of Belfast, and illustrates many practical points. The entire surface of the cornea, and the greater portion of the conjunctiva scleriotica, were literally paved by fine particles of iron. A young man in an iron foundry was drilling a hole in the cylinder case of a steam engine; he stooped down to observe his progress, and holding a lighted candle, there was an explosion of some gas that had collected between the cylinder and case. The eye was scorched, and the particles from the drilling thrown on its surface. The cornea was scraped of its epithelium, and the particles of iron removed to an extent that saved the eye, and rendered it useful. Deformity, however, remained from the stains on the cornea, and the presence of some of the iron in the conjunctiva, from which but a small quantity of the filings was extracted. Some of the metal was even under the conjunctiva."

"In the sixth volume of the 'Dublin Medical Press,' we also find an account of a nail in the eye: Mary Barron, admitted on the evening of the 29th May, 1841, into the Richmond Surgical Hospital, stated, says the reporter, that about two hours previously, when shaking a carpet, a small nail, which had been concealed in it, was jerked against the centre of her right eye. The shock it caused was very great, and slight nausea and general weakness followed. A nail supposed to have done the injury, was found in the carpet. Very shortly afterwards she observed, on her apron, a small glairy substance, which from her replies to questions respecting it, was probably the crystalline lens. The palpebræ of the right eye were much inflamed and swollen, the chemosis being so extensive as almost to cover the cornea, which presented a depressed appearance with a slight oozing of blood. Not the slightest trace of anything like a foreign body in the eye could be discovered. Up to the 15th of June there was scarcely any alteration in the state of the eye, notwithstanding the vigorous antiphlogistic measures which had been pursued; and on the evening of this day the eyeball was found greatly enlarged, very painful, and, apparently, hopelessly disorganized. Under these circumstances Dr. O'Beirne thought it advisable to make a free opening into the cornea; the incision was followed by a considerable discharge of purulent matter mixed with serum, and the patient expressed herself relieved. Whilst making this incision, Dr. O'Beirne felt the point of the lancet grate against some hard resisting body, and at the moment stated his decided opinion to the class, that there was some foreign body in the eye. He thought proper, however, to desist from any further examination on this day; but two days after, having by a careful examination satisfied himself of the presence of a foreign substance, he proceeded to extract it with a pair of long-limbed forceps, and readily succeeded in drawing from the eye a small black nail, of about three-quarters of an inch in length, having a broad and flattened head very nearly a quarter of an inch in diameter. It is a singular fact, that the point of the nail was in front. Immediately on the withdrawal of the irritating body, the distressing symptoms ceased."

Certain structures of the eye, like other parts of the system, may become ossified,—for instance, the capsule of the lens is sometimes ossified—when it acts as a foreign body and requires removal. The choroid and the hyaloid membranes; the retina and all the textures of the eye have been found more or less ossified, and calcareous concretions within the eye have been observed. We find the following examples of this kind of degeneration:

"From a man who was blinded by lightning forty years before death. The choroid membrane contains several small thin plates of bone at its posterior part; the lens is absorbed, and its capsule ossified.

The eye of a blind man in which large plates of earthy matter or bone are formed in parts of the retina and on its inner surface. There is a second specimen similar to this.

Parts of an eye dried; the lens, converted into a mass of white compact earthy substance, nearly retains its natural size and form; some irregular portions of earthy substance, extend also from it into the vitreous humor.

The sclerotica has been found partially ossified. Mr. Middlemore alludes to the *post-mortem* examination of an idiot boy, at St. Bartholomew's, where ossification of the greater part of the globe of each eye existed.

In the museum of St. Bartholomew's Hospital, there are sections of the eye of an adult, showing that the retina has disappeared and its place become occupied by a thick layer of dense osseous substance.

A remarkable case of the impaction of a grain of duck-shot in the optic nerve, where it lodged for six years and six months, is recorded in the "London Medical Gazette," for 1834, vol. xiii. The shot entered the eye at the inner side of its surface, near the cornea. Occasional and intense pain, for four years and a half, and the serious disturbance of functions of the other eye, induced the patient, contrary to medical advice, to have the body sought for. The lens, which is said to have been partly bony, partly calcareous, was removed with the hope of affording relief, but without benefit; pain continued, and the sight of the other eye being endangered, an attempt to find the shot was made, but unsuccesfully. The sufferer now determined to have the eye extirpated, and the shot was found impacted, as the report says, in that part of the optic nerve which expands and forms the retina. The right eye was daily getting into health when the last communication was sent to Dr. Butter, the operator."

A long chapter is devoted to affections of the eyelids; but as these may be treated according to the common principles of surgery, we shall not stop to notice any special points discussed. We refer to the work itself for ample details.

Chapter Sixth treats of "*affections of the Puncta—the Canaliculi and the lachrymal tube.*"

From the minuteness of these structures, they are seldom the seat of disease, although they may suffer from mechanical injury—contractions from ulcerations proceeding from burns. To give the reader a distinct idea of the excellent views laid down by the author on the subject, embraced under the above head, would require more space than we devote to this subject; we therefore again refer to the text for full details.

Caries of the orbit next demands the consideration of the author; but we confess we have not read his observations on this point.

Chapter Eighth goes at great length into the examination and treatment of *nævi materni*—a pathological point, which we had imagined appertained to surgery proper.

We must here close this notice, with the recommendation to our subscribers to procure the work and read it. The plates are well executed and the text neat and well printed. T. L. White, No. 105 Canal street, keeps the work for sale. A. H.

II.—*Dr. Hooper's Vade Mecum: or a Manual of the Principles and Practice of Physic*, by Wm. A. Gry, M. D., Cantab, author of Medical Jurisprudence, 1853.

We have heretofore spoken in condemnatory terms of "Manuals, Vade Mecums," and their congeners; in the present instance however, we must except from this class, the compendium of Dr. Hooper's work by Mr. Guy. In Great Britain, the work has already passed through many editions with additions and emendations; and at this time it is very popular with all classes of practitioners in the old world.

As a treatise on the practice of physic, we know of few books better calculated to guide the doubting, or give confidence to the wavering in perplexing and trying emergencies than the compendium before us.

No theoretical doctrine or mooted question occupies a place in the pages of the work, it presents only those well ascertained and leading facts of the science, which are absolutely required of every general practitioner who undertakes to cure diseases.

The Book is arranged under the following distinct heads:

1st. The characteristic symptoms by which diseases are known.

2d. The causes from which they most frequently have their origin.

3d. The circumstances that more especially point out the difference between diseases which resemble one another.

4th. The signs which influence the judgment in forming a prognosis of their event.

5th. That mode of treatment, which in the present improved mode of treatment is deemed most appropriate and which experience has sanctioned.

At the close of the volume, a select collection of useful Formulæ are given which renders the work more valuable. White, 105 Canal street, has the book for sale. A. H.

III.—*The Practice of Surgery.* By James Miller, F.R.S., F.R.C.S.E., Surgeon in Ordinary to the Queen of Scotland, etc. Third American from the Second Edinburgh edition, with additions by Sargent. Illustrated with 319 engravings. 1853. Philadelphia.

We have from the first been a warm admirer of the writings of Mr. Miller, because he is sound in doctrine, simple and graphic in style, and practical in his aim. In proof of this, read the first chapter on "Oper-

ations," in which he vindicates surgery from the opprobrious charges which have been leveled at it, because it achieved good through blood and much suffering. Thanks to chloroform, this latter objection can be no longer urged with reason; whilst the former is terrible only to the child and the highly nervous. Within the last quarter of a century the knife has been resorted to only in extreme cases; the bloody bistoury and reeking knife have been superceded by the more mild and persuasive means so profusely scattered throughout the materia medica. The progressive spirit of the age has been conspicuously shown in the surgical department of our profession. *First*, in a better knowlege of the mechanical means requisite to correct the deformities of the physical system. *Second*, in a more thorough acquaintance with the diagnosis and treatment of disease.

Surgery, in the strict sense of the term, has been narrowed down to a mere appendage of the healing art; and Dr. Physick long since remarked that the merely *operative* part of surgery in any one of our large cities would not support one medical man in a respectable style. If this was true 20 years since, it must throw a damper upon the enthusiasm of those who aspire at the present day for fame as operative surgeons.

Mr. Miller's work is progressive in tone and matter; it teaches the science of surgery as well as the art. He is conservative, because he is enlightened on the general principles of medicine, and is therefore the best guide that the general practitioner can follow. It is, in a word, the best work of the day on the principles and practice of surgery. We would advise all who wish to understand the "*healing art*," to secure a copy of this truly useful work.

J. B. Steel, 60 Camp street, has copies for sale. A. H.

IV.—*A Treatise on the Venereal Disease.* By John Hunter, F.R.S. With copious additions, by Dr. Philip Ricord, Surgeon of the Hospital *du Midi*, Paris, &c., edited with notes by Freeman J. Bumpstead, M. D., Physician to the North Western Dispensary, New York.

Ricord and Hunter! names which stand in such bold relief before the Medical world, and whose writings have enriched the pages of many a book, whether the chosen theme has been the fluids or the solids of our *vile compound* of "*mud and blood*," whether in a disquisi-

tion upon the blood alone, or in treatises, or most *ravishing* letters upon syphilis, need not the aid of some humble plea to point out their excellencies, if at times, (since to err is human) a vein of *gentle* satire might pass upon their foibles. The present treatise on venereal disease is by John Hunter, with copious additions by Ricord; the notes are by F. I. Bumpstead, M.D., of New York.

There are some important points, upon which the two names which we have noted just now with exclamation, have widely differed: and first upon Gonorrhœa, we quote from Hunter.

"It sometimes happens that the parts which become irritated first, get well, while another part of the same surface receives the irritation, which continues the disease, as happens when it shifts from the glands to the urethra."

From this circumstance of all Gonorrhœas ceasing without medical help, I should doubt very much the possibility of a person getting a fresh Gonorrhœa while he has that disease, or of his increasing the same by the application of fresh matter of its own kind.

From all this it is reasonable to suppose that such a surface of an animal body is not capable of being irritated by its own matter; nor is it capable of being irritated beyond a certain time; and, therefore, if fresh venereal matter were continued to be applied to the urethra of a man having a Gonorrhœa, that it would go off just as soon as if no such application had been made, and get as soon well as if great pains had been taken to wash its own matter away. The same reasoning holds good in chancres.

Mr. Hunter carries the point still further, and asserts "that the parts become less susceptible of the venereal irritation; and that not only a Gonorrhœa cannot be continued by the application of either its own or fresh matter, but that a man cannot get a fresh Gonorrhœa, or a chancre, if he applies fresh venereal matter to the parts when the cure is nearly completed, and continues the application ever after, or at least at such intervals as are within the effect of habit. I can conceive that in time the parts may become so habituated to its application as to be insensible to it: for by a constant application the parts would never be allowed to forget its irritation, or rather never become unaccustomed to it, and therefore this supply of fresh matter could not affect the parts so as to renew the disease till they first recovered their original and natural state, and then would be capable of being affected again."

The above sentences embrace the opinion of Mr. Hunter on this head. With due reverence to the authority, doubt might naturally

arise as to the correctness of the doctrine; and first, is it a fact that all Gonorrhœas cease without medical help? If so, how comes it that the urethral discharge continues after the inflammation has subsided, or until the patient, worn out by distress and annoyance consequent upon the irritation produced by such discharge, seeks medical aid as a *dernier* resort? And if, as it is asserted by Mr. Hunter, "the venereal matter formed in a Gonorrhœa does not assist in keeping up that Gonorrhœa," of what avail is it that we order careful ablution for the removal of "matter" from this and other surfaces upon which it is formed? The objection is reconciled by Mr. Hunter in the following words: "that no matter of whatever kind can produce any effect on the part that forms it." Yet may we not again urge, that the *stimulus* of the discharge increases the inflammation? With the view of arresting the discharge we employ astringent agents in the abortive treatment, a decrease of inflammation being generally consequent thereon.

Opposed to the opinions of Hunter, as already stated, we have those of Ricord in these words: "The more numerous attacks of Gonorrhœa have been, the more easily the disease is contracted anew; and the succeeding affection is developed with more ease and rapidity in proportion to the traces remaining of the previous attack.

It is well known that the most common cause of relapse, especially in Gonorrhœa, is the too hasty repetition of sexual intercourse before the cure is perfect, and even before convalescence commences."

The different modes recommended at the present day, for the varieties of stricture, by their respective advocates, and the annoyance, (not to mention the more serious consequences) resulting from the malady, may, perhaps, have induced Mr. Hunter to allot many pages of his book to the discussion of this subject.

Authorities have generally agreed to assign (not to say solely) the cause of stricture, to Gonorrhœa; of course perineal injuries (which frequently give rise to the most serious strictures) must always be excepted.

Mr. Hunter, however, goes so far as to question if stricture ever arise from gonorrhœa; yet keeping in mind the fact, that any source of irritation to the uretha, if sufficiently long continued, will produce stricture, and bearing still farther in mind the fact, that a gonorrhœal discharge does produce urethral irritation, so that its walls, at certain parts, are contracted, as is evidenced in the forked stream of the urine; we have, it would seem, a refutation of the opinion of Mr. Hunter; even were we to admit, (and which circumstance does not admit of doubt,) that

the urine itself may be so changed from its normal standard as to be a cause of irritation, so much so as to produce stricture. Upon this head Mr. Babington holds the following words: "Many well authenticated facts disprove the common prejudice which attributes stricture invariably to gonorrhœa or to the use of injections. But when the author (alluding to Mr. Hunter) goes so far as to question whether it ever arises from these causes, his opinion is contradicted both by reason and by experience."

Passing over the primary and simplest manner of treating stricture, by dilatation with the sound or bougie, (and which observation and experience furnish abundant proof, is very frequently palliative rather than curative,) we recur to Mr. Hunter on the application of caustic to strictures, who, according to his annotator was not the first to employ this method of treatment; Alphonso Ferri and Ambroise Paré having employed cauterization at an earlier date.

Mr. Hunter observes, "about the year 1752, I attended a chimney-sweeper laboring under a stricture. He was the first patient I ever had under this disease; not finding that I gained any advantage, after six months' trial with the bougie, I conceived that I might be able to destroy the stricture by escharotics; and my first attempt was with red precipitate. "I applied to the end of a bougie some salve, and then dipped it into red precipitate." The mode of thus applying an escharotic, which, to effect any good, must reach the strictured part, will best account for its failure; and as might have been anticipated, Mr. Hunter continues, "I found that it brought on considerable inflammation all along the inside of the passage," The subsequent plan which he adopted, a rude porte-caustic of the present day, soon had the desired effect.

Mr. Hunter, prior to his death, improved upon his original plan of applying caustic; yet upon the whole, he seems not to have been well pleased with the result, for his concluding opinion is, that, "caustic has no advantage over bougies in respect to the permanent cure of the malady. Experience has fully proved that after a stricture has been removed by caustic, bougies are still necessary, and that unless they are occasionally passed, it is almost certain to recur."

Ricord recommends cauterization under two varieties of circumstances, which may be briefly expressed as follows: "Whenever the stricture still permits the passage of the urine, but resists the introduction of every other instrument;" and again, "when dilatation is without effect, or is too slow, or when the stricture remains stationary, after having yielded to a certain degree."

The same writer, in conclusion of this subject, remarks: "It must

be confessed that some strictures will not yield to dilatation, and are aggravated by caustic. Such are, in a large number of cases, those which depend on cicatrices, bands, callosities, inodular indurations, and fibrous degenerations. Here, however, art still offers to true practitioners resources which all speculative theories cannot gainsay; for in these refractory strictures, we may, though difficulties still confessedly remain, oppose the action of cutting instruments, of which Hunter does not speak, though they were employed before his time."

We will now take up another subject or two, not so fully treated of in the book under consideration. The intimate relation, by sympathy or otherwise, which some of the diseases of the bladder bear to the whole of the urinary apparatus; and the serious and sometimes fatal consequences which ensue from diseases of that viscus, (the bladder) might reasonably have lead the reader to hope for, and to expect something more than a summary dismissal of this branch of the author's writings; especially when we consider his marked ability, and his deservedly high reputation.

One of the most painfully distressing and annoying (not to mention those of graver character, which demand the most prompt interposition of the surgeon) diseases of the bladder, is extreme irritability, not caused by obstructions to the passage of the urine, and which in the course of time so completely exhausts the nervous system, that in a greatly emaciated state the patient succumbs.

Mr. Hunter, in the brief allusion which he has here made to this source of disease says: "the symptoms of this disease are very similar to those arising from obstructions to the passage of the urine in the urethra, but with this difference, that in the present disease the urine flows readily, because the urethra obeys the summons and relaxes; however, there is often considerable straining, after the water is all voided, arising from the muscular coat of the bladder still continuing its contractions."

Regarding irritability of the bladder most frequently to arise, (next to stone and tumors forming on the inside, in which event obstructions to the passage of the urine would almost infallibly occur,) from an altered quality of the urine, whether it be in an excess of acids or alkalies, or in a deposit of mucous with needle-shaped crystals, and knowing that the urethra will very generally take on an irritable state from the same cause, which irritation, (granting its continuance a sufficient length of time,) will produce stricture; we must again call in question the correctness of the statement of Mr. Hunter, as given at page 234, "that in the present disease the urine flows readily, because the urethra obeys

the summons and relaxes," (!) when in fact, the only *summons* to which the urethra will *readily* respond, is upon the application of a bougie!

Truly does Mr. Hunter observe in reference to stone, etc., in the bladder, as well as from irritability arising from other causes, "in such cases the straining is violent, for the cause still remains, which continues to give the stimulus of something to be expelled, and the bladder continues to contract till tired, as in the case of simple irritability, and then there is a respite for a time; but this respite is of short duration, for the urine is soon accumulated." As the best means of procuring temporary relief, when the irritability of the bladder is not dependent on stone, or other local affection, Mr. Hunter recommends opium, and to be most effectual, given in the form of clyster, or "a blister applied to the perineum, the lower part of the small of the back, or upper part of the sacrum if more convenient." To these might have been added the application of leeches to the perineum, or some sedative and anodyne ointment, as the extract of belladonna, or hyoscyamus, blended with some camphor pomade, and applied to the same part; but those who have been frequently called on to treat these painful affections, cannot but have marked the too common failure of each, and all these appliances, to accomplish any more than, as Mr. Hunter justly observes, "temporary relief."

In turning to the close of the book, we quote a curious passage or two from Mr. Hunter, without comment, under the head "of diseases supposed to be venereal, produced by transplanted teeth."

Since the operation of transplanting teeth has been practised in London, some cases have occurred in which the venereal infection has been supposed to be communicated in this way, and they have been treated accordingly; nor has the method of cure tended to weaken the suspicion; yet when all the circumstances attending them, both in the mode of catching the disease, and in the cure, when they have been treated as venereal are considered, there is something in them all which is not exactly similar to the usual appearance of the venereal disease when caught in the common way; especially, too, when it is considered that some of the cases were not treated as venereal, and yet were cured; and, therefore, the cure of the others, which appeared to be from mercury, are not clear proofs of their having been venereal." Mr. Hunter then cites several cases which came under his care, the time, in which to accomplish a cure, varied from a few weeks to about three years.

He had great difficulty in arriving at a definite opinion on this subject:

indeed, his last remarks rather withhold, than give such. "If we take some of the above cases," (alluding to those which he has given) "and consider them as they at first appeared, we shall almost pronounce them to have been venereal. If we take the others, we shall pronounce them absolutely not to be venereal. And if we consider every circumstance relating to those, probably, venereal, we shall, as far as reasoning goes, conclude that they were not venereal.

I cannot conclude without intimating that undescribed diseases, resembling the venereal, are very numerous; and that what I have said is rather to be considered as hints for others to prosecute this inquiry farther, than as a complete account of the subject."

The work before us is embellished with several plates, contains much valuable reading, and the names with which we have prefixed our comments, will be the certain indices of its sale.

It is to be found at White's, Canal street.

G. T. B.

New Orleans, November 28, 1853.

Part Fourth.

MISCELLANEOUS MEDICAL INTELLIGENCE.

I.—*Yellow or Malignant Bilious Fever, in the vicinity of South-street Wharf, Philadelphia*, 1853. *Read before the College of Physicians, Aug.* 3 *and* 7, 1853. *With an Appendix.*

BY WILSON JEWELL, M.D.

This is a most interesting pamphlet of 40 pages, extracted from the *Quarterly Summary of the Transactions of the College of Physicians.* If our limits would permit, we would insert the whole of it, as the subject is one of thrilling interest; but as it is, we can only make room for the most material parts of the narrative, omitting the fully reported cases and all comment upon the treatment. The respected author is evidently in a quandary as to whether the disease originated in Philadelphia or was introduced from Cuba, but states the *facts and attendant circumstances* so fully and candidly as to give the reader a fair opportunity to form his own opinion. This is exactly as it should be; our opinions may pass for what they are worth, but our statement of facts should be as full and authentic as possible. It takes patient and persevering labor to get up facts, and that labor is not always performed by those who undertake to give accounts of epidemics. But let us see what Dr. Jewell says about the yellow fever of Philadelphia.

"During the past month (July) our usually healthy city was thrown into a state of great excitement, from a suspicion that yellow fever, with its fearful concomitants, threatened once more, after an absence of the third of a century, to find a 'local habitation and a name' in our midst.

A careful investigation into the circumstances giving rise to this alarm, has resulted in the development of the following facts, having a direct bearing upon the origin and history of this much dreaded visitation:

On the 25th of June last, the bark Mandarin, Captain R. N. Campbell, sailed from Cienfuegos, Cuba, for this port, all in good health, with a cargo of sugar, molasses and cigars. Her crew consisted of twelve men. On arriving at the

Lazaretto, July 12, after a passage of seventeen days, she was visited by the officers at the station, and, on oath, the captain reported 'cases of small-pox and fever' at Cienfuegos when he left. That he had lost two of his crew on the passage with fever. The statement of the Lazaretto physician is, that 'the crew, numbering ten souls, were examined and proved to be of good health; notwithstanding this, it was considered prudent that the bark should be detained until thoroughly ventilated, cleansed, and fumigated; the bedding and clothing of the deceased sailors were destroyed, the vessel whitewashed and fumigated in every part with chloride of lime, the bedding of the crew aired, and their clothing washed; she was detained an entire day; and, before being allowed to proceed to the city, all on board were separately and minutely examined; all hands were on duty, and apparently free from disease. The captain spent a portion of the day on shore, and before being admitted up, declared on oath that 'all on board were in good health,' and that no sickness, except that resulting in the death of the two seamen, had occurred during the voyage."

On the evening of the 13th, the Mandarin reached the city, and came to at South-street wharf. On the 16th she was hauled up to the lower side of the first pier below Lombard-street, where she discharged her cargo. The crew having been previously discharged, the captain and mate remained by her, sleeping on board. On Wednesday morning, July 20, seven days after her arrival, she dropped down to the lower side of the first pier above Almond-street, (her several positions being designated in the accompanying diagram,) where she remained until the 26th, when she was removed by order of tha Board of Health to the cove below the Navy Yard, from whence, on the 28th, she was remanded to the Lazaretto, in order to undergo a more rigid and thorough purification.

There was no development of disease of a malignant type in the vicinity of where this vessel lay, as far as has been ascertained (and the strictest inquiry has been instituted by Dr. Gilbert, the Port Physician, to whose politeness we are indebted for many of the facts here recorded,) either before or during the time of discharging her cargo, and it is still to be made known that any of her sailors, or any of the laborers employed in removing her cargo, have since been sick.

There appears to have been no cause for alarm until the cargo was out of the vessel, when it was noticed that a very offensive smell proceeded from her hold. After she dropped down to the pier at Almond-street wharf, on Wednesday morning, the 20th, the stench became intolerable, rspecially whenever the pumps agitated the bilge water, contained under the limber planks or flooring of the hold.

The first case of suspicious fever which occurred in the neighborhood was on Tuesday, the 19th of July, the day before the Mandarin left her position at Lombard-street for Almond-street wharf—Joseph Sharp, a young man, eighteen years of age, who drove a furniture car, and whose stand was on the upper side of South-street wharf. This case proved fatal on the 26th, seven days after the inception of the disease. He died at the corner of Almond and Swanson streets.

The next case was that of Captain George Robinson, of the British brig Effort, which vessel lay in the Lombard-street dock, next above the pier where the Mandarin discharged. Captain R. slept on board his brig, but took his meals at the Champion House, kept by Mr. Charles Koehler, near the N. W. corner of South-street and Delaware Avenue. He sickened on Wednesday, the 20th, and died at the Champion House, on Saturday, the 23d, at 3 P.M.

On the evening of the same day, the 20th, Mr. Koehler's son, aged nineteen, a ship-carpenter, who passed daily along the wharf in going to and from his business, took sick, and died on the 27th, at his father's, the Champion House, where he boarded.

G. W. Kerkeslager, who kept the Red Bank Ferry House, on the S. W. corner of South-street and the Delaware or Wharf street, was the next victim. He was taken on the afternoon of Wednesday, the 20th, and died on the 25th. His wife, Mrs. Kerkeslager, was taken on the same evening, the 20th, and died on the 26th.

Charles Burrows, the second mate of the bark Mazeppa, lying on the north side of the pier, first below Lombard-street, where the Mandarin discharged her cargo, sickened on Wednesday evening, the 20th,* was removed to the Pennsylvania Hospital on the 22d, and died the same night.

Frederick H. Kellog, mate of the Mandarin, was taken sick on Thursday, the 21st. On the 23d, was removed to the Pennsylvania Hospital, and died the following day.

The next one attacked was Fanny Martin, a maid-servant at Mr. Koehler's, Champion House. She sickened on Thursday, the 21st, and died on the evening of Tuesday, the 26th. On the same day, another son of Mr. Koehler, aged seventeen, and a daughter aged nine, both took sick of the fever, but recovered in a few days.

Honora Stanton, residing at No. 16 Little Water-street, above South, was taken sick on Friday, the 22d, and died on the 27th.

Silas Green, a laboring man, at No. 21 Little Water street, took sick on Saturday, the 23d. This man came from the country on Wednesday, where he had been for several weeks. On Thursday and Friday evenings he had visited the avenue, and sat for an hour or more on the wharf at Almond-street, where the Mandarin lay. He was not sensible of any bad smell. Recovered.

Capt. R. N. Campbell, of the Mandarin, took sick on Friday, the 23d, and was removed to Mr. Clement's Hotel, Delaware Avenue, between South and Lombard streets. Recovered.

On the 25th, John Shellcott, steward, and John White, mate of the brig Effort, already alluded to, were taken sick on board that vessel, lying at Lombard-street dock. On the 27th, they were removed to the Pennsylvania Hospital, where John Shellcott died on the 30th; but John White recovered.

Captain David Murray, of the brig Reform, lying on the north side of the pier above Almond-street wharf, where the Mandarin lay last (see diagram,) was taken sick on the 26th, and on the 29th entered the Pennsylvania Hospital. On the 1st inst. he was doing well. Recovered.

James Markley, another of the crew of the Effort, was taken sick on the 27th, removed to the hospital on the 29th, and August 1st was reported doing well. Recovered.

A man about fifty years of age died on the night of the 27th of July, at No. 8 Little Water street, said to have been with yellow fever; but, upon strict inquiry, there is not a doubt but that his death was from *typhoid* fever, caused by intemperance and exposure.

Between the 19th and the 27th of the month (July,) there were other cases of sickness in the the neighborhood of South-street and the Avenue, but those above enumerated are all the deaths that are known to have taken place, and all the cases of disease, as far as could be ascertained that bore any resemblance to fever of a malignant grade.

The following are the pathognomonic symptoms exhibited in several of the case of yellow fever and furnished to Dr. Gilbert and the writer, by the physicians in attendance.

Joseph Sharp, aged 18, sickened 19th, died 26th July. Fever without re-

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* The account given by Burrows, at the hospital, was that he sickened on the 17th, but the above date was procured by Dr. Gilbert, at his boarding-house, and is no doubt correct.

mission until the end of third day; then prostration, without reaction; sallow skin and eyes.

Captain Geo. Robinson, of brig Effort, taken with fever 20th; died 23d. Fever without intermission for thirty-six hours, then prostration without reaction; black vomit, hemorrhage from mucous surfaces, and bronzed color of skin.

G. W. Kerkeslager, sickened 20th, died 25. Excruciating pain in loins; fever without remission for three days, when pulse became natural and sank; black vomit, and sallowness of conjunctiva.

Wife of last case sickened two hours after, and died twenty hours after her husband. Symptoms the same, except black vomit.

I. D. Koehler, aged 19, sickened 20th, died 27th. Pain in loins; no remission of fever until end of third day; yellow skin, black vomit, hemorrhage from mucous membranes.

Fanny Martin at Koehler's sickened 22d, died 26th. Symptoms same as last case, except the yellow skin.

J. M. G. Koehler, aged 17, taken 21st; and Pauline, daughter, aged 9, taken same evening. Febrile excitement of the same character as the others; abatement on third day, followed by reaction and recovery.

Case of Honora Stanton. Could not discover any symptoms of yellow fever in the account of this case as given by the husband, family and neighbors, nor did her body after death present any symptoms favorable to such a view. The certificate of her physician' Dr. Gegan, said *bilious fever*, although the doctor insisted upon it that she had the black vomit.

Silas Green, taken on the 23d. Fever without intermission for several days, with vomiting of bilious matter, and considerable gastric distress; intense pain in the head, back and limbs; fever abated on the 26th, and recovery was rapid.

Captain R. N. Campbell was attacked on the 23d. Symptoms were accompanied by headache; fever without remission for several days; nausea, without vomiting; was convaslescent on the 27th.

We are not advised that the treatment in the above cases differed particularly from the usual method pursued in our ordinary bilious fevers, unless by the early introduction of decided doses of quinia in several of them.

Of the case treated in the hospital, Dr. Gerhard has furnished the following very brief but emphatic report:

"*Dear Doctor:* I inclose you a list of cases of *yellow fever* admitted into the hospital; two of them were from vessels from Cuba, the others were from the English brig Effort, lying near the Mandarin, and one from the Reform, lying in the same neighborhood.

Two of the fatal cases came into the hospital dying; one had black vomit, the other had the same black secretion contained in his stomach. The steward of the Effort had abundant black vomit in the last twelve hours of his life. These cases have all been genuine *yellow fever*, bearing no resemblance to any other variety of febrile diseases.

The examination after death, in the three cases, gave us nothing but the usual lesions; that is, the peculiar yellowish hue of the liver, coupled, generally, with a little gastritis and a slight enlargement of the spleen.

All of them had a yellowish hue, and one was exceedingly jaundiced. The treatment we adopted was quinia in large doses, together with cupping in some cases.

Striking out the two cases that came in dying, we have lost one out of four."

Before the arrival of the Mandarin, and up to the 19th of July, the day on which the first case of fever occurred, the vicinity of South-street and the wharf, as well as the entire city, enjoyed its usual degree of health. Certain it is, that no epidemic was prevalent. For the week ending July 9, the deaths

in the city and liberties were 229, and only one death from fever of a bilious type. For the week ending July 16, there were 206 deaths, including one from intermittent fever. For the week ending July 16, there were 206 deaths, including one from intermittent fever. For the week ending July 23, there were 218 deaths, of which four were from fever. Three of these were in children; one was recorded fever, one bilious, one congestive, and one remittent.

As yet, no one with whom we have conversed has ventured to intimate a doubt as to the agency of some uncommon and virulent poison, diffused through the atmosphere, as the pestilential cause of the malignant or yellow fever, a few cases of which have made their appearance recently in the vicinity of South-street wharf.

The essential character and origin of this poison may afford an opportunity for the speculative inquirer to indulge anew, either in an effort to demolish some favorite, though long established, yet not the less false theory of the origin of malaria; or to build upon the ruins of theories, once accredited as ingenious and popular, some more modern system of causation, which, as we advance in medical science, may be destined to meet the fate of those which have preceded it, however elaborately and industriously they may have been exemplified and sustained.

But, while we leave the discussion of this subject to others, it will not, we conceive, be questioned, when all the facts are clearly and minutely examined into, that this poison, whatever may be the nature of its character, must in part be ascribed to a morbid effluvia generated under the limber planks, in the hold of the bark Mandarin, from the putrescent state of her bilge-water.

Upon the first glance at the Manderin, and the history of her voyage previous to her arrival at Philadelphia, the advocates for a contagious germ for yellow fever, or, in other words, a principle emanating from the sick, and capable of being conveyed from one person to another, as the focus for the fever which has threatened our city, may imagine they have discovered another instance in support of their favorite theory. This, however, we are persuaded can hardly be the case, although we are desirous that a careful review of the facts connected with this ill-fated vessel should speak for itself.

The Mandarin left Cienfuegos on the 25th of June, with a healthy crew. No *epidemic* was prevailing there when she sailed, although the captain, on oath, admits that a "few cases of small-pox and fever" did exist. He states that his crew lived on board the bark while in port, anchored off the town, were seldom on shore, and as far as his knowledge extended, none of them had visited among the sick.

The captain admits that the hold of his vessel had often been in a foul condition, as all vessels were that carried cargoes of sugar and molasses. Eight days out from Cienfuegos, July 3, one of the seaman sickened with fever, and died on the 7th. On the ninth day at sea, July 4, another took sick with fever, and died on the 9th, being the fifth day of his illness. This last man was thrown overboard after the vessel was within the Capes of the Delaware.

On the 12th of July, seventeen days from the time of leaving Cienfuegos, she hoisted a whiff on approaching the Lazaretto, for a visit from the doctor. She remained at the station one day, to undergo a certain amount of cleansing, as a precautionary measure only, there being at the time no sickness on board.

On the 13th, she was permitted up, and at Lombard street wharf proceeded to unload. The seaman were discharged, and, up to this date, we are yet to learn of any one of the eight having had any sickness; nor can we ascertain that any of those who worked on board, during the time of her discharging cargo, have either died or sickened.

Not a case of fever, supposed to have had its origin from the malaria arising out of the foul condition of the bilge-water of the Mandarin, occurred

uutil the 19th of July, six days after her arrival, and not subsequently to the cargo being discharged.

All the cases of fever known to have taken place up to this date, August 3, seventeen in number, and in the vicinity of the Mandarin, occurred between the 19th and 27th of July. Of these, eleven, nearly two-thirds died.

The poison appeared to be most active between the 19th and 22d. During this interval, twelve cases were reported, and of these ten died.

Of the seventeen cases recorded, we have no direct evidence of the existence of black vomit but in eight, and all of these perished.

On the 20th of July, the Mandarin was warped down to Almond-street wharf, below South-street, about three hundred yards from her berth at Lombard-street.

All the cases that have occurred up to this date, either resided in, or did business in the vicinity of South-street wharf, nor is it known that any case originated below, or south of where the vessel had been last moored, nor has any case come within our knowledge north of Lombard-street.

The prevailing winds during the week ending the 27th, varied from north-north-east to south-east and south, while the average gauge of the thermometer was $79\frac{1}{2}^{\circ}$.

From all the facts above recited, we may be warranted in drawing the following conclusions:

1. That no disease of a malignant type was prevailing in our city previous to the arrival of the Mandarin.

2. That none of the seamen discharged from the Mandarin have sickened.

3. That none of the laborers employed in unloading the Mandarin have taken the disease.

4. That the fever did not develop itself until after the cargo was discharged; when, it is believed, the noxious emanations which had been latent in the hold, under the limbers of the vessel, had an opportunity (acted upon by certain exciting causes, as heat and moisture,) to disseminate itself, and, coming into contact with other elements of decomposition existing on shore and in the docks, soon poisoned the atmosphore of the immediate neighborhood where the bark lay moored.

5. That in no instance can the disease be traced to any individual, except among those who either visited or resided in the immediate vicinity of South and Lombard-street wharves.

6. In no case has the disease been communicated to any persons visiting, or engaged in attendance upon the sick.

7. Up to this period, not a single instance can be met with, having its origin to the south of where the Mandarin lay last.

[*Read September* 7, 1853.]

Since our communication of August 3, read before the College, on the subject of yellow fever in this city, we have come into possession of some additional particulars, and a number of new cases that are well authenticated, and beg leave to submit them to your consideration.

On the 26th of July, as already stated in our first communication, the bark Mandarin, by order of the Board of Health, was taken in charge by the health officer, Wm. McGlensey, Esq., who put on board of her a crew of five men, including the watchman. These men remained on board from the 26th up to the 29th of July, when she was safely anchored inside the island of Little Tinicum, opposite the Lazaretto, and placed in charge of the quarantine master. Neither the health officer nor any of his men, nor did the watchman who remained on board until after her purification and returned to the city, experience an hour's sickness.

While at the Lazaretto, where she remained from the 29th of July to the 2d of August, several of her limber planks were removed, but no mud or

other collection of filth was found concealed there. She was then scuttled and filled with water, which remained in her twenty-four hours. The holes were then plugged up, and the water pumped out. After this operation, she was thoroughly whitewashed, and in every part of her chloride of lime was freely distributed. During this process, the quarantine master, with several of the barge-men at the station, were on board and at work in the hold, cabin, and other parts of the vessel for several hours during each day—the weather excessively hot, without any inconvenience to their health.

On her return to the city, the 3d of August, she was moored at Noble street wharf, Northern Liberties, where she remained until near the close of the month; then was removed to the screw dock, Kensington, for repairs; and, on the 6th of the present month, was taking in cargo between Arch and Race streets, preparatory to a voyage for New Orleans.

No sickness whatever has followed in her track, since she left Almond street wharf, on the 26th of last July.

The population within the district where the disease first appeared, and where it seems to have been almost altogether confined, is by no means crowded. Many of the houses, however, were filthy in the extreme, with dirty yards, and full and foul privies.

The docks along the Delaware line, between Lombard and Almond streets, as usual, contain large accumulations of offensive mud and other filth.

Upon the authority of a highly respectable shipwright, who, in his official capacity, very carefully examined the Mandarin, we learn that her pumps are so constructed as to render it impossible to remove all the water in her hold. Being a tight vessel, the bilge-water remaining in her will smell in a very few days after pumping her clean. His language is, 'If you draw those pumps every five minutes in the day, there must remain in her twelve inches of water. My opinion is, that the pumps do not go down low enough; they do not go down within twelve inches of the keel, whereas they ought to be at least eight inches lower.'

We present this fact in evidence of the impure state of the hold of the Mandarin, six days after she was pumped clean at the Lazaretto.

In addition, however, to the prevalence of the morbific atmosphere which we have clearly shown developed itself on board the Mandarin—but not until her cargo was discharged—and which so sensibly affected individuals on approaching her when she lay at Almond street wharf, we must not for a moment conceal the existing causes in the immediate vicinity of South-street wharf, sufficient to justify the supposition of their agency, in the development of disease of a malignant type, when subjected to a high thermometrical influence which prevailed throughout the months of June and July. Not the least mischievous of these causes in the production of an unhealthy atmosphere, was the outlet of the sewer into the dock at South-street ferry, belching forth continually putrid masses of animal aud vegetable filth, accumulating around its mouth, and exposed at low water to the rays of the sun, exhaling streams of unwholesome and poisonous gases into the surrounding air. Besides this agent, there was a most foul wharf at the upper side of South-street; a filthy avenue, between Lombard and South streets, without any properly constructed surface drainage; numerous damp and confined cellars, subject to an overflow by the ebbing and flowing of the tide-water of the Delaware; and various minor causes that might properly be added to the above category, fruitful in the production of atmospherical changes injurious to health.

In summing up, however, there is one prominent feature in the chain of our narrative that must not be passed by without notice, viz:

No yellow fever existed in our city until six days after the arrival of the Mandarin; that it broke out immediately abreast of the wharf where she first hauled to, and, although there were existing causes in the vicinity—on shore—for the production of disease, there were 'plague spots' in other parts of our city, remote from South street wharf, where, had the question been asked, we

should have unhesitatingly located the first appearance of fever of a malignant type, independent of the suspected existence of a foreign focus of infection, competent to exercise its morbific influence on an atmosphere already tainted.

In presenting the above, it is proper to say that we are influenced solely by a desire to arrive at the true cause for the origin of the yellow fever in our city. That we are no blind adherent to any favorite theory for the spontaneous or domestic origin of yellow fever, nor yet an uncompromising opponent of those who advocate the doctrine of a contagious principle, capable of being carried about from place to place, and, under a train of favorable circumstances, productive of disease. An honest inquirer after truth, we would disguise no fact, that might tend in any way to elucidate a question so intricate, that for years it has been controverted by the ablest pens in our profession, and yet remains a mystery.

In our first account of this disease, we narrated briefly the history of seventeen cases, bringing them down to July 27, and gave the pathognomonic symptoms of several of them as far as they could be obtained. Since then, we have collected twenty-seven additional cases, that may be relied upon, and presenting in all respects, the evidences of genuine yellow fever.

The accompanying table embraces the entire list, in their numerical order, since July 19, including the name, age, sex, place of birth, where the disease was contracted, the date of attack, where attended, by whom, the duration of the disease, those that were accompanied with black vomit, those that died, and those that recovered.

From this table we learn that 9 of the cases were under 20 years of age; 18 between 20 and 30; 10 between 30 and 40; and 7 over 40.

Twenty five were males and nineteen females. Of the whole number, twenty were born in Ireland, four in Germany, five in England, and fifteen in the United States.

Thirty-seven of these cases may be traced directly to the infected district. In four, the origin is doubtful, and in three, no clue could be had as to where the disease was contracted.

Of the forty-four cases, thirty-four died. The mean duration of the disease in those who died was four days; not including, however, Dr. Jackson's case, No. 23, and that of James Genton, No. 30. The former was protracted until the fourteenth, and the latter to the twelfth day.

In twenty-six of the forty-four cases recorded, there occurred the peculiar dark-colored 'coffee-grounds' ejection from the stomach, known as black vomit.* This substance, when placed within the field of the microscope, exhibited the true blood-corpuscles, denoting its sanguineous character, with the exception of that in Ellen Parr's case, No. 35, examined by Professor Gilbert, whose testimony disproves its identity with blood, and consequently throws a doubt on the genuineness of the case. This patient recovered, and was one of the two instances of recovery where the black vomit was said to have occurred.

In eleven of the thirty-four deaths a *post-mortem* was made; in all of which the yellow or ochre-colored liver was detected in whole or in part. In all of them the 'coffee-grounds' fluid, or melænic blood, was found, either in the stomach or intestines, with other evidences of a pernicious form of fever.

The mortality, according to the table, has been fearful; eqnal to 80 per cent., or 5 to one on recoveries.

APPENDIX.

In addition to the 44 cases of yellow fever enumerated in the preceding articles, as read before the College of Physicians, we have, after considerable labor, gathered the statistics of 126 more; bringing the report up to October 7, 1853, the date of the occurrence of the last case. Thus making in all 170

* In the seventh case, there was no black vomit before death, but it was found in the stomach at the *post-mortem*.

cases of yellow fever in Philadelphia from July 19 to October 17, inclusive, a period of 80 days.

Of these 128 died ; equivalent to 1 in every 1.42 hundreths, or 75 per cent.

One hundred and forty-seven of all the cases may be traced to a direct intercourse with the infected locality. Twenty-two are of doubtful or unknown origin ; and one was contracted at the Lazaretto, while at work on board the ship Caledonia Brander, from New Orleans.*

One hundred and eighteen were treated in private practice,twenty-four at the Pennsylvania Hospital, eighteen at the Blockley Hospital, seven at the Bush Hill or City Hospital, and three at St. Joseph's.

Ninety, equal to 53 per cent of the cases, were accompanied with black vomit; all of which died, with the exception of four. The genuineness of the discharge in one of these four cases has been doubted, as stated before. As the discharge of black vomit from the stomach of a yellow fever patient has generally been considered a fatal symptom, we should always be inclined to question the character of the matter vomited where the patients recover, who are said to have had black vomit, no matter how high the authority from which they emanate, unless it had first undergone a careful examination under the microscope, and then found to contain blood-corpuscles.

The greatest number of cases that occurred in any one day was ten; this happened on the 11th and also on the 19th of September.

The duration of the cases that proved fatal was from two to twenty-four days; the mean period was a fraction less than six days.

Ninety-three of all the cases that occurred were among males, and seventy-seven in females. That period of life which appeared most susceptible of the fever was between 20 and 30; one-third of all the cases happened in this decade.

Out of the whole number of cases recorded, 68 were born in the United States, 62 in Ireland, 19 in Germany, 18 in England, 1 in Scotland, 1 in France and 1 in Spain.

In no instance have we learned that the fever has been communicated to others by those laboring under the disease. At the Pennsylvania Hospital the yellow fever cases were placed in the wards promiscuously, with other patients, without any attempt to prevent intercourse between them; and not an individual, either among the patients, nurses or visitors, contracted the disease. The same immunity was observed at the Blockley, St. Joseph's and City Hospitals. In private practice, although many of the cases were treated away from the infected locality, where they contracted the fever, we have not been able to hear, after the strictest inquiry, that the disease spread itself in any case beyond the individuals infected, although there was unrestrained intercourse between them and their friends. An evidence of the non-contagiousness of yellow fever, or in other words, that the poison which produces yellow fever is incapable of being generated in the body of the sick, so as to produce a similar disease in those attending upon them.

We have been unable to calculate the proportion of cases, or even of deaths, to the population, from yellow fever, in the infected district. The information required to make an accurate estimate not being reliable, all we can offer on this subject is, that the population involved was by no means a crowded one. The number of resident inhabitants in that locality, where the first 19 cases happened, bounded by the south side of South street, the west side of Little Water street, Lombard street, and the Delaware front, did not exceed 100. The remaining portion of the infected district to which the fever seemed to be confined, and to which we have ascribed limits, extending from Union street on the north to Queen street on the south, Second street on the west, and the Delaware front on the east, did not by any means contain an excess of population.

* This vessel had yellow fever on board when she arrived.

The inference therefore is, not only, as we have already shown, that the deaths to cases have been large, but that the cases to the population, as well as the deaths to the population residing within the above limits, have been proportionately large.

While the fever in general has been confined to the limits above named, it is evident that a few cases of genuine yellow fever have appeared beyond these boundaries, between which and the infected locality no direct communication could be traced. Showing either that isolated cases of malignant fever may occur in our city when the mean of the thermometer is above 76°, or that an epidemic influence was extending its treacherous march to remote and varied sections of our metropolis.

We must omit a beautiful map of the infected district and a comprehensive table of the cases. F.

The New Orleans Medical and Surgical Journal.

VOL. X.] NEW-ORLEANS, JANUARY 1, 1854. [No. 4.

TO THE SUBSCRIBERS OF THE JOURNAL.

The undersigned most willingly completes the unfinished labors of his lamented friend, Dr. Hester, in bringing out this number of the Journal. He regrets the delay in its appearance that has occurred; but begs leave to assure the subscribers that on account of numerous and pressing engagements, it could not be avoided. In regard to the continuance of the Journal, he is authorised to say there will be no interruption to its progress, and its business affairs will be conducted in the same manner as heretofore. It is not yet determined who is to be its future permanent Editor, but, until finally disposed of, the subscribers may rest assured that it will appear regularly at its appointed times under the direction of the undersigned. Those wishing to remit money may do so as usual, and new subscribers may forward their names and payments in advance, which will be properly attended to.

The undersigned would most respectfully remind the many subscribers who are still in arrears with their payments, that they cannot in a better manner testify their regard for the late editor, who struggled so perseveringly to maintain this Journal, than by promptly remitting the amounts they owe, for the benefit of *her* who is left desolate by his untimely death. Communications may be addressed as usual to the Editor of the New Orleans Medical and Surgical Journal.

E. D. FENNER.

DEATH OF DR. HESTER.

Died, of Epidemic Cholera, on the morning of the 1st December, 1853, Dr. ABNER HESTER, aged about 40 years, late Editor and sole Proprietor of this Journal.

In the course of a somewhat eventful and checkered life, the writer has but seldom performed so melancholy a task as that which now devolves upon him. When the ties of friendship which have been knit for years by close and confidential intimacy are severed *during life*, it never fails to cause a pang in the sensitive bosom; yet, if there be no violation of *honor* in the breach, there ever lingers a hope that it may be healed at some time; but when those ties are severed by *death* and the grave closes upon the dearest objects of affection, how sad and gloomy is the void that is left and how melancholy the workings of the mind ever clinging to the memories of departed worth!

The eloquent divine who attended the funeral of Dr. Hester made something

like the following touching remarks: "My friends, I have come here this evening, not alone in my official capacity, to perform the last sad service to a fellow mortal, but *to bury* a *friend*—one whom I have known since his boyhood—who was my school fellow, and gained my affectionate regard at a period of life when the affections are warmest and most pure." Our own acquaintance with the deceased does not extend so far back, but we may say that for the last twelve years he has been our most intimate friend and companion. We have struggled together in adversity and enjoyed together such pleasures as we were able to find along the rugged walks of life—we have worked together in the great cause of medical science and literature, which enchained alike our ambition and our energies, and we both had the gratification to see our efforts crowned with some degree of success. Now he comes to an untimely end—snatched away at the meridian of life and the full development of his fine mental powers. By the force of his talents and indomitable energy, unaided by wealthy friends, whose influence his proud spirit scorned to court, he made himself one of the first physicians of New Orleans, and if he had lived ten years longer, would have amassed a splendid fortune.

As none of Dr. Hester's relatives reside in this city, we are indebted to some of his old acquaintance for the following memoranda of his early life. We learn from one of his most intimate friends, who has known him from his boyhood that he was born in Mecklenburg county, Virginia, but that his parents emigrated to the West when he was but a child, and settled themselves in Montgomery county, Tennessee, near the Kentucky line, where he was brought up on a farm. His parents were of that unostentatious but independent class of farmers constituting the best portion of the population in the Western States, whose chief ambition is to educate and elevate their children above the sphere in which they themselves have moved. Indeed this is the class from which the most talented and energetic men of our country have sprung. Born to independence and equality in point of moral position with all around them, yet sufficiently cramped by the *res angustæ domi* to protect them from the enervating influences of luxury and indolence, they soon feel the soul-stirring impulses of pride and ambition, and even with limited education, are sent forth into the world armed with an energy and firmness of purpose calculated to overcome whatever obstacles they may meet in their future careers.

At an early age Dr. Hester displayed talents and ambition which induced his parents to bestow what assistance they intended to give him, in the way of a *good education.* At about sixteen years of age he was sent to Cumberland College, situated in Princeton, Kentucky; an institution which at that time had an able faculty and was in a flourishing condition. It was under the special direction of the Cumberland Presbyterians, and was conducted on the manual labor principle; that is, the time of the student was occupied partly in mental application and partly in laboring on the farm. The Rev. Wm. A. Scott, now one of the most distinguished Ministers of the Gospel in this city was at college with Dr. Hester, and knew him intimately. Dr. Scott says he was a good student, very generally esteemed, and prided himself on being well prepared for his recitations. After graduating at this college he went into the

office of Dr. Walter H. Drane of Clarksville, Tennessee, for the purpose of studying medicine. Dr. Drane at that time commanded a large practice and was considered one of the ablest physicians in the State. After going through the usual preliminary course of reading, Dr. Hester went on to Philadelphia, where he attended two courses of lectures in the University of Pennsylvania, and graduated in the spring of 1837. On returning home, he remained but a short time before concluding to come further South, and selected the flourishing village of Holly Springs, in North Mississippi, as a place of residence. Here he practised his profession with much credit to himself until the fall of 1839, when, finding the field too small for his ambition, he resolved to leave it and plunge at once into the great emporium of the South-West, New Orleans. His fortunes were now desperate, having spent the last of his patrimony in acquiring his profession and as yet made but little by the practice. It was certainly a bold step for a young man without money or acquaintance to come and settle himself in this extravagant city. Inspired, however, with confidence in his own abilities, he boldly trusted to fortune for an opportunity to display them, though his daily bread depended on the cast. The difficulties he encountered were perhaps even greater than he had anticipated; yet by close attention to his meagerly furnished office and the observance of the most rigid economy, he managed to keep himself afloat. His genteel outward appearance and ever cheerful countenance doubtless led many to suppose he was living in affluence, whilst he was actually denied the comforts of life.

We came to New Orleans on a visit in the winter of 1840—41, and then made the acquaintance of the deceased. In the winter of 1841—42 we settled ourself in the city, and since that time have been intimately associated with him. We found him oppressed with poverty, but full of energy and hope. Being in somewhat similar circumstances myself at that time, a fellow-feeling generated a mutual attachment between us, which was only disturbed by the intervention of death.

In 1843 Dr. Hester was so much discouraged by his slow progress in practice that he thought seriously of again changing his residence and seeking his fortune in a foreign land. He was persuaded by a sea-captain whom he happened to attend professionally, to go to Merida in Central America, and had actually packed up his clothes ready for starting, and waited only for the captain to call for him. He was disappointed—the captain did not call, and thus he was forced to renew his efforts for a living in New Orleans. If he had left here at that time his subsequent career would probably have been altogether different.

In January, 1844, Dr. Hester and myself, both then in rather desperate circumstances, embarked in an enterprize which has proven to be of no trifling importance to the Medical Profession throughout the Southern States; we allude to the establishment of this Journal. In the first volume of my *Southern Medical Reports*, published in 1850, we drew up a brief sketch of the origin and progress of this Journal which, we trust, will not be deemed inappropriate to the present occasion. It is as follows:

"This work was commenced by its present editor, and the editor of these

reports in 1844, and is the oldest of the Southern journals now in operation. Happening to be thrown together in the city of New Orleans and finding our fortunes alike desperate, "a fellow-feeling" gave rise to an intimacy between us which it is hoped will endure through life. Without money, with but few acquaintances, and dependent on a precarious practice which barely afforded the most economical support, we determined to project the hazardous adventure of a Southern medical journal and trust to the liberality of the medical profession for its support. The field was ample, rich, and entirely unoccupied, but it was difficult to see how the experiment could succeed without having *one cent of capital* to start on. We actually had the Prospectus printed *on a credit*, one of our booksellers being willing to go that far at all hazards, and we paid the bill, *eleven dollars*, out of the first spare money we had. The Prospectus being out and distributed throughout the country, we were fairly committed to bring out the work, but as yet, could find no person willing to undertake the publication. All we had to give was our own labor, which was cheerfully offered, but something more substantial was required. We appealed to the booksellers, to the proprietors of the city newspapers, and finally, to the Medical College and leading physicians of the city, for a guaranty of five hundred dollars, but all to no purpose. The enterprize was *conceived in poverty*, and *poverty brought it forth!* At this stage of our gestation, we had the good luck to come across a poor *French printer*, who had *a handful of type and nothing to do.* Him we persuaded, by means of *flattering promises*, to bring out the first number; and thus the New Orleans Medical Journal saw the light! Each number made out to pay its own way, but left no surplus on hand. In this manner we struggled through the first volume and were entering upon the second with prospects somewhat improved, when an unexpected rival appeared in the field. The Professors of the Louisiana Medical College issued a prospectus announcing the early appearance of a new Medical Journal from their school. An union was effected between the two, and the late Professors Harrison and Carpenter joined us in the publication of the New Orleans Medical and Surgical Journal. In 1848 we voluntarily withdrew from the Journal, and in less than two years, Drs. Carpenter and Harrison were removed by the hand of death, leaving the present worthy editor "alone in his glory." And nobly has he devoted himself to the discharge of the heavy duties devolving upon him. It affords us much pleasure to say, that this Journal is now in a very prosperous condition, having a list of subscribers numbering about one thousand, and constantly increasing. It has given a decided impetus to the cultivation of medical science in the South, thus fulfilling the highest object of its original founders. Its circulation is confined chiefly to the Southern States, though some of its contributions, particularly thoso of Dr. Bennet Dowler, have attracted much attention both at the North and in Europe. We trust this historical notice will not be unacceptable to the reader, as it adds another to the numerous instances on record, of the triumph of energy and perseverance over obstacles apparently insurmountable. We heartily wish the editor and his work all manner of success."

From that time the Journal has continued to progress with brightening prospects until it has become one of the most repectable in the Union, and if well conducted in future, will doubtless prove a lasting monument to Dr. Hester's memory.

But let us notice some other events in his life. In the spring of 1846 war was declared between the United States and Mexico, and under a pressing emergency, General Taylor called upon Louisiana for three Regiments of Volunteers. The call was promptly responded to, and Dr. Hester was appointed Surgeon to one of these Regiments. From that time we may date the

commencement of his rise as a practitioner in this city. For seven years he had persevered in his efforts to get into practice, but had only succeeded so far as to make a bare support. His face was familiar to hundreds of his fellow-citizens, but few of them had ventured to employ him. With a proud aspect of independence he walked the streets of New Orleans, relying upon his merit alone and scorning to stoop to any means of getting practice that were at all derogatory to the dignity of the Profession. When he went into the army he was brought in contact with a regiment of his fellow-citizens who were forced to discover his merits as a physician, and they were astonished to find them of a superior order. His regiment was only in service about three months, but it was a trying time for new recruits who were suddenly removed from the comforts and luxuries of city life to the hardships of the camp. There was a good deal of sickness amongst them, but scarcely a man was lost. When he returned to the city he resumed the practice of his profession under much brighter auspices. He had now a *regiment of friends* around him who had formed a high estimate of his professional abilities and were disposed to do all they could to promote his advancement. The result was that he soon got into a lucrative practice which he ever afterwards sustained with signal ability. Soon after his return from Mexico he was elected Secretary to the Board of Health, with a salary of six hundred dollars per annum, which was of immense advantage to him by keeping his name constantly before the people in the publications of the Board. In 1847 there was a great epidemic of yellow fever, in which Dr. Hester did an extensive and lucrative practice. He then became really independent and triumphed at last over all the obstacles which had so long opposed his progress. He was not able to support a horse and carriage till 1846—indeed, he could hardly afford to ride in the omnibus or car previous to that time; consequently he had to practice almost entirely on foot.

In 1848 Dr. Hester was appointed by Governor Johnson a member of the Board of Medical Examiners for the Eastern District of Louisiana, and continued in that office until the Board was abolished in 1852.

He was appointed Visiting Physician to the Charity Hospital upon numerous occasions, and served as Visiting Surgeon during one term.

On the re-organization of the Board of Health last summer he was elected Port Physician, which office he held till his death—in fact, the office expired the very day he died.

He was one of the founders of the Medico-Chirurgical Society, of which the late Dr. Luzenberg was President, and which was suffered to expire with him. He was a member of the Physico-Medical Society when he died, and his memory has been duly honored in that Society by the passage of the complimentary resolutions that follow.

At the annual meeting of the Louisiana State Medical Society held in March last, Dr. Hester was elected one of the Vice Presidents, and held that office at the time of his death.

Such is a brief summary of the offices and honors conferred upon Dr. Hes-

ter, all of which were deservedly bestowed and faithfully discharged. As Editor of this Journal from its foundation, he deserves much credit for his persevering efforts to sustain it through all its difficulties, and they have been by no means inconsiderable. By his urbanity and conciliating disposition he ingratiated himself into the favor of his subscribers, most of whom display a deep interest in the success of the Journal.

Dr. Hester's sudden and unexpected death caused quite a panic in the city. He was seen practising his profession in different parts of the city till past six o'clock in the evening; by three o'clock the same night he was a corpse, and on the following morning his friends were invited to attend his funeral. The consternation thus produced by the death of one so generally known and esteemed may readily be conceived. A number of cases of Cholera had already occurred and caused considerable alarm in the city; but the death of Dr. Hester at the St. Charles Hotel created a shock which was felt far and wide. About two weeks previous to this attack he had one which would undoubtedly have taken him off, but for the prompt and efficient medical aid he obtained. From that time he looked badly, as if he had not fully recovered, but it seemed even that serious warning could not put him properly on his guard. He knew he had a predisposition to bowel-complaint which always endangered his life when Cholera was in the atmosphere, yet he could not be prevailed on to be prudent or use proper precaution against this insidious disease. In the last number of this Journal he predicted the re-appearance of Cholera in this city before a great while, little thinking perhaps that it was quite so near, and that he himself would be numbered amongst its victims. If he had used the precautions which he recommended to others, we cannot doubt that he would be with us still; but there really seems to be something in the nature of this disease calculated to deceive the sufferer and lead him on under a false sense of security into the very jaws of death. We see this not only amongst the gnorant, but often with the most intelligent. But it is vain now to recount the irretrievable errors of the past, unless it may serve to warn some future sufferer of the danger of trifling with this insidious foe. No fatal disease gives such fair warning of its approaches as Cholera, and there is none more manageable in its early stages, yet we see thousands upon thousands falling beneath its deadly grasp.

It remains to say a few words respecting the professional abilities of the deceased, before closing this imperfect sketch. It will be conceded by all who knew him that he was a physician of rare skill and judgment, of ample resources, bold and prompt in action, and untiring in his attention to the sick. But few physicians in this city command the confidence of their employers to a greater extent than Dr. Hester did. He was ever willing to consult with his honorable brethren when desired, but most of his patients were content to trust their lives to his skill and judgment. He was remarkable for his fine personal appearance and the urbanity of his manners. He had the happy faculty of ingratiating himself with his acquaintance and gaining their affection. But few men had warmer or more devoted friends than he, as was evinced by the large and respectable cortege that attended his funeral. We see in his life an illus-

tration of the triumph of talent and perseverence over great obstacles. He came here a stranger oppressed with poverty; he died possessed of a very handsome competence, beloved and regretted by a large circle of admiring friends.

A few short months ago he led to the altar a beautiful bride, upon whom he had concentrated the whole force of his affections; he leaves her independent, it is true; but naught to compensate his own irreparable loss. By his death the Medical Profession has lost one of its most useful members. For ourself, we must be permitted to say we have lost an intimate friend and companion whose place we may scarcely expect ever to have filled.

Vale! longum, longum vale! E. D. F.

We insert some of the notices of Dr. Hester's death that appeared in the daily papers. There were equally complimentary notices in the other city journals, which are not now at hand.

From the N. O. Evening Delta, December 1.

Death of Dr. Hester.—We regret exceedingly to announce the death this morning of Dr. Hester, editor of the Medical and Surgical Journal, and the New Orleans Port Physician. He was a man of high and deserved reputation in his profession, an experienced and skilful practitioner, and a writer of much ability and acuteness. It is somewhat strange that he was the first to announce the approach of the disease to which he has fallen a victim. His prophecy (the "sunset of life gave him mystical lore," though his sun has set early) can be found in the last number of his periodical. His premature death is a source of regret to all who knew his estimable character, and a serious loss to the medical profession of this city. We have only space to announce the melancholy fact and to wish his soul the peace of the righteous.

From the N. O. Daily Crescent, Dec. 2.

Dr. A. Hester.—The death of this gentleman at an early hour yesterday was not less mournful than unexpected to his numerous acquaintances and friends. There were few abler members of his profession than the subject of this notice, and none whose death would have been more feelingly regretted. Dr. Hester was Physician of the Port and Editor of the New Orleans Medical and Surgical Journal. He was an able and polished writer, and in his profession few of his age had attained greater skill. Doing a large practice, and enjoying the confidence of the community as a successful practitioner, his loss will be deeply felt. To the members of his family we tender our sympathies in their unspeakable bereavement.

From the Daily Picayune, Dec. 2.

The remains of the late Dr. Hester were yesterday followed to the grave by a large concourse of mourners. His sudden death, in the full maturity of his powers, when the promises of future usefulness were so bright, has caused a deep sensation among the medical profession and with the public. An ardent devotee of science, and a man of study as well as of observation, he held a high rank among his brethren, and was much esteemed by the public. It is but a few months since he married a charming lady of this city, for whose bereavement, in this overwhelming affliction, the expressions of sympathy are universal.

The annual supper of the Medical Society, which was appointed to take place on Saturday evening next, will, in consequence of Dr. Hester's death, be postponed to some later period, to be selected hereafter.

The following notice was taken of his death in the Physico-Medical Society:

Anniversary of the Physico-Medical Society of New Orleans.

This Society held its annual meeting in the Medical College on Saturday evening, December 3d, the President, Dr. Axson, in the chair. After listening to an interesting address from Dr. C. R. Nutt, the orator for the occasion, the following gentlemen were elected officers of the Society for the ensuing year:

President, Dr. R. Bein. Vice Presidents, Drs. P. B. McKelvey and S. Choppin. Corresponding Secretary, Dr. T. S. Clapp. Recording Secretary, Dr. D. McGibbon. Treasurer, Dr. G. T. Browning. Curator, Dr. Boyer. Orator, Dr. A. Mercier.

The customary anniversary festival of the Society was indefinitely postponed on account of the recent death of Dr. Hester, and the following preamble and resolutions adopted unanimously:

Whereas, it has pleased Almighty God, in his inscrutable providence, to remove from amongst us in the person of the late Dr. Abner Hester, one of the most talented, honorable and useful members of the medical profession in this city; and

Whereas, the Physico-Medical Society recognizes in the sudden and unexpected death of Dr. Hester, the loss of one of its worthiest members, and a gentleman whose labors in behalf of medical science and literature have been productive of most beneficial results, and entitle his memory to be cherished with respect aud esteem by the medical profession of this country in all time to come; therefore,

Resolved, as the sense of the Physico-Medical Society, that the untimely death of Dr. Hester is deeply regretted by every one of its members, as a loss not only to us, but to the whole community in which he lived.

Resolved, That the sympathies of this Society be communicated to his bereaved wife and distant relatives, through the medium of this preamble and resolutions, to be forwarded to them by the Corresponding Secretary.

A true copy from the records.

D. Macgibbon, M. D., Recording Sec'y.

HEALTH OF THE CITY.

In the last number of this Journal Dr. Hester announced the subsidence of the great epidemic which had so severely scourged our unfortunate city, but at the same time predicted the reappearance of Epidemic Cholera at an early day. This prediction has been fulfilled, and is rendered more impressive from he fact that the Prophet has fallen a victim to the anticipated scourge. From our last date, Yellow Fever continued steadily to decline until it has now so nearly disappeared that we only occasionally meet with an isolated case. On the 19th of December the writer discharged his last case of this disease at the

Charity Hospital—a young man who came over from Ireland about the middle of October and went to work on the levee, fourteen miles above the city. He was there seized with a violent fever, and on the 11th December, the 5th day of sickness, was brought to the Hospital. He was then turning yellow and had slight hemorrhage from the nose and gums. He soon convalesced and was discharged cured. We know of no case in the Hospital or any other place at this time.

The re-appearance of Cholera in our city, after it was thought to have vanished entirely, caused much alarm and has seriously injured the business of the season. The injury, however, would not have been so great, but for the exaggerated reports that went abroad in all directions. We will briefly review some of the prominent facts in connection with the late Cholera at this place.

On the 21st of October, in one of our wards at the Charity Hospital, we found a case of Cholera in complete *collapse*. He had arrived from St. Louis the day previous, and had suffered from diarrhœa several days. On the 22d October we found another case in the same state. This man had just come down the river from Evansville, Ind. Both said they had heard of no Cholera where they had been.

On the 29th of October the newspapers contained notices of the introduction of a considerable amount of Cholera amongst the European immigrants arriving at the Quarantine Station, New York, but nothing of the kind was observed at this port. Still it created uneasiness and we expected to have the disease introduced in a short time. Before this took place, however, Dr. Hester himself was the subject of a well-marked attack on the 12th of November. There may have been other cases about this time. On the 19th of November the following announcement of the approach of Cholera by sea appeared in the *Picayune* newspaper:

The Cholera.—A despatch from the South West Pass, dated yesterday at 2 P. M., announces the arrival below of the British ship Blanche, from Liverpool, with four hundred passengers. She has lost nine passengers during the voyage, by cholera, and one was buried yesterday after her arrival.

This need not, however, create any alarm. The Board of Health are providently supplied with the means now at Forts Jackson and St. Philip for receiving and taking care of at least six hundred persons; so that if due diligence be observed, and we have every ground to believe that no effort will be wanting, the cholera imported into the Balize will be no nearer New Orleans than it was at Liverpool. Agents have been despatched by the Board of Health to see to this matter at once.

On the 20th we found a case in complete *collapse* at the Charity Hospital. The man said he had been in the city *fourteen days* and was last from the interior of Texas. The same day Dr. Hester, Port Physician, examined a vessel just arrived from Europe, which had lost nine emigrants with Cholera on the voyage, but had no sickness at that time. This was probably the Blanche, which anchored off Slaughter-house Point on the opposite side of the river. On the 21st, there were several cases at the Charity Hospital and the Touro Infirmary. At this time and for a week or two previous the weather was warm and wet, and the streets so dirty as to attract comment in the newspapers. On

the morning of the 22d we found that twelve cases of Cholera had been admitted into the Charity Hospital within the previous 24 hours, and cases were heard of all over the city excepting the 4th District. From this time the disease increased so rapidly that the weekly report of the Board of Health of the 27th November contained 129 deaths from Cholera. On the evening of the 30th, Dr. Hester was attacked a second time and died in less than nine hours after he took his bed. This melancholy event caused quite a panic in the city. The next weekly report from the cemeteries contained 214 deaths from Cholera. On the 8th December there was a sudden change of weather—turned very cold and windy. This had a marked effect on the progress of Cholera. The next weekly report showed a diminution of deaths by Cholera of nearly one half—*one hundred and eleven*. This was on the 11th December. The report of the 18th showed only 37 deaths from Cholera—and that of the 25th only *five;* three of which occurred at the Charity Hospital.

Such is the history of this brief and limited epidemic. It must have sprung up spontaneously amongst us, for surely no one will contend from the facts presented, that the disease was imported from Europe this time, whatever may have been the fact in December, 1848. We have heard of two deaths on the river near Princeton, some 500 miles above here, but none anywhere else. How long we shall continue to be occasionally visited by this scourge remains to be seen.

It is somewhat remarkable that for six years past we have had a warm and wet spell of weather about the last of November or first of December, which has invariably been attended by an outbreak of Cholera, and a change of weather has invariably been the signal for its disappearance. The reader may determine for himself what relation these two facts bear towards each other. Although Epidemic Cholera may not arise from *local causes alone*, there is no fact better established by observation in different parts of the world than that *this disease appears first, rages worst and prevails longest* in localities otherwise unhealthy, and the filthiest portions of those cities, towns and neighborhoods which happen to lie across its pathway in its mysterious progress over the world.

The city of New Orleans is again without a Board of Health, the last having expired by limitation on the 1st of December; so, there is at present no special organization or police department to which is entrusted the important business of removing the many local causes of disease that exist here or making any sort of effort to improve the health of the city. If this state of things continues, we are doomed to suffer the dreadful consequences which invariably await *neglect of duty;* but how awful the responsibility that rests upon those who have charge of the city government! If the best sanitary measures that could be advised were at once put in force, it would probably require five or six years to develop its beneficial effects; but to this we must come at last, if we are ever to be delivered from pestilence; and therefore, the sooner we begin the good work, the better. We need not enlarge upon this topic, as our remarks will be read by but few of our fellow-cifizens. What we have said we consider due to the medical history of the time.

Our city is remarkably healthy at this time.

CITY MORTALITY,

For the 10 *weeks ending Jan.* 1*st*, 1854.

		Total.	*Yel. Fever.*	*Cholera.*	*Other Dis.*	*Not Stat.*
October	30	193	26	——	148	19
November	6	79	9	——	56	14
"	13	115	5	——	95	15
"	20	110	8	——	86	16
"	27	261	3	129	102	27
December	4	372	——	214	141	17
"	11	255	——	111	127	17
"	18	153	——	36	107	10
"	25	*91	1	5	68	17
1854.						
January	1	118	——	9	102	7
		1747	52	504	1031	159

MORTALITY FOR THE YEAR 1853.

It will appear from the reports published in this Journal, which are not precisely correct, that the total mortality of the city for the year has been 15,085.

From Yellow Fever, 7,899

From Cholera, 607

This is the greatest mortality that ever occurred in New Orleans in a single year. May we never witness the like again!

* Not quite full.

REPORT OF THE HOWARD ASSOCIATION.

The transactions of this celebrated Association in the most memorable epidemic that ever visited our ill-fated city, makes a neat pamphlet of 28 pages, three fourths of which are taken up with the names of the contributors from all parts of the Union. It presents the most magnificent display of active benevolence, not only in dollars and cents, but also gratuitous labor for the benefit of the afflicted by fever perhaps ever witnessed. Contributions came in from cities, towns, villages, neighborhoods and individuals in nearly every quarter of the Union, and in sums varying from above *fifty-eight thousand dollars*, down to *one dollar*—the latter, perhaps, like the *widow's mite*, displaying as much *true charity* as the former. Our city being the first afflicted by the pestilence, many cities and towns in this region promptly forwarded most liberal contributions, though doomed too soon to require a helping hand themselves. By the time the pestilence reached them it had abated here, and the Howards flew to their relief with experienced physicians, nurses and money.

The following extract from their report gives a summary of their receipts, disbursements and labors.

"It will be seen by reference to the above statements that the total receipts of our treasury since the 14th July, on which day we regularly organized for action in view of the impending epidemic, have amounted to $228,927 46; and that the sum expended from the same date is $159,190 32, which, together with an approximated estimate of $3500 for outstanding debts, is $162,690 32; leaving a balance this day of $66,237 14; of which balance $30,000 is invested in mortgage on real estate, bearing 8 per cent interest per annum, and 30,000 specially deposited in bank, subject to draft after 15 days notice, and bearing 4 per cent interest per annum.

The total number of cases of yellow fever attended to in this city, as per returns of the members, is 11,088, to which we might add several thousands of cases in the surrounding country, where the offices of the association have been extended, and where, in many instances, in addition to the means forwarded, our members have attended in person.

It will also be borne in mind that we have given special relief in this city to the indigent, whose sufferings, either directly or indirectly, grew out of the fell pestilence that stalked among us, and thus, whilst our expenditures may seem large at the first glance, they will appear moderate when compared with the number to whom we have afforded comfort during their sickness—sustenance in their convalescence. The stranger, the poor, the widow and the orphan have all been cared for. In practice, the association ever since its formation in 1837, has adopted as its motto the noble sentiment, '*I am a man—whatever concerns mankind concerns me!*"'

It is with painful regret that we feel impelled by a sense of duty to our profession to point out *one dark blank* that mars this otherwise most interesting document—it contains not one word of thanks, not a single expression of gratitude to the generous, charitable and self-sacrificing physicians who worked day and night and followed wherever the Howards pointed out the abodes of sickness and distress—not even an expression of regret for those noble fellows who died in their service without any expectation of pecuniary reward—who labored as hard as they did, and in far greater danger, for these were mostly unacclimated. When the Howards proclaimed the presence of the Epidemic and their readiness for action, they at the same time called on the medical profession for aid in their benevolent services to the poor. This call was responded to by about *forty physicians;* of whom some four or five fell victims to the pestilence. They surely were worthy of a passing notice in the report of the Association. Now, it is well known that the medical profession does more gratuitous service for the afflicted than any vocation in the world, but the people of this goodly city seem to think it is not only our duty to attend the poor *gratis*, but actually make us pay a tax for the privilege. There is injustice in this, and it ought to be corrected.

We highly appreciate the noble benevolence of the Howards, but they should not forget their worthy coadjurors in the glorious work of doing good.

HISTORY OF THE GREAT EPIDEMIC.

The undersigned has prepared a full and minute history of the late Epidemic Yellow Fever of this city, which is now in press and will be ready for delivery about the 18th instant. Part of it will appear in neat pamphlet form and part in muslin binding. Dr. Hester, before death, had ordered a sufficient quantity of this work to be added as an *Extra* to the present number of the Journal, but, unfortunately, it cannot be gotten out in time. The writer happened to have in one of his wards the first case of black vomit that occurred at the Charity Hospital, and commenced at that time noting facts and circumstances with the view to making this history as minute and authentic as possible. The work will be about 75 pages, and will be furnished on the following terms:

Pamphlet, single copy, - - -	50 cents.
" 100 copies, - - -	one third less.
Bound in muslin, single copy, - -	$1
" " 100 copies, - -	one third less.

Address E. D. FENNER, M. D.,
No. 5 Carondelet street, New Orleans.

RESPECT FOR THE DEAD.

We most cheerfully insert the following communication, displaying as it does proper respect for departed worth:

At a meeting of Physicians of Port Gibson, Claiborne county, Miss., Nov. 24th, 1853,

It was resolved, that the members of this meeting wear the usual badge of mourning for thirty days as a mark of respect for the memory of our late friend and brother, Dr. D. D. Irwin, who died of yellow fever in this town, September 26th, 1853; and that this resolution be published in the American Journal of Medical Sciences, Philadelphia, and the Medical and Surgical Journal, New Orleans.

Signed, R. W. HARPER, President.
W. McN. RUSSELL, Secretary.

DEATH FROM CHLOROFORM.

A young female, whose great toe was about to be amputated by one of the visiting surgeons of the Charity Hospital, suddenly expired while under the influence of chloroform. It was some time before she could be brought fully under the effects of the anæsthetic; she finally, however, became completely insensible, and before the operation was concluded, she sank and rapidly expired in spite of the most strenuous and judicious efforts of several medical men present. The usual precautions were used in its administration, and no censure can justly be attached to the surgeon or his assistants for the untimely result of the case.

A post-mortem was made by the Professor of Physiology in the University of Louisiana, and all the organs were found to be perfectly healthy. A. H.

DR. J. MARION SIMS.

It is with much pleasure we learn that this talented and skilful surgeon has completely regained his health and settled himself permanently in the city of New York, where he has established an *Institution for diseases and injuries incidental to Parturition.* We congratulate the great Metropolis upon this acquisition to her able medical faculty, and sincerely hope she may duly appreciate his superior professional abilities and his real worth as an exemplary citizen. Dr. Sims' brilliant surgical operations in the city of Montgomery, Alabama, where he has resided for the last ten or twelve years, and his writings in the leading Medical Journal of this country have gained him a high and well-merited reputation. Driven from his beloved Southern home by the unfriendliness of the climate, we trust he may find at the North not only a larger field for the display of his talents, but also a clime that will prolong his usefulness for many years to come. He has devoted special attention to those injuries and accidents which too often befall the unfortunate *parturient female*, such as Vesico-vaginal Fistula, Rupture of Perineum, etc.—injuries which entail upon woman incessant pain, the most loathsome inconvenience, and have hitherto almost defied the skill of the profession. The great improvements he has made in the treatment of these diseases require only to be more generally known to ensure him a world-wide fame. In the central position he has now taken, we really think he may render almost as valuable services to the medical profession as to the unfortunate females who may come under his care, for the most of us do but little more than palliate the sufferings which he by his ingenious appliances very often permanently cures. We therefore think it the duty of the profession to send all such patients to Dr. Sims, who will certainly give them the best treatment that has as yet been devised. We are pleased to see he has republished his interesting paper "*on the treatment of Vesico-vaginal Fistula,*" which first appeared in the *American Journal of the Medical Sciences*, for January, 1852. His present residence is at No. 79 Madison Avenue, New York, where he may be addressed or applied to at any time.

LORD PALMERSTON ON FASTING AND PRAYER.

We take the following admirable letter with its editorial preface from one of our city newspapers, the *Orleanean.* Its religion and philosophy too are worthy the great statesman who dictated it.

Lord Palmerston, although having no identification with our citizens, and remotely situate from our latitude, would appear, notwithstanding, to have our people, our authorities and our city in his mind's eye, as clearly as though he had resided amongst us, when replying to the Presbytery of Edinburgh, who addressed him a letter, inquiring whether the British government intended to appoint a national fast on account of the prevalence of cholera. We annex it:

WHITEHALL, Oct. 19, 1853.

Sir—I am directed by Viscount Palmerston to acknowledge the receipt of your letter of the 15th instant, requesting on behalf of the Presbytery of Edinburgh to be informed whether it is proposed to appoint a day of national fast on account of the visitation of the cholera, and to state that there can be no doubt that manifestations of humble resignation to the Divine will and sincere acknowledgment of human unworthiness are never more appropriate than when it has pleased Providence to afflict mankind with some severe visitation; but it does not appear to Lord Palmerston that a national fast would be suitable to the circumstances of the present moment.

The Maker of the universe has established certain laws of nature for the planet in which we live, and the weal and woe of mankind depends upon the observance or neglect of those laws. One of those laws connects health with the absence of those gaseous exhalations which proceed from over crowded human beings, or from decomposing substances, whether animal or vegetable; and these same laws render sickness the almost inevitable consequence of exposure to those noxious influences. But it has at the same time pleased Providence to place it within the power of man to make such arrangements as will prevent or disperse such exhalations, so as to render them harmless; and it is the duty of man to attend to those laws of nature, and to exert the faculties which Providence has thus given to man for his welfare.

The recent visitation of cholera, which has for the moment been mercifully checked, is an awful warning given to the people of this realm, that they have too much neglected their duty in this respect, and that those persons with whom it rested to purify towns and cities, and to prevent and remove the causes of diseases, have not been sufficiently active in regard to such matters.

Lord Palmerston would therefore suggest, that the best course which the people of this country can pursue, to deserve that the further progress of the cholera should be stayed, will be to employ the interval that will elapse between the present time and the beginning of the next spring, in planning and executing measures by which those portions of their towns and cities which are inhabited by the poorest classes, and which, from the nature of things most need purification and improvement, may be freed from those causes and sources of contagion which, if allowed to remain, will infallibly breed pestilence, and be fruitful in death, in spite of all the prayers and fastings of an united but inactive nation. When man has done his utmost for his own safety, then is the time to invoke the blessing of heaven to give effect to his exertions.
I am, sir, your obedient servant, HENRY FITZROY.

To Rev. W. H. Gray, Moderator, Edinburgh Presbytery.

ABSTRACT OF A METEOROLOGICAL JOURNAL FOR 1853.

BY D. T. LILLIE & Co., at the City of New Orleans.

Latitude, 29 deg. 57 min.; Longitude, 90 deg. 07 min. West of Greenwich

WEEKLY. 1853.	THERMOMETER.			BAROMETER.			COURSE OF THE WIND.	FORCE OF THE WIND, Ratio 1 to 10.	Number of Rainy Days	Quantity of RAIN — Inches.
	Max.	Min.	Range.	Max.	Min.	Range.				
Oct, 27	74 .00	49 .00	25 .00	30 .30	29 .80	50. 0	E.	2 .¾	4	1.645
Nov. 5	79 .00	52 .00	27 .00	30 .40	30 .05	35. 0	SE.	2 .¼	0	0.000
" 12	77 .00	48 .00	29 .00	30 .30	30 .10	20. 0	SW.	2.	2	0.035
" 19	80 .00	54 .00	26 .00	30 .27	30 .10	17. 0	SE.	1 .¾	0	0.000
" 26	79. 00	59 .00	20 .00	30 .23	30 .10	13. 0	E.	2. ¼	2	0.450
Dec 3	72 .00	52 .00	20 00	30 .20	30 .05	15. 0	NW.	3.	3	6·560
" 10	74 .00	40 .00	34 .00	30 .30	30 .00	30. 0	SW.	2 .¾	0	0 000
" 17	69 .00	50 .00	19 .00	30 .10	29 .70	40 .0	NNW.	3 .¼	2	2 620
" 24	64 .00	33. 00	31 .00	30 .27	30 .00	27 .0	E.	3 .½	5	5.930
" 31	66. 00	39 .00	27 .00	30 .15	29 .70	45 .0	NE.	2 .¾	3	0.055

The Thermometer used for these observations is a self-registering one, placed in a fair exposure. Regular hours of observation: 8 A. M., 2 P. M., and 8 P. M.

REPORT OF THE CHARITY HOSPITAL,

(NEW-ORLEANS,)

For October, November and December, 1853.

	SEX.	OCTOBER.	NOVEM.	DECEM.
ADMISSIONS -	Males	691	720	809
Do. - -	Females	179	203	190
		870	923	999
DISCHARGES -	Males	506	514	677
Do. - -	Females	159	162	144
		665	676	721
DEATHS - -	Males	165	126	130
Do. - -	Females	30	32	41
		195	158	170
BIRTHS - -		0		
Do. - -		0		
STILL-BORN -		0		

ANNUAL REPORT FOR 1853.

Total Admissions,	Males,	10,126	
	Females,	3,633	—13,759
Discharges,	Males,	7,971	
	Females,	2,762	—10,733
Deaths,	Males,	2,426	
	Females,	738	— 3,164
Remaining January 1s, 1854,		508	
Births during the year,		159	

REMARKS—It will be seen that the number of patients admitted into this Hospital is smaller than usual. This proceeds from the fact that the Hospital was greatly relieved by the Howard Association, which established four Infirmaries during the great epidemic. Four others were established by the Board of Health.

THE

NEW-ORLEANS
MEDICAL AND SURGICAL
JOURNAL.

MARCH, 1854.

EDITED BY

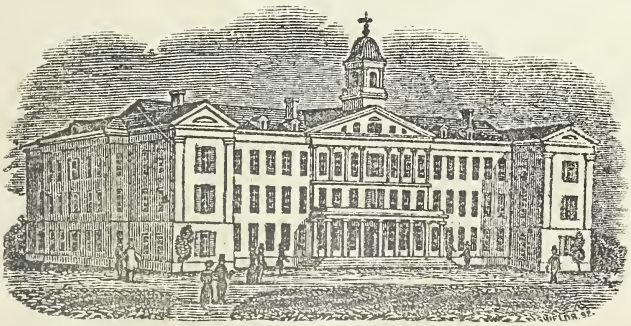
BENNET DOWLER, M. D.,

Corresponding Member of the Academy of Natural Sciences of Philadelphia; Fellow and Honorary Vice President of the Medico-Chirurgical College of the same city; Fellow of the Medical Society of Virginia; Corresp. Member of the Society of Statistical Medicine of New York; Fellow and a Founder of the Royal Society of Northern Antiquaries of Copenhagen, &c., &c.

EDITOR'S OFFICE : NO. 30 CAMP STREET;

Residence : Tchoupitoulas Street, between Louisa & Poyfarre Streets.

SUMMUM BONUM MEDICINÆ, SANITAS.—*Galen.*



N. O. Charity Hospital.

NEW-ORLEANS:
PRINTED BY JOSEPH COHN, 12 ST. PETER STREET.
1854.

TO READERS AND CORRESPONDENTS.

Our correspondents will place us under obligations, by communicating to the Journal the sanitary condition of their respective regions of country; and describe the cause, symptoms and treatment of any endemic and epidemic disease that may come under their observation. Short and practical papers are preferred.

Correspondents who may desire their papers to appear in the Journal, should forward them to the Editor at least *one month* previous to publication.

All communications should be addressed to the Editor of the New Orleans Medical and Surgical Journal, and they will receive prompt attention.

Papers for publication have been received from Dr. W. Taylor of Talladega, Ala.; Dr. M. Troy of Cahaba, Ala.; Dr. S. S. Grier of Adams County, Miss., and Dr. J. S. Davis of Salem, Miss.

Since our last we have received the following books, pamphlets, etc., for review:

A Treatise on Diseases of the Eye. By W. Lawrence, F. R. S. A new edition by Isaac Hays, M. D., Surgeon to Well's Hospital. Philadelphia, Blanchard & Lea, 1854. (From the publishers, T. L. White.)

The Medical Formulary. By B. Ellis, M. D., etc.. 10th edition, revised by R. P. Thomas, M. D., Prof. Mat. Med., etc. Blanchard & Lea. (From the publishers.)

Dental Chemistry. By A. S. Piggott, M. D., etc. Lindsay & Blakiston, Philadelphia. (From the publishers.)

A Text Book of Anatomy, and Guide in Dissections. By Washington R. Handy, M. D., etc. Lindsay & Blakiston, Philadelphia, 1854. (From the publishers.)

On the Use and Abuse of Alcoholic Liquors in Health and Disease. By W. B. Carpenter, M. D. Philadelphia, Blanchard & Lea, 1853. (Prize Essay.) (From the publishers.)

An Inquiry into the Nature of Typhoidal Fevers, based upon a consideration of their history and pathology. By Henry F. Campbell, M. D. Philadelphia, 1853. (From the author.)

Professional Letters from Europe, written during the summer of 1852. By P.

.F Eve, M. D., Professor of Surgery in the Nashville University. (From the author.)

A Case Book, to be used at the bed-side. By G. F. Cooper, M. D., of Savannah, Geo. (This is a well-arranged Blank Book, very convenient for keeping Notes of interesting cases. Every physician should have one. *Ed.*)

We have received all our exchanges regularly, with the addition of the American Medical Monthly. New York: Edited by Edward H. Parker, and conducted by Horace Green and six other physicians. (A very neat journal.)

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THE NEW-ORLEANS

MEDICAL AND SURGICAL JOURNAL.

MARCH, 1854.

Part First.

ORIGINAL COMMUNICATIONS.

I.—THE EPIDEMIC YELLOW FEVER OF MOBILE IN 1853.

Communicated to the Sanitary Commission of New Orleans.

BY J. C. NOTT, M. D.

GENTLEMEN :

I herein transmit to you such facts as I have been able to collect, bearing on the Epidemic Yellow Fever which prevailed in and around the city of Mobile during the summer and autumn of 1853.

The disease this season has pursued such an unusual course, as to bring under discussion again the long neglected idea of *contagion*, which I, in common with most members of the profession, had regarded as obsolete. However it may be explained, the fact is none the less certain, that the disease has extended not only to all the little settlements within five or six miles of the city, but to Citronelle, the present terminus of the Ohio Railroad, thirty-three miles from town ; and to the various towns on the rivers tributary to our Bay as far as steamboats have gone and no farther—to Montgomery and Demopolis, for example, to say nothing of many intermediate points.

The first cases of Yellow Fever which occurred in Mobile, it is conceded on all hands, were imported from New Orleans on board the barque *Miltiades*; and for the following facts I am indebted to Doctor Walkly and Mr. Cox, one of our most respectable stevedores—Doctor Walkly's information was derived from the captain of the barque and the second mate of the steamer Daniel Pratt, which acted as lighter to her.

The Miltiades sailed from Portland, Maine, to New Orleans, where she lost several of her crew with Yellow Fever; from thence she came to Mobile Bay and anchored below Dog River Bar, some fifteen or twenty miles below town, on the 11th July; and on the 13th Peter Johnson, one of the crew, was sent to our Marine Hospital in the back part of the city, one mile from the wharves, where he died with black vomit. Dr. Lopez, surgeon of this hospital, informs me that this man entered on the 11th instead of the 13th, in *articulo mortis*, and that he had been sick at sea five days with Yellow Fever.

On the 14th, three days after the arrival of the vessel, the stevedores went on board to load her with cotton for Liverpool. One of them, John Johnson, was taken down with Yellow Fever on the 19th or 20th, and was brought to town on the steamer Daniel Pratt, and placed in the "Sailor's Home," where he died with black vomit on the 25th. On the 25th four others were brought up from the vessel sick by the same steamer. One was taken to No. 9 Government street; one to Franklin street, below Eslava and another went to the hospital.

On the 1st of August the second engineer of the Daniel Pratt was taken down with the same disease and recovered. Dr. Levert saw a stevedore, David Nichols, with Yellow Fever, from the same vessel, on the 27th July.

These, as far as I can learn, include all the cases from this vessel. There were, however, other imported cases, preceding the appearance of the disease among our citizens, as the following facts will show; and these, like the former, cannot be questioned.

On the record of our "*City* Hospital" the following entries are made of Yellow Fever cases: July 23d one; 25th two; 26th three—all of whom were laborers that had fled from the Epidemic in New Orleans, and were either sick on arrival or taken soon after. It may be worthy of remark *en passant*, that I was informed by the Sisters of Charity that the disease did not spread among the inmates of this hospital until some time after, when it had become epidemic throughout the city.

After diligent inquiry among the physicians, the first case I can trace

among our citizens who had no communication with the Miltiades, was Mr. McDowell, a patient of Dr. Levert; he slept at Hollywood, a watering place on the opposite side of the Bay, and came to town every day on the steamer Junior; he sickened on the 31st of July, and recovered.

A few days after this, rumor was busily at work, and cases were talked of in distant parts of the town, having no connection with each other. On the 18th I made a memorandum in my notebook, to the effect, that up to that date, from the best information, there had been in the town about thirty cases. I inquired among the physicians as to their dates and localities and could trace no connection amoug the cases; they seem to have been sown broad-cast over a mile square. I kept, as is my custom, the range of the thermometer, the winds and rain, from the 1st of May until frost, and could see nothing in the season to account for disease. May, June and July were temperate, showery, pleasant and remarkably exempt from all febrile diseases. Nor was there any thing in the type of diseases to foreshadow Yellow Fever. Yet I predicted, a month before its appearance, with great confidence, that we should have a terrible epidemic in Mobile, and simply from the fact that I had never known the disease early in the season to attack Vera Cruz, West India Islands and New Orleans, without completing the circuit of the Gulf. I expected unusual virulence, because this had been its character every where it had gone, and I shall be greatly deceived if the same disease does not attack cities on the Atlantic next season, and particularly Philadelphia. The germ is sleeping, but not dead.

It should be remarked that our corporate authorities had shown unusual activity in cleansing our city, and long before the appearance of the disease every thing had been done which foresight and prudence could do, to ward off the scourge.

The foregoing statement includes, as far as I know, all the essential facts connected with the late epidemic in the city. I now propose to give what information I have gathered relative to its extension from this point to others around the city and along the rivers.

"*Spring Hill*" is part of a sandy, pine hill region, west of Mobile, 150 feet above tide water, and six miles distant from the wharves of the city; it has been a summer retreat for many years, is watered by excellent springs, and heretofore has been considered exempt from Yellow Fever, or any form of malarious disease. This settlement covers about three fourths of a mile square, with the virgin pine forest still

standing, and includes about thirty families, together with St. Joseph's College, which contains about 200 resident pupils. The epidemic commenced its ravages at Spring Hill about the 5th of September, and we shall give the history of its progress.

On the 12th of August, just about the time the Yellow Fever began to assume the epidemic form in Mobile, and one month after the first imported case, I was called to see a young gentleman, Mr. Alfred Murray, with a well marked attack of the disease, at a boarding house in Mobile, on St. Louis street, near St. Joseph, and on the 14th had him removed on a bed to the house of his brother-in-law, Mr. Wheeler, on Spring Hill, about the centre of the settlement. He recovered, and twenty days after he entered the house, 5th September, two of Mr. Wheeler's children were attacked with the epidemic, and about two weeks after two other children were attacked; three had black vomit and two died.

On the 22d August Mr. Stramler moved his family from town to Spring Hill, and occupied the house of John B. Toulmin; on the 27th he carried out a negro woman sick with intermittent fever, who died on the 31st, under circumstances which I need not detail, but I have every reason to believe she did not have Yellow Fever.

Mr. Greer moved with his family to the same house on the 29th from town, carrying a daughter convalescing from Yellow Fever; another daughter sickened on the 8th; three of Mrs. Flemming's children, in the same house, on the 10th; and Mrs. John Greer on the next day; Mrs. Flemming on the 15th, and John H. Greer two or three days after. This house is about 300 yards north-west of Mr. Wheeler's.

My father-in-law, Col. Deas, lived on a lot about 100 yards north of the last named house, and his household, white and black, consisted of sixty persons. On the 7th September one of his negro women was attacked on an adjoining lot; on the 8th his daughter-in-law, Mrs. John Deas, and on the 9th Mrs. Brown, his daughter—each being in a different enclosure and 100 yards from each other. The disease then spread rapidly through the families of the three adjoining premises, attacking whites and blacks indiscriminately. Fifty-four were attacked out of the sixty, and in 14 days the whole tale was told—five whites, two mulattoes and one black were dead with black vomit, and the rest were convalescent. One half of the whites attacked died, and I had never in twenty-five year's practice witnessed such a scene, among a class of people well lodged, in clean, well ventilated apartments and surrounded

by every possible comfort, and this, too, on a high, barren sand hill, nearly six miles from the city.

Cases existed simultaneously at Wm. Stewart's, Mr. Wheeler's and Mr. Purvis's and Toulmin's houses, widely separated from each other; and in the latter part of September and through October, the disease visited the houses of Capt. Stein, McMillan, Rev. Mr. Knapp, Mrs. George, Dubose's, John Battle's and some others. The disease skipped about in an extraordinary manner; some houses escaped entirely, some had but one or two cases. I could see no connection between the houses or inmates to explain the order of attack. There was scarcely a fatal case among those attacked after the 16th September—not more than two or three.

The great majority of the subjects on Spring Hill had had no communication with the city for many weeks, and it is worthy of note that the disease had attacked most of the country between the Hill and town before it reached the Hill, though some neighborhoods, as the Nunnery, and around it as far as Hubbell's, escaped. As far as I can learn, the disease did not spread among the country population beyond Spring Hill, which is sparse.

Citronelle. This is the name of a village which has sprung up in the last twelve months, 33 miles from Mobile, at the present terminus of the Mobile and Ohio Rail Road. It is situated on a beautiful plateau of pine land, about 400 feet above tide water, and has been considered, like all these pine hills at the South, perfectly healthy.

The following is an extract from a letter of Dr. James S. Gaines, a a most promising and estimable young gentleman, who witnessed the facts. This letter was dated 4th October, 1853, and published in the Mobile Advertiser of the 6th:

"The local population of Citronelle is 250; adding the boarders at the hotels and different boarding houses, say 100, it will make our population about 350. This estimate of the population does not include over 100 hands in the immediate vicinity of Citronelle. I have seen and treated since the 16th of August 53 cases of Yellow Fever; 13 out of this number have died. There have been seven other deaths since this date; they were not seen by me, but from what I could learn, five out of this number were from Yellow Fever; making the total number of deaths since the 16th of August up to date, 20. That will just make an average of 1 death to 17 of the population. The first case that I was satisfied of its originating here occurred on the 11th of September, since which time there have been several clear cases

and within the last ten days the number has been increasing, some of them of a very malignant type. I have no idea that the disease could have originated here, had it not been for the frequent communication between this point and Mobile; and it is not singular that it should have done so, when we reflect that the baggage cars are almost air-tight when closed, running from Mobile to this point in two hours."

The Doctor, unhappily, did not live to tell the whole tale—he himself fell a victim to the disease soon after this date. Many more of the population died, and 16 out of 18 of the employées on the Rail Road, besides many laborers. There are no data for accurate statistics, but from what I can learn, something like a fourth or a fifth of the population along the road from Mobile to Citronelle died. According to Dr. Gaines' statement, there was just a month between the first case imported into Mobile and the first at Citronelle.

The Dog River Cotton Factory is situated south-west of Mobile about five miles, and has within its enclosure of some 20 or 30 acres about 300 operatives, including their families. The houses are built in a hollow square and form a complete village. Mr. Charles Wattleworth, one of the most efficient and intelligent officers of the establishment, under date of 21st November, writes the following reply to certain queries:

"Dear Sir—In answer to yours of yesterday, I send you the following account of the deaths and recoveries from Yellow Fever in our immediate neighborhood.

The first case we had was a man that had been to New Orleans; he was taken ill on the 18th August, (two days after his return) and died on the 22d—a man that waited on him died about the same time.

The next cases that occurred were about the 1st September; they were about six in number, and the parties had been in the habit of going frequently to town.

The first cases that appeared here among parties that had been in no way connected with the city, or with the sick, occurred on the 9th October; there were five cases on the evening of that day, and about the 13th there were five more. Other cases have occurred since that time and there are three sick at present (21st November) one of which is not expected to live.

The whole number that died with Yellow Fever up to date is twenty-three, and forty-six have recovered.

Yours, &c., Chas. Wattleworth."

What is called St. Stephen's road goes off from Mobile in a north-west direction, and is so densely populated for five miles as to present much the appearance of a continuous village. My friend, Dr. E. P Gaines, lives about four miles from town on this road, had ample opportunities for investigating the epidemic, and to him I am indebted for the following facts :

The following cases all occurred from two to four miles from town, on the St. Stephen's road, or in other words, between the Creek and Gen. Toulmin's residence. August 23d, two cases ; 24th, one ; 30th two. September 1st, one. These were all contracted in town.

The following were the first originating in the country : September 4th, one ; 7th, one ; 9th, one ; 11th, two ; 12th, two ; 16th, one—Miss Wilson, the first death with black vomit, and from this date the disease became decidedly epidemic.

Dr. Gaines thinks the disease contagious, and narrated to me some instances which are difficult to explain on any other ground. The disease extended out in this direction some ten miles, into the neighborhood of William Cleveland.

Heretofore in Mobile the colored population, except in 1819, have escaped Yellow Fever ; this year they have been as generally attacked as the whites, but with less fatality ; there have been at least 50 deaths among them this season from Yellow Fever, and the mulattoes have suffered more than blacks.

Children, who heretofore have been little liable, this year have been generally and violently attacked. No acclimation, short of an attack of Yellow Fever, has served this year as a protection ; not only many who have lived here fifteen or twenty years and passed through several epidemics untouched, but grown up natives, and even those advanced in life, have been fatally attacked. There were very few secoud attacks. I saw but one clear case.

It is remarkable that not only some neighborhoods around the town escaped, within three or four miles, but many houses in town. Mrs. McKnight, a milliner, lived in Claiborne street, between Dauphin and St. Francis, and she with eleven unacclimated girls escaped entirely. Other examples of the same kind occurred.

Elevation seemed to have no influence over it. The Battle House, a large and superb new hotel, had just been completed and occupied but a few months; it was as clean as any building could be and as well ventilated. The female Irish servants slept in the fifth story, and the males in the basement. They were nearly all attacked and about one

half died. The cleanest parts and best residences in the city suffered as much as the small buildings in filthy alleys.

On the opposite side of the Bay, while many cases occurred at isolated houses and some sixty deaths between the village and Point Clear, yet Freeman's and the Point Clear Hotels, having more than one hundred regular boarders each, escaped almost entirely, though cases were brought to them from Mobile and New Orleans.

Contagion. Under this head, according to my view, two distinct questions have been confounded, viz., the *contagiousness* and the *transportability* of a disease. A disease may not be contagious, in the proper acceptation of the term, that is, communicable from one human body to another, like small-pox; and still it does not follow that the germ or *materies morbi* may not be transported from one place to another in a vessel or baggage car, and there be propagated.

With regard to the *contagiousness* of Yellow Fever, my mind is still undecided, nor is my conviction yet complete with regard to its *transportability.* In the epidemics of Yellow Fever which I have witnessed on former occasions, 1837—'9—'42—'43—'47, the evidence seemed to be decidedly against contagion, while in 1853 the facts have been so conflicting as to leave me still in doubt, though my leaning is rather in favor of the contagiousness of this epidemic.

The reader need not be told how completely we are in the dark with regard to the laws by which epidemic diseases are propagated, to say nothing of their obscure origin. It is a common opinion that the decomposition of animal or vegetable substances may and do produce certain gaseous emanations which rise into the air, are diffused through it, and thus produce Yellow Fever; but this theory will not bear a moment's examination. If a gas, the cause of Yellow Fever must obey the laws of gases, and be very soon diffused, by changing currents of wind, all over a city, from a given point. Yellow Fever, on the contrary, is extremely erratic in its course. It prevailed this season in Mobile for more than two months as an epidemic, and attacking new houses every day in different parts of the city; houses on opposite sides of the street, or beside each other, were attacked at intervals of several weeks, and many houses escaped entirely, or had but one or two cases, in the very heart of the city. If the cause was in a gaseous form, how could it thus skip from house to house in town, and travel in the same erratic way for miles around the town? It is a curious fact that Montgomery, Demopolis and Spring Hill were attacked about the same time, viz., between the 1st and 15th of September, while Selma

and Dog River factory were not attacked till about the 8th of October. Spring Hill and Dog River factory are within five miles of Mobile, while the other points are 200. Intermediate points, like the houses in town beside each other, were attacked at irregular intervals.

The above facts would seem to disprove the idea that the cause of Yellow Fever exists as a gaseous emanation, and we must seek some more plausible hypothesis. An examination of the facts tends more to show that the cause exists in an organic form and possesses the power of propagation and progression by organic laws. The *transportability* of Yellow Fever, to say the least, rests upon much more stable support than its contagiousness, for however conflicting the minor details may be, the broad fact stands out that the disease was not only a traveling disease, but traveled to those points on the Gulf of Mexico frequented by vessels and railroads, and *no farther*. When on former years Yellow Fever visited Vera Cruz, the West India Islands and New Orleans early in the summer, it has almost invariably extended along the coast of Mississippi, Alabama and Florida. So in 1853, after it had marched from Rio to New Orleans, I predicted with certainty that it would continue its march around the Gulf; and although we had had a cool, showery, pleasant summer in Mobile, and extraordinary sanitary precautions had been taken, I advised my friends to fly, and was called an alarmist.

It has been, too, the invariable habit of Yellow Fever, when it has visited Mobile, to commence first in the city, and not to attack the surrounding country for several weeks. Why, if it depends upon an atmospheric cause, should it not attack the settlements around for five miles, as soon as the town?

It is a fact worthy of note that the Yellow Fever this season has visited every point on the Lake where the New Orleans boats have touched, while Portersville, where they did not touch, has escaped—Biloxi, Pass Christian, Pascagoula, &c., have all been attacked. At Portersville, where several hundred people were assembled, and about 150 in one enclosure, no cases occurred, though five imported cases were brought in, nursed by different persons, and two died with black vomit. These facts I have from Dr. J. W. Moore, a very intelligent gentleman who lives at Portersville, and saw every case of sickness that occurred there.

Other facts favor the transportability of the germ or *materies morbi*. It is admitted that a vessel may go from an infected to an uninfected

port, carry the *materies morbi* with her, and that persons at the latter port may go on board the infected vessel, take the disease and die with it; hundreds of examples of this kind have occurred, and the barque Miltiades, alluded to above, is a case in point. The stevedores of Mobile, as did two men from the steamer Daniel Pratt, which lay alongside of her, took Yellow Fever from her. It is by no means an unreasonable idea to suppose that the *materies morbi* may have been transmitted to the Daniel Pratt, that was carrying freight to her for some days and by her brought to the city.

It is also a fact perfectly well established, that Yellow Fever has in many instances started in an alley or other point in a city, and gradually extended itself through the whole or part of a city; this has occurred twice in my day in Mobile—1842 and 1843—each year taking several weeks to travel half across the city, and each year prevailing in different parts of the city. In '42 the disease commenced in the southern part of the city and spread over one half; and in '43 it commenced in the extreme north and covered the part of the city untouched the previous year. This fact and others lead me strongly to believe that Philadelphia will be scourged next summer, and probably other Atlantic cities.

It is notorious that Yellow Fever has repeatedly spread from a point in Philadelphia and New York. So slow has been its progress that they have fenced it in, and in some days after, discovering that the disease was progressing, they have moved the fence to keep pace with it. A very reliable old gentleman, who was a member of the Board of Health in New York in 1822 or '23, when Yellow Fever prevailed, told me that by actual calculation it travelled forty feet a day on that occasion.

If, then, the *materies morbi* of Yellow Fever can be transported in the hold of a vessel from one port to another; and if it can be propagated from a single point in a city throughout that city, why may the disease not make its point of departure an infected vessel lying at a wharf, as well as an infected alley, or other point of land? As far as reasoning goes, I confess I can see no difference, and the spread of Yellow Fever in 1793 from a vessel in Philadelphia, and numerous other examples since, would seem strongly to favor the idea that a city may, under certain unknown circumstances, receive and propagate the *materies morbi* of Yellow Fever from an infected vessel. It is true that infected vessels have often arrived in ports without communicating the disease, but the same may be said of small-pox and other strictly contagious diseases—a negative does not disprove a positive fact.

Nor can the admission of the occasional importation of Yellow Fever into New Orleans or Mobile conflict with the fact, that sporadic cases or epidemics may spring up from germs which have been long slumbering in these cities. The facts do not conflict.

A doubt was long ago started as to the indigenous origin of Yellow Fever in America. Many have contended that it is an imported African disease, and I confess that my mind is by no means free from doubt on this point. Cholera, small-pox, measles and scarlet fever are all Asiatic diseases, all imported into Europe since the Crusades, and into America since the conquest. So recent is scarlet fever in this country that Dr. Rush remarked, fifty years ago, that the disease was so rare that one physician would not be likely to see it more than once in his lifetime! It was never known as far South as the Carolinas before about 1830, and yet how common has it become. These diseases have all the habits here which they had in their original country; they lie dormant for a time and then wake up to their work of destruction; they travel from place to place in the most erratic manner, by laws impenetrable to us. Some may be transported by contagion, others not; some may be transported both by epidemic laws and by contagion. Scarle fever, for example, may break out and prevail as an epidemic without its origin being traced, or it may be transported by contagion.

Some five years ago I published an article in the New Orleans Medical and Surgical Journal, to show that the *animalcular* hypothesis explained better the erratic habits of Yellow Fever than any other, and every day's experience and reflection since have strengthened those views; but I will not here repeat them. I am fully aware of the numerous and ingenious objections which have been urged, and among others those in the recent paper of Prof. Leidy, in which he pronounces the idea "absurd."

I am not disposed to open the discussion at present, but must be permitted to say, that ingenious and philosophical as are the experiments of Prof. Leidy, they are wholly inconclusive to my mind. Prof. Agassiz, whose authority will be allowed in any scientific assembly, regards all microscopic observations heretofore made in this department as so defective, that he informs me he has not assigned the infusoria a place in his classification of the animal kingdom. Microscopic observations are yet but in their infancy, and in reaching the causes of disease it is as far behind reality as we know chemistry to be.

In reasoning from analogy, the "Insect hypothesis" of Sir Henry Holland explains best the habits of certain epidemic diseases, and it is he part of true philosophy to abandon such theories as the old malarial

one, which is in accordance with no known laws, and to explore in a direction towards which rational hypothesis points. Prof. Leidy says "none of the well known animalculæ are poisonous. At various times I have purposely swallowed large draughts of water containing myriads of *Monas*, *Vibrio*, &c., &c., without ever having perceived any subsequent effect." He might have swallowed the poison of the Viper with the same impunity. By what various means the poison of insects or animalculæ might be communicated through the air or directly to individuals, we know not. During the past summer I knew a lady of very nervous temperament to be kept for weeks in a nervous, nauseated state, from the effluvia of certain insects on trees in the yard, while no one else perceived it, or was affected by it; she did not recover until the season for the insects had passed over. Here is a perfect analogy to the *Rhus Vernix* and other vegetable substances alluded to by Prof. eidy ,as capable of poisoning the air. Similar analogies abound.

Table of interments in the Mobile Cemeteries during the Yellow Fever Epidemic from 1*st August to* 1*st November*, 1853.

		Deaths			Deaths			Deaths
August	1	6	September	1	40	October	1	8
	2	3		2	45		2	9
	3	4		3	29		3	6
	4	2		4	31		4	15
	5	5		5	36		5	9
	6	2		6	39		6	4
	7	2		7	30		7	9
	8	2		8	44		8	5
	9	3		9	33		9	6
	10	4		10	28		10	7
	11	4		11	31		11	4
	12	11		12	30		12	8
	13	6		13	32		13	6
	14	7		14	26		14	6
	15	9		15	40		15	8
	16	8		16	25		16	5
	17	10		17	31		17	11
	18	12		18	21		18	8
	19	15		19	18		19	9
	20	11		20	21		20	11
	21	8		21	18		21	8
	22	11		22	21		22	7
	23	12		23	14		23	6
	24	12		24	20		24	4
	25	20		25	13		25	5
	26	15		26	18		26	6
	27	26		27	17		27	3
	28	28		28	8		28	4
	29	39		29	9		29	5
	30	15		30	12		30	6
	31	25					31	6
		337			780			214
								780
								337
								1331

The epidemic had so exhausted itself by the 26th October that the Medical Board announced it was at an end, and discontinued their daily reports; scattering cases, however, continued to occur throughout the months of November and December, and I find on examining the records of the Sextons, that 25 deaths in November and 15 in December are placed to the Yellow Fever list. The last death was on the 16th of December, but other cases, not fatal, occurred later.

The above table includes deaths from *all causes*, and we possess no data by which we can classify with accuracy the different diseases for those months, but we can approximate the number of deaths from Yellow Fever alone sufficiently near for all practical purposes. The aggregate from *all causes* during the three months was 1331, and those informed on the subject will allow that 15 deaths a week, or 60 a month, would cover the mortality at this season of the year from all other causes than fever, and particularly during the prevalence of an epidemic. According to this estimate, the three epidemic months would give an aggregate of 180 deaths from causes exclusive of Yellow Fever. The facts may be tabulated as follows:

Deaths during August, September and October from all causes, - -	1331
" for same period from other causes than Yellow Fever, - -	180
" " " " Yellow Fever alone, - - - - - - - -	1151
" during November and December from Yellow Fever alone, - -	40
Total of Yellow Fever from 1st August to 16th December, - - - -	1191

About fifty of the deaths from Yellow Fever were among the colored population, and this class was almost as universally attacked as the whites, which shows a degree of malignity unknown in Mobile since 1819, when the disease attacked creoles, negroes and Indians.

The winter population of Mobile is at present about 25,000, of which at least one third were absent during the epidemic; some of the latter remained in the vicinity, and many went to the interior or other States. It should however be borne in mind that our city cemeteries are the repositories of most of the dead for several miles around the city, as well as for the steamboats; and that our bills of mortality may therefore exhibit a larger per centage on our population than truth would justify. But mitigate the facts as we may, 1331 deaths in 90 days is a terrible mortality, and had the population remained in the city, I see no reason to doubt that the white portion would have been more than decimated. Certain it is, that in many villages along the Gulf States, where the number of inhabitants could be closely approximated, and where none were "acclimated," this fearful epidemic committed ravages far beyond decimation.

Mobile, December 18, 1853.

II—AN INQUIRY, ANALOGICAL AND EXPERIMENTAL, INTO THE DIFFERENT ELECTRICAL CONDITIONS OF ARTERIAL AND VENOUS BLOOD.

BY JOHN GARRIE, M. D., OF APALACHICOLA, FLORIDA.

The existence of organized bodies depends upon two properties of matter—the one peculiar to it, and called the vital, the other common to all matter, and known as the physical. The concurrent action of both is generally recognized as indispensable to our idea of life in its ordinary acceptation; yet each is manifestly antagonistic of the other. Under the exclusive control of the former, organized structure would retain whatever form and composition it received at its creation. As in the seed of a plant, the ovum of a bird, or an hibernating animal protected from the action of physical stimuli, it would maintain an inert and dormant vitality, unrecognizable by our senses, for an indefinite period—it would be immortal. Placed under the sole influence of physical forces, it would instantly undergo numerous modifications in the composition and condition of its materials, which would be subversive of its distinctive character; death and decay would be cotemporaneous with conferred existence. Applied to man, the union of the two properties is necessary to the maintenance of that beautiful, and in its results, conservative law which consigns his body, after a certain degree of maturity, and the fulfilment of duties of high importance in the scheme of creation, to temporary decomposition and destruction.

The phenomena of organic matter have always been objects of profound interest to men of science. Their investigation has long and ardently engaged the attention of ingenious minds; and a clear and just solution of their nature has ever been looked upon as more important than the most successful inquiry into any other department of knowledge. Agreeing, generally, in viewing them as under the control and direction of both vital and physical forces, physiologists have yet differed widely as to the degree of influence that should be assigned to each. The leading tendency among them at the present day is to view the functions of life as mainly dependent upon physical laws. Indeed, by a few, vitality is deemed a mere hypothesis—"a chimera of the dark ages"—supported neither by analogy nor experiment, and introduced into scientific discussion to hide the ignorance of pretending philosophers. Life, in their view, is merely some undiscovered physical or electrical condition of the material particles constituting the substance in which it exists. All the supposed proofs of its separate existence are consid-

ered only figurative representations of ideas incapable of being plainly expressed because not susceptible of being clearly understood; and therefore substituted to render, without the labor of explanation, its effects cognizable by our faculties. They are considered as retarding progress, and keeping physiology in the rear of all other sciences. Their only effect, like the mirage of the desert, is delusion; they show us deceitful views of refreshing lakes and shady forests, while the consequent disappointment discourages us on our march to the true fountains and groves of science.

In the prevailing spirit of modern science to simplify and bring under the cognizance of the senses all natural causes, attempts have been made to reduce the phenomena of life to the common laws of mechanical force; but the more general view of material physiologists is, that with these are united "all the peculiarities of chemical forces, and of the not less wonderful cause, which we regard as the ultimate origin of electrical phenomena."*

But the existence of a vital principle, superadded to the physical forces, and, distinct from any known physical property, operating as the source of growth and reproduction of organic bodies, is generally recognized by physiologists. That the influence of this force has been too widely extended and too implicitly submitted to, has been also generally suspected. The idea, inseparable at the dawn of science from every examination of the living organism, of a vital principle distinct from the organism itself, is scarcely admitted by any intelligent modern physiologist, yet many still consider that vital activity has nothing in common with the causes which produce motion or change of form and structure in organic matter. And the fullest examination of the peculiar properties of living beings shows that they are influenced by forces which no present knowledge of the action of physical and chemical agents will explain. Indeed, all our observations and experiments confirm the view that the nature of life can only be investigated with a prospect of success when many of its manifestations are regarded as resulting from the agency of forces as distinct from those of physics and chemistry as these are from each other.† Invoking the aid of physical forces, vitality still maintains its independence; and while it subjects the former to its paramount power, it never permits them to usurp its natural dominion.

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* Liebig's Animal Chemistry, p. 63, Philadelphia edition.

† Carpenter's Principles of Human Physiology, p. 219, American edition.

As belief is essential to the acquisition of knowledge, so incredulity is necessary to that discrimination which constitutes its accuracy; and every doubt, which, leading to the examination, tends to detect the falseness of a reputed fact, tends also to promote science. If applied to the received dogmas of physiological science, its effect is to remove properties of animals from the dominion of vital to that of physical laws; it tends to bring the mysterious principle of vitality itself more within the compass of human comprehension. An object of this and a subsequent communication will be to show that one function, at least, of the animal economy—that of the often discussed one of the capillary systemic circulation—is plainly susceptible of such a removal.

That living beings are endowed with the general properties of all natural bodies, is too evidently in accordance with the analogies of nature and the conclusions of experimental science, to be disputed. Even in the more recondite forms of caloric, light, molecular attraction and repulsion, and chemical affinity, the influence of physical forces over the structure and functions of organic matter is universally recognized. Although differing from vital laws, yet their analogies are sufficiently numerous to show that there is an affinity between them, which is fully equal to the purpose of producing a modification of each other's effects. The separate and combined action of both sets of forces—their concurrence and opposition—their analogy and antagonism—appear to human comprehension inextricably blended, and certainly present complex problems well calculated to rouse the genius and engage the labors of philosophers in their solution. The physiologist who reduces either within its natural and true boundaries, effects an object of high interest to mankind; or, falling short of demonstrating such a result, proves a new analogy, antagonism, difference or concurrence between them, renders a valuable service to science.

Enquiries into the nature of new properties of bodies show such a similarity and conformity in all nature's operations, that the employment of comparison and hypothesis in assigning analogous functions to analogous causes, is fully justified. This, indeed, forms a rule of logi from the practical application of which no science is fully free, and i indispensable to the successful prosecution of physiology. Notwithstanding the endeavor and pretension of physiologists to adhere rigidly to th Baconian system of induction, we perceive that much of our knowledg of the laws, or most general principles of organic structure, is intimatel connected with and dependent upon comparison and analogy. Withou the valuable indications which these principles of reasoning afford, hov

much should we know of comparative anatomy and pathology, or even therapeutics?

In every science there are axioms that require no proof, and there are operations of laws, which, from their being above the reach of experiment, admit of no demonstration, and yet are equally entitled to belief. That invaluable but inexplicable property of the human mind, common sense, compels it to receive many unprovable, and even incomprehensible ideas, because they carry an evidence of truth which necessitates their admission. In the department of knowledge treating of the nature of the vital forces, how many propositions are there which can be proved experimentally? And yet we readily admit there are very many, deduced from analogical reasoning and hypothesis, that are not only probably, but absolutely true. In the germination of the grains of wheat, exhumed after a burial of thousands of years in the catacombs of Thebes, we have evidence that vitality may exist in a dormant state for an indefinite period, and we may infer that under the same circumstances it would exist for ever; but it is obviously impossible to prove it. Even in the generally exact and demonstrative science of mathematics, we are incapable of proving experimentally, or even logically, beyond the possibility of doubt, many of its most elementary positions. How can we prove that those which assume a continuance throughout all space, or for eternity, are true?

Hypothesis, as affording a rational means of attempting a description of instinctive ideas, and of giving form and expression to self-evident, though not demonstrable propositions, is admissible in scientific enquiries. As an agent by which we are enabled to concentrate our views of phenomena, and direct our investigations to rational experiment, it is a requisite aid to almost every advance in knowledge. A discovery, not preceded by an hypothesis, is an accident; and, however valuable, confers no merit on the discoverer. But an hypothesis founded upon the analogies which facts in a cognate science, and enlightened reasoning afford, is not only admissable and meritorious, but indispensable to all our attempts to complete the boundaries of any science in which few discoveries have been made and defined by the senses. Though it may not admit of direct experimental proofs, yet if it is founded npon known analogies, and accounts for phenomena with a probable adaptation of means to ends, it is entitled to the favorable consideration of philosophers.

The attempts of physiologists to withdraw the phenomena of life from the dominion of purely conjectural vital laws, and bring them under that of the more demonstrative principles of physics, required hypothe-

sis, and have been both impeded and aided by its use. In the early efforts, the mind was bewildered in the vain endeavor to trace the mazes of doubt in which the whole subject of physiology was involved, and suggested hypotheses which were as wild and fruitless as any they were put forth to supersede, until the light afforded by the fortunate detection of one fallacious principle, induced the proper examination of another, and thus led us along the path of discovery. Very soon after the attention of mankind was generally directed to the connection of the physical forces with the functions of life, the belief became common, and the expression of it floated alike on the lips of the philosopher and the sciolist, that electricity was generated in the animal organism, and was a necessary property of organic existence. But having no other basis than simple conjecture, it failed to command the general approval of philosophers. And unaided by experiment, or the parallelism afforded by the cognate sciences, to verify or render probable its reality, the conviction or conjecture cannot be considered as contributing any thing to the extension of knowledge, "and as the induction of enthusiasm, rather than any solid reason, is not to be admired for its wisdom." But, as illustrating an instinctive idea of truth, and as an evidence of the general persuasion of the existence of an important natural law, it urges speculative minds to attempt its discovery in the legitimate path of experiment, and is therefore not to be contemned as a mere guess.

Experimental researches in physico-chemical science have evolved the laws and properties of this "ubiquitous, imponderable, exhaustless energy, power, spirit, substance—*quocunque nomine gaudes*—called electricity. Throughout the material universe we find that every change in matter, whether of form or place, develops it in one form or another. A state of electrical tension is produced whenever a body is submitted to any kind of mechanical motion; it is set free by every chemical change of composition; it is developed by every disturbance of temperature; it results from every variety of substance placed in galvanic or magnetic arrangements; and lastly, it is displayed unequivocally in all its known forms by certain animals. Electricity is vaguely believed by many to be the source of all natural motion; the general cause of animal and vegetable life, and its decay; the source of increase and growth from that of the inorganic crystal, and the most imperfect plant or minute insect, up to a tree or a whale; and under a different name, as the power whence springs the revolutions of satellites around planets, and planets around suns.

The analogies afforded by physical science, and physiological experiment, so far as it has been made, show that electricity is the probable,

perhaps the undoubted cause of muscular contractility and expansion, the various capillary circulations, nutrition, secretion, the generation and exhaustion of animal life. No physiological fact is more certain than that it is capable of performing, at least vicariously, many of the functions considered as dependent on vital principles. And certainly, if we consider its laws in connection with the various composition of the living structure, it may well be doubted whether any bodies in nature or art unite more of the conditions necessary to produce a subversion of the balance, or a restoration of the equilibrium of electricity. The whole animal body exhibits in its incessant composition and decomposition the appearances commonly assigned to electro-chemical force; while all the phenomena constituting the inexplicable power called life, manifest effects which may rationally be considered as resembling a series of electrical actions and reactions.

The connection of electricity, established by experiment, with parts of the animal structure, may very properly be extended by analogy to the whole. The subject divides itself into several branches, partly corresponding with the progress of our knowledge of the facts that have been gradually developed, and partly depending upon the divisions into which it has been made by the science of natural phylosophy.

Static electricity, as evinced by the spontaneous projection of sparks from the skins of the lower animals, and sometimes of man, appears to have been observed, though recorded in obscure and scattered facts, and often under the influence of an imagination which magnified and exaggerated them into supernatural appearances, at a very early period of philosophical history. As the result of friction, in particular states of the weather, it is among the best attested electrical facts of modern days. Saussure, Larrey, and other writers, detail instances where electricity in this form has attained an intensity that placed animals of the quadruped class, and even man, in rivalry with the torpedo, or the common electrical machine.

Galvanism, when first discovered, was supposed to be exclusively an animal electricity; and though it has been found a property common to every kind of matter, yet its evolution must be regarded as a prominent quality of the animal structure. Phenomena analogous to those developed by the ordinary forms of galvanic arrangement, are readily made manifest, by combinations of different kinds of animal matter, made into a circuit by means of a metallic conductor. Experiments with such an instrument have produced the most wonderful physiological effects, but as they scarcely afford any support to the position that there is an independent electricity of the living body, it is not necessary

to repeat a description of them. Aldini, the nephew of Galvani, believing that the nervous fluid itself was identical with, or developed the galvanic fluid, performed some remarkable experiments in which very considerable muscular contractions were excited without the intervention of any metal; and thus was the first to demonstrate the independence of electricity of inorganic matter. Under the influence that this simple but novel experiment produced, Galvani, Aldini and most of the men of science of that period assumed that there was a proper animal electricity inherent in the living body; and the hypothetical agent which for many ages, under the name of the nervous fluid, had reigned as the vital principle, now gave way to electricity, through which "the decrees of the understanding and the dictates of the will were conveyed from the organ of the brain to the obedient member of the body."* But this opinion, however attractive from its simplicity, was too crude and too feebly supported by facts to command the assent of all the contemporary physiologists, electricians and chemists, and was generally repudiated in the succeeding age. Volta, and after him Davy, offered as the true explanation, that the electrical excitement was due to the mutual contact of two dissimilar substances; or rather to that decomposition of electricity which takes place on the combustion of any three kinds of matter.

Recently, Matteucci, improving upon the experiments of the early electrical physiologists as well as upon his own, has proved the correctness of Galvani's hypothesis, by demonstrating the generation of electrical currents in the muscular system of the living body. By the happy combination of the rigorous method of physical experiment with the close reasoning of physiological research, he has fully established the important fact of the existence of an electric current, directed in its development from the deep to the superficial parts of the muscles. Contrary to the deductions of Volta and other experimenters, he has determined that this electricity pervades every muscle in every species of animal which has been made the subject of examination. He has also shown that the intensity of the current is greater in the warm than in the cold blooded animals.†

The question whether there is an identity between the magnetic forces and any of the phenomena presented by the animal body, has received some examination from men of science, and has frequently been answered in the affirmative. But in general the demonstrations of this identity, though more numerous than those in regard to the resemblance

* Lardner's Popular Lectures on Science, vol. 1, p. 363.

† Lectures on Physical Phenomena of Living Beings, p. 178.

between galvanism and animal electricity, can scarcely be considered either so conclusive or satisfactory. Experiments from which obvious and uniform effects were expected, have been attended with results, from which the most vague and discordant conclusions have been drawn. Thus Pouillet, Buland,* Prevost, David,† Beraudi,‡ assert, as the result of numerous, varied and careful experiments, that a needle introduced into a nerve of a living animal becomes magnetic. On the other hand, Person,§ though believing in the electrical properties of the nerves, was unable to detect any electrical effect from them upon the needle of the galvanometer. And after him, Matteucci, experimenting in his precise and critical manner, on the large crural nerve of a horse, says, "I must confess, that whenever the experiment was well made, I never obtained evident and constant traces of the electric current," by a deflection of the galvanometer from irritation excited in the nerve. How far this failure may be dependent upon the small quantity or low intensity of a current, and the inadequacy of our means and mode of experimenting to develop it, we do not know; but considerations on these subjects will be hereafter presented to the reader from which he can draw his own conclusions. In the present state of our knowledge, it is at least doubtful whether electricity or any other fluid is transmitted along the nerves.

Although the proof of the existence of electro-magnetic currents in the nerves is of difficult or doubtful attainment, yet the action of the muscles in inducing this effect is by no means equivocal; for, independent of direct experiments showing the fact, it is well ascertained that needles, after being used in acupuncturation, show magnetic power.

Thermal electricity being the result of every disturbance in the equal flow of heat, must necessarily exist as a property of matter so subject to changes of temperature, and so capable of transmitting their effects as the animal structure. So vast is the quantity of electricity of this peculiar form developed by animals, that with a pile, carefully made of a large number of the elements usually employed for manifesting its effects, united to a galvanometer, the warmth of the hand may be made sensible at the distance of thirty feet. Indeed so conspicuous is this species of electricity, or so sensible and delicate a measurer of it is the instrument

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* Ranking's Abstract, vol. 1, p. 244.

† Cyclopedia of Practical Medicine, vol. 2, p. 291.

‡ American Journal Medical Sciences, vol. 9. p. 483. Lectures, op. cit. p. 259.

§ Dunglison's Philosophy, 7th edition, vol. 1, p. 119.

referred to, that by its means the comparative warmth of different insects may be ascertained.*

The most full and complete evidences of free electricity in living animals are to be found in certain species of fish, the best known of which are the Raia-Torpedo, and the Gymnotus-Electricus. In these fish the identity of animal with common electricity is proved, not only by the similarity of the effects upon the feelings produced by their shocks, but also by the most striking resemblance between their other effects and those of the various kinds of electricity. From the former, Blainville and Flourens obtained magnetic deflection; Dr. John Davy magnetized the needle, and was able to effect electro-chemical decomposition of common salt, nitrate of silver and super-acetate of lead; and more recently Linari and Matteucci gave produced the spark. But the Gymnotus in the hands of Farraday has more readily, and in a more striking manner, all the proofs required of the identity of its power with that of common electricity.†

In the preceding cursory view of the connection and identity of electricity derived from the animal structure with that from purely inorganic sources, we have advanced nothing that is not universally recognized as true by philosophers, and easily demonstrated to the senses. But it is not acknowledged, nor do the facts now mentioned prove that electricity is a special property of the living organism, (at least of warm blooded animals) and still less that it directs or influences the functions. Even if the experiments adduced be admitted as affording evidence of electrical action on the solids and semi-solids, they cannot be considered as proving any operation on the different circulations, nutrition, the secretions, the production of animal heat, or any of the functions that are referred to vital principles as their cause. To prove a functional power in electricity, it is necessary that it should be made apparent as a distinctive property of organic fluids. That appearances are presented by the fluids of the animal system that can only be rationally assigned to the influence of the forces of attraction and repulsion, is acknowledged; but a polarization of these forces, or a reciprocal action of each upon the other, rendered distinct to the eye of observation, is required to demonstrate that they are the effects of electricity, and this has not yet been effected in a conclusive or satisfactory manner. It is true that M. Donne, a physiologist favorably known to medical men as a microscopist, and who, in this instance, appears to be a patient in-

* Noad's Lectures on Electricity, page 241.

† Ibid, page 248.

vestigator of animal electricity, has presented to the public some highly interesting statements, which tend to show that the polarization of animal fluids has been accomplished. If the experiments he has reported have been carefully observed, and there is no mistake of cause, electromagnetic force is proved to exist as a function of the glandular system, and secretion is resolved into a mere play of electric affinities. The principle results he obtained show that the external cutaneous and internal mucous membrane represent, through their secretions, the two poles of a galvanic circle, the electrical effects of which are appreciable by the galvanometer. He affirms that on placing one of the conductors of the instrument in connection with the mucous membrane of the mouth, and the other in contact with the skin, the magnetic needle deviated fifteen, twenty and even thirty degrees, according to its sensibility; and the direction of its deviation indicated that the internal mucous, or alkaline membrane, took negative and the cutaneous or acid membrane positive electricity. He further asserts that between the acid stomach, and the alkaline liver, extremely powerful electrical currents are formed.* The only probable objection to the conclusions of M. Donne appears to be, that the different electrical effects he obtained may be attributed to the decomposition of salts, and the action of acids, after they had been thrown off from the system as mere excretory inorganic matter upon the wires of the galvanometer, rather than as the action of living fluids.

Having these ample evidences of electrical manifestations in organic structure, the hypothesis of different electrical states in arterial and venous blood readily suggests itself; and the object of the remainder of this essay will be to show by analogical and inductive reasoning that it is founded on probability, and to demonstrate by experiment its certainty.†

The blood is known as the common material from which all the tissues of the animal body are built; as the grand source of stimulus to the nervous and secretory functions; as the fountain of vitality, and the fluid with which, of all parts of our body, we associate most intimately and indissolubly our idea of life. On account of the very important part which it performs in every function of the animal economy, it is necessary for the object in view that notice should be taken of every one of its supposed properties, and its real ones ascertained and examined.

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* Dunglison's Physiology, vol. 2, p. 257.

† London Medico-Chirurgical Review, 1837.

Upon pure physiological considerations we should infer a difference of electrical excitement between arterial and venous blood. The former is recognized as the real and exclusive source from which all parts of the body are formed and developed, while the latter is only the receptacle of such products of the metamorphosis of the tissues as are not carried off by the secretions. The very great difference of office assigned to each fluid would alone indicate that the arterial blood must be more eminently charged with the forces of attraction than the venous. We find, too, that in the execution of the glandular functions of the organism, peculiar elements of the blood, tending to alter its electrical properties, are dismissed from the circulation. Thus the kidneys, in forming urine, separate directly from arterial blood a large proportion of nitrogen—an electric high in the scale of negatives—and, of course, the blood, returned to the circulatory system by the renal veins, cannot possibly be in the same electrical condition as that in the renal arteries. But independent of such a fact, it is difficult to suppose that "the different transmissions and transformations of the arterial blood, by which its constituents are converted into fat, muscular fibre, substance of the brain and nerves, hair, bones," etc., can take place without the presence and exertion of those forces, in their appropriate quantities, which accompany all changes in organic matter; or that venous blood, having no similar duties to perform, can be charged in an equal degree with electricity.

The general physical properties of the blood show a marked difference between its arterial and venous varieties; and indicate, upon known principles, a higher electro-positive state in the former than in the latter. In this part of our enquiry little assistance can be derived from the investigations of the anatomist; but that little is eminently favorable to our view. The structure of the blood, divested of the fanciful properties with which the enthusiasm of microscopists has endowed it, seems, equally with that of the solids, to present the conditions necessary for developing electricity. According to the most generally recognized opinions it is composed essentially of cells or vesicular globules, enclosing a dense, high colored liquid of obviously a different chemical composition, and floating in a liquid, which is less consistent, and of course also of a different chemical composition. There are here not only the physical elements of matter necessary to every simple galvanic circle, but the form and arrangement of them most favorable to the polarization and transmission of electricity. All the examinations of the blood indicate that the arterial is richer in globules than the venous,

and hence it may be inferred that it is endowed with a higher vitality and a greater quantity of electricity.

Among the physical differences of the blood contained in the two sets of vessels, the arterial is found to yield a fuller, firmer coagulum, and therefore it must contain a less quantity of the more aqueous serum than the venous; which is an indication of a larger quantity of globules and electricity generating matter.

The color of the two kinds of blood is very different; that of the arterial being in the higher order of animals, of a richer and vermillion redness, dependent alike upon a larger proportion of globules and a surcharge of the electro-negative principle—oxygen.

Although the relative temperature of arterial and venous blood, simple as the determination of the question would seem to be, has not been ascertained to the satisfaction of all physiologists, yet the weight of authority seems to be in favor of the opinion that the temperature of the former is higher than that of the latter. Independent of observation with the thermometer, we might infer such a result from the absorption and solution of oxygen by the arterial, and the setting free of carbonic acid by venous blood—upon the simple physical principles that condensation produces heat and expansion generates cold. Conjoined with the fact that the specific heat of arterial blood is also somewhat greater than the venous, we may rationally infer that the former has, from this cause, a higher electrical tension, and that a current of electricity always flows towards the latter.

Mechanical friction is well known as the most simple and general means of putting electricity into action; and this effect takes place whether it be between solids, or solids and fluids. If mercury, after being subjected to the friction of agitation in a glass tube be presented to an electroscope, it will give distinct signs of positive electricity. Mr. Farraday has shown, in his observations on the source of electricity in the hydro-electric machine, that the forcible ejection of a liquid through small tubes, or orifices, may be made the most powerful source, in both quantity and intensity, of statical electricity.

In the structure of the arteries of the systemic circulation we see a nice adaptation of means not only to the generation or evolution of electricity from the blood, but also to its preservation or insulation. "These vessels are flexible, elastic tubes, principally composed of membraneous matter, formed into distinct layers, and comprising what are called the coats of the arteries. Of these coats anatomists usually describe two, as possessing a sufficiently determinate structure to be easily distinguished from each other; the outer one partaking more of the cellular

structure, and therefore called the eellular coat; and an inner membrane, white, firm and smooth, possessing more of the physical properties of tendon. Between these membraneous coats is situated a stratum of transverse fibres, which have been termed the muscular coat."*

The chemical composition and anatomical arrangement of these coats suggest an obvious and beautiful design for checking that tendency to equilibrium which is so marked a property of electricity, and for retaining it in the blood, in furtherance of any functional duties it may have to perform. Electricians appear to consider the animal system as nearly equally divided into conductors and non-conductors of electricity; the aqueous fluids and muscles being considered as belonging to the former class; the oils and all adipose matter to the latter. Indeed, the structure of the whole animal system seems, among its other objects, to be admirably adapted and intended for the retention and economical dispensation of the energizing fluid. All the organs of the body, whose functions are considered essential to secretion or reproduction are found protected to a greater or less extent by non-conducting investments. The position of the blood in this relation does not seem to have been defined or examined, but fulfilling in its systemic circulation the great duties of nutrition, secretion and the vital functions, probably connected with the evolution of electricity, we should readily suppose it would be well provided with non-conducting envelopes, for moderating that tendency to equilibrium which would otherwise take place too rapidly. Experiment fully confirms this view of the electrical properties of arteries. The aorta of an ox was found, even in its recent state, to be a non-conductor of electricity, high in the scale of insulators—classing apparently above parchment or leather. The arteries are therefore good electrics, because the properties of bodies which render them good insulators, render them also good electrics.

"In the arteries there is a very great degree of friction—of the blood globules against the arteries—of the arteries contracting round the blood like an obstacle—and of the particles of blood amongst each other, by the confused and vertical manner in which they are propelled."† On the other hand, the greater size of the veins and the consequent slowness with which the blood flows in them would lead us to suppose that venous blood must be nearly free from friction; while, from the thin and dense texture of the venous coats, they must be incapable of insulating electricity. We may, therefore, reasonably conclude that

* Bostock's Physiology, vol. 1, p. 271.

† Haller's Physiology.

the projection and transmission of the arterial blood through the tortuous, non-conducting and electric walls of tubes, presenting before their termination a very small calibre, must generate a portion of that electricity which we believe is necessary to the animal system. At least we must infer that, from the difference in the motion of the blood and the properties of the vessels, it must be in a higher state of excitation in the arteries than in the veins.

The next analogical confirmation of our hypothesis is to be found in the chemical constitutions of arterial and venous blood, and the well known law of electricity that bodies differing in chemical composition exercise different electrical relations towards each other; it being considered that that body, whether simple or compound, which is most highly charged with electro-negative principles or elements, has always a tendency to give out positive electricity to bodies presenting the opposite, or a different electrical relation.

The determination of the chemical composition of organic structure, is always a difficult task, and subject to much greater diversity of result than that of inorganic matter. This is fully exemplified in the attempts to analyze the blood; for no two agree in all respects with each other. Considering the striking difference between the physical qualities of arterial and venous blood, it excites some surprise that chemical analyses should show so great a uniformity as is found in the composition of their organic elements. In the absence of an apparent or easily ascertained diversity in the chemical constitution of the two kinds of blood, we recognize a strong objection to our electrical hypothesis; but, besides that organic elements do not form the whole of the blood, a minute and careful examination shows that there is a sufficient difference in composition to enable it to possess electricity enough for any duty it has to perform. All the experiments and discoveries in animal electricity (except those in electrical fishes) evince that it exists in a state of exceedingly low intensity—corresponding with the slight difference in chemical composition of the animal tissues. Nature, in this instance, as in her usual practice, works out the end she aims at gradually,silently, imperceptibly, and with the greatest economy of means; yet with these means she always obtains the most complete as well as the most magnificent results. In particular cases, the small quantity of the forces she employs in her operations is compensated by the greatness of their intensity; but more commonly she supplies deficiency of intensity by vastness of quantity. Unlike the narrow resources of art, she can make up for limited power by a rapid renewal of her ope-

rations; for low intensity by great quantity of force: and for small space by endless convolutions of surface, or infinite divisions of matter. In the circulation of the blood through the capillaries of the systemic system, it is divided into an innumerable multitude of minute streamlets, each so small, as, perhaps, to admit but a single layer of its corpuscles; and in these, therefore, the surface which is placed in contact with their walls is so enormously extended, that the degree of friction and exposure to electrical and chemical attraction must be almost beyond calculation. Hence, it is possible to understand how an electric force, adequate even to perform all the functions of the animal body, might be obtained from an electrical excitement of very low intensity.

According to M. Lucanu, who may be regarded as among the highest authorities on this subject, the immense number of substances blood contains, are found in both kinds and in nearly equal quantities. Beyond the physical appearances already mentioned, arterial seems to vary from venous blood only in the more decided character of its peculiar odor, the greater abundance of globules, fibrine and fixed material, a diminished proportion of carbon and a higher one of oxygen in combination.* In the quantity of albumen, and of extractive, saline and fatty matter, both kinds of blood are about equal.

But it is in the comparative quantities of the inorganic, mineral and ærial constituents of the blood—particularly iron, oxygen and carbonic acid—combined and free, that the two kinds of blood chiefly differ. Respecting the manner in which oxygen—the quantity and electrical properties of which render it far the most important in the present consideration—is combined with the blood and conveyed to the peripheral system, there are two theories prevalent among physiological chemists—those of Liebig and Mulder. Liebig refers the change of properties that venous blood undergoes in becoming arterial solely to the absorption of oxygen by the iron in the corpuscles;† while Mulder maintains that it is entirely owing to the oxidation of proteine compounds.‡ There is reason to conclude from the experiments of M. Denis, as well as the researches of Mulder, in regard to the difference between the fibrine of arterial and venous blood, that oxygen derived from the inspired air enters into actual combination with this element. Doctor

* Gibert on the Blood, page 12.

† Animal Chemistry, page 55.

‡ Ranking's Abstract, vol. 1, page 287.

Carpenter* adds, that the same may be probably true of other constituents of the blood; and therefore the theory of Mulder is probably ths more correct one. But the operations comprised in both theories may be carried on; for while arterial blood undoubtedly contains more assimilated oxygen, and is more highly animalized than the venous, "the red corpuscles are the chief carriers of the free gases to be interchanged between the capillaries of the several systems of the vessels." The truth and accuracy of either theory, however, is not absolutely essential to the attainment of the present object. The experiments and deductions on which the conclusions of both are founded show the important fact, that there is more of the electro-negative element, oxygen, in arterial than venous blood.

The difference in the quantities of the free oxygen and carbonic acid in the blood is of higher consequence than that of the combined, in an enquiry into the comparative electricity of its two varieties. In the investigation of this subject we are aided by the recent and splendid researches of Magnus on the function of respiration. He has made a great many experiments with a view to elucidate the nature of arterialization and the relation of the gases to the process; and they are deemed so accurate and deserving of confidence, that they are quoted with approbation in most late treatises on Physiology. They were not, as from the nature of gases they could not be, entirely accordant with each other; but a minute examination of them by Gay-Lussac, with the object of refuting the conclusions of Magnus, and a repetition or extension of part of them by Majendie, afford corroboration of the correctness of his views on respiration.† In consequence of the objections of Gay-Lussac, these experiments have been repeated and extended by Magnus himself and their accuracy confirmed; they have also been repeated by Prof. Shultz of Berlin, and by Doctors Stevens and R. E. Rogers, of this country, with a general verification of the results.‡ A table deduced from Magnus' experiments represents the average absolute quantities of free gases in the blood of horses and calves as follows, viz:

	Cubic Centimetres.	
For 100 parts of arterial blood	10.4276 of gases composed of	6.4967 carbonic acid. 2.4178 oxygen. 1.5131 azote.
For 100 parts of venous blood.	7.6825 of gases composed of	5.5041 carbonic acid. 1.1703 oxygen. 1.0081 azote.

* Human Physiology, page 588.

† Ranking's Abstract, vol. 1, p. 295.

‡ Dunglison's Physiology, vol. 2, p. 112.

This view of the subject presents results which are of the highest interest to our theory of the electrical relation of arterial to venous blood. They show that—

First. There exists in the arterial blood a larger quantity of gas than in venous blood.

Second. The quantity of oxygen found in arterial blood is double that which exists in venous blood.

Third. The ratio between the oxygen and carbonic acid is from one third to almost one half in arterial blood, while it is only from one fourth to even one fifth in venous blood.*†

The importance of these beautiful experiments in explaining the function of respiration cannot be doubted; nor are they less valuable as corroborative of the view of a higher state of electrical energy in arterial than venous blood. Oxygen and carbonic acid gases are negative electrics of a very high order; and, according to recognized principles of electricity, they must cause any substance containing them in one quantity to transmit a current of positive electricity to auother containing a less quantity, on the completion of a galvanic circle. Minutely diffused and circulating rapidly with the blood throughout the animal system, and constantly receiving new accessions from the atmosphere, they must necessarily, in their contact and affinities with the tissues, and their course to the venous system, evolve very large quantities of electricity. Viewed in both their physiological and electrical relations, there can be little doubt that the changes in the blood, which are effected by the function of nutrition, have reference, in great part, to the proportions of these gases which the arterial and venous systems hold in solution, and the quantity and kind of electricity they evolve.

It is, perhaps, not the least of the recommendations of this hypothesis, that it proves an adaptation to and supplies a direct and positive want of organic structure. This fitness of means to ends is an invariable characteristic of the Deity's works; and the highest degree of deducible evidence of its existence is indispensable to the support of every hypothesis.

In explaining the phenomena of the organic functions, it has been customary to commence with the proofs of digestion and terminate with

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* Note. Though the absolute quantities of oxygen and carbonic acid are greater in arterial than in venous blood, yet the relative quantity of the latter to the whole amount of the gases is greater in venous blood; it being in arterial 62.3 and in venous blood 71.6 per cent.

† Matteucci. Lectures on the Physical Phenomena of Living Beings, p. 127.

that of nutrition. After the chyle is seen to be formed, its course in connection with the lymph and venous blood, is traced to the heart, thence to the lungs, and thence, after an examination of the changes by which the whole is animalized and vitalized, it is retraced to the heart. The arterial blood is then followed through the arteries till it is lost in the capillary tissues, and finally resigned to the inexplicable laws by which the tissues are made to appropriate the parts suitable to their purposes, or the glands to separate the molecules required for secretion. The remainder of the blood and the residue of the tissues are considered as then, in some unknown way, transferred to or absorbed by the veins and changed into venous blood, in order to renew the cycle of an ever varying but continuous existence. In the details of this elaborate prosecution and enquiry the possible causes of the various effects are minutely examined, but the examination has never resulted in satisfactory conclusions. A broken series of causes, tending rather to render mysterious the nature of the powers by which the individual is preserved, continued and renewed, than to furnish an adequate explanation of the effects, has been adopted. The idea of a power received with the ærial inorganic elements from the atmosphere, which, by mixing with and vivifying the blood, is, in connection with other known physical causes, sufficient to produce the whole series of phenomena, has been either disregarded or slightly and partially examined. Though often surmised or hinted at, the possibility of demonstrating its existence seems never to have been suggested.

The ordinary changes in the properties of matter from inorganic to organic are the most wonderful, as they are the greatest that take place in nature. But organic existence presents manifestations of a power—at one time normal, at another abnormal; sometimes increased, sometimes diminished in energy—which seems to be superadded to the ordinary evidences of vitality. This power is seen to be exerted in the embryo to develop it into the perfect-formed and new-born animal; and is farther continued till it attains the powers and magnitude of maturity. It exists in the reproductive power by which the inferior animals renew lost parts—as the snail its head, the newt its legs, or the crab its claws. It is evinced in its most extraordinary form in the power of parents to procreate and transmit their own form and features to a numerous offspring. But it also exists in the morbid growths of animal structure, constituting hypertrophy and tumors; and must be considered as operating in the opposite effects of atrophy and attenuation. All these effects of this extraordinary power, on the hypothesis that life de-

pends exclusively upon a vital principle, must be attributed to a redundancy of vitality—a superfluity of life, or a deficiency. Upon this view life must be supposed to increase or diminish with every change in the volume and form of a living being. With our most enlarged views of the nature of vitality can such attributes be considered possible? and under the more generally received idea of its unity and immateriality, are they not too absurd to be logically assumed?

Every view that we are able to take of the recondite and mysterious phenomena of the functions, demands that the agents through which they act should be endowed with very energetic and very peculiar properties. Electricity is the only known power which presents the least probability of a capacity to work such extraordinary results. In assuming that this force exists in arterial blood in a greater quantity and of a higher tension than in venous, or indeed any other structure, we assume a power which supplies the link, hitherto wanting, in the chain forming the connecting bond between the organic functions, and fully adequate to explain them. The general properties and analogies we have assigned to it we deem sufficient, on recognized principles of philosophy, to explain the nutritive functions; and if we add to them its power of indefinite accumulation, condensation and expansion in the whole or part of a structure like that of the animal body, we may be able to understand how those extraordinary appearances, which, on the vital hypothesis, require a superabundance of life, may be produced. If, then, we have proved by argumental evidence that electricity exists in an exalted condition in arterial blood; and more especially if we shall hereafter demonstrate the fact experimentally, we shall have proved the existence of a cause adequate to produce the effects we have assigned to it.

(*To be continued.*)

III.—AN ACCOUNT OF THE YELLOW FEVER WHICH PREVAILED IN THE TOWN OF WASHINGTON, LA.

In the latter part of the Summer and Fall of 1853, *with Remarks.*

BY T. A. COOKE, M. D.

I propose to give an account of the Epidemic Fever which prevailed in the town of Washington during a portion of the Summer and Fal. of this year; and in noticing its origin, type, symptoms and treatment, shall occasionally indulge in such remarks as the subject will necessa-

rily suggest. It is in truth one which has acquired a fearful interest in the history of Louisiana, and indeed of the whole South.

We hear of its introduction into New Orleans in the early part of summer ; in the month of July it assumes an unparalleled severity ; its march is onward in defiance of all opposition ; it consumes the aliment which a long period of security had accumulated in too ample abundance ; and then it slowly departs, lingering in its footsteps, and still proclaiming its irresistibleness in the daily additions it makes to the long list of the dead.

But to the city of New Orleans its ravages are not limited. The disease is transmitted to all the towns and villages in the State ; indeed to all the towns in the Southern States, with few exceptions, having communication with that city of pestilence and of death ; and from the towns it is even carried to the large neighboring plantations.

The effect of a fatal pestilence in towns or the country cannot well be conceived by those who have not witnessed it. It spreads alarm ; the people are panic-stricken ; and every death adds to the consternation, which sweeps over the land. Soon all direct communication is arrested between the country and the unhappy town ; intercourse even in the country is restricted ; every house becomes a barricaded castle ; then ensues a disruption of the bonds which hold men together, and for the time society is dissolved. Such a calamity as a fatal epidemic in the country is not easily forgotten. Years cannot efface the recollection of the disastrous effects of such a visitation. In the country the dead were known to all, and the remembrance of virtue and of merit is not buried with the mortal remains of the dead. But the city, after such a visitation marking its progress with desolation and misery, sweeping into the untimely grave whole families, is like the sea, more tranquil after the tempest has subsided and the surges cease to roll. The public mind, instead of being aroused by scenes the most fearful —intense beyond the reach of the imagination—pictured only in reality, falls back into the coldest apathy, and the past is forgotten.

With regard to the origin of this disease, it is to be remarked that there was a weekly communication between the town and Orleans up to the 15th of August, the date of the last arrival of a steamboat from the city. Every trip the boat brought various kinds of merchandize for the merchants of our town and for the people in the country. In the last trip she brought back an individual, who, though suffering with fever on his arrival, went his way to his home in the country, and expired on the second or third day with all the symptoms of yellow fe-

ver. By the trip of the boat before the last, four individuals, free persons of color, after the death in the city of their mother, a brother and a sister, of the prevailing epidemic, with their baggage returned to the town, from whence a year ago the family had removed to New Orleans. Soon after their arrival one of these persons had an attack of fever, but it was not of such severity as to attract particular attention. The members of the family in whose midst these people from the city were received, were evidently seized with great fear and anxiety, as is proven by the contradictory statements which were made by them relative to the baggage, etc., brought by these people, and by the fact that a trunk remained unopened for many days. At length the brother of Simon Lepp, the proprietor, determined to open this trunk, and the clerk of the store relates that the smell from the articles it contained was so offensive as to sicken him, and to cause him for a time to desist. All the articles were finally taken out and exposed to the air in the back yard; and it appears that an individual from the country, named Miller, passed an hour or two exposed to the effluvia from these clothes thus exposed. This man, the proprietor, the clerk and brother of the last, and a free man of color living in the next house, were all taken sick within a day or two of each other. Miller and Lepp's brother died after about three day's illness; the other three got well. The clerk had a violent attack. Lepp was not long sick, and Sterling, the colored man, had a mild attack.

About this time other cases occurred. A child of Lelah, free colored woman, who had received into her house the girl from New Orleans a few days after her arrival and seizure with fever, fell sick; and here it may be remarked that soon every member of the family, seven in number, one after the other was violently attacked. A child of Sterling, before his recovery, was seized with fever; and in rapid succession the wife, three remaining children and two slaves were attacked. Mrs. King, living at a distance from the point of infection, passed several hours in the immediate neighborhood; very speedily she sickened and died. All the members of the family in whose house she died, fell sick in rapid succession. Several individuals attended the funeral of Mrs. K.; one of them, it is reported, died with black vomit. Two others I know were attacked and recovered. In another part of the town, at this early period, a case occurred, Mrs. Jenkins, who no doubt contracted the disease in daily recent visits she received from a female member of Lepp's family. Among the first cases reported was that of A. Folain, who, nevertheless, had had the disease many years ago. He is the owner of a large ware-

house and receiver of goods from the city. And also in the country the case of Mr. Valensart was reported; a case undoubtedly of a violent attack of fever. This gentleman had been often in town, but in what manner he contracted his disease I have not been able to learn. These cases occurred in the practice of Dr. Tallis, a gentleman of high reputation here. Mr. Valensart came to town several weeks after he first fell sick, contracted the epidemic fever, and in three days died.

In a short time after the occurrence of these first cases, not only individuals and whole families living in the neihhborhood of the house into which the poison of yellow fever, in a concentrated form, or in quantity unusually abundant, or most potent in its procreative energies, was directly imported from New Orleans, were soon attacked with a fever, distinct in its symptoms, malignant in its type, sui generis, wearing an unmistakeable livery, but a great many individuals living in different parts of the town, but in the habit of daily communication with the infected region, also fell sick with the same fever, and thus in a brief period the fever reigned all over the town, and ere long the number of the sick at one time was reckoned at two hundred—nearly one half of the whole population then remaining.

Louis Lambert, a most estimable youth, replaced the clerk of Lepp, immediately after the attack of the latter; in a few days his whole system, apparently saturated with the morbific cause of the fever, yielded to the disease. He was with difficulty on the second day removed to his home in the country. Medical aid was fruitless. Black vomit closed the scene in almost a few hours. His father, mother, brother and two little sisters were in town several times during his brief clerkship. They all and alone of the family fell sick; four were severely attacked; all distinctly presented strong symptoms of the epidemic influence. As said, none but these, of this large family, who contracted the disease in town, took it. They were visited by a large number of persons with impunity.

V. Goubert attended the funeral of Mrs. King; he had a mild attack. but a severe jaundice retarded his recovery. He lived on an adjoining plantation containing some sixty souls; none contracted the disease from him.

About ten members of her family from the country visited Mrs. Sterling on her death-bed. On their return home—and some remained but a few hours in town—her father, mother, a sister and two brothers and two servants fell sick. They were all mulattoes except the slaves. To this house in the country, immediately after the death of his wife, Ster-

ling, his four children and two slaves were removed. He and one child were already sick; the other three and the two slaves were speedily attacked.

There were ten patients in one small house in the country at one and the same time, and as many as four in one close room. Under these circumstances, not only several persons who had been in town, but all the attendants on the sick who had not been in town, escaped the disease, which was in this family of a most fatal type. It would be a superfluous but easy task to cite numerous examples similar to the above.

In the years 1837, '39, '42 and '53, about fifty persons contracted the yellow fever in Opelousas and Washington, the disease only prevailing in the last place in 1853, and only in Opelousas in the other three years; they died almost always with black vomit at their country houses, and in no one instance did any attendant on the sick catch the disease.

As far as I have been able to learn, no doubt exists this year that this fever has been transmitted directly or indirectly from New Orleans to every place in which it has prevailed as an epidemic; and I believe that the opponents of the importation of it into New Orleans must admit that there was this year, at least, a remarkable coincidence between the actual importation of its morbific principle and the actual occurrence of the disease; and it may be here remarked, inasmuch as in hundreds of certified instances which have occurred from year to year up to this time all over the country, and embracing also New Orleans, this pestilence can be legitimately referred to an imported cause; that in the absence of overt facts proving its importation, a strong probability of this importation should be regarded as satisfactory as if proof positive existed. In the country towns the fact of importation can be, and is always verified; and another admitted fact—indeed a necessary consequence of the preceding one—is, that it never prevails in the towns unless it previously exists as an epidemic in New Orleans.

With regard to the manner in which the morbific cause is conveyed, no doubt exists that it may be effected through the media not only of sick persons, but of fomites—that is, through every description of merchandize. The only difference between the two modes of producing the disesse is, that fomites convey the actual morbific cause, and consequently will, when accumulated in sufficient quantity, produce the disease in individuals predisposed to it, under any circumstances and every where; whereas the effluvium from sick persons is perfectly innocuous,

unless the surrounding medium be so constituted or modified, as by its addition to assume the morbific character necessary to produce the disease, or to afford sufficient food for the development and perhaps rapid reproduction, of the germs of the disease emanating from the sick body. An infected atmosphere may thus originate from a sick person, and the bed-clothes used, as well as from the actual materies morbi imported through the medium of fomites; and hence arises much confusion of ideas in relation to what is called contagion and infection. The facts in proof of the importation of the disease through the medium of such persons, and of merchandize, are as numerous as the leaves on the trees, at least in the opinion of most country people destitute of prejudice, or a taste for metaphysical disquisitions. For my part, I sincerely believe that if the origin of the disease in New Orleans could always be accurately ascertained, it would be found to consist invariably either in the infection of the sick, or of merchandize from an infected port, or in the actual infection imprisoned in the holds of vessels coming from such ports.

I will here introduce a fact, familiar to the old people of Opelousas, which, though now a matter of tradition, is as well authenticated as any other whatever. About the year 1826 Mr. L. Louaillier, a merchant of Opelousas, imported late in the fall from the city, during the prevalence of yellow fever, some merchandize. Three individuals, who were present on opening some of these goods, contracted a violent fever; one died, and all were said by an experienced physician to have had yellow fever. In 1828 the same individual, with M. Lazaretti, another merchant, imported goods from the city during the prevalence of the fever. Mr. Louallier, ever regretting the error he committed in 1826, for he believed in the importability of the poison or cause of yellow fever,—opened his boxes and bales, and ventilated his goods for three days before taking them to town, and no disease followed from contact with them; whereas of four individuals present on opening the goods of Lazaretti, brought directly to his store in Opelousas, three died with black vomit, and the fourth narrowly escaped with his life.

And facts are not wanting to prove that this disease can be excluded from the towns, even nearly in the same latitude with New Orleans. Natchez was always protected when her quarantine was vigorously enforced; and this year the exemption of Franklin, by her quarantine, and also of Opelousas by her sanitary regulations, are worthy of particular attention. These quarantines, especially that of Franklin, were enforced vi et armis; indeed with a rigor commensurate in severity

with the evil to be warded off, and for the successful execution of them every praise is justly due to the people and the public authorities. The disease was directly transmitted from the city to the small villages of Pattersonville and of Centreville, just below Franklin, with her population exceeding 2000. It was imported into the town of Washington, six miles only from Opelousas. It came also directly from the city, and burst forth with a fatal violence in the town of Lafayette. Franklin and Opelousas, protected by their quarantines against the invasion of a foreign enemy, worse than fire and sword, alone are exempt. The history of the origin of the disease in all the interior towns of the State, affords satisfactory evidence to every one that the cause is always imported from some other place in which the disease existed. Other conditions are also indispensably necessary for the production of an epidemic; to wit, a more or less crowded population, and the previous existence of summer heat for a certain length of time. A fourth condition, equally indispensable, is a peculiar condition or constitution of the atmosphere; certain strange meteorological conditions of the earth and air. Dr. Rush has said "an atmosphere charged with miasmatic effluvia, or pestilential exhalations."

Last year (1852) a man arrived in this town about the middle of September; immediately after leaving the city, three days previously he was seized with violent fever. Dr. Heard was called to him on his arrival, and on his first visit sent for me in consultation. It was a case of pure yellow fever, which from neglect and bad treatment had run a rapid course; death in a few hours ensued. He died in nearly the centre of the town, in a hotel containing a large unacclimated family and numerous boarders. He was well nursed and seen by many people. From this individual no disease spread. There were present apparently all the circumstances required for its propagation. There only existed this year a higher degree of temperature, and more rains than last year, two circumstances not very material, for yellow fever prevails not only in warm and wet, but in cool and dry summers. Looking to the facts that have been mentioned, we are forced to the conclusion, that in 1852 that peculiar atmospheric constitution favorable to the increase and diffusion of the cause of the disease, nay, indispensable to its development, did not exist. During last year it did not grow even in New Orleans into an epidemic; but 72* deaths, I believe, were reported. For several years previously no epidemic had occurred in the city; and no one can doubt that last year the city was full of material

* 456 deaths from yellow fever in New Orleans in 1852.—Ed.

for the disease to prey upon, if the atmospheric condition of its existence had been fully established. Exemption from epidemics for several years had lulled strangers and emigrants into a false security; and this year, unfortunately trusting to the same false hope, the number of unacclimated persons had greatly increased. If we deny the doctrine of contagion, and admit as necessary to the production of the disease the importation of a foreign cause—two conclusions to my mind as clear as any medical facts whatever—it inevitably follows that the poison, or cause of yellow fever, after its introduction, must, before it can engender an epidemic, find pabulum for its development, increase and extension; or by combining with, and vitiating the peculiarly modified atmosphere it meets with, produce a potent cause of disease.

There are many interesting facts which show that the cause of the disease may for a long time be confined to limited districts, and unless contracted by individuals visiting the infected region, and communicated to other districts, its extension will be very slow; but when it once gets a foothold in an atmosphere favorable to its increase and multiplication, its progress, though it may be slow, is certain; nor can any thing arrest it but freezing weather.

Unlike last year, not only in New Orleans, but in every town and village in the State, and even amongt the negroes on many large estates, this year a constitution of the atmosphere most propitious to the devolopment of the morbific cause existed. Three summer months, as by some considered indispensable, had not elapsed before the disease in the city assumed the character of an epidemic. In every village and town of the whole South, a single germ of the pestilence had but to be cast, and universal flight alone could prevent its rapid development and a ruthless decimation of the inhabitants.

The prevalent opinion amongst medical men, and particularly in former years amongst the faculty of New Orleans, was opposed to the importability of the morbific cause of this disease into cities. It is by many said to arise sporadically; that is, as the result of local causes, which are generated without the aid of any foreign cause whatever. That in Mobile, New Orleans and elsewhere, cases do often occur, even in the winter months, is a circumstance, which, instead of proving its indigenous origin, rather in my opinion shows the reverse. These cases are of rare occurrence; they appear to me rather to reveal the fact, that in these commercial cities, having a large population and constant commercial relations with Cuba, and other places, in some one of which the disease is constantly prevailing, the cause, confes-

sedly limited in its operation and short-lived, is accidental and foreign, than that it is the result of domestic influences, unaided by a foreign agent. We know that when the epidemic begins early in the summer, it ceases long before frost; and that a freeze invariably arrests it when it originates late in the season ; and yet in each instance, after the cessation of the epidemic, cases occasionally occur for weeks and months. If the cause of these cases be not foreign ; if these diseases have not been contracted from exposure to the infection imported in some vessel, it appears to me more reasonable to assign them to the previous epidemic cause, which in localities and under peculiar circumstances favorable to its preservation, may have existed in a form isolated, but in sufficient strength to excite the disease. A case occurred in the winter of 1842 in Opelousas, long after ice was seen, and several weeks after the fever hud ceased to exist. It was a violent case of yellow fever. No one will admit that the cause of this case was domestic in its origin, or developed by influences native to the town. If the cause of this disease be indigenous to New Orleans, why does it never prevail in the other towns until after it has existed as an epidemic in the city. The violence of the epidemic this year, considered in connection with the known corruption and profligacy of the times, lends, it must be confessed, some importance to the theory, that as civilization progresses, involving social and moral improvements, the disease will gradually disappear. It appears to me that if even in one instance the epidemic in New Orleans or Mobile can be traced directly to a foreign cause, these two cities are as dependent upon importation for the disease, as that they do unquestionably transmit it to all the other towns.

The doctrine that it depends on miasm, heat combined with dryness or with moisture, putrid exhalations from filth in any or all forms, upon organic or inorganic poisons, meteorological conditions of earth and air, as incident to the United States, combined in any possible manner or singly, meets with contradictions, and affords no satisfactory solution of the presence and absence of the disease. Believing in its foreign origin, and knowing the fact that it often delights in its severest manifestations in places remarkable for cleanliness and their general salubrity ; believing that the most rigid observance of hygienic rules, and the freest use of disinfecting agents, are impotent in the prevention of the disease, I would yet advocate as strongly as any one the use of every means to promote cleanliness in the streets and back-yards, alleys and suburbs.

The animalcular origin of this disease is with marked ability presented and supported by Dr. Nott; in which doctrine I would willingly acquiesce, if its distinguished advocate would admit as invariable the foreign origin of the animalculæ. I have long been inclined to the opinion that the time is fast approaching, when most febrile diseases will be attributed, and justly, to a similar cause—to an animalcular origin.

It has been my lot to witness and to pass through, in active professional employment, four severe epidemics of this terrible scourge, and the remarks which I have and shall make are almost entirely suggested by the facts which I have witnessed. It will be readily inferred from what is already said that I regard the establishment of a quarantine around New Orleans as not only indispensable to the safety of the city, but of the whole State; and that it is the duty of the Legislature to pass laws for its establishment and most rigorous enforcement.

From numerous and unquestioned facts, it would appear to me to follow, as indispensable conclusions, that New Orleans and the interior towns, in their liability to yellow fever, hold the same relations; that the disease is not the native product of either; that the cause being foreign, must be imported; that it is innocuous unless it meets a peculiarly affected atmosphere; that this atmosphere is dependent on summer heat of a certain previous duration and on a crowded population; that through the influence of a numerous and crowded population, the cause of the disease may be developed to the extent of producing occasional cases, while the same year it cannot be developed in country towns.

What do we know of the cause of any idiopathic fever? Are we as well acquainted with the circumstances and laws which govern the causation of any of these complaints as we are of those which attend the production of yellow fever? The facts and circumstances accompanying the development of these affections present themselves to my mind with more clearness and precision than those we assign to the development of any idiopathic fever whatever.

The numerous facts already cited appear to me to divest this disease of all the attributes of contagion. I cannot believe, from what I have seen, that the effluvia from sick persons crowded together, however abundant it may be, can per se communicate the disease. I refer to the example of eleven cases in one small house in the country, all of an aggravated character, and the entire exemption of the nurses from the disease. The exclusive occurrence and limitation of this affection to thickly peopled districts and towns and the impossibility of its communication in the country through the direct medium of patients, are attri-

butes which do not belong to contagion. The great characteristic of a contagious disease is a disregard of zones, climates and seasons ; of country and of town. Contagion demands no infected atmosphere; it requires no known adjuvants for its communication ; its action is confined within a narrow sphere or circle, beyond which it cannot travel. Its poison is not self-productive or progressive ; it cannot engender a prevailing distempered atmosphere. In all these respects the poison of yellow fever differs from contagion. There is a resemblance between the contagious and yellow fever, in their exempting patients from second attacks; and yet in this respect the resemblance is not complete, for individuals have been known to have had several attacks of yellow fever. I believe this second attack is rare in persons who continue to reside at the South ; but that a prolonged residence at the North may renew the susceptibility to the disease. One of the severest cases I saw this year was a young man, who declares he had a severe attack in the city in 1847, and who has ever since continued to reside in the South. He was a clerk in a large store, which was closed early. Before the epidemic ceased this clerk returned, unbarred the doors and windows, overhauled the goods, and was thus engaged for two days, exposed to a concentrated imprisoned infection, when he was taken sick and narrowly escaped. Of nineteen individuals, who it is well ascertained had had the yellow fever in former years, who remained in Washington throughout the epidemic, and who discharged towards the sick and dead all the duties which man could perform, not one—though the disease spared scarcely another human being—experienced the slightest indisposition ; a fact strongly in favor of the opinion, that a genuine attack will prevent a repetition of the disease.

I will here introduce the entire chapter, entitled "General Conclusions," from the pamphlet* of the late Professor Carpenter on the Origin of Yellow Fever.

* This pamphlet, entitled "Sketches from the History of Yellow Fever," showing its origin, etc., was published in the year 1844. It may I think with a regard to truth be said to have fallen still-born from the press. It was my good fortune to have enjoyed the friendship of the author, who to a most noble nature and most refined feelings, added all the high attributes and rare qualities of a mind, which, eminently practical in its turn, was indeed strictly disciplined in the rigid rules of philosophic enquiry ; and who, fully imbued with a deep love of his profession, and profoundly versed in all its important collateral sciences, would, had life and health been spared him, have successfully labored for the extension of the medical domain. He died in 1848, in the prime of manhood. This pamphlet, presenting a long array of facts, the result of observation and laborious research, while it rebuked the "hesitation of the selfish," scattered to the winds all the cobweb fancies, the hypotheses and sophistries of the imaginative and scientific. No wonder it fell still-born from the press.

"1st. Yellow fever is a disease not native to the continent of America, but of foreign origin, introduced first from Siam, and afterwards aggravated in its type by the importation of the Bulam Fever. [See Historical Notice.]

"2d. No well authenticated case of the specific disease called yellow fever has yet been known to occur on the American continent under circumstances which precluded the possibility of infection, or even rendered it probable that it originated independent of transmission, either by going into infected localities, opening boxes or parcels from infected places, visiting boats or vessels from infected towns or opening rooms closed during the prevalence of an epidemic."*

"3d. Since the introduction of yellow fever into America, it has always existed on some part of its coast. It prevails almost perpetually near the Equator, where the temperature of winter is rarely low enough to destroy the infection; and it is carried by commerce to the countries lying north during the portion of the year between February and November, and to the regions to the southward from August to May. Thus in Surinam and Demarara it is indifferent as to seasons; in Campeachy, Vera Cruz and Havana it begins from February to May; in the United States from June to October; while at Pernambuco and Rio Janeiro it prevails generally from November to May. In this way it prevails perpetually, changiag its place as the seasons vary, visiting the cities as soon as they are filled with fresh subjects, and where commerce offers facilities for its introduction.

"4th. Yellow fever is a disease sui generis and peculiar, and not a grade or type of bilious fever, as is shown by the fact, that as in the plague, measles, small-pox and other specific and infectious diseases, one attack diminishes the liability to and almost exempts from a second attack; while it in no way diminishes the liability to bilious fevers at all. Neither do attacks of bilious fevers of the severest grades in any manner diminish the liability to take the yellow fever.

"5th. The yellow fever is not produced by a crowded population; neither by heat, moisture, marsh air, miasm, filth, nor by any combination and concurrence of these; otherwise it should always occur, when these concur, and should not occur when the particular combination is absent, neither of which we find to be true.

"6. The transmission of yellow fever depends exclusively on inter-

* In large cities the complicated relations of daily life render it difficult to trace up the infection to its source, and it is only in small towns that we can always arrive with certainty at the desired information.

course and commercial relations; any city being liable to infection in the precise ratio of its proximity to and of its unrestricted communication with ports or places where the disease is epidemic. It is from this circumstance that quarantine derives its pre-eminent efficacy in the exclusion of this disease from cities. Dr. Townsend remarks, that "although the imperfection of medical science places the cure of the disease too often beyond our control, and that our means of counteracting its progress are limited, a wise Providence has indemnified us for these losses by putting into our hands an effectual method of totally preventing its occurrence, by shutting out its introduction from abroad. It is left for us to carry into execution what our own judgment must now teach us is the only resort that is left.' 'I firmly believe that the source of the disease can be only cut off by a rigorous code of quarantine restrictions.' [Townsend, p. 228.]

"7. Yollow fever requires for its transmission a moderate summer temperature, a certain accumulation of people, as the crew of a vessel, or the population of a town, city, etc.

"8. Under certain circumstances of population and temperature, the introduction of persons with yellow fever, or of the air from places where the disease is epidemic, will frequently give rise to new cases, and finally to an epidemic of the disease.

"The infection may be conveyed:

"1stly. In boats or vessels which remain at the wharves, etc., in the infected city, receiving and discharging freight, and then closing their hatches upon the contained air, may become the real vehicles of the transmission.

"2dly. Boxes or bales containing goods, particularly woollens, if packed and closed in an infected atmosphere, may convey the infection.

"3dly. The clothes, bedding, etc., which have been used by persons with yellow fever, have been known to communicate the disease.

"9th. Certain ports are almost annually subject to epidemics of yellow fever. These during the summer we should always regard as infected, and during that period we should enforce quarantine against all vessels sailing from or touching at them. Now such rigor against the towns of the United States would be unnecessary, for should cases of yellow fever occur in any of our towns we should hear of it by mail before it could become epidemic in the place, and often in less time than it would be required for a vessel to arrive here from there. But with the West Indies the case is different; for we learn that at the quarantine in New York, the first intimation they have of the disease

prevailing in these places, is from cases of, or death by it occurring on vessels arriving from there ; so that if we only enforce quarantine against those places where the disease is known to prevail, we may have "cargoes of infection" introduced into our cities before the health officer even suspects any place of being the seat of an epidemic.

"10. The healthy state of a vessel's crew is no proof that she may not be infected; for the crew may all be acclimated, while the infection may be sealed up in her hold, or contained in the cargo, etc., and may only exhibit itself after the arrival at a healthy port, and among the unacclimated persons who may visit, or receive freight from her. [See Bayley's letter, Townsend, p. 92.]

"11. Cleansing and ventilation do not always destroy the infection of a vessel. Therefore quarantine, with its precautionary measures is not a sufficient guarantee for the public health. [See case of the brig Enterprize, also case of ship Diana, which introduced the disease into Brooklyn in 1823.

"12. The only measure by which the public safety can be guarded is, to prevent all vessels coming from sickly ports or places from coming above the quarantine ground, whether their crews be sickly or not. Provision should be made, enabling them to discharge and receive freights, safely and expeditiously, and arrangements should be maintained by which the freight so discharged should be delivered to the consignees as soon as the time expires which may be deemed necessary for its perfect ventilation and disinfection.

"13. The principal difficulties against which it will be necersary to guard in establishing quarantine for New Orleans, is presented by the Tow Boats engaged in towing vessels from the mouth of the river. These boats, by communicating with infected vessels, or towing them up abreast, or even at hawser's length, become liable to infection, and in turn become the medium of infecting the city. The fact is established beyond a doubt, that in two of the years during the time the quarantine was in force here, the introduction of the disease was mainly attributable to these boats ; and experience should lead us to provide against similar disasters in future.

"14. It would certainly be safest to prohibit Tow Boats from towing up vessels from infected places at all ; as in the various turnings of their course and shifting of the wind, it is possible that infection would be communicated at even hawser's length ; at any rate whatever the law provides should be most strictly and scrupulously enforced, and Tow Boats should be placed under the heaviest bonds to answer their observance of all the particulars of the law.

"15. An accurate account should be kept by the quarantine establishment of the state of health on board of each of these boats, which should be subject to the same kind of examination as vessels coming in from sea; they should be required to report the occurrence of any case of fever, or any infectious or contagious disease occurring on board; and during the period from the first of May to the 1st of December, these sick should immediately be sent to the quarantine infirmary.

"16. In case of their crews becoming infected with any of the contagious or infectious disorders contemplated by quarantine arrangements, they should be subjected to the same restriction and rules as the other infected vessels.

"17. They should be particularly prohibited from taking as passengers any person from vessels declared in quarantine, or from receiving any freight, box, parcel, or package, from on board of such vessels.

"18. A landing or wharf should be provided for Tow Boats at a point not in front of the thickly inhabited portions of the city."

With regard to the type or character of the disease which prevailed in Washington, it may well be doubted if in malignancy it was exceeded anywhere else. Dr. Bayles, who, on the part of the "Howard Association" arrived amongst us toward the close of the epidemic and saw a good many cases, told me that neither in the city nor in the towns which he visited during the epidemic, did the disease present any greater violence or malignancy than some of thecases he saw in Washington. If not so fatal, it was as malignant, if not more so, than the epidemic which prevailed in Opelousas in the year 1837.

Perhaps the annals of medicine do not present a more violent and malignant form of disease than that which was prevalent in our late epidemic in many of the free mulatto families living in and near the town. In one house I saw five out of ten die. In all these cases, except those that died before the third day, there were seen after the third day petechiæ with a point of pus in the centre, interspersed amongst a thick crop of small accuminated pustulous eruptions; and amongst these, in two cases, were seen large pustules, filled with white, thick, opaque matter, and some as large as a pea. In some of these cases these large pustules were so numerous as to excite an apprehension of a combination of the varioloid with the disease. What was remarkable, we saw in the same neighborhood the almost complete extirpation of one family, and there was in an adjoining family not a single death.

Amongst the blacks, contrary to what has hitherto been observed, the characteristic symptoms of the disease, particularly at the outset,

were generally most violent. In some of the fatal cases there was hemorrhage from the nose and anus, but never, that I know of, black vomit. Black vomit occurred amongst the mulattoes, but it was not so common as amongst the whites.

Of the different classes, as near as I can form a judgment, the mortality amongst the mulattoes was the greatest; next that of the whites; and it was the least amongst the blacks. Of about 400 who were sick in Washington during the epidemic with a fever, the mildest cases of which presented clear evidences of the epidemic influence, there died in our town 70 persons; that is, at the rate of something over sixteen per cent. This great mortality was aided much by other causes than the actual severity of the disease; as for example panic; the want of good nursing; relapses from imprudence in eating; delay in obtaining medical aid; extreme old age in several cases; in a few intemperance; and more particularly drastic medicines and calomel—medicines presumed by some to be required by the severity of the disease, and freely taken without medical advice.

Whatever may be the cause of this disease, the conditions necessary to its existence, and which it is useless here to recapitulate, are demonstrable. Whenever it has affected a person once, it loses its power of affecting him a second time, or the system loses its susceptibility to its influence. The exceptions to this fact are rare; and we see in many, nay in all epidemics, that some individuals may, with entire impunity, inhale the infectious atmosphere. Even in this town, during the late disease, some few persons who never had the disease escaped. In some instances on the second, most generally on the third day after exposure to the infection, the disease manifests itself. I have noticed that the country cases occurred almost always during the third or beginning of the fourth day after the individuals had left town; very rarely later than the third. I have never known one taken after the fourth day. I know not how to explain those examples in town of speedy and of procrastinated attacks of persons equally exposed to the cause of the disease, except on the supposition that the latter, after absorbing it, excrete it from their systems; that the morbific cause, yielding to the resistance which life invests the tissues with, is discharged without injury, perhaps through the kidneys and alimentary canal. The eighteen persons who in former years had had this fever, and some twenty-five others who never had had it, and all of whom escaped this year, were constantly for two months exposed to the infection in every form and under all circumstances. It would be folly to presume they never took the poison into their systems. Then it was excreted from their systems;

and their continued good health demonstrates, that in these cases the poison could not incorporate with and contaminate the blood, affect the nervous system or paralyze the molecular nutritive actions on the remote tissues of the body. As these persons continued to receive it into their systems, so they continued to discharge it. In conversation with many of them, I have been assured that during the epidemic their passages were occasionally of a dark or black color, and sometimes unusually offensive; that they also observed a peculiar disagreeable smell from their urine; sometimes as if slightly scented with garlic or the root of valerian. They all complained occasionally of a strange muscular debility; some of spasms; these probably were the effects of fatigue.

But unfortunately the unacclimated are rarely able to resist the cause of yellow fever; and here, for the purpose of illustration of what remarks I may make on the mode of action of the morbific cause on the system, as well as to present the history of our epidemic, I will endeavor to describe such symptoms as in a majority of the cases marked the progress of the disease to both a happy and fatal result. Having felt no previous indisposition, the patient in the enjoyment of a good and unimpaired constitution, rises in the morning with a feeling of heaviness or weariness; he is not disposed to take exercise; complains of a dull feeling or one of tightness across the forehead; his appetite, though impaired, does not prevent his eating some breakfast; he walks out, but exercise brings no relief; his skin is moist and cool; by and by he has some pain in the head, qualms of the stomach, and chilly sensations striking through his body; a rigor or a chill, varying in intensity and in duration, compels him to take his bed. Within the space of from a few minutes to an hour, the hot stage comes on. The fever is established. The pain in the head, back and limbs is very intense. The whole surface assumes a reddish color. The eyes more or less red; look dull and heavy, and are the seat of a burning sensation. There is nausea, and the breakfast, which from two to six hours previously was eaten, is readily discharged in an undigested state. Abundant perspiration ensues naturally, or is easily brought on. The tongue is moist, with reddish eyes; the taste is much corrupted; the lips and mucous membranes are very red. His thirst persists, but is easily gratified for the moment. He may throw up at the onset some yellow bile. His pulse strong and hard at first, beats 120 in the minute. His bowels are easily moved; urine at first pale, but soon assuming a red color, is discharged in ordinary quantities, and may impart a disagreeable

smell. Restlessness exists, and the countenance expresses suffering and anxiety.

During the second day the leading symptoms may remain unchanged, or present no important modifications. But yet on a close examination the skin will be found to have lost its heat in a considerable degree; the pulse has also lost its resistance; it gives a wavy, rolling sensation to the touch, and is sometimes fuller; it still retains its frequency, and when the case was progressing favorably, at this early period, could be often detected evidences of improvement; the tongue would be found more coated, without increased redness of edges. The pains much abated; eyes more natural; less nausea; increased secretions, particularly of urine.

On or during the third day, when in the midst of our pestilence, our fears were confirmed or dissipated in regard to the result of such cases as I am describing; when tending to a favorable result, a sensible improvement would present itself in the general appearance of the patient; his prostration would be great, but without restlessness or anxiety; he feels more naturally, and will tell you he is very sick, or that he thinks he is better; the eye looks brighter, the complexion less dusky; the body, shrunken and considerably emaciated, feels warm and hard to the touch; his pains are slight or entirely gone, except that of the eyes or forehead, which never ceases before the fifth or sixth day; his tongue is moist; nausea ceases to annoy; the dejections are more natural; the urine is abundant and of a strong odor; he retains light food, as chicken water, beef tea, strained gruel; his taste is improved. With patience and prudence these symptoms announce a speedy and happy recovery.

But if the case tended to a fatal termination, we saw, before the end of the third day, a manifest aggravation of the symptoms. Great restlessness, an indescribable "malaise," a lethargy of the moral, intellectual and sensational functions ensue and go on increasing. Every organ in the body appears oppressed, as if they had been overtasked; if sensible of his danger, he will sometimes manifest a painful vigilance to catch the opinions of his attendants; or he lays in a listless condition, sleeping unless roused, when, with an apparent effort to gather together his mental faculties, and will answer you sensibly and rationally. Again he falls asleep, or he is heard muttering something indicating an unsettled mind; his eyes have an anxious, wobegone expression; his complexion becomes swarthy; a dirty tinge is visible about the eyes and neck, and a dark red streak encircles the neck, extending up to the ears. The tongue is of a dusky red, often dry and smooth; the throat

is moistened by a tenacious, viscid fluid, exceedingly annoying; thirst is considerable, but a sensation of fulness of the stomach makes him afraid to drink. The gums are swollen, soft and red. Vomiting ensues, consisting of the contents of the stomach and mucus, and is very distressing to the sufferer. The bowels are full and doughy to the touch, and discharge frequently small quantities of thick mucosities, or a matter more consistent, like dirt and water mixed. The entire absence of bile showed that the liver had ceased to act. The urine ceases gradually, and no desire to urinate is felt. These symptoms foretell the approach of black vomit, and with it the patient may live for many hours, sometimes breathing out his life calmly and equably and in the full possession of his senses; or with delirium more or less wild; or in deep coma, or writhing with convulsions, he gives up the ghost.

This awful disease, running its course in a few days, is now considered an idiopathic fever; that is, one consisting in functional, not anatomical lesions. The entire absence of structural lesions on dissection, in many most violent cases, the speed with which the disease hastens to a termination, the rapidity of death, the rapidity of recovery—all repudiate the idea of inflammation, and point emphatically to a poisonous impression made on the tissues and organic functions, involving the whole system in disease. The poisonous shaft has sunk deep into the vitals; the sources of life are vitiated; the organs which furnish the aliment of life, are stricken with paralysis of their functions. Let us, keeping in mind the phenomena of this disease, endeavor to establish some theory of the modus operandi of the cause. We have seen that beyond a doubt this cause, in many individuals who never had the disease, as well as in those who have had it, is innocuous in its effects. It unquestionably is introduced into the system of these persons; it traverses the body through its sanguineous current; and with out affecting the latter, or that other system, which, like the watchful sentinel, is never slow to utter the cry of alarm whenever nature, in the remotest ramifications of her being, is affected—the nervous system, it passes out an innocuous excretion. With the masses of unacclimated persons the result is different. They absorb the poison. In many instances the health-restoring power of nature successfully for a time combats against the deleterious effects of the poison; but finally this power succumbs. The infection penetrates the system; it attattaches itself to, it incorporates itself with, it spends its power on the remote tissues. The molecular movements, the nutritive actions taking place in the remote tissues between the arterial blood and their elements, are more or less interrupted; and in the fatal case which has

been described, there is no proof, there is not a symptom which indicates that they have throughout the disease even unto death been resumed; on the contrary, there is every evidence that they have very speedily been entirely arrested. With and in proportion to the impairment of the action of the nutritive system, there ensues incessantly a derangement of every organ of the body; because the blood is thereby deprived of many elements, and to an extent proportioned to the intensity of the lesions of the nutritive system that are required for the formation and constitution of its natural venous and arterial character; and with the announcemeut of pain in the head and spinal marrow, the energies of the heart are called into high action, and what is called fever, an effort of nature to expel an offending cause, is seen to exist. We see in numerous cases, like the one detailed, not a symptom marking structural lesion throughout the disease, and the nervous system, impaired no doubt in its functional action, appears to me in many instances to manifest less derangement than any system or organ of the body. As the disease progresses with giant steps and in a manner visible to the eye, to a fatal termination, disclosing the deep and fatal hold it has on the functions of life, parching up the tissues, arresting the secretions, vitiating all the fluids of the body, we often see the nervous system apparently unaffected, for it announces no sensational or intellectual alienation; indeed it would appear from some innate force to retrieve its energies, and the mind will be brighter and calmer as death approaches. As said, all the secretions in the body are diminished in quantity and vitiated in quality. If we draw blood from the arm at the onset, we shall in all cases find that this fluid has already undergone peculiar modifications of color, consistency and of coagulability. It has already lost its uniform, dark, venous hue, and it presents a striated appearance, with a mahogany color; being left at rest, it forms a semi-consistent coagulated mass, with a scanty supply of serum. As the disease progresses, we shall find this fluid still more changed; its color is now red; its consistence is much diminished; its coagulability lessened. As it proceeds to a fatal termination the blood continues to present more marked evidences of a departure from its healthy condition. It loses its reddish hue, which is replaced by that of a dirty pale, yellowish color; the whole mass apparently has undergone a complete dissolution or change of character. It sometimes has lost entirely its property of coagulation, or if it has not entirely, it presents no healthy looking serum. In no other disease—in no other condition of the system, have I ever witnessed this yellowish color and dissolved condition of the blood. For my part I do not doubt its existence in all bad and

fatal cases of purely idiopathic yellow fever, Now it is we see the skin presenting a dark white or waxy hue, or a pale yellow tinge becomes more or less visible about the breast, neck and eyes. The patient often presents all the appearance of one bloated by intemperance, and whose system seems oppressed by prolonged dissipation. A heavy load seems to press upon every organ, arresting every function except those indispensable to life itself. The diluted blood finally exudes through the weakened mucous membranes, no longer affording a barrier to its exit.

In the matter discharged from the stomach, mucous flocculi, resembling perfectly pieces of spider's web, of a sooty black, settle at the bottom. The coloring matter increases until genuine black vomit is discharged in more or less abundance. In black vomit, the color is derived from the blood, which, mixing with the contents of the stomach, more or less acid, gives sometimes a black, sometimes a reddish color. In some cases this matter is darker than the color produced by mingling blood with acids. But again, towards the close of the disease, in same cases, as the heart loses its force, and the blood seems hardly to circulate in the capillary tissues, another change takes place ia the color of the blood. It now assumes a dark hue. A few hours previously of a yellow color, it is now discharged from the mucous membranes of a dark hue; and it has seemed to me sometimes to grow darker even on exposure to the atmosphere.

This remarkable phenomenon appears to me to present in the symptoms of the disease an explanation of its production. In the entire prostration of the digestive organs and of the vital organic actions in the remote tissues, the element of carbon, so necessary to the functional actions of all the secretory organs, ceases to be present in the blood. The lungs in the meantime appear to be endowed with a power superior to that they possess in health. The blood receives rich supplies of oxygen. In the progress of the disease we mark the period of stimulation and its gradual abatement; the supervention of entire prostration; the debility; to use the expression, the fatally intoxicated condition of every organ and tissue of the body. The tissues become flaccid, and soft, and swollen; sometimes we feel distinct crepitation, and we discover gas in the swollen blood-vessels. It appears to me now that the superabundant oxygen is forced to leave the blood, or if even previously it has commenced to diffuse itself through the tissues, that now the natural affinity between it and carbon, existing perhaps abundantly in the tissues, is manifested in a free evolution of carbonic acid gas. Entering the blood, the acid restores the venous dark color. I

have come to the opinion, after the closest observation in my power of all the symptoms of the disease and the best reflection I could give to every possible mode of their production, that the effect of the cause of yellow fever is in the mild cases an impediment or interruption, in the severe, a prostration or suspension of the nutritive actions which take place between the arterial blood and the elements or atoms of the remote tissues, in which operation there is a depuration of the useless and effete matters, and a substitution of new vitalized elements ; in the matter depurated, the most important and without doubt the most abundant element, is carbon ; one which largely predominates in the constituents of bile, and enters into urine to a certain extent, and one whose presence in blood must be indispensable to life. Observe the character of the blood in all of its conditions in the different phases of the disease, its loss of venous and arterial color, of consistence and of coagulability —the diminished and perverted secretions—the long array of fatal symptoms, and it appears to me impossible to avoid the conclusion that all this results, and from necessity, from super-oxygenation of the blood and from the absence of carbon, the indispensable element of most secretions, and the antidote of oxygen in the blood. Other most serious changes no doubt occur in the blood, but we can hardly conceive that any could be more deleterious than such as would necessarily result from the condition I presume to exist.

I have sat and watched at the bedside of many, and I have thought that I could mark the progress of the disease both to a fatal and a favorable result ; in some instances silently and insidiously it proceeds ; again, with the boldness of an enemy aware of its mastery, it unmasks itself and exhibits the manacles with which it has bound the energies of nature ; and then, as if to diversify the intensity of its character and the awful majesty of its power, it attacks some organ or system, and will induce such agony as will beggar all description ; and again we think we can see the antagonistic forces arrayed against each other—that of nature, contending with all her might against the enemy—now thwarted in her efforts—finally successful ; and during the contest hope and fear alternately prevail, for in it no quarter is asked or given, and the result must be life or death.

Yellow fever, it is true, runs a rapid course; but nevertheless I have satisfied myself that a loss of flesh, a shrinking of the tissues was always a favorable symptom ; and on the contrary, a retention of embonpoint, aocompanied as it is almost always with a doughy sensation on pressure, was unfavorable. With emaciation the secretions are abundant and hemorrhage rare ; and yet under such circumstances we often

see fever continuing for several days; but it is a frank, open affection, which readily yields to rest and mucilaginous drinks.

A Dr. Stephens wrote a book for the purpose of showing (if I remember correctly, for it is now twenty-one years since I read the book) that yellow fever, the autumnal bilious fever, Asiatic cholera, etc., presented but one great pathological condition, to wit, an excess of acids in the blood, or in other words, the absence of the saline elements. The remedies were of course such as were calculated to supply the absent ingredients in the vital fluids. Now the effect of acids on the blood is to bleachen it. The venous blood is supposed to derive its color from the carbonic acid, the result of the combination of the oxygen taken into the blood in respiration, with the carbon received through the chyle and lymph; or more directly eliminated from the tissues in the process of nutrition. Now in yellow fever the appearance of the blood shows that there are no permanent acids in it whatever until after decomposition of solids and fluids commences. If acids are formed during the first days of the disease, they must combine immediately with some base, and form such salts as impart to the blood the red or yellow hue.

My object in dwelling upon the chemical changes in the blood is to call the attention of chemists and physicians to a subject, a full investigation of which will throw a flood of light not only on the disease in question, its cause, pathology and treatment, but will reflect important light upon all diseases, particularly of the class of fevers. We know the symptoms, "the cries of suffering nature" in yellow fever, and the question of importance is, can the chemist enlighten us as to the nature of the changes which the solids and fluids are experiencing during the whole progress of the affection, and knowing, to learn from them the means of counteraction. Is the doctrine of Stephens, which I remember reading with a good deal of interest, and which derives some semblance of respect from the appearance of the blood in our congestive fever and in Asiatic cholera, correct with regard to yellow fever? It has never, that I know, been refuted. Perhaps its ridiculousness has rescued it from refutation. How are we ever to acquire true knowledge of the nature of those diseases in which functional action is alone involved, and from which result diminished or vitiated secretions and impeded or arrested molecular actions, if the chemist shall not come to our aid and disclose the true constitution of the body?—in disease, what healthy elements are absent, what noxious ones present; the excess or deficiency of any; what new compounds may exist. If the morbific causes of these diseases be beyond the reach of chemical analysis

let us yet hope that persevering, systematic and successful investigations will be made on the perverted constitution of the solids and fluids, establishing results or facts which will present the characteristics of fevers; what it is that tends to eruptions, to putridity; upon what depends fever and congestion; the phenomena of continuousness and remissions and intermissions; and which will form a better starting for reasoning on, if they do not explain the nature of causes. Diseases are but effects, and though the cause may be the same, individuals, in consequence of a difference of constitution and diathesis, will present modifications of symptoms, and some more symptoms than another, justly attributable to the same cause. Directly opposite symptoms ought, in my mind, rather to indicate a difference of causes than mere modifications of tissues and systems; and I believe the time is approaching when many diseases now assembled into one group or family, considered mere modifications of a type, having a presumed identity of cause and similarity if not identity of nature, will take separate and distinct places in nosology.

Returning from this digression, it is certainly a matter of paramount importance to determine whether or not there is in this disease an absence or deficiency of carbon in the blood, resulting from an impairment or suspension of the functions of digestion, nutrition and absorption. I will simply state that the diluted condition of the blood, its loss of the venous and arterial color; the diminution, vitiation and arrest of the secretions; indeed all the symptoms appear to me to sustain the ideas I have expressed, to wit, that in yellow fever the great primary derangement is a modification of the nutritive atomic actions, from which result two other elements of disease, persistent in their nature, and which, though but secondary links in the chain of effects, become in their turn the immediate and prolific causes of that long train of symptoms, which, according to their intensity, stamp upon the disease its varied character, in its different periods and forms, and that they consist in—1st, a deficiency or absence of carbon, and 2d, a superabundance of oxygen in the blood. In yellow fever the great primary and persistent morbid elements, the first links in the chain of morbid effects, the immediate and prolific causes in their turn of that long train of symptoms, which give to the disease oftentimes an aspect the most terrible and trucculent, consist in an absence or deficiency of carbon, and a superabundance of oxygen in the blood.

I will now give the history of a few cases, whose leading peculiarities and symptoms and whose treatment are fresh in my memory. The wife of Lelasling, aged about 25, mother of four children, a large

fat woman, about three-fourths white, of a temperament bordering on the lymphatic, complained of general indisposition on the 30th of August. Still feeling unwell, she on the 31st, in the absence of her cook, prepared breakfast and dinner for the family, of which latter meal she partook with a ravenous and insatiable appetite. She ate enormously; four times as much, she told me, as ever before at one meal. Two hours after she was taken with a violent ague. At 7 o'clock that evening she presented the common symptoms of the fever; hot skin, redness of face and eyes, with a dull heavy look, intensely severe pains in the head, eyes, back and limbs. Pulse was not full, but very quick, great restlessness, slight evidences of delirium; tongue clean, with reddish edges; intolerable nausea, with a sensation of suffocation, but no vomiting. Prescribed immediately a small dose of ipecacuanha, which, with a draught of warm water, excited vomiting, in which she discharged an incredible amount of undigested food. Ordered a foot bath; pills of rhubarb, comp. ext. of colocynth and blue mass, five of 2 grains each, to be followed speedily by injections, and 20 ounces of blood to be taken from the back of the head and neck; teas and common drinks ad lib.

September 1st, 6 o'clock in the morning. During the night there was perspiration and purging, and she slept, as was said, well. The cupping was neglected. I found her this morning worse in every respect. Pains less violent, tongue redder, nausea, quick pulse, dry skin, marked disposition to sleep. She was cupped immediately on the back of the head, on the neck and between the shoulders; blood not flowing freely in consequence of obesity and flaccidity of tissue. The cups were repeated in the course of the day. Bicarb. of soda with sweet spirits of nitre, emollient injections, foot baths, and cold applications to the head, etc., during the day, failed to ameliorate a single symptom. During the ensuing night coma with convulsive twitchings of the muscles, occasionally amounting to spasms, ensued, and suppression of urine.

In forty hours from the time of her attack she was in a moribund condition, and in a little over two days she breathed her last. An irresistible desire to sleep, the patient hardly being able to keep awake to answer questions, though when fully aroused the intellect and senses appear natural, is a most unfavorable, if not one of the fatal symptoms in this disease. A full development of the adipose tissue is one of the most unfavorable circumstances in the condition of the patient. The disease is almost always fatal in very fat subjects, according to my experience. The lymphatic or scrofulous temperament, and the venereal

poison in a secondary form, are also very unfavorable. The compli cation of the latter is always, indeed, as far as my observation extends' fatal.

2d. Mr. ———, aged 30, of medium size, very strong and robust, of sanguineous temperament, a working man, occasionally will take a frolic. At 5 o'clock on the 5th of September, was taken, without premonitory symptoms, with a chill. The hot stage soon came on, and with it great suffering. At 10 o'clock at night I found him slightly delirious, with high fever, pulse full, strong and resisting and 110 in the minute. Tongue clean and red, some nausea, excessive pains, great restlessness. The blood looked as if it would escape from the lips and skin of hic face ; eyes were very red, but dull. I immediately bled him to about 45 ounces, with general but moderate relief; ordered a hot mustard pediluvium, warm teas, and a dose consisting of 20 grains of quinine, one and a half of opium and 10 grains of blue mass, to be followed in four hours by a table spoonful of castor oil. He was restless during the night, but sweated freely and was moderately purged.

Early next morning, 6th September, I found him restless, with dry, hot skin, full, strong pulse, great pain in the head, &c. He was bled to the amount of 25 ounces with relief, and ordered mustard pediluvia, warm teas, mucilaginous drinks, with sweet spirits of nitre, bicarb. of soda and injections. In the course of this day the pain in the head returning with severity, he was cupped in the temples with complete relief of the pain.

During the second and third days his condition was far from being satisfactory. Nausea continued to increase ; his restlessness was such that he would go from bed to bed. His complexion became dark and swarthy, his eyes very dull ; he would draw deep sighs, and complain of very distressing sensations in the stomach. His mouth was dry and his throat moistened with a sticky, viscid mucus. Towards the fourth day he began to throw up serous looking matter with black cobweb deposite. The chloride of soda, in half teaspoonful doses, partly diluted, was administered every two hours, and between them small quantities of the carb. of soda, with the occasional addition of some morphine. Brandy frictions and injections were freely employed. He had now also almost complete suppression of urine ; what he discharged being very small in quantity and very red. He bled also excessively from the nose. Black vomit ensued in abundance, and in a genuine form, but under the use of the remedies above prescribed it entirely ceased before the close of the fourth day.

But now appeared other most alarming symptoms; old venereal sores broke out afresh. The scrotum began to redden and swell, and wherever the skin had been broken in other parts of the body it began to inflame. An erysipelatous inflammation succeeded them, and around those believed to be venereal it spread with great rapidity. The sores themselves began about the seventh day to assume a gangrenous aspect, the scrotum to swell more and more, and also to be threatened with gangrene. In the meantime, that is, during the fifth and sixth days, the patient in all other respects appeared to be doing very well. His evacuations had become healthy, the urine abundant, appetite good; indeed he expressed himself as being well, with the exception of his sores, which were painful.

On the eighth day, notwithstanding the use of bark and charcoal, creosote and chloride of soda, red wine, beer, carrot, and vegetable poultices, and the free use of tonics and stimulants, the parts began to mortify and he died on the ninth day of his disease.

Another case, which I did not see, occurred, that was represented as very much like the one given. In both subjects there was no doubt of syphilitic taint; and to it I attribute death in both instances. To the chloride of soda, which was freely given in my case, aided by the free use of brandy in injections and by frictions, I also attribute the entire cessation of black vomit.

3d. M. Daneo, about 40 years old, of robust frame, with great muscular development, of a bilious temperament and excellent constitution, had been absent from town amongst friends twenty miles distant. On the morning of the 5th of September feeling badly, he started for town. He was soon seized with a severe ague, which compelled him to lie for several hours on the ground. He reached home with difficulty, riding on horseback, by 10 o'clock at night and immediately sent for me. He told me he had suffered a great deal and had got home with great difficulty. In fact, on his arrival he had to be lifted from the gate to his bed, for he could not walk. At eleven o'clock at night, about fifteen bours after the chill, his pulse was neither strong nor quick, though full; skin moderately hot; tongue moist but red; great thirst and restlessness. He had some pain in his head and back; he complained of an insupportable sense of fatigue; he felt sore and bruised, as if he had been beaten with a stick.

Ordered an injection with mustard, 20 grains of sulphate of quinine, 2 of opium and 10 grains of blue mass at a dose; mustard foot bath and peach leaf tea, cold or warm as he liked, with other drinks, water

in particular, ad libitum ; also to be sponged with weak brandy and water.

Next morning, at an early hour, found that he had perspired moderately ; his face and eyes were very red, skin dry and hot, severe pain in the head ; pulse about 90, full and strong ; restless, thirst, slight nausea, with the same feeling of fatigue. He insisted on being bled, to which I consented and drew away 35 ounces of blood with marked abatement of pain. Prescribed 20 grains of quinine, to be followed, after free perspiration, with oil, and the common adjuvants. All the medicines seemed to have most happy effects, and he looked and expressed himself much better.

During the third day (17th September) of his disease nearly all the symptoms were satisfactory; very little pain, no nausea, all the organs seemed acting well, but there was that extreme lassitude and feeling of soreness, of which he never ceased to complain.

On the morning of the 8th, the fourth day of his sickness, I found him quite lively ; he represented himself as being well, and indeed after a minute examination I saw no indication for any other prescription than rest, perfect quiet and chicken water. But he still felt fatigued, and as if he had been bruised, and his complexion was of a waxy hue. A few hours later I dropped in to see him, and notwithstanding my great anxiety for him, his account of himself and the aggregate of his symptoms, deluded me with a conviction that his recovery was certain. But at 2 o'clock of the afternoon this delusion was entirely dissipated. The insidious disease could no longer conceal its fatal inroad on life itself. At the door the nurse told me Mr. D. had wandered occasionally in mind since I saw him ; that he slept too much. I found him asleep, and his face bore that peculiar expression, with a falling of the muscles, which indicates the approach of death ; his complexion was more waxy ; his nose, the tips of his fingers and toes were of cadaveric coldness. I asked if he had vomited, and was shown the vessel in which he had vomited black matter. I awoke him; with a marked effort he collected his thoughts and answered me rationally ; he complained of great malaise, an indescribable weariness ; otherwise he said he felt well. All hope for him was fled. A consultation was asked for by a friend. The consulted physician, inexperienced in the disease, ridiculed my prognosis. It was impossible Daneo could live ; but in six hours afterwards, black vomit having increased, the poor man perished. Great soreness of muscles, with a feeling of excessive fatigue, I have noticed in some cases, and I have always regarded as very unfavorable. But in the pre

ceding case I attributed it to the long ride he took on the day he fell sick, after his fever commenced.

Nelson King, small in stature, of a nervo-sanguineous temperament, feeble muscular development, 30 years old, cooper by trade, of steady habits, was deeply afflicted by the recent loss of his wife ; fell sick on the morning of the 8th, presenting all the symptoms of the prevailing disease. He was bled in the beginning, and immediately took twenty grains of quinine. This medicine caused some perspiration with the assistance of foot baths and warm teas, but in other respects its action was evidently pernicious. The brain became bewildered, he would kick the blankets off, his skin was soon hot and dry, his pulse though continued to be full and to beat at about 100 in the minute ; his tongue became dry. I resorted to cups on the back of the neck, mild purgatives, as manna and rhubarb infusion, gave freely mucilaginous drinks, with nitre and soda alternately ; repeated mustard pediluvia ; emollient injections. He was well nursed, but black vomit declared itself on the fourth day. He threw it up for 24 hours ; at first in small quantities, presented in the form of the dusky black flakes of soot, or pieces of spider's web of the same color, sinking in a sero-mucous matter ; soon the whole matter discharged was intensely black, then it gradually changed to a substance more like dissolved incoagulable blood, which finally ceased entirely. During this period the quantity of urine passed was so small as to indicate, in connexion with an absence of much desire to urinate, almost complete suppression of the secretion. In cases which had occurred in my practice already, the common remedy recommended in black vomit, acid chloride of soda, in which I reposed great confidence, had entirely failed, and I was induced to try the creosote. This medicine was resorted to immediately and in drop doses, in four table spoonsful of water, repeated every hour three times. Its effect was most happy, for it relieved the patient of the fulness he complained of in the stomach, changed immediately the secretion of the throat and mouth, retarded the efforts at vomiting, and under its use, with the aid of brandy toddy, brandy injections and frictions after the first three doses being given at more and more lengthened intervals, the black vomit entirely disappeared, urine was discharged and soon in abundance ; his stomach relieved ; then water gruel, and he rapidly recovered.

On the seventh day from his attack, that is on the 14th of September, he was discharged cured. He regained his strength very rapidly. In a few days he was riding about and superintending his business. About

the 25th there occurred a sudden change from very warm to very cool, almost frosty weather. I met him returning home from a long ride late one night, dressed in summer apparel, in frosty weather. The next day, the 27th of the month, symptoms of tetanus came on. In the course of this day they rapidly augmented. Hot fomentations, opium and quinine, assafœtida, &c., were used, in vain. His spasms affected all the muscles, voluntary and involuntary, except the heart, and in four days he died in great agony.

There were three circumstances which I thought imparted considerable interest to this case, to wit, the manifest injury which resulted from quinine, the success of the creosote in arresting black vamit, and the occurrence of most violent tetanus, developed by cold, and to which the previous attack of fever must, in my opinion, be considered the predisposing cause.

Mrs. Hinckley, aged about 30, and Mr. N. Offutt, aged about 45, each of weakly constitution, of impaired or feeble digestive organs, and possessing what may be called the dry temperament, were attacked—Mad. H. on the 19th and Mr. O. on the 24th of September. The disease in each case presented almost identical symptoms. There was in each case constant and distressing nausea, which resisted every remedy used, as soda, morphine, hyoscyamus, peach leaf and cinnamon tea and charcoal, effervescing draughts, chloride of soda, warm and cold fomentations and mustard over the stomach, and ended in black vomit towards the close of the fourth day. Pulse frequent, beating 125 per minute; eyes dull; great languor and occasionally much restlessness and malaise; skin dry and warm but not hot; dull heavy pain in the head, eyes and back. With the exception of 20 grains of quinine given Mrs. H., and cups to relieve its distressing effects, the same routine of medicines was used in both cases; mild purgatives and injections and pediluvia need only to be added to those above mentioned. So soon as black vomit declared itself the creosote was resorted to, in doses of one drop in two table spoonsful or more of water, and repeated every two hours, for several doses.

In the case of Mr. O. the black vomit was speedily arrested. Mrs. H. threw up an immense quantity before it was stopped. Each of these very intelligent patients was aware of their danger, for they believed the black vomit a fatal symptom. They recovered very slowly but completely. Mrs. H. during her sickness complained of a severe pain in her right knee, which finally ceased. After her recovery the calf of the same leg became swollen and extremely painful; œdema and erysipelas ensued; finally pus collected in a diffuse form under the muscles.

She suffered for three weeks intensely. So soon as fluctuation was discovered a puncture was made ; an immense quantity of very unhealthy pus was discharged, and she has rapidly recuperated since. Mr. O. presented a very languishing condition for many days after a cessation of all the worst symptoms. The clean, extremely dry and cracked tongue, and continued perverted taste, indicated a sad condition of the stomach. The subnitrate of bismuth, in 100 grain doses, administered in a saucer of gruel three times a day for two days, was followed by a great improvement in the appetite and digestion and by full secretion of mucus in the mouth, and by it I believe his recovery was greatly hastened.

Mrs. Read, aged 18, brunette, medium size, constitution excellent, of moderate embonpoint, recently married ; had waited a good deal on the sick ; without noticeable premonition was attacked with chill, which soon ended in a hot fever on the morning of the 13th, with all the symptoms in great severity of the prevailing epidemic. Thirty ounces of blood were drawn from her arm, with great relief and softening of the pulse. A purgative of blue mass, extract of juglans and rhubarb, was given, which operated speedily and effectively. Six hours after her attack the pains had returned, with nausea and restlessness. The foot baths, etc., had been freely used. She was again bled to the amount of twenty ounces, with moderate relief; she fell asleep, and her arm continued bleeding and before any one was aware of it a considerable quantity of blood was lost, but with the happy effect of almost complete relief from all pain. Her nausea was distressing ; all taste completely lost. I gave her brandy and water to sip, and every two hours 10 drops of chloride of soda, to be continued all night unless it should clearly disagree with her. Next day nausea much diminished, general appearance better, eyes brighter ; tongue furred, moist, and redder than in health ; urine was freely discharged ; skin warm and moist ; less restlessness. Ordered the brandy and water to be taken regularly ; occasional injections ; soda powders were occasionally given to promote alvine evacuations. Nothing else was given. She rapidly grew better and better, and though the case commenced with considerable severity, the recovery was the most speedy and happy one I ever witnessed.

Mr. R., the husband of the preceding patient, immediately after her recovery was also severely attacked with all the symptoms marking the incipient stage of the fever. His attentions to the sick had been unceasing by both day and night, of excellent constitution, and accustomed to

labor ; he hoped to avoid the disease by means of occasional purgatives, and had been well purged two days previously. At the onset he informed me that his urine was very freely secreted. Prescription, hot foot bath, mucilaginous drinks with sweet spirits of nitre, towels to his head wrung out of cold water, and a mild purgative injection during the day. Under this treatment, which was continued without intermission, all the symptoms, as fever, pains in the head and back, nausea, redness of face and eyes, considerable restlessness, etc., gradually abated, and the patient was able to sit up on the fifth day, complaining only of great prostration. The above remedies were continued because he perspired freely, and because of excessive urinary secretion ; with which was hourly perceptible a gradual improvement in the condition of the patient. His emaciation was excessive for this disease ; prostration was also most marked. A case of rapid recovery from a pure attack of this disease, is, in a pathological point of view, as interesting as one in which we mark every step the case takes to death. In the one case, as health returns, the secretions are more abundant and natural, and their constituent elements are derived from no food taken in from without, but from the remote tissues, whose molecular movements, once interrupted or paralyzed by the morbific cause, are now renewed with increased activity by the influence of the fever itself, and the elements necessary to secretion are furnished to the blood ; and this fluid, by means of the carbon and other elements it receives, is relieved of its surplus of oxygen; and the energies of life now sink to a state of prostration proportioned to their previous intensity of action. It is but a temporary debility ; the machine has received no injury. It is speedily reanimated. But in the other case, progressing to a fatal termination, we see from the beginning that a contest exists within the system. All the powers of life manifest an intensity of action and the presence of a formidable enemy. The patient, until stricken down, has been indisposed for hours, or he has cheated himself with the hope that he was well. A friend tells him, hours before the attack, to lie down and rest, for he must be sick, or been indulging too freely in wine, for his eye is dull, his cheek injected ; his walk is languid. Already the blood is corrupted, from a diminution of the necessary supplies from within, and from the constantly augmenting proportion of oxygen. Fever comes on ; the heart is tumultuously and violently roused ; and now the secretions are diminished and vitiated from the start. The powers of life grow weaker and weaker. Every organ appears inebriated ; a viscid mucus sticks in the throat and glues together the coats of the intestines. Medicines are impotent. Incipient decomposition ensues ; black blood exudes from the mucous membranes, in dependent parts it

stagnates and forms black spots; and death under these circumstances soon closes the scene. I have been told that the lamented Doctor Diggs, after wiping black grumous blood from his lips, observed to his attendants, "this is the matter of black vomit; I know that I am dying, but it is remarkable that I do not suffer, nor feel disease in a single organ in my body."

Two little girls, each aged ten, of the lymphatic temperament, blue eyes, red hair, passed through the disease with apparent rapidity. The daughter of Capt. Marsh, on the fifth and sixth days discharged by the mouth and anus an incredible quantity of pure black vomit. The other, daughter of Mullen, discharged by the bowels a considerable quantity of this black matter. The creosote was administered to both, and for the whole period to the daughter of Marsh, while she discharged the black matter. The prescription was 6 drops to 8 ounces water and a tea spoonful every two or three hours. Brandy frictions and stimulants were freely employed. To one champagne was given. All the unfavorable symptoms ceased, and for two days they both promised to recover, but then an irritative fever commenced, with restlessness, pain in the head, red tongue, hot fever, small frequent pulse, irritable stomach, beating of carotids, delirium, finally coma, and after six or seven hours they perished, having presented all the symptoms of typhus fever.

Mr. B——, aged about 30, of medium stature, dark bilious complexion, intemperate, and for several weeks more so than usual. The appearance of this man, who led a rather solitary life, and seems to have been intimate with nobody, had attracted attention for some days, in consequence of his haggard looks. He had had a chill, with some febrile reaction, several days previous to his attack, for which medicine had been prescribed. He was reading a newspaper when he fell from his chair in a fit, marked with spasms, which seemed to affect more particularly the muscles of the upper extremity and face, and with insensibility. There was some froth in the mouth, which could be opened, but he could not swallow; his eyes half opened, pupils very much dilated and contracting on the approach of the candle. The spasms gradually diminished; pulse at first was very weak, but in a few hours it argued considerable fever; then again it sunk. In the course of about fourteen hours he looked like one in a comatose condition, and died in about 28 hours from the commencement of the fit. I am very sensible of the great imperfection of the above description. I saw the case only twice, in company with Dr. Bayles, who arrived in Washington towards the close of our epidemic, and to whom is due every consideration of res-

pect and the deepest gratitude for his assiduous and successful attention to many sick persons. In the sufferings of this unfortunate man we did not discover the leading symptoms of either apoplexy or epilepsy. The case resembled neither in its general appearances or symptoms, which indicated congestion of the base of the brain and spinal marrow. It was known that he had been complaining for several days, and that the brain and mucous system had but short respite from spirituous stimulation.

Another case, and in my opinion an illustration of the preceding and fully worthy of record, has often been detailed to me by a venerable gentleman, now deceased. He and another clerk were serving a great many years ago together in a mercantile concern in New Orleans during the prevalence of a very malignant epidemic. Late one night he parted from his comrade, who did not complain of being sick. The latter next morning did not arrive at the usual hour at the office, but finally entered in a hurried manner, with his clothes soiled with mud, with a haggard look and injected eye, and approaching his employer, told him that he had left Louisville that morning, and was happy to present to him the compliments of his friends in that city. His appearance inspired great anxiety ; he confessed that his unprecedented feat had greatly fatigued him, and consented to take repose on a bed in an adjoining room.

Dr. Flood, a name familiar to the old inhabitants of the city, was immediately called in. After seeing the patient, he was about leaving when he was reminded that he had made no prescription. I can make none, says he, other than to advise the speedy engagement of the services of the priest and undertaker. In fact the poor man died in a very few hours.

Both of the above cases have their parallels in the history of this disease. For my part I do not doubt that the death of B—— was due to the poison of yellow fever, which had been preying on his vitals for several days ; that the alcohol had retarded the eruption of the painful malady, stimulating his secretory organs, imparting activity to the nutritive actions, and doubly supplying carbon to the blood, thus enabling him to contend for hours, nay for days, against the enemy, which though never ceasing to make its forays on his system, finally overcame perhaps in a momentary withdrawal of the alcoholic stimulus, all opposition, and involved in irretrievable ruin certain portions of the nervous system, which, on the cessation of artificial excitement, were left an unresisting prey to any general morbific influence to which the system might have been subjected.

In this case, the base of the brain—the organs of sensuality and of the animal passions—those organs which make man the mere animal, and the abuse of which make him a brute; organs no doubt predominating in the case, were less able to resist the morbific cause, and the man died foaming at the mouth, with head thrown back, with convulsive twitchings, with an entire loss of the little intellect he ever had.

With regard to the other case, there seemed to exist a mental intoxication, an aberration of mind. His habitual application to business, involving a constant exercise of thought, connected in all probability with a peculiar mental idiosyncrasy, rendered the organs of thought more obnoxious to the cause of the disease, and he manifested derangement, perversion of the mind.

In 1837 a case of peculiar interest occurred in my practice. The gentleman (subject of the case) in moments of relaxation was the gayest of the gay; he was engaged in very extensive commercial business in New Orleans; nor had his house escaped the universal blight with which the whole commerce of our country was at that time visited. His attack was ushered in with an exhibition of gaiety and an uncontrolable desire to dance. This peculiarity ceased in a few hours; indeed it was replaced by deep gloom and despondency.

I feel almost an irresistible impulse to add other cases, which appear to me to merit attention, from the light they throw upon the disease. But I have already written much more than I had any expectation of doing at the start; for the moment I overleaped the province of the mere narrator of events witnessed. I have become involved in the discussion of abstract subjects, and I have announced opinions requiring support and illustration, and which I believe highly important, even though they may not attract the notice of any one. I present these opinions with all humility, but with the satisfaction that in doing so I have discharged a duty; and with the conviction, that if they should attract attention and merit investigation, however visionary they may be, the facts upon which they rest will be found no "phantom of the imagination," but nature's truths.

I must now proceed to a hasty consideration of the leading symptoms and their modifications, as manifested in our late epidemic, and finish with some remarks upon the treatment, or the chief remedies used.

Premonitory symptoms, which in a very large majority of cases were complained of, have been noticed. It has happened to me frequently to beg individuals to cease from exertion, to seek repose and quiet, and to censure many after the disease was on them for not taking to their beds. Such facts are constantly observed in every epidemic of

yellow fever, and they are peculiar to the disease. One of the primary effects of the cause of this disease—an effect already noticed—is a delusive hope, oftentimes indulged in, and manifested frequently in a strange exhibition of gayety, even until the patient is stricken down and forced to take his bed.

The fever sometimes came on very slowly, and you could mark every step it took in its development until it became violent and confirmed; and in some of these cases there was no chill at the beginning. But in nine-tenths of the cases a cold stage, greatly varying in intensity, from simple stretching or cold sensations to agues and rigors, more or less violent, and presenting periods of duration of from a few minutes to several hours, preceded the hot stage.

Fever.—With few exceptions, there was extreme heat of skin at the onset of the disease, with a large and full pulse, variable in frequency from 90 to 125 strokes per minute, and averaging about 110. In some cases it was strong aud resisting; in others, though full and frequent, it was soft and yielding to pressure. In a few individuals of weak and impaired constitutions, it was small and frequent from the beginning. The fever varied in duration, in some cases declining at the expiration of 30 hours; in others lasting for three days. In some fatal cases, at the end of 48 hours the fever had entirely ceased, and there ensued that remarkable prostration, which at some period is certain to arrive. The patient for some 24 hours will present a condition of restlessness, with nausea and great malaise, without being sensible of any local suffering, and then a tumultuous beating of the heart, with occasionally throbbing of the carotids and increased heat of skin, with an aggravation of all the symptoms, will come on; and frequently before black vomit or death he will for hours be tranquil and present the most deceptive appearance of recovery. In some cases presenting favorable symptoms, towards the fourth day considerable fever was renewed after it had also almost entirely ceased for 24 hours; but with this fever there were increased secretions and other favorable symptoms, which announced, without disappointment, a speedy recovery. Towards evening there was in many cases an increase of the febrile symptoms, but upon the whole the fever consisted of but one paroxysm.

In the midst of the epidemic, surrounded by yellow fever patients, a medical confrere had a severe attack of fever in the purely intermittent form. I saw him as the hot stage of the second paroxysm was passing off, and his affection presented the symptoms, the general physiognomy of yellow fever. But the fever ceased, and during the intermittence

which continued, as well as I remember, about 14 hours, he was free from bodily pain. After the third paroxysm the fever did not return, and he recovered as speedily as one generally does from intermittent fever.

Though intermittence, according to my experience, is not an element of yellow fever, yet I would not deny that exceptional cases of it might occur. I saw a case which commenced with fever in an intermittent form. With the third paroxysm the disease assumed the complete character of yellow fever, and in its progress, which resulted in recovery, it presented many very unfavorable symptoms, as retention of urine, great anxiety, sighing, an ashy countenance, and as death approached the pulse lost its force and frequency, and medicine for several hours before dissolution ceased entirely to be felt.

In many cases, after complete convalescence and during its progress, the pulse, retaining its fulness, falls in frequency considerably below the healthy standard. I am particularly reminded of three young men, Jews, whose pulse during this period beat, in one but 38 per minute, in the other two 40; otherwise there was nothing remarkable in their convalescence.

Tongue. The appearances of the tongue in this disease are not always to be relied on. In some few cases it did not for some days exhibit any diseased appearance at all; but in a majority of instances it was from the beginning slightly furred or coated in the middle, with its borders and end slightly redder than in health. Even when dry, parched and with fissures or cracks on its surface, it is not always a bad symptom; sometimes in the progress of the disease it becomes smooth and dry, swollen and of a purple color; sometimes moist, then dry. In some fatal cases of negroes it became perfectly yellow. Most patients in yellow fever have a peculiar mode of showing the tongue. When asked to let you see it, and before you have finished, they sud denly protrude it, with a spasmodic jerk.

A spongy, swollen condition of the gums is a common and always an unfavorable, though not necessarily a fatal symptom. It merits the closest consideration on the part of the physician, as the indicator of serious derangement of the mucous membranes.

Symptoms of the Stomach and Bowels.—More or less nausea was almost an universal symptom. In some cases of children, for the first two days the stomach would retain nothing. In cases of grown persons, notwithstanding this symptom, medicines were generally retained. In some instances when the stomach would not tolerate wa-

ter, it would retain medicines. This nausea, always accompanied with a sensation of fulness of the stomach, as the case improved generally diminished and passed away; though in some cases, which resulted favorably as late as the third and fourth day, it continued to distress the patients. These symptoms increase as black vomit approaches, and yet some patients complaining of a deadly sickness and distension of the stomach, would not towards the close of the disease vomit at all. In many of these cases, after death, abundance of matter like that of black vomit flowed from the mouth.

According to my observation, and all that I can learn, black vomit occurred in a large majority of cases.* Death took place in many instances before the morbid condition necessary to black vomit existed: before that tenuity of the blood existed enabling it to exude from the debilitated mucous tissue; from congestion of important organs or from an exhaustion of the vital forces. In many of the cases in which it was not discharged, it is highly probable it would have been found in the stomach on dissection. Certain it is that many of these cases which I witnessed there was presented the greatest possible intensity of disease, a more appalling aspect of irretrievably vitiated solids and fluids than in cases in which it occurred. Some of these cases, running to the seventh and ninth, and one in particular to the thirteenth day, were disease personified—awfully repulsive—terrible to behold. In some of the cases which I witnessed, this matter was thrown up with some considerable effort or straining; but in general it was discharged suddenly and without effort.

In 1839 I saw a patient, a powerful man, without retching or straining, eject, as if it were spouted, a stream of black vomit twenty feet from the bed on which he was lying; then he rose on his feet, walked a short distance, was about falling when he was lifted back to his bed, and died almost in an instant.

Black vomit, according to my observation, occurs in this disease under two distinct conditions. In some of them it is not a fatal symptom; in the other it is the infallible precursor of death—nay, one of the effects of incipient dissolution. In the one condition there has taken place a great change in the blood, as regards color, consistency and coagulation; but as yet there is no decomposition of organic matter. Through the mucous tissues, rendered patulous by disease, the diluted blood passes out, not passively, but in obedience to the vis a tergo, the

* It occurred in about three-fourths of the whites, in about three-fifths of the mulattoes, and in one instance among the blacks, that I could learn.

impulsive efforts of the heart. We see active, lively hemorrhages from the nose and other parts, followed often by great improvement; and at this time we may have black vomit, the result of an hemorrhage in the stomach. In this condition, though imperfectly performed, there is still capillary circulation. At any rate that state of the capillary system in which the blood merely permeates the minute cells, often stagnating in them, does not yet exist. Up to this time the recuperative energies of nature are not abolished. Nature may yet, and she often does, resume her energies, re-excite the forces of life, re-establish the nutritive actions, re-commence operations in her laboratory, and excrete from the remote tissues, and throw into the channel of the circulation the elements which alone can neutralize the morbific influence, and bring back health. But in the other condition, incipient decomposition has assailed both solids and fluids. The forces of life are paralyzed beyond resuscitation. Chemical changes are commencing in the tissues and blood; oxyde of carbon and perhaps other various gases are being produced and in quantity in an increasing ratio. They make the blood black. The mucous tissues, in their tumid and patulous state, cannot contain this blood. Black vomit, under these circumstances, is but an effect of partial death.

In an article many years ago on the epidemics of Opelousas of 1837 and '39, I stated that black vomit was a fatal symptom, according to my observation. Subsequently I received from my friends, Dr. Hill of Opelousas and Mallard of Grand Coteau, a full statement of all the particulars of a case occurring to each in his private practice, of a recovery after genuine and copious black vomit. I regret very much that the histories of these cases, minutely and circumstantially related by their authors, have been mislaid and cannot be found.

Pain, from pressure on the stomach, existed in some, and not in other cases; and sometimes this gastric sensibility was extreme, and I observed it in not only fatal, but in cases that recovered. In two cases, particularly, which resulted in health, this sensitiveness in the epigastrium was so exquisite, that they would not permit that region to be touched. There was no other evidence of gastric irritation. Nor do I believe there existed any. It appears to me like many other similar fevers elsewhere, to be neuralgic in its nature. Some complained of an emptyness; others that the stomach felt folded up, or its coats glued together.

I saw two patients, slightly affected with hiccough, get well. It was present in many of the fatal cases; I should think about one-fourth.

Occasionally it was so violent, that every paroxysm, easily heard at a great distance, appeared to agonize the patient. It is one of the most distressing, and most generally a fatal symptom.

It is not often that a sinking in of the anterior walls of the abdomen is seen in this disease. I saw it occur in a negro man some days before his death, which took place on the thirteenth day from the attack. It may be said, indeed, that almost universally pressure over the bowels imparts a feeling of tumidity, a flatulent or doughy sensation to the touch. They were occasionally the seat of pain, in different periods of the disease, but this was rarely an object of serious attention. Sometimes the bowels were with difficulty moved; but as a general rule the mildest purgatives sufficed.

Castor oil acted very well in this epidemic. There were some cases in which patients had taken drastic medicines, which purged severely, and in which afterwards no evacuations could be procured. They were invariably fatal. The colon may, in such cases, throughout its course so contract its calibre as to be impermeable. Such a case I once saw on a post-mortem examination.

The discharge from the stomach and bowels varied in its nature. In the beginning there was occasionally thrown up bile, that is, a yellowish fluid, thin and containing much mucus; but there was always, as far as I observed, an absence of that bright yellow color, of that peculiar bitterness of taste, and of the peculiar consistence and general physical appearance of true, healthy bile. It was but two days ago that I called the special attention of an old and esteemed physician, Doctor Acher of Opelousas, to this peculiar matter. Some few hours after her attack—quite a servere one—this matter was puked by Miss T. It was a pale yellowish fluid, which on casual examination would have been pronounced pure bile by every one. It was discharged into a purely white vessel. Shake it as much as you pleased, it left not a tinge on the side of the vessel. The Doctor examined it minutely, and decided that the most important constituents of bile were very imperfect or entirely absent in it. He emphatically denied that it was bile. The departure of this fluid from the healthy appearance of bile varied in extent in different cases. In the above case its vitiation was complete.

In the beginning of the disease the first alvine evacuations often indicated the presence of healthy bile, but purgation soon put an end to it, and then we saw sometimes yellowish evacuations marked by the presence of some imperfectly elaborated bile, or matter discharged without any appearance of bile whatever. I am forced to the conclu-

sion, that in a violent or malignant form of yellow fever the elimination of pure healthy bile from the liver has never been seen, and is an impossibility, which results from no nervous derangement, or structural lesion of this organ, but from the simple fact that the elements in the blood required for the elaboration of healthy bile—and in particular carbon and its compounds—are deficient or entirely absent.

Dark brownish, consistent evacuations, I regarded as almost the certain harbinger of a healthy convalescence. The passages were sometimes very copious and very offensive, which last circumstance was regarded as favorable. When destitute of smell, or having the odor of macerated flesh, just before putrefaction, when thin, half mucus or watery, of a clay or ash colored hue, or app earing like the washings of meat, sero-sanguinolent, or of a black, fuligenous appearance, or looking like black vomit—in all these cases they were very unfavorable, and if connected with other bad symptoms, fatal. I saw several patients, who during convalescence passed thick, tarry matter, apparently with advantage.

Urine.—In some few cases this fluid presented no remarkable change in color or quantity. In two instances particularly it was discharged in very increased quantities, and with the happiest effects. In one of these cases it was redder than it is naturally. As a general rule I believe it was diminished in quantity and much reddened in color. In the case of a child it was also discharged in very considerable quantity and for some time involuntarily. It was of an orange color in this case, and when shaken in the vessel, left on the sides a yellow tinge and stained the linen of the same marked color.

I saw one case of retention of urine from the beginning, in an individual of a bilious lymphatic temperament, of impaired constitution. It gradually diminished in quantity until the catheter proved complete suppression, and he died in the beginning of the fifth day of his disease with black vomit. He took of his own accord, in the first eight hours of his disease, 60 grains of quinine in two doses.

I must regard retention, as well as gradually diminishing secretion of urine, very unfavorable. Suppression of urine is in my opinion a fatal symptom. It may result from a lesion of structure ; but dissections rarely showing such lesions, we are bound to look to the general symptoms of the disease for an explanation. And to what else, after a due examination of them—of the diseased blood, and vitiated and diminished and arrested secretions—than to an absence in the blood of the constituent elements of urine ?

Hemorrhage.—This symptom, in its connexion with black vomit, has already been noticed. The great tendency of the morbid condition in this disease is to hemorrhage. In the mild cases, in those in which, according to my views, the morbific cause has merely modified the nutritive functions, and in which nature, on the accession of fever, resumes, often to an excess, her chemical nutritive actions, we do not see hemorrhages. Whenever these actions are imperfectly performed, tardily resumed or arrested, the diluted blood is discharged through the diseased mucous membranes. This hemorrhage is of itself an evidence, not only of an increased liquidity of the blood, but also of diseased mucous membranes.

Bleeding from the nose occurred in a large number of cases. In very many instances it was decidedly beneficial; when it co-existed with a red face, injected eye and pain in the head, I have seen it as late as the fifth day dissipate these symptoms and greatly comfort the patient. This symptom occurring from the nose, mouth or anus, with evidence of a more or less active capillary circulation, and without black vomit, seems to exude from the membranes, has assumed a darkish aspect; when pressure on the skin leaves a pale color, which gradually ceases; when it is accompanied with a waxy color of the skin or swarthiness, and when preceded by great nausea and great but ineffectual efforts to vomit—we may regard hemorrhage as a fatal symptom, and be prepared for copious discharges of black vomit.

In the case of a lady 55 years old, of considerable severity, within the first 24 hours there ensued a considerable hemorrhage from the womb. She had ceased to menstruate since six years. Within a few hours the discharge ceased and her symptoms were aggravated. I bled her to the amount of 25 ounces. The hemorrhage was resumed and flowed with increased abundance, and she recovered in a very happy manner, with the aid of a mild purgative, mucilaginous drinks, a foot bath, and nothing else.

In one young lady, who had a very violent attack, on the third day a copious hemorrhage from the womb commenced; with it the symptoms improved and she recovered. Her period was not expected for a week. In another young lady, who was attacked five days after her period, there ensued a sanguineous uterine discharge.

Abortion occurred in two cases; one in the seventh, the other in the eighth month of pregnancy; both children were still-born and the unhappy mothers perished. A relapse, resulting from excessive imprudence in the one case, and tampering with "No. 6" in the other,

brought on these accidents and death. In many other instances pregnant women passed safely through the disease.

Skin.—It is generally red and hot. In some lymphatic or scrofulous persons, in the first days of the disease, the complexion is of a pale white color, which improves as the fever diminishes. In some persons of the sanguine temperament the blood appeared as if it would burst out through the pores of the face. Such persons are peculiarly subject to hemorrhage. Perspiration, except in some few cases, and those mostly children, was easily induced; and when easily maintained I regarded it a most favorable symptom. From the secretions, etc., of the patients there was always observed a peculiarly disagreeable smell, hard to describe and an ashy complexion; one of a waxy or greenish hue, with viscid sweat, a death-like coldness of the end of the nose, fingers and toes, were bad symptoms—sometimes accompanying (and then fatal) black vomit.

In a majority of the fatal cases, a yellowish tinge, more or less evident, was visible in the face, neck, breast, and sometimes in other parts. After death I was told the body, in a very large majority of cases, became yellow; in some few it was of an unnatural pale white. The yellow appearance was totally unlike the jaundice, as seen every day under other circumstances. It was not the dark yellow of common jaundice; it was of a pale orange tint. I do not believe it was the result of absorption into the blood of secreted bilious matter. Neither the symptoms during life, nor the scalpel after death, reveal the slightest evidence of a secretion of bile in these cases; or if they do, most rarely.

In those instances in which I saw the yellowish matter discharged during the first one or two days, and which ended in death, there was no discoloration of the skin during life. I allude to two individuals particularly, and I do not know whether they turned yellow after death. If the jaundiced appearance depends on reabsorption of bilious matter, why does it so often never appear until after death, and then as it were instantaneously, and in quantity sufficient to impart the peculiar tinge to all the surfaces within reach of vision?

Though clearly inferrible from previous remarks, I will nevertheless express the opinion that in this affection, at its onset in all severe attacks, there existed more or less imperfectly constituted elements of bile in the blood, from which was occasionally eliminated a very imperfect bile; that in proportion to the energies of the nutritive system and correspondent absorption furnishing elements to the blood, would the bili-

ous secretion be less vitiated; that in the arrest of all functional action of the liver, as often unquestionably exists at an early period of the disease, these imperfectly constituted elements of bile in the blood, either find an exit through the kidneys or internal mucous membranes, or they remain in the blood, and are probably deposited in the skin at that period of incipient decomposition which precedes death, and immediately after death are separated from the blood, staining with a yellowish deep tinge the whole periphery of the body, presenting, except sometimes in isolated portions, no previous discoloration at all. I express opinions founded on general observation. There are cases in which, after the fever has ceased, and nature has resumed healthy action, these elements of bile are deposited in the skin, producing a universal jaundiced complexion. The liver and kidneys refusing them they are, as it were, excreted from the blood chiefly in the cutaneous tissue.

In three fatal cases of negroes, young and remarkably robust, of the bilious temperament, the conjunctiva, mucous membranes, every tissue, as far as the eye could determine, seemed completely injected with a dirty yellowish coloring matter. The tongue, in its entire surface and substance presented this color. Every tissue and fluid of the body was changed in their very composition. The principle of life alone existed. One died on the sixth and one on the seventh. The third died on the thirteenth, and this condition in his case originated during a relapse.

Nervous System.—If a full exercise of the functions of mind, of will, of sensation, of respiration, of voluntary motion, be indicative of a healthy condition of the nervous system, then in a great many cases, from the beginning of the disease unto death, this system would appear to continue in a state of integrity. But whatever may be the peculiar derangement it may sustain, dissections show that it and the membranes are as little liable to structural derangement as any other organs of the body; indeed the great nervous centres less so. Chemical changes, and black vomit, the last links in the lengthened chain of morbid phenomena will very often ensue, without any manifestation of nervous derangement whatever, except of that function which is necessary, or may possibly be necessary for healthy action of the nutritive function, or for its restitution. It is indeed passing strange, if a primary affection of this system be the fons etorigo of the physical and functional changes which constitute the essential character of this disease.

But how the nervous system or portions of it should be so deranged in rare cases as to give rise in some instances to delirium, at different periods of the affection; sometimes to coma towards its end; occasionally to nervous twitchings and even convulsions, and how at the onset the most atrocious and speedily fatal convulsions, does not appear strange; rather is it remarkable, considering the peculiarities of temperament and idiosyncrasies, natural and artificial, that these nervous phenomena are not more frequent. Such nervous derangements ought not, it appears to me, to be regarded pathognomonic, but exceptional symptoms of the disease. How often did it happen that our most painful apprehensions were excited by the very absence of all those symptoms which announce suffering of the nervous system; nearly all of its functions appeared intact; yet the patient was perishing. Is it a vitiated innervation which corrupts the blood before the fever is established, which produces the ever varying modifications in its character, which diminishes and vitiates the secretions, which exsiccates the kidneys and liver, which finally decomposes the living tissues, and discharges tho diluted blood through the mucous membranes, while in other respects there seems to be no lesion of the nervous system. That in the nervous tissue there is derangement of the molecular nutritive actions, as there is in all the tissues of the body, admits, it seems to me, of no doubt; but the role which this system plays throughout the affection, seems to me to reject the doctrine of a primary vitiation, and to divest it of all claims to a serious consideration in discussing the pathology of the disease.

Treatment.—The history of the treatment of this disease derives, I fear, very unimportant accessions from observation of, and experience in, our epidemic; already but a bundle of strange and contradictory recipes, it has not, it is to be feared, ceased to contribute to the disgrace of medical aid, or at least to disclose the profound ignorance of the profession, as regards the nature of the disease, and our inability to counteract its effects and oppose its march. It may without a fear of contradiction be asserted, that notwithstanding the admitted necessity of medicine and counsel, there is not in the catalogue of grave affections one which can present a more varied treatment, or one which really demands less medication than yellow fever. Mercury has had its day; but it is now remembered only in connexion with its victims. A few years since and quinine was enthroned high above its compeers; and it was an act of extreme temerity even to question its infallible supremacy in the disease. After a short reign it has been deposed;

but the throne is left vacant. Blood-letting boldly practised, active emetics, purgatives, etc., have had their advocates and their opponents; the former lauding their beneficial effects, the latter condemning them as dangerous.

The fact appears to be that this disease, though produced by a specific cause, and presenting in its genuine form, in my opinion at least, specific lesions of tissue and of function, can, in our imperfect knowledge of its nature and of animal chemistry, be opposed by no specific remedy. In idiopathic fevers, it appears to me, that recovery can only be promoted by a medication which exerts the special influence of aiding and abetting nature, in her efforts to restore all the functional actions, the arrest or modification of which is the first link and fruitful cause of all the morbid effects which make up the disease ; and hence perhaps there is a great error on the part of many in assigning to medicines an inherent immediate action for good. We have no remedy which neutralizes or eradicates the morbific cause. In truth we have not the least conception of the causes of idiopathic fevers. Does quinine neutralize and destroy the miasmatic poison which we imagine to be the cause of intermittent fever? or does it aid nature to restore functional action and to excrete it from the system ? And thus it is that in regard to yellow fever, a disease, often in its severe form cured solely by the efforts of nature, and it is to be feared more often aggravated than benefitted by medicine, we witness a great variety of opinion as regards the effects of remedies, as well as the great reputation which certain medicines have acquired in consequence of favorable circumstances.

Emetics.—Having witnessed frequently in this disease a remarkable proneness to gastro-intestinal irritation, I resorted to the use of emetics only in the beginning of the disease, and only when the stomach was surcharged and oppressed by offending matter, as undigested food or muco-bilious secretions. I used indiscriminately the ipecacuanha, warm water sometimes alone, sometimes with salt and mustard, or with one separately. It was an easy matter to procure free emesis with any of these substances. A tumbler of water with half a tea spoonful of mustard and the same quantity of salt, or with a tea spoonful of either, evacuated the contents of the stomach readily and did not tend to purgation.

In consultation with another physician, tartar emetic was given to one patient, a fat subject, presenting under any treatment a poor prospect of recovery. Hypercatharsis ensued and the patient died. A

grain was given every ten minutes; after the second dose he puked copiously; the patient threw up yellowish matter in abundance, and commenced purging without a third dose or any more being given. From its effects in this case, and from what I have known of it in other cases, I think it ought never to be used under any circumstances in this disease.

Purgatives.—A mild but efficient purge seems called for, and I always gave one so soon as perspiration was established, or combined it in the beginning with quinine. When the stomach would retain it, I still used the old remedy, an infusion of manna and rhubarb with a small addition of magnesia; but when irritable, as it was in a vast majority of cases, a dose consisting Ext. Juglans, of Dandelion, Blue Mass and Rhubarb, each 5 grains, made into pills, and repeated when necessary, acted as a pleasant and effectual purge. When I desired more speedy purgation, I added to the above 5 grains of Comp. Ex. of Colocynth, and never saw drastic effects from it.

According to the demand for an action on the bowels, injections were used; sometimes purely mucilaginous; again, of molasses and soap and water; and again with mustard, a tea spoonful of which in warm water was certain to evacuate the lower bowels.

In the progress of the disease purgatives were sometimes required; and of all medicines, a half table spoonful of castor oil, with a tea spoonful of brandy, operated most speedily and gently. In some cases of tardy convalescence, and in those accompanied with yellowish skin, it was often necessary to administer blue mass and comp. rhubarb pill; and when, as happened sometimes, the dejections were black and tenacious, it was necessary to use castor oil or comp. ext. of colocynth.

Frequent, thin, whitish, or dark colored evacuations from the bowels, occurring at any period of the disease, is a very unfavorable symptom. In the use of emetics and purgatives, beware of gastric and intestinal irritation, for in yellow fever there is undoubtedly a tendency to it; and with it urinary discharges rarely continue long.

Opiates.—In the severe epidemic of Opelousas in 1839, having an abundant supply of pure Thridace, or Lactucarium, obtained for a special purpose in Paris, I used it in every case, and with such happy effects as seemed almost to entitle it to the character of a specific. Certainly I saw in its use no injury inflicted on the emunctories, but all the other happy effects of opium. But here it should be observed that the

fever of '39, though intensely violent in its incipient symptoms and accompanied with pure black vomit in all fatal cases, should rather after all be called a self-curing disease, for with a full bleeding at the beginning, a mild purgative, a foot bath, and a good dose of Lactucarium ninety-seven in every hundred got well, and speedily. In the late epidemic I had no Lactucarium. Occasionally I used Hyoscyamus and Dover's Powder; sometimes Morphine in a solution of Soda. I used these preparations only when Opium was imperiously required, sometimes with good effects, again without any noticeable improvement in the case.

Quinine.—I used this remedy in a great many cases in doses varying from fifteen to twenty-five grains in grown persons, repeated sometimes twice or three times; in some cases uncombined, in others with from one to two grains of Opium (with ten grains of blue mass) and again combined with both opium and blue mass; and I cannot attribute a single case of cure to this unaided remedy. In almost all cases it produued a relaxation of the system and copious perspiration, but it did not effect an arrest or abortion of the disease. In some instances it affected the brain in a most distressing manner, for the relief of which cups, cold applications and foot baths were impotent. It is very true a great many patients who took quinine got well; but a good many died.

It must be borne in mind that in Washington the type of the fever was of a most malignant character, presenting in some cases, in the first days of the disease, typhoid symptoms; and in many cases and in blacks particularly, terminating in the pure typhus of old authors. In '37 the fatality of the epidemic in Opelousas was horrible. Of the first fifty cases I do not believe five got well. As already remarked, that of 1839 was perfectly manageable. The mortality did not exceed four per cent. In 1842 it was again very fatal. In neither of these epidemics was quinine used in heroic doses. In the late Washington epidemic it was employed, and in many cases its strongest advocates could not have made any objection as regards the quantity used. I saw it given to the extent of 30 grains. If in my experience I cannot concur in an indiscriminate recommendation of the remedy, neither will I condemn it. I can well conceive that in the Opelousas fever of '39, as many recoveries would have taken place from the employment of quinine, as actually did take place without it, and that the results would have been confirmative of those subsequently obtained in New Orleans. But in the late epidemic, neither quinine, with all the adjuvants

that medicine could muster and skill apply, nor any other remedy as yet known, could have warded off death in all cases. It was inevitable. To say that quinine would not cure this disease, that is, cut it short, ought not to be construed into a condemnation of the remedy. Upon the highest authority its happy effects in the fever in previous years has entitled its employment to the epithet of the "abortive treatment." The resumé of my experience with it amounts to this—that in our late epidemic it did not cut short the disease; that a majority of those who took quinine recovered; that some who took it died; and that in a few cases it exerted a decidedly injurious influence; and it will be justly inferred that this resumé would be applicable to all remedies that were employed.

Adjuvants.—Under this head I will consider a large number of remedial agents, which, though commonly regarded as auxiliaries, merit rather in my opinion to be considered the heroic remedies in the disease. They are, mustard pediluvia, warm teas, mucilaginous drinks, cold or warm, rendered alkaline and nitrous; injections, purgative and emollient; warm and cold frictions, and fomentations with cloths wrung out of hot brandy or whiskey and water, all over the body; weak brandy toddy and injections; the use of "eau sedative" and chloroform locally, etc., and injunctions of perfect quietude of body and mind; the recumbent position should be added. How many grave attacks of yellow fever in every epidemic, and in the practice of every physician, have resulted in the most happy recovery from the employment of only the above means, used in part or in whole? For my part, I could fill a volume with the history of such recoveries.

In our ignorance of the nature of the cause of this disease; in the utter absence of any theory, whose plausibility has entitled it to more than an ephemeral reputation, of the mode in which this cause acts on the system; without any light upon the modification of the solids and fluids, derived from chemical analysis, it might beforehand be confessed that medicines, according to the circumstances, and particularly the period of the disease in which they were applied, and more especially according to the type of the fever prevailing, would be followed by different effects, that for example emetics, purgatives, quinine and blood-letting, while under certain circumstances they would be followed by happy results, under other circumstances, or in a different type of the disease, they would exert no salutary effects. And such, in truth, is the history of the effects of the leading remedies in yellow fever. Hence

in my opinion it follows, that in the present state of our knowledge of its pathology (by which I mean not only the mode in which the cause acts, but the nature of, and relations between the effects produced) and the uncertainty of the effects of potent agents employed in the disease, it is most natural to place our chief reliance in those remedies termed "adjuvants," and to promote their favorable action by the use of an emetic, a purge, quinine or venesection, as may be indicated in the particular case, or which in the peculiar type of the epidemic experience may sanction.

By some writers it has been questioned, whether perspiration in this disease was beneficial or not. They have witnessed recoveries which were not accompanied with perspiration, and they have seen death occur notwithstanding the most copious sweating. For my part, I have always regarded this symptom, whether occurring spontaneously or promoted by foot baths and common teas, and frictions, etc., as of favorable augury in the disease. With it the sufferings of the patient almost always diminish, the burning heat ceases and the pulse grows softer. In many cases the symptoms manifest such evident improvement with the perspiration, that the physician feels justified in prescribing only such remedies as tend to encourage it, contented with the proof manifested in the improving condition of his patient, that nature is resuming those functions which have been modified or impeded by the action of the morbific cause. Mustard or stimulating foot baths, besides promoting perspiration, oftentimes, by their revulsive effect, greatly relieve the head. Warm teas, as orange leaf, sage, black teas, etc., are generally agreeable and conducive to perspiration ; and for this purpose a glass of cold water will frequently answer better than any thing else. As the fever begins to abate, and the period of debility approaches, nothing is so soothing and grateful as brandy or whiskey frictions, with occasional injections of either, properly diluted with water. From both patients derive great benefit. I can entertain no doubt of the happiest effects from the frictions from the beginning of the disease ; the liquor in the early stages of the disease should be only more diluted. It is I believe a singular fact, that in yellow fever the use of alcoholic frictions, injections and drinks may be pushed to a greater extent with impunity than in any other disease—nay, than in health. Towards the middle and decline of our epidemic I made a more free use of brandy and good whiskey externally and internally, and always I thought with great advantage to the sick. The case of Mrs. Read will be remembered. Brandy was given to relieve nausea ; its happy effect induced its continuance. She took a much larger quantity of it

than she could have taken in health; its effect was soothing, not stimulating.

The late Dr. Harrison records the case of an Irishman,* who evidently cured himself with whiskey. He did not see him until the fifth day after his attack, which was a severe one, and he was assured that the patient, previously to his visit, had taken nothing but free potations of whiskey. He recovered. Cases analogous to the above have been observed by almost every physician of large experience in this disease. I could cite many instances in which I gave brandy freely from an early period of the disease, and particularly of individuals who indulged in it freely in health. Spirituous drinks, taken with moderation, and a generous diet, constitute, I believe, in the opinion of the experienced, not only the surest prophylactic against the disease, but furnishe to the tissues that vigor of recuperative action favorable to recovery, and the loss of which, entailed by the strictest diet and abstemiousness, has consigned to the grave more talent and virtue than any other auxiliary employed in the service of this fatal disease.

An intelligent and honorable gentleman of this town, a few days ago gave me the following statement; in its truth I put full trust, but give it as a second hand fact for what it is worth. He had the facts directly from the captain of the vessel:

Captain Genish left Boston in 1837 with a cargo of flour for Matanzas. On his arrival in port he found the yellow fever prevailing, and he was prevented from discharging his cargo by some temporary restrictions which the Government had recently imposed on such articles as his freight. He had not long been in port before the epidemic seized on his crew, consisting of seven sailors and first and second mates. He employed the best medical aid and yet his seven sailors perished. The survivors, the captain and two mates, agreed to dispense with medical aid, and to treat each other in the event of their falling sick; and the treatment determined on was the free use of ice to the head and brandy and water by the mouth. They all three fell sick and got well under the above treatment. The great confidence I have in the relator is my reason for citing this case. But it is not more apposite in the illustration of the nature of the disease than many other analogous cases fully known and proven. But there exists in this disease very often such irritability of the stomach or tendency to it, that nothing stimulating can be taken. Whenever the stomach tolerates it, I believe, properly diluted, it will prove beneficial. I attribute the

* It is quite likely that what is good for an Irishman would be equally good for an Englishman.

good effects of alcoholic frictions, &c., to absorption; to the carbon entering the blood and combining with a portion of the oxygen of which that fluid contains a superabundance, leaving the remaining elements of the brandy to form water, of which the tissues stand in so much need; and thus it is that I explain, not only the impunity with which whiskey in some cases may be drank, but its probable good effects, and the marked amelioration which always results from alcoholic frictions. How muchsoever in violation of all previous theories and teachings, I nevertheless feel a conviction that all carbonaceous substances, not poisonous in their nature, but presenting the carbon in an innocent combination and readily absorbable, will prove eminently serviceable in the treatment of this disease; and that if in the dispensations of an offended God the country shall again be swept over by this besom destruction, that they will merit the attention of the profession.

Alkaline and Nitrous Drinks.—As a general rule, the supercarbonate of soda, in small doses, with the addition of sweet spirits of nitre, at intervals more or less long, during the first two or three days of the disease, were given, with the view of correcting acidity and promoting the urinary secretion. The chloride of soda (Labarraques' solution) merits, in my opinion, more than a passing notice. I saw in 1839 a young man, of the nervo-bilious temperament, get well under the use of this preparation alone. I saw him for the first time on the fourth day. He presented a strongly marked ataxic condition, a jaundiced skin and hemorrhage from the mouth, nose and anus. The chloride of soda was administered by the mouth and anus, and contrary to all expectations he got well.

In many cases in our late epidemic it arrested nausea when nothing else would. I gave it in doses of from 10 to 50 drops in as much water as the patient would drink at a draught, and repeated it every one, two or three hours, according to circumstances. In two cases the black vomit yielded to it. As soon as nausea ceased or was greatly diminished, the intervals between its administration were lengthened, and the medicine finally suspended.

Creosote.—This preparation, as has already been stated, arrested black vomit in five cases, three of which entirely recovered; but in the other two cases, both children, after an apparent improvement in all the symptoms, a typhus affection ensued and destroyed life. The creosote is the only remedy that I know of that will, by exciting, heal these secretions, relieve the patient of that distressing and unfavorable

symptom, which consists in a thick, viscid, glutinous matter in the mouth and throat, and which excites a constant but ineffectual hawking, and is always accompanied with restlessness, more or less dryness of skin, &c. In such cases the creosote often arrests nausea and that sensation of distress and fulness of the stomach—both of which are apt to be present—acting like a charm.

In some cases the creosote and chloride of soda were given in alternate doses, every one or two hours. When the condition of the patient is hourly deteriorating, with such spmptoms present and declaring themselves, as a swollen, clean, red, more or less dry tongue, a glistening eye, but dull look, a swarthy complexion, a lowering, anxious expression, increasing restlessness, pains in the legs, a heaviness in the epigastrium, sometimes a painful and exhausting straining to vomit, diminished and vitiated secretions, dry skin or clammy sweat—I know of nothing in which I can repose so much hope and confidence as in half drop doses of creosote in as much water as the patient can conveniently drink at a time, and repeated every half, every hour or at least longer intervals, according to circumstances, aided with repeated and continued frictions and injections with strong brandy and water. The first two or three doses are apt to be thrown up; but nausea and all the horrid train of symptoms mentioned will often soon disappear. But then the treatment should not be stopped; the remedies should be continued at longer intervals, according to circumstances. The phosphate of lime was a remedy unknown to me, until I had the pleasure of meeting with Dr. Bayles of New Orleans, late in the epidemic. I saw it prescribed by him under unfavorable circumstances and with happy effects. In a case now under treatment I gave it on the third day of the disease, with a view to relieve some nausea and gastric distress, in six grain doses, repeated every four hours, with a good result.

Cupping and Blood-letting.—In many of the cases, after practising venesection, some blood was drawn by cups. In nearly all the cases in which no blood was drawn by bleeding, it was drawn in quantities more or less considerable by cups. The peculiar suffering complained of in the head, eyes and back, strongly indicates the necessity of this therapeutic agent. Upon the whole I am inclined to think it was beneficial, its good effects being more marked, when it was employed early in the disease. In some cases, though practised to considerable extent, it failed entirely in affording any general or local improvement of the patient, Again at the onset of the disease, and particularly in children, I saw it dissipate pain and delirium as by a charm,

promote perspiration and apparently exert a powerful salutary effect upon the system.

With regard to blood-letting, I am still a strenuous advocate of it in particular cases and when practised in the first hours of the disease. To doubt its beneficial effects, nay its indispensable necessity in some cases, would be to doubt one's senses; to disregard the most clamorous demand for its employment. I repeat that I am not an advocate for bleeding in this disease, except at its beginning, and rarely after the first 24 hours, believing that when the loss of blood may after this period be required, for any local complication or symptom, cups would be preferable to the lancet. In cases of a cachectic or scorbutic habit, of the lymphatic or scrofulous temperament, in individuals whose constitutions were impaired by intemperance or by chronic disease or old age; when the pulse was small and quick I never resorted to the lancet. Nor in cases of robust constitutions and sanguine temperament, marked by a free or excessive action of the kidneys and abundant perspiration, would I use or advise the lancet. As examples illustrative of my views, I would refer to the cases already detailed of Mr. and Mrs. Read. The lady is young and healthy and of a constitution of ordinary vigor. I saw her early after her attack, suffering with much pain, highly injected face and eyes, &c. I bled her and repeated the bleeding soon after, and her arm again bled considerably in consequence of a derangement of the bandage. Mr. R., of sanguineous temperament, of most robust constitution, was not bled at all, nor even cupped; though he presented all the symptoms and his pulse was soft and full, his skin perspiring well, and the urinary discharge superabundant. It was an interesting case, and every three hours, at often repeated visits, it was manifest to me that nature was all sufficient to accomplish her work of rapid recovery.

The case of Mr. Graham and wife will illustrate this question of blood-letting. Mr. G. fell sick during the night of the 7th of September; his temperament partakes of the nervous, the bilious and sanguineous, apparently in equal proportions. He is a hard-working man, and strong, unimpaired constitution. I saw him at two o'clock on the morning of the 8th. It was the case of a strong man contending against a violent disease; his pulse was full, strong, hard, and 120 in the minute; face red, eye injected, tongue red, mouth dry, skin hot, restlessness extreme; occasionally he was delirious, &c. I bled him 50 ounces with scarcely any abatement of his suffering. Gave him a foot bath and an active purge, that is pills of Juglans, rhubarb, comp. ex. of colocynth and blue mass. In the morning his situation was

about the same ; bled him 30 ounces ; ordered cathr. enema and foot-bath with teas. The bleeding greatly relieved him ; the medicine with aid of injection purged well ; his skin began to perspire freely ; and he became composed. The headache returned in the course of the day, and he was cupped to the amount of 8 ounces with partial relief. For the next three days the treatment consisted of soda, cold mucilaginous injections, and similar drinks, with sweet spirits of nitre, foot baths, whiskey frictions, occasionally a dose of the chloride of soda, during which period his recovery was very doubtful, but on the fourth day his improvement was evident and his recovery rapid, unmarked by more than ordinary debility.

Mrs. G. was the counterpart of her husband in temperament, physical development, &c. She was of large frame and of great powers of endurance. She was violently attacked immediately after her husband's recovery. The action of the heart, turgescence of blood vessels, redness of face and eye, pains, &c., were excessive ; her fright, anxiety and restlessness were extreme. She refused positively to be bled or cupped. I was almost on the point of leaving her, but consented to make a prescription. I ordered a mustard pediluvium and injection, cold cloths to the head, to be constantly renewed, and immediately after the action of the injection 25 grains of quinine, 2 of ipecac, with 10 grains of blue mass. Abundant perspiration ensued, and for a time she appeared to be doing well ; but the symptoms returned in a worse form, accompanied with evident cerebral engorgement. She obstinately objected to cups and the lancet. The repetition of the quinine was contra-indicated. In the course of twelve hours she became delirious, which soon terminated in spasms and coma, and death took place in 36 hours from her attack.

But for the lancet I am very certain the husband's career would have ended like that of his wife. I believe the wife would now be alive had she been properly and at the start depleted. In the epidemics of 1837 and 1839 in Opelousas, the beneficial effects, nay the imperious necessity of blood-letting was confirmed by the experience of every one. In 1837 nine-tenths of the first patients treated exclusively by medicines perished. Subsequently the mortality was greatly diminished ; and in my practice it could have been attributed only to the free employment of the lancet. In 1839 we bled, with scarcely an exception, and we literally saved all of our patients. The opponents of blood-letting are constantly invoking attention to the lesions of the nervous system, to which alone their attention seems to be directed, and to the great muscular prostration which inevitably en-

sues as the fever declines; and the objection they apply to bleeding is, that it accelerates nervous prostration and aggravates the muscular debility. I saw nothing in 1837 or '39 or in our late epidemic to substantiate such an argument.

In almost every family which I treated, it is easy to institute a comparative estimate of different treatments. I will cite several. The family of G. Carantain. It embraced seven white persons, all of whom had genuine attacks. Four were well depleted at the beginning, three were not bled; of the latter one was cupped twice. The recovery of the four that were bled was speedier, more happy and less marked by prostration, nervous or muscular, than was the recovery of the other three.

Of four black negroes, all of whom were attacked, there were two very severely attacked. One of them was copiously bled, the other was not depleted at all. The first recovered very speedily, and in the case of the second the prostration was so excessive, as for several days to excite the most serious doubts of a recovery.

In the family of Mr. T. M. Anderson four young gentlemen were successfully treated in violent attacks of the fever. Their constitutions and temperaments were very similar. Three of them were copiously bled in the beginning of the attack; two of them were twice bled. The speedier and more happy recovery of these three than of him who was neither bled nor cupped, was a matter of general observation and of surprise to many. The prostration of the latter was more intense and prolonged than what was experienced by the other three.

The results in the large families of Capt. Marsh, Capt. Hinckley, Mr. N. Offutt, &c., coincided completely with those detailed. With regard to 61 fatal cases, I can state from personal knowledge and reliable information, that only in 14 was venesection practised. Hence in 47 cases it was not practised; and in the 14 cases in which the lancet was resorted to, I know that in more than a majority of them it was employed under unfavorable circumstances; and in six of them not before the second day. In one case death resulted from the supervention of another distinct disease. In one case the disease appeared to expend its force upon the brain of the sufferer, and there was confirmed lunacy and aphony from the beginning. In another the recovery was almost certain, when from great imprudence the fever returned and resulted in death.

Amongst the free colored people no blood-letting was practised in the first and fatal case. In the eight succeeding cases blood was drawn more or less copiously, and four were fatal. In all, with the exception

of two fatal cases, blood was drawn within a few hours after the attack; and in one of these two cases not until twelve, and in the other not until sixteen hours had elapsed. With such unfavorable results I desisted from blood-letting amongst the mulattoes; but the subsequent cases I saw were mild in comparison with those of the family in which five died.

Now, according to my observation I do not pretend to say that in every case marked by youth, previous good health and the sanguineous temperament, blood-letting is indispensable; but the important question is, are there not many cases in which it is indispensable to a cure? It appears to me as clear as the noonday sun, that there is a plenty of such cases; and yet the eloquent descriptions given of the deep and profound lesions of the nervous system in this disease—of the fast approaching debility which invariably prostrates the mighty and the strong, as well as the infirm and effeminate, and the direct and positive opposition made by some against venesection at any stage of the disease, cannot but frighten the inexperienced from even occasional employment of the lancet, the rash and improper use of which has no doubt often accelerated death. But if, as has been said, this instrument can vie with the sword in the number of its victims, I am myself fully persuaded that in yellow fever, to its non-employment is to be attributed many a death; perhaps more so in the country than in the city; in which latter place the disease hunts up its most numerous victims amongst those whose constitutions are either enervated by poverty or impaired by dissipation.

To obtain the good effects of blood-letting in severe attacks of yellow fever, it should be practised at an early period. A few hours often suffice for the complete establishment of those modifications of both solids and fluids, in which not only blood-letting, but all active therapeutic agents are not only useless but pernicious. When the action of the nutritive functions is suspended or paralyzed; when the blood has materially changed, in color, in consistence and in coagulability, then bleeding, and, a fortiori, its repetition, would be useless, nay highly injurious.

The great object of blood-letting is, not to bleed the poison out of the system, but to elicit and renew the energies of the nutritive functions in the remote elementary tissues. During the early hours of the disease, when the red blood has injected the face, the eyes, and the skin; when the head, the eyes, the back and limbs are tortured with pain; when the heart, goaded to supernatural exertions, propels with dangerous celerity the super-oxygenated blood through the delicate

tissues of the organs, I would bleed, that I might lessen the labor of the heart; that I might diminish the danger to which organs would be exposed from a virtual excess of blood; I would bleed quickly and copiously, that by lessening the quantity of the intoxicating fluid I might awaken in these tissues the recuperative energies,—the instincts of life, paralyzed by the poisonous cause of the disease, that by exciting into renewed action the nutritive functions and the absorbent system, these elements might be supplied to the blood, necessary to the healthy functional actions of all the nutritive organs, to the probable elimination of the poison itself, and to the restoration of health.

I have said a great deal more on the epidemic of Washington than I intended; for I had proposed to myself simply the discharge of a duty which circumstances devolved on me; to give an account of the origin and progress of the disease—its symptoms and general treatment.

Almost unknowingly to myself, and yet with a diffidence increasing as I became involved in the abstract discussion, I have been led to an enquiry into the pathology of the disease—that is, the peculiar action of the cause and its effects. I am well aware that books have been written by the ablest men to establish peculiar theories; that by Sydenham, perhaps the ablest of them all, the humoral doctrine has been so reconstructed as to challenge even at this day the favorable judgment of men; that a Cullen has applied his intellectual acumen to the investigation of fever; that Clutterbuck has labored to establish that inflammation of the brain is the primary derangement; that the modern Sangrado of France, in his eloquent diatribes against entities, ontologism, and non-essentiality, has yet given to fever a local habitation and a name, and can see in it nothing but a gastro enteritis; that Clanny and a host of others, even including him whose name I venerate in medicine above all living names—George Andral, has again vindicated the claims of a more rational humoralism—and yet in mine ignorance I have felt that a vacuum still existed. I have presumed that the causes of fever, including the congestive, and the cholera, &c., were, though unlike in nature, yet similar in form, and that to produce their effects, they must necessarily be introduced into the system. I have passed in array before my mind the varied morbid effects, as announced in the symptoms of all these febrile complaints, and I have been bewildered in the attempt to explain how effects so varied and opposite could result from the primary vitiation of either the blood or the nervous system, or from an encephalitis, or a gastro-enteritis. In

looking at the human body I have thought that it should be regarded as one great whole, consisting of many parts and systems; that though some of these systems play more important roles than others, they are no more liable to disease in fever than all the other elements which constitute the other tissues of the body, and that of all the functions of this complicated body the most important one is that, which, from the elements derived from without, and in part from within, is engaged constantly in the nutrition and renovation of the tissues, and particularly in the elimination of the effete matters. The blood, I fully believed, might be charged with the morbific causes of fever, and yet be compatible with a healthy exercise of those actions which take place between the ultimate atoms of the solids and fluids, from which the life of every organ and tissue emanates, and consequently from which emanates the totality of movements or actions which constitute life; and that this poison might be excreted without affecting the organization of the blood, or impressing a peculiar derangement on the nervous system or exciting any local inflammation.

In cholera and congestive fever, for example, I was unable to account for the rapid prostration and the sudden and extreme emaciation which take place, and for the thick black blood in the vessels, upon the sup position that the causes primarily produced the above mentioned derangements; when I remembered that analogous causes according to the different theories, producing the same primary derangements, developed an altogether different train of phenomena in yellow fever, typhus, &c. I came to the conclusion from an observation of the effects and phenomena characterizing those states of the system to which the inapplicable term "fever" is given, that the first link, the invariable antecedent in the chain of these effects must be a depravation of the functions of life itself, which exists only and exclusively in the nutritive atomic actions; a full and complete exercise of all and every one in every tissue of the body is necessary to perfect health; that these causes, thus modifying and affecting these functions, inherent in all the tissues, and upon which life and health depend, according to their nature and constitution produce the various effects we witness, as for example, in congestive fever and in cholera, in scarlatina and small-pox, in the continued, remittent and intermittent fevers; in all the atomic or typhoid; and that in yellow fever, the simplest type or form of these complaints, we can mark the progress of the morbific cause in its primary attack upon the nutritive actions, and the subsequent vitiation of the blood and fluids, engendering in their turn, according to individual susceptibilities,

sometimes a scarcely perceptible, sometimes a predominating derangement of the nervous system, as well as all the other symptoms.

Certain elements are necessary to healthy nutritive action, but their existence in improper proportions will derange the functions of life and finally destroy them. Oxygen is indispensable in the animal chemistry, to allow nature to dispose of the carbon, and to manufacture those constituents which are necessary to health; its superabundance as well as that of carbon is prejudicial to healthy atomic actions. Either pushed to a certain extent will arrest these actions and kill. If the atomic nutritive actions are not affected, so that the natural supply and waste continue, it is immaterial what noxious poisons are present in the blood; they are innocuous and health continues. The narcotic poisons, embracing all those which, in conseqeence of their affinity with its atoms or elements act directly upon the nervous system, must necessarily arrest the nutritive actions and thus induce death. And with regard to the causes of fevers, according to their intensity, will be the modifications of the sources of life, and their effects may be a speedy arrest of the nutritive vital functions and death, or such a train of symptoms and effects, which characterize these affections in all their grades; effects dependent for their modifications, in every individual, upon temperament, or rather the physical constitution, which always imparts an individual character to every case of fever whatever.

A thorough revision—a pruning of redundancies—the expression of many ideas in simpler and more comprehensible language—the adoption of more order and system in its arrangement, might tend to make this article acceptable to the Physician. But engagements which have been neglected, and the necessity of an immediate departure for the North, almost deny a re-perusal of what I have written. This much I will say, that I have given the subject the best reflection I was capable of, and that if in the history of our epidemic, as faithful as it was in my power to give, I have supplied a single fact—if I have suggested a single truth, or succeeded in eliciting attention to phenomena which hitherto have been neglected, and whose full investigation is, in my opinion, indispensable to a better knowledge of the nature of the disease, I shall feel that I have fully accomplished my undertaking, and paid a portion of that debt which the divine and noble art of medicine exacts from all of its disciples.

V.—LETTERS ON YELLOW FEVER AT MEMPHIS, TENN., IN 1853.

By Geo. A. Smith, M. D., Physician to the Memphis Hospital, and W. J. Tuck, M. D., of Memphis.

(We commend the following letters to the special attention of our readers. *Ed.*)

Memphis, Tenn., Dec., 1853.

Editor N. O. Med. and Surgical Journal:

Dear Sir—With the belief that it may be a matter of some interest to trace the phenomena of Yellow Fever when carried away from the head quarters of its epidemic fury, the following remarks have been written upon some cases which were brought for treatment to the Memphis Hospital.

The city of Memphis is well known to be by river about 800 miles above New Orleans, and is the lowest point of any considerable population which escaped the fearful epidemic of 1853, having been not only free from Yellow Fever, but favored with a much healthier summer and autumn than has fallen to its lot for several years. The mortality from May to August, inclusive, (generally the unhealthy season here) was in 1853 considerably less than half what it had been the two preceding years.

The nearest point to Memphis at which Yellow Fever prevailed as an epidemic was Napoleon, Arkansas, about 200 miles below; and it ascended to no point above the city; so that Memphis was the lowest point on the Mississippi river, of any considerable population, which enjoyed this happy immunity. On the other hand, it prevailed with virulence at Natchez, Vicksburg, and several of the smaller towns intermediate.

All the cases which were brought to the Memphis Hospital, in number 62, were landed from the river, and contracted the disease in New-Orleans and its immediate vicinity. The mortality, as might be expected, was very great, from the fact that they were not landed till the disease had taken full possession of the system, and then carried the distance of a mile from the wharf-boat to the Hospital, on an open dray. Of the 36 who died, 17 lived less than twelve hours after their arrival.

Three methods of treatment were adopted, of which the most successful, in fact the ouly successful one, was the following: On their arrival, after a hot mustard foot-bath and being warmly covered in bed, a laxative, if necessary, was administered, of an infusion of senna,

rhubarb and manna ; cups and mustard to epigastrium, with ice given solid and iced mucilage ; opiates, particularly the Black Drop, and milk punch ad libitum. Several cases were cured under this treatment, after the black vomit had been well established. The other methods tried were that of calomel aud opium in broken doses, and quinine in scruple doses, with twenty drops of laudanum—both entirely without success.

The question of the contagiousness of Yellow Fever was fully tested, and the result confirms the impression, that without the pre-requisite conditions of atmospheric and climatic derangement, Yellow Fever is no more liable to be communicated from one subject to another than gout or rheumatism. At any rate the 62 patients who were treated in the Memphis Hospital were all of them distributed promiscuously through the wards of the institution ; treated there throughout their progress to recovery or death, and no single instance occurred of the disease spreading beyond the patients imported with the disease upon them.

The above brief statement is respectfully submitted to you. In relation to so terrible a scourge as the epidemic of 1853, it is presumed that as large an accumulation of facts as possible is most desirable—facts derived from observations made under all possible circumstances; and, provided they are plainly and truthfully stated, ought to be acceptable, whatever the sources from which they are derived. The circumstances under which my observations were made were in this respect peculiar, indeed almost unique, as Memphis was probably the only place where a considerable number of *imported cases* could have been treated, in a region itself exempt from epidemic influences.

Respectfuily,

Geo. A. Smith.

Remark.—We are at a loss to conceive how any one could expect benefit from doses of 20 grs. quinine and 20 drops laudanum in the advanced stages of Yellow Fever—say the third or fourth day. We have seen no such treatment recommended by any advocate of quinine, bnt on the contrary, always forbidden. Calomel and opium in this stage is not much better. F., *Ed.*

Editor N. O. Med. and Surgical Journal :

Memphis, Tenn., March 19, 1853.

My Dear Sir—I applied a few days since to Dr. Smith, Physician of the Hospital here, in relation to the snbject of your enquiry, and he

informed me that he had recently transmitted all the statistical information connected with the subject to Dr. Hester, and which probably reached its destination about the time of his death. The Doctor wished me to refer you to that letter. He informed me at the same time that there were 62 cases of Yellow Fever admitted into the Hospital, which were taken from the boats landing at this place and 36 deaths, aud that in no instance was the disease communicated to others. As well as my memory serves me, we had only three cases of Yellow Fever in this city; two of these had been exposed to the epidemic in New Orleans, and probably contracted the seeds of the disease there; the third was a citizen of our place and had not been absent, and it was regarded at the time as a sporadic case, such as occasionally occurs in this city.

You request me to give my views in regard to the contagiousness of Yellow Fever. So far as I have been competent to investigate this subject, and so far as my own observation extends, I am a decided non-contagionist. During my residence in New Orleans, in the summer of 1841, and while one of the attending Physicians of the Charity Hospital during the epidemic of that summer, I think it was the unanimous sentiment of all the prominent physicians of the city that the disease was not contagious, nor do I think any one pretended at that time that the epidemic of that season was imported from abroad. Such, I believe, have been your own statements, as evinced in your writings on this subject. Whether the opinions of the physicians of your city have undergone any change in relation to this matter since the breaking out of the late epidemic, I have not been able to gather any accurate information. Still the intimation has been thrown out, that circumstances have been developed in connexion with the recent epidemic, which have led some to alter their opinions and to favor the doctrine of contagion. However that may be, we are all perfectly well satisfied *here* that the disease could not be communicated by contagion in the atmosphere of our city; and it seems to me, if I understand the meaning of the word contagion, in its proper acceptation, that contagious diseases may be communicated and reproduced from one person to another, without any regard to the temperature or latitude; as for instance, small-pox, scarlet fever, measles, etc.

Now, let us look for a moment to the facts of the case in our city. We adopted no quarantine regulations; we had instituted no especial sanitary regulations with the view of preventing an incursion of the epidemic. Boats freighted with merchandize, saturated with the atmosphere of an infected city, landed at our wharf almost every day; a

number of persons affected with the disease were carried through our streets to the Hospital, and some of them dying in private families, and yet not a case was communicated to any of the nurses, friends or physicians.

It certainly must be a very singular and unprecedented sort of contagious disease which could not be communicated in a single instance, under such favorable circumstances! when the streets of our city were as filthy as usual; when the weather was very warm; when there was no avoidance of exposure, and when every condition existed to promote contagiousness! Does any person suppose for a moment, that there could be brought into our city such a number of cases of any of those diseases ordinarily termed contagious, without the occurrence of a single instance of communication to the unprotected and exposed? During the month of September it fell to my lot to be called upon professionally to attend some cases of Yellow Fever occurring on the steamer H. D. Bacon, from New Orleans. When the boat arrived here some ten cases were taken off and sent to our Hospital; I was retained professionally to attend some of the officers to their point of destination, St. Louis. I must confess that I had some apprehension of danger when I first went on board the boat; not from fear of contagion, but from the infected atmosphere, which I supposed might still have remained to some extent. The boat was a large one, heavily freighted with merchandize, and a large number of the crew and passengers were sick, some of whom died before reaching here. Here, certainly, was as fine and fair an opportunity for an unacclimated and unprotected person to contract a contagious disease as we can well suppose to exist; and yet what are the facts of the case? Not a passenger, out of some dozen or more, who took passage on the boat, above the infected region, suffered from any symptom of the disease. And yet the second clerk, and the captain, who was on duty until he reached St. Louis, had contracted the seeds of the disease in New Orleans, and after I returned home to Memphis I learned that both had died of Yellow Fever several days after their arrival at St. Louis. I am happy to state that the several cases which were under my care were convalescent when I reached St. Louis, and I returned immediately home; and although exposed constantly day and night to whatever source of contagion there might have been, yet my health remained not only unaffected, but was better than usual.

It is said that the epidemic which has swept with so much fatality over the fairest regions of our southern country, differs in some respects from the preceding epidemics. We are disposed to believe the differ-

ence exists rather in degree than in kind. The cases we have seen here, imported from your city, have been just the same in symptoms as those we witnessed in New Orleans in 1841. It is true, the recent epidemic has been much more malignant and fatal than any of the previous ones; and it is stated that many more of those supposed to be acclimated were affected with the disease than at any previous period; but the main peculiarity that has been insisted upon is, that the epidemic extended over a much larger scope of country than it has been previously known to do and occurred in many towns and villages, where it was never known before to exist; thus appearing to favor the opinion entertained by some, that the disease was transmitted to those localities by contagious influence, and not originating from local causes.

Now it is a very important question to determine, whether, in those cases, the disease did *originate* in certain localities, or was trans mitted by persons affected with the disease, steamboats, etc.

We have long since learned enough of human nature to know that the citizens of any particular town or locality are very slow to acknowledge their location a sickly or unhealthy one; and this is very natural; and no doubt many of the citizens of various villages in Louisiana and Mississippi are disposed to attribute the visitation of the epidemic among them to importation from New Orleans. Natchez has been in the habit of doing this for the last thirty years; and yet with all her quarantine regulations, as likewise those of Vicksburg, the disease has prevailed in those places with as much or more fatality than in New Orleans. It is true the quarantine regulations were not as rigid as they might have been; yet if we are correctly informed no steamboats from New Orleans were permitted to land, nor were any of the sick on those boats taken care of until the epidemic made its appearance in those towns. Now compare this condition of things with that which existed in Memphis, not much more than a day's travel on a fast boat. Here we imposed no restrictions—had no quarantine. As before mentioned, large quantities of goods, which had been remaining for weeks in New Orleans, were daily put off at our wharf and distributed through the city; many of these goods of a molient character, well calculated to act as fomites to convey contagious effluvia; and moreover, a number of persons affected with Yellow Fever, daily taken from the boats and dying within the precincts of the city, and yet not a single instance of its communication occurring.

But the enquiry is raised, if the disease was not communicated or transmitted from one point to another, in the region South of us, why did it occur and how did it originate in some of those localities hitherto

regarded as healthy and where the epidemic was never before known to exist? We reply to this by asking, how did it originate in New Orleans, Mobile and other places, at the time preceding which the epidemic was never known to occur in those latitudes? The fact is, a combination of *local causes*, we believe, sprang up at that time; which combination had never existed previously.

We are very much inclined to the opinion, that there existed in the whole region of country embraced within a certain line of latitude South of us, a *certain predisposing epidemic constitution of atmosphere*, which aided in giving rise to so extensive an epidemic. We believe that in the same way there is a predisposing and peculiar constitution of atmosphere prevailing when cholera appears in our country, only the latter is more extensive, and is but little influenced by climate, whereas the former is confined almost exclusively to hot latitudes. The probability is that the increased fatality and malignity of the epidemic this year was the result of a greater degree of this predisposing influence in the atmospheric constitution. This being the case, and other causes, such as animal or vegetable miasm, being the same as in previous epidemics, we may readily explain why the recent one should have been more fatal and malignant as well as extensive in its scope. Now supposing this predisposing atmospheric constitution to exist throughout the whole of a certain region of a warm climate, may we not thus account for the development of the disease in certain localities where it has never occurred before, without resorting to the doctrine of communication or contagion?

This opinion would seem to be corroborated by the fact, that in some of these localities it has been fouud impossible, from the best information we can obtain, to trace out the least communication of any kind with those towns or places where the epidemic was prevailing. And may we not in this way account for the exemption of our city and that of all other towns on the river north of a certain latitude, say that of Napoleon or Lake Providence? In this city surely we had enough of animal and vegetable filth to produce disease, as well as every circumstance to favor contagion, but we did not have the predisposing constitution of atmosphere. We are therefore disposed to believe that the disease is of local origin and not contagious; and we offer the above theory as a sufficient one, we believe, to account for the extension and prevalence of the disease in Southern localities not hitherto subject to it, without the necessity of resorting to the doctrine of contagion, importation or transmission.

Yours, respectfully,

 W. J. Tuck.

V.—YELLOW FEVER IN PLAQUEMINE, PARISH OF IBERVILLE.

BY J. B. HACKER, M. D.

An Epidemic Yellow Fever prevailed in this town from the first week in September until the latter part of October, mostly among the unacclimated at first, but subsequently attacking those that had resided many years in the State, even natives who had never left the place and negroes.

The first case occurred in a German, employed in a cabinet maker's shop. He went to the city during the epidemic, but did not remain there more than a day or two. Two or three days after his return he was taken with Yellow Fever and recovered.

A few days after (about the 25th of August) another man in the same house was attacked and died of black vomit; before he died his brother and mother got sick and both died of black vomit; the mother on the 3d and son on the 6th of September. These four cases occurred in the same house.

About the same time with the first case, a man came from Baton Rouge in the evening and remained all night in a small cabin on the river. Next morning he took lodging at the Iberville Hotel, took sick and died on the 26th of August of black vomit. I am told this man had been in the city a few days before. No other case occurred in this house.

After these few cases the disease increased rapidly and continued until the latter part of October, when it declined gradually, with an occasional case to this time, 9th December. As the disease declined in town it prevailed more in the surrounding country, on the coast above extensively and below Plaquemine and on Bayou Plaquemine. For a considerable time it remained in the first street from the river, and the few cases that were observed at first in the back part of town were in those that had been in contact with the sick.

In the majority of cases that came under my observation, the fever began with a chill of greater or less severity, followed by violent headache, referred to the forehead over the eyes, pain in the back and abdominal extremities, burning heat of the skin, face flushed, eyes injected, tongue covered with a thick white fur in the middle, red on the edges and at the point, generally large; not much thirst, urine scanty, bowels easily moved; sensibility of stomach on pressure; nausea, and in some cases vomiting; pulse full and frequent, but with the exception of a few cases, not hard, the number of pulsations ranged between 100 and

120 in a minute. These symptoms continued three days, with a very well marked exacerbation at the approach of every twenty-four hours. On the fourth day a complete state of apyrexia supervened, and with well directed care the patient recovered. When black vomit supervened, it was generally on the fourth day; hemorrhage from the gums, bowels, nose, etc., were frequent accompaniments and generally happened on the third or fourth day; in a few cases the epistaxis was profuse and obstinate.

A number of blacks were attacked, but the disease seemed less fatal. As far as I observed in my practice, the cases were easily managed and recovered favorably, although precautionary measures were not as strictly observed as with the whites.

I shall conclude this brief history of the disease in this place by mentioning an instance of its appearance on a plantation about ten miles from this, on Bayou Plaquemine. I will simply relate the facts as they occurred.

On the 17th of September I was requested to call at Captain S.'s house, to see the pilot of the steamboat Pitser Miller, then in front of the house. I found him with high fever, headache, pain in the back and limbs, and all the other symptoms of Yellow Fever. By my advice he was taken from the boat into a room distant from the Captain's house about sixty feet, where he received all the attendance necessary, and recovered in the course of seven or eight days. Soon after and while he was convalescent and still on the plantation, several cases occurred; the first in a black man, who nursed him and was most of his time in the room; the second case was the Captain's child, then his mother, and subsequently several negroes, making in all nine cases. I will remark that none of those attacked had been in town during the epidemic, and that there was no Yellow Fever in the neighborhood at the time.

I might relate other similar instances, did I not fear to occupy too much space in your estimable Journal.

[It is to be regretted that the author did not state whether the pilot of the P. Miller had been at any place where Yellow Fever was prevailing a short time before he was attacked. As Bayou Plaquemine is seldom open to the river in September, it is presumable that the boat was laid up at the time. *Ed.*]

VI.—YELLOW FEVER AT FRANKLIN, LA.

BY J. W. LYMAN, M. D.

FRANKLIN, La. Feb. 14, 1854.

Dr. E. D. Fenner:

DEAR SIR—Yours of the 4th instant reached me four or five days since, and I take pleasure in employing my earliest leisure in giving you, as far as in my power, the required information.

In this town there were only five cases of Yellow Fever last fall, and these all occurred in the house of one of our most valuable citizens, Mr. Simeon Smith. I will first, as briefly as possible, give you a history of these cases, and make a statement of the collateral circumstances existing previous to and at the time of their occurrence.

CASE 1.—Mrs. Smith, aged 45 years, was taken with a chill just before daylight, October 19th, which was speedily followed by fever, accompanied with pains in the head, back, epigastrium, etc. At first I looked on the case as an ordinary but pretty severe attack of remittent fever, but before the end of the second day, my mind was fully made up that I had to deal with a genuine case of Yellow Fever. The fever yielded at the end of the third day, and the patient ultimately recovered, after extreme prostration and a tedious convalescence. The three next cases occurred in persons who were in close attendance on Mrs. S. from a very early period of her attack.

CASE 2.—Mrs. Johnson, aged 30 years, stepdaughter of case 1, had chilly sensations at ten o'clock, A. M., October 23d, soon followed by fever. This case differed in no material respect from No. 1, except that she had moderate bleeding from the gums and retention of urine. She slowly recovered.

CASE 3.—Mrs. McMillan, aged 32 years, sister to the above, was taken with the chill about one o'clock, A. M., October 24th. Hers was a very violent case from the first moment of attack, and progressed steadily on from bad to worse, to a fatal termination, which took place at 6 o'clock, A. M., October 28th. This patient was six months advanced in pregnancy, and aborted 24 hours before death. Several hours before death she threw up small quantities of black vomit.

CASE 4.—Simeon Smith, aged 52 years, husband of case No. 1, and father of cases 2 and 3, by a former marriage. Taken with a chill at daylight October 24th. This case was at first apparently no severer than cases 1 and 2; but in the evening of October 28th, the patient without any very assignable cause, fell into a state of profound coma,

and died October 29th, at 11 o'clock, A. M. After death, black vomit passed in considerable quantities from his bowels, as I was informed by an experienced nurse; one who has seen much of the disease in New Orleans and Mobile. In the night of the 24th, the same day on which the two last cases were taken, we had a pretty heavy frost, and small quantities of ice were seen by one or two persons in the neighborhood. Another heavy frost occurred on the night of the 29th.

CASE 5.—Ella, aged 11 years, daughter of cases 1 and 4. Taken with a slight chill November 24th. Being unable, from indisposition, to leave my house, this case fell under the care of Dr. C. M. Smith. For the first two or three days her case was regarded as one of our autumnal remittents, but this opinion was changed on the third or fourth day, when she threw up a large quantity of black vomit. This was saved and has been shown to several physicians conversant with its appearance, and no one expresses the least doubt of its character. The patient recovered after a lingering convalescence.

In relating the above cases I have avoided entering into a minute detail of symptoms; it being my design to give, in as few words as possible, such a history of the cases as would leave no doubt in your mind, as there is none in mine, as to the true nature of the disease. In relation to the treatment, I have nothing new to offer. I adopted what I believe is usually called the expectant plan, viz., mustard foot baths, a cathartic to procure free evacuations within the first 24 hours, diluent drinks, flaxseed injections subsequently, if deemed requisite, minute doses of morphine to procure rest, when not positively contra-indicated. In fine, after the first day the patient was trusted pretty much to nature and the watchful care of attentive nurses. Of the various modes of treating this insidious disease I am by no means properly qualified to speak understandingly; but I must confess that I am far from being satisfied with my own treatment, nor have I any more confidence in that which I have known pursued by others.

Here it may be proper to refer to the local causes of epidemic disease which existed in Franklin, where the disease commenced. It is an undoubted fact that our little town, never very notorious for its filthiness, was never in a cleaner condition—was never freer from filth and other local causes which are so often appealed to as the originators of pestilential diseases, than it was on the 19th October, when the first case occurred.

Our Town Council, with commendable zeal, had been more than usually active in their endeavors to improve our sanitary condition. In fact, no appreciable local causes existed in or about our town; and if a

scientific committee had been appointed to select the dryest, cleanest and healthiest spot in the whole town, it would most certainly have selected the residence of Simeon Smith; and yet, strange to say, here, and here only, did the fell destroyer begin and end his ravages. In the absence then of any such causes, where shall we look for one adequate to the production of the disease? Whence came the infection, and in what manner did it reach this family? These are questions much easier asked than answered, and I, for one, am unable to solve them; but I submit the following statement of facts, which although of apparently trifling importance, may assist, in connexion with facts gathered from other sources, in throwing some light on the mode of propagation of this disease.

Some time about the 25th of September, in consequence of positive information being received here that Yellow Fever was prevailing in Centreville—a little village five miles below Franklin on the Teche—our Town Council adopted pretty stringent measures to prevent a free communication between the two places. Persons living in Centreville and having business to transact in Franklin were permitted to remain here but three or four hours during the day, and our citizens were warned that if they visited any infected place, they would have to submit to a nine days' quarantine before they could return to their houses. The latter part of this arrangement was not always enforced, and at *this time* physicians were exempt from the restriction. A barrier was drawn across the road half a mile below town, and a guard stationed there to admit or reject persons from below. This barrier was kept up with tolerable efficiency until about the tenth or twelfth of October, when Centreville being reported healthy, the prohibition was withdrawn for one or two days.

On the 14th October Mr. Smith (case 4th) sent his negro man with horses and wagon to the store in Centreville, in which he was a partner, to bring up some goods to his store in Franklin. The clerk informs me that the following articles only were brought up, viz., one one bale of oakum, box of axes, box of children's shoes, and a paper package containing India Rubber coats. These goods had arrived in a vessel from New York, and had been but three days in Centreville. Mrs. S. (case 1) was in the store when these goods arrived and were opened.

This day (October 14th) the fever again broke out in Centreville, and a system of non-intercourse more rigid than the first was established. On the 19th Mrs. S. was attacked, and the only known com-

munication of the Smith family with an infected place was as stated above. At that time the family consisted of Mr. S. and wife, five children, a clerk and five or six servants. Mrs. M., (case 3) living in Franklin, and Mrs. J. (case 2) living three miles from town) both came to their father's on the day Mrs. S. was taken. Mr. H. Smith, eldest son of Mr. S. Smith, who arrived from the North a few days before Ellen (case 5) was attacked, and who had been pretty constantly in attendance on her, escaped. Of all the nurses and medical attendants, not one was attacked.

This, I believe, comprises all that is worth relating in regard to the Yellow Fever in Franklin ; above this point there was not one case, while the lower end of the parish suffered to a great extent. And now a few words in reference to our famous quarantine, and I have done. This town had suffered severely by the yellow fever in 1839, and to avoid, if possible, another such calamity, our Police Jury met and adopted certain quarantine regulations, in which most good citizens acquiesced, believing that in their existing uncertainty as to the contagiousness or non-contagiousness, transmissibility or non-transmissibility of yellow fever, it would be the part of prudence and humanity to adopt such reasonable measures as would be calculated to stay the pestilence and thereby escape its fearful consequences.

A quarantine station was established, and a house prepared for a hospital near the junction of the Atchafalaya and Teche, and a physician and attendants appointed to examine steamboats and other crafts plying in this trade. All vessels from infected regions were required to undergo nine days' quarantine, dating from the time of leaving such infected region. Another station was established on the Atchafalaya, a short distance below Pattersonville, and a physician appointed to examine the mail boat and vessels coming in from sea.

Shortly after the quarantine went into operation, (about the 20th of August) the steamboats Pitser Miller and Planter arrived at the station. The latter served out the required time and came up the Bayou without opposition. The former, instead of remaining at quarantine, crossed Grand Lake to Fausse Pointe, discharged part of her cargo and passengers, returned to quarantine and landed a young man who was in the last stage of yellow fever, and who had black vomit in half an hour after he was placed in charge of the Physician, and died next morning. Under these circumstances the quarantine physician properly refused to grant a permit to the boat to proceed up the Bayou.

Notwithstanding this refusal, the commander of the boat being, one would suppose, somewhat wearied with such an inactive and unprofitable life, and withal not over-abundantly supplied with the creature comforts, determined to take the responsibility of leaving and risking the consequences.

The boat then left, discharged at Pattersonville what cargo she had for that place, and proceeded up the Teche to within half a mile of Franklin, where she received warning to proceed no farther. After some little time spent in parleying and deliberation, and seeing a pretty formidable posse collected on the wharf, determined to dispute her passage, she returned to quarantine ground, discharged the portion of her cargo intended for this and other places on the Teche, and recrossed the lake.

Some considerable difficulty, which came near being attended with serious results, occurred with the mail boat Fairy, principally in consequence of some of her passengers being unable to take the oath required by the Board of Health, and the refusal of the Fairy to proceed to quarantine. After detaining her below Franklin for two or three hours, the difficulty was adjusted without bloodshed, by a sort of compromise, not very satisfactory to any of the parties concerned.

The quarantine was not generally so rigidly enforced as it should have been, in order to test the question of the transmissibility of yellow fever; but much more so than could have been expected from persons who were so little acquainted with the best modes of organizing and sustaining it.

So far as I have heard, the first case of yellow fever in this parish occurred in a young man from New Orleans, who crossed the lake on the Fairy and was permitted to land three miles above Pattersonville, on condition that he should remain in the country and not in the villages. After remaining on a plantation a few days, he was taken sick, and the proprietor, fearing he might have yellow fever, had him conveyed to Pattersonville. This man ultimately recovered, and I am informed that one of the physicians of Pattersonville pronounced his disease yellow fever. Be this as it may, it was not long before cases occurred in rapid succession, and of so unequivocal a character as to leave no doubts of their nature; and it is a fact of notoriety that this same physician (Dr. Grant) pronounced the prevailing disease to be yellow fever long before the other resident physicians, some three or four in number, would acquiesce in his opinion.

I have heard many other reports in relation to the spread of the epi-

demic at Pattersonville and Berwick's Bay, but as I can give nothing as facts, I should be going beyond my intentions in giving mere rumors.

This concludes all I have to say at present on yellow fever, and should you be of opinion that any thing contained in this communication may be made subservient to the interests of humanity and science, you are at liberty to use it as you may think most conducive to those ends.

Yours, &c.,

J. W. LYMAN.

VII.—THE YELLOW FEVER AT GRAND GULF, MISS, IN 1853.

BY E. M'ALLISTER, M. D., PORT GIBSON.

Editor N. O. Med. and Sur. Journal:

DEAR SIR—I propose giving you a brief account of my practice and observations in the late yellow fever epidemic of Grand Gulf.

The disease assumed the character of sthenic. The pulse ranged from 120 to 180 beats per minute, full and strong; the skin intensely hot, cheeks flushed, eyes injected, pains in the muscles as if neuralgic, tongue whitish on the dorsum and red at tip. A disease of one paroxysm, with slight remissions, terminating in three or four days, in death or convalescence. The tendency to death seemed to be by congestion of some vital part; of the lungs, brain, stomach, liver, &c. The most plausible theory of black vomit is, that it is caused by congestion; and the effusion of blood into the cavity of the stomach as its result. It has been pretty well demonstrated in pathological researches, that the jaundiced condition of the skin and eyes is produced by obstruction in the liver, gall-stone, structural or functional derangement. The idea that it can or ever has been produced by any other cause, is perfectly gratuitous, according to the lights before us, as we have no evidence of the fact.

Conforming my practice to the above diagnosis, I adopted the system of cooling and depletory remedies, to unload oppression and reduce excessive excitement—to wit:

The sick room was freely ventilated:

When the fever rose and the skin became hot, either with or without perspiration on it, the patient was put into a cold plunge bath, in which he was allowed to remain until his skin became cool and his pulse reduced to near its natural heat. This required from three to ten min-

utes. Cold water was at the same time poured on his head, and his skin rubbed with towels. He was then put to bed and two or three blankets thrown over him. If a natural perspiration came on, the blankets were allowed to remain on him six or eight hours, and a cup or two of warm tea administered; but if the fever rose again, the blankets were removed, plenty of cold water given to drink and another bath given—some one of the following: Plunge bath, shower, wet sheet pack, sitz bath, head bath and sponge. Iced water was applied to the head when it could be had; and if the feet became cool, they were put in hot water. If the patient was of a weakly habit, water of milder temperature was used to the body. Throughout the whole course of the disease some one of these baths were used whenever the fever rose.

After quieting the irritability and excitability of the system to some extent, which could generally be effected in a few hours by the above means, I gave from two to four of the following doses, made into pills, at intervals of one hour: Calomel, grs. v; blue mass, grs. v; ext. hyosciamus gr. 1. If this did dot operate on the bowels in due time, Cook's pills, comp. cath. pills or castor oil was given. These medicines agreed well with the stomach and operated gently on the bowels, bringing off dark bilious matter. A few grains nitrate of potash, largely diluted in water, was also given every three hours; also an occasional opiate.

In some few instances, in which there seemed to be great torpidity of the secretive functions, I gave ipecac emetics with good effect. If after using the above means 20 or 30 hours, the violence of the symptoms continued to recur, with a full, strong pulse, I resorted to the lancet. This I found necessary in four cases, all of whom I bled freely from the arm, and with the happiest results, and all of them recovered. One of these cases deserves special notice. She was a lady of a rather delicate frame, and about seven months advanced in pregnancy. After being sick about twenty-four hours and using means, her symptoms rose to a violence I had never before witnessed. The commotion of her heart, arteries and nerves suggested to my mind the idea of a tornado existing in her system. Believing this state of things could exist but a short time without a destruction of organization; and that the lancet was the only means by which relief could be obtained, I accordingly bled her from the arm. The first bleeding the blood was allowed to flow to the amount of thirty-two ounces, as near as I could guess, with but slight impression on the pulse. I then stopped the bleeding and waited two hours, at the expiration of which time her symptoms had

risen to their former violence. I again corded the same arm and from the same orifice allowed the blood to flow to the amount, I suppose, of at least twenty-four ounces, while she remained in a sitting posture on the bedside. The desired effect was then produced, a relaxation of the pulse, when all the sympathies of her system acted in harmony. After this I did not find much difficulty in conducting the case to a convalescence. She went her full time in utero-gestation, was delivered of a fine boy and is now in good health.

Many cases which occurred were of so mild a type that nothing was required but cold water to the head and one cathartic.

In treating my cases, I avoided the use of all articles at all tonic, stimulant or excitant.

Such is a brief outline of a system of medical practice pursued by me in the late epidemic of Grand Gulf, the good effect of which, in relieving the sufferings of the sick and restoring them to health were so signally manifest, that all who witnessed them were filled with admiration.

I treated fifty cases, all white except eight, and all recovered except one, a child about four years old.

As regards the cases treated here on the quinine, stimulating and heating plan, I can say nothing from experience, for I treated none in this way; but from what I can learn from others, very few of the bad cases to whom this practice was administered recovered; and when used in mild cases, it produced a great deal of unnecessary suffering.

According to my observation, the disease was no more severe, under similar circumstances, with persons who had lately come to the country than with natives, or those who had been here a long time.

In all classes of people, I think the young and middle aged suffered most.

As regards the contagious nature of the disease there is much contradictory testimony afloat. I know of no incontestible instance in which the disease seemed to have been communicated from one person to another. It is also perhaps useless to speculate on the origin of the disease in Grand Gulf and Port Gibson. Some attribute it to importation.

Grand Gulf is situated on the river bank, on a level plain. Port Gibson is seven miles back in the country, but on rather a level locality; both places on rich alluvial soil.

For the last eighteen years in this country I have been a pretty close observer of the seasons, and I am sure I never before witnessed such

repeated floods of rain, attended with such excess of thunder and lightning, and succeeded by such hot, sultry days, as occurred here the latter part of the last summer. Many times the localities on which the villages stand presented the aspect of marshy districts—with all the concomitants of heat, moisture and vegetable matter, exposed to the sun in a state of decay; and I will venture the opinion, that whenever there is a similar combination of circumstances, they will be succeeded by a similar or some other epidemic disease.

Port Gibson, Miss., Dec. 28, 1853.

Part Second.

EXCERPTA.

I.—ON THE ZYMOTIC THEORY OF ESSENTIAL FEVERS AND OTHER DISORDERED CONDITIONS OF THE BLOOD.

Extract from the Prize Essay af S. G. Armor, M. D., of Cleveland, O.

There are few inquiries in pathological science of more interest than those which relate to changed conditions of the blood; for whether we regard it as endowed with a distinct vitality and obedient to the general laws of cellular growth, development and decay; or as ministering to the nutritive and textural wants of the system in the elaboration of fibrin from elements furnished by primary assimilation; or as connected with important chemical changes essential to a healthy action of the system; whether we regard the blood as contributing to one or more of these purposes in the animal economy it becomes at once evident that destruction of its vitality, or change in any of its constituent elements, must be followed by serious constitutional disturbances. Hence the interest with which its diseases should bes tudied and the importance of understanding, in a curative point of view, the primary and secondary impression of disease upon this fluid.

It must be confessed, however, that the question of priority or sequences, although of much importance to him who thinks or reasons about the nature, origin and phenomena of disease, is often one of difficult solution. But to arrive at greater certainty on this point if possible, so far at least as relates to the essential features, is the object of this Essay, and if I shall succeed in any degree in pointing out the distinction between symptoms of diseased action and diseased action itself, I will have, to some extent at least, accomplished my object.

In M. Andral's classifications of Lesions, in which he makes all disease to exist, he embraces some in which no notable change of either organization or composition can be detected. Yet it is worthy of inquiry as to whether this eminent pathologist has not included in his lesions some which are but symptoms, not properly diseases—actions and not states.

It is not my purpose, however, at present, to enter this field of inquiry. I desire to call attention to another question in which no such controversy can arise.

In the essential or idiopathic forms of fever, it is evident that change has been induced in the blood by the admixture of foreign matters. The proof of this consists in the fact—

1st. That diseases analogous to those fevers have been induced by injecting putrid matter into the veins of animals.

2d. These fevers are readily produced by the introduction of animal poisons into the blood, as in the case of small-pox, measles, &c.

3d. These poisons are known to operate through the medium of the air, by thus obtaining access to the blood through the lungs.

4th. The non-contagious fevers, such as intermittents and remittents, are universally admitted to depend upon a poisoned or changed condition of the atmosphere.

5th. Actual observation establishes the fact that the blood is altered in all essential or idiopathic fevers.

The best point of departure, therefore, is the general fact (for it should be regarded as such) that all essential fevers depend primarily on a poisoning of the blood, and the proof as to primary impression will be given in illustration of the facts already cited.

It must not be inferred, however, that I am laboring to establish the identity of fevers. No such inference can be legitimately drawn from any fact or reason which I shall present. True, so far as the general fact is concerned that all foreign matters, when introduced into the blood, change either its physical, chemical or vital properties, all essential fevers may be regarded as a unit. Yet observation abundantly establishes the fact, that different poisons act differently on the human constitution, and upon the peculiar and specific character of each depends not only the destructive effect on the blood, but the local lesions that will ensue. Urea and its compounds, if retained in the blood, affect the brain and nervous system, and are apt to give rise to a low grade of inflammation in serous and sero-fibrous tissues ; while mucous structures will suffer but little. But the small-pox virus spends its force upon mucous and cutaneous structures, and leaves unharmed the serous and fibrous structures.

There can be no explanation given of this other than the general fact that the tissue or viscus affected seems to be that which has an affinity for the poison which has to be eliminated from the blood. In this process of elimination, inflammation and its sequela are excited and local disease becomes manifest. Hence all essential fevers should be regarded as distinct in species, according to the circumstance of the primary sedative impression. This is the only true and rational classification of fevers.

We feel authorized in asserting then, as a starting point in our reasonings, what observation abundantly establishes, that each specific miasm has its own peculiar and distinct law of development. But in the absence of all reliable information, as to the essential nature of these miasms, it would be idle to speculate. Our knowledge on this point must, at least for the present, rest on observation.

But it will be at once perceived that our knowledge of the action of remedial agents is not more certain. Indeed the perfect analogue of one is found in the other, and the reasoning applied to one applies with equal force to the other. Thus, that mercury will excite inflammation of the salivary glands ; arsenic, the mucous structures ; belladonna, the skin; ergot, the uterus, etc., has long been a matter of observation. But why they should be so is just as obscure as why the typhoid poison should select for its destructive action the glands of Peyer, or the small-pox poison should spend its influence upon the dermoid structure. The articles of the materia medica furnish a just illustratinn of the action of all foreign substances in the production of disease. Mercury, arsenic or croton oil, if uncontrolled by the judicious skill of the physi-

sician, is capable of giving rise to diseased action with as much certainty and as varied in its manifestations as either of the animal poisons to which I have alluded ; and analogy would lead us to suppose that if we could control the one, as we can the other, miasmatic poisons might be used as therapeutic agents. That all agents that affect the vitality or composition of the blood bear certain pathological relations, cannot be doubted ; but that by no means proves the doctrine of identity. As well might we assert the identity of small-pox and typhus fever, from the fact that the fibrin of the blood is found defective in both.

An important point however to be established, before conclusions are drawn, is the fact, that the blood does undergo change in disease, and from medication, diet, etc. ; for if this be denied, our conclusions will be without a predicate, and therefore unsound. But the chemists have happily settled this point by furnishing us accurate analyses of the blood both in health and disease. In the condition of health the venous blood of a man, as represented by the number 1000, is composed of—

Serum, - - -	869,1547
Globules (fibrine included)	130,8458
	1000

This varies, however, according to sex, age, temperament, kinds of food, evacuations, etc. The rapidity with which some of the solid constituents of the blood are diminished by blood-letting, for example, is very remarkable. Thus, according to the researshes of Dumas, the blood of a robust young man, of 23 years of age, gave—

At the first venesection,

Water, - - -	780,210
Globules, - - -	139,129
Albumen, Salts, Fatty and extractive matters,	80,661
	1000

At the third venesection,

Water, - - -	853,46
Globules, - - -	76,19
Albumen, Salts, Fatty and extractive matters,	70,35
	1000

The more solid constituents of the blood, it will be seen, are rapidly supplied by a compensating quantity of non-sanguineous fluid ; and hence the value of blood-letting when it is desirable to promote absorption.

Diet and drinks also very readily affect the constitution of the blood. Ac-

cording to M. Denis, in the blood of a young man 21 years of age were found—

Water, - - -	770
Globules, - - -	154
Albumen, &c., - -	76
	1000

After forty days' use of watery drinks—

Water, - - -	804
Globules, - - -	111.9
Albumen, - - -	84.1
	1000

It will be thus seen, that the blood is very readily changed in its constitution by blood-letting, diet and exercise. It sustains direct relations also to the air we breathe, to the water we drink, to the food we eat, and to the excretions of the body, by which it is purified; and that a fluid which is presented to us in such a compound and complicated form, and sustaining so many relations to the various modifying influences which surround it, should not become a frequent seat of disease, would be indeed an anomaly in nature.

Among the various hypotheses to account for fever, a zymosis, or fermentation of the blood has prevailed, under one form or other, from remote antiquity. But it has been so inseparably connected with the old humoral pathology, that it has received little consideration. Recently this hypothesis (for I shall regard it as such at present) has been rendered, to say the least of it, very plausible by the researches of the distinguished Liebig.

In his Animal Chemistry he calls attention to the fact that no other component part of the organism can be compared to the blood in respect of the feeble resistance it offers to exterior influences, and the reason assigned is, that "it is not an organ which is formed, but an organ in a state of formation." The following quotation embodies in a few words the main leading thought of the author on this subject:

"The chemical force and the vital principle hold each other in such perfect equilibrium, that every disturbance, however trifling, or from whatever cause it may proceed, effects a change in the blood."

This then is an important starting point in our reasoning process; for if it be really possessed of a low vitality, we may logically arrive at the conclusion, by an *a priori* argument, if we knew nothing of the facts in confirmation of it, that all fevers produced by endemic, epidemic or infectious causes, have their origin in a primary diseased condition of the blood.

A zymotic change of the blood is due, according to Liebig, to a decomposing organic molecule in the interior of the human body. This molecule, by a law of catalysis, induction, or contact, has the power of imparting its own motion to another molecule, with which it may be in contact. Hence chemists have defined it to be "decomposition by contact," or the "action of presence." We have illustrations of this law in the power which small quantities of substances, in a state of change, possess of causing unlimited quantities to pass into the same state; and it is an interesting fact, worthy of note in this connection, that all substances which readily suffer this transformation are, without exception, bodies which contain nitrogen. A large portion of the blood

being composed of this element, we might readily conclude that it is the vital principle alone that keeps it from spontaneously passing into this condition of transformation. If the catalytic force be greater than the resistance offered by the vital principle, the blood must pass into a condition of decomposition.

It may be asked, however, with reference to this law of zymosis. or induction, is there any evidence to show that the introduction of putrid matter into the animal system does give rise to effects which are at all comparable with those of fever ? If not, the law which has been announced is but a speculation, and at best, an hypothesis. But let us see. " It is a fact," says Liebig, " that subjects in anatomical theatres frequently pass into a state of decomposition, which is communicated to the blood of the living body." And the fact observed by Magendie, that putrifying blood, brain, eggs, etc., laid on recent wounds, cause vomiting, lassitude and death, after a longer or shorter interval, has never as yet been contradicted. Numerous experiments have demonstrated that putrid matter injected into the blood of healthy animals will give rise to a set of symptoms which are very analogous to typhus. "If a small portion of putrid matter," says Dr. Armstrong, " be accidentally introduced into the blood during dissection, or if the experiment be made upon the lower animals, it produces fever having exactly the character of typhus under its continued form, and no individual could confidently pronounce that it differed from it." Bernard has also shown that by injecting yeast or sugar into the circulation, many of the ordinary kinds of fermentation are excited, giving rise to a disease very analogous to typhoid fever, accompanied by prostration of strength, bloody fluxes, ecchymosis, and a black and uncoagulated condition of the blood. " Lastly, it is," says Liebig, quoting from Henle, " a universal observation that the origin of epidemic diseases is often to be traced to the putrefaction of large quantities of animal and vegetable matters; that miasmatic diseases are endemic in places where the decomposition of organic matter is constantly taking place, as in marshy and moist localities ; that they are developed epidemically under the same circumstances after inundations ; also in places where a large number of people are crowded together, with insufficient ventilation, as in ships, prisons, and besieged places."

It is also worthy of note that these factitious fevers, produced by the introduction of deleterious substances directly into the blood are analogous, both in their symptoms and pathological lesions, to those produced by the sting or bite of certain animals ; they present, also, the same general class of symptoms that are present in small-pox, malignant scarlatina and other eruptive diseases.

In Mr. Walker's work on Grave Yards, he also presents an array of facts, which prove, beyond all controversy, that putrid animal exhalations have given rise to diseases that have raged like a pestilence or epidemic. He cites an instructive instance, which occurred in 1733, at the parish of St. Saturnine, in Burgundy. A sexton, while letting down a corpse into the vault, accidentally broke a coffin which contained the body of a fat man that had been buried 23 days. A discharge of sanies followed, which greatly annoyed the assistants ; and, " of one hundred and twenty young persons of both sexes who assembled to receive their first communion, all but six fell dangerously ill, together with the cure, the grave-digger and sixty other persons." The disease is described as a putrid, verminous fever, accompanied with hemorrhage, eruption and inflammation.

Facts in support of these views might be accumulated at great length. Dr. Francis Home communicated measles by means of a drop of blood from a patient affected with the disease. And the experiments of M. Gendrin, as given in Williams' Principles of Medicine, is a striking one in point. " A man who

had been skinning a diseased animal was seized with a putrid fever, attended with an eruption of sloughing pustules. Some blood taken from this man was injected within the cellular texture of the groin of a cat; the animal was soon after affected with vomiting of bile, dyspnœa, frequent, small and irregular pulse, dry, brown tongue, slight convulsions, and died seven hours after the injection." The same pathologist induced in animals various and severe symptoms, followed by death, by injecting into their veins the blood of persons laboring under small-pox. MM. Dupuy and Lauret also communicated the malignant pustular disease known as "Charbon," by injecting into the veins of the healthy horse a minute quantity of blood of the diseased animal. Andral relates an extraordinary case in which a malignant fever, followed with pustular eruption and death, was occasioned by the mere contact of the lips with the diseased blood of an animal.

May we not then infer from these facts, that the blood is the hot-bed in which many malignant diseases are propagated, whether by ova, parasites, cell germs, or zymotic action.

But our proof does not rest here. Clinical observation has long since established the alteration of the blood in diseases which are termed putrid; the blood appears to be in a partial state of dissolution; its vitality is destroyed, and its fibrine either not elaborated, or dissolved in the process of putrefaction. As a result of this decomposition, an increased quantity of hydro-sulphate of ammonia has been found in the blood of patients suffering from typhus and other malignant diseases; and hence the alkaline reaction of the urine that is so often observed to be present in these fevers.

These observations have been made the basis for the support of a great group of maladies which go by the name of zymotic diseases, and include, according to the statistical nosology of Mr. Farr, small-pox, chicken-pox, all eruptive diseases, influenza, scurvey, purpura, ague, remittent fever, yellow fever, typhus, puerperal, plague, hospital gangrene, etc. And in proof of their zymotic origin the fact has been offered—1st, that the vitality of the blood is low, and that it therefore readily suffers transformation; 2d, that we can produce in animals and man factitious diseases by inoculations or injections of putrid or contagious matter having all the characteristics of the essential fevers; and 3d, that clinical observation establishes the fact that the blood is changed. And that the febrile phenomena present in these fevers indicate a condition of the system independent of inflammatory action, I infer from the fact—1st, that in the absence of complication during the progress of the disease, there is no evidence of inflammation revealed by post mortem inspection; and 2d, that the symptoms co-exist with a diminution of the fibrin of the blood, and diminished tolerance of the loss of blood. In some of the most malignant forms of fever—those in w hich the fibrin of the blood is at its minimum—there is often not much heat of body and but little increase of pulse. The patient often dies in the cold stage of such fevers, without in fact having any fever! Evidently, therefore, the term fever may be used in two very different senses—in one, signifying a collection of symptoms depending on local inflammation; and in the other, a condition of the system entirely independent of such inflammation. In one the term indicates the name of a disease, and in the other the name of a symptom. Hence the distinction between essential and symptomatic fever; and hence the inference, also, that the essential fevers have their origin in certain qualitative changes of the blood, caused by the introductian of foreign matters.

Further proof that the general class of diseases which have been termed zymotic have their origin in the blood is drawn from the symptoms usually present. These will be found accurately detailed by all standard writers on General Pathology, under the head of "Necraemia, or death beginning with

the blood," such as petechiæ and vibices on the external surface, the occurrence of hemorrhage in internal parts, the general fluidity of the blood, its frequently dark and otherwise altered aspect, its proneness to pass into decomposition, the general prostration of all the vital powers, the dark tongue, sordes on the teeth, suspended secretion, and the general arrest of molecular nutrition. Indeed the very universality of the diseased action points to a cause more general than can be found in any individual function.

I have thus far spoken of admixture of foreign elements in the blood from without. There are causes, however, which operate upon it intrinsically as well as extrinsically. Thus, defective excretion is followed by a direct backward action on the blood, resulting in changes of its chemical or vital properties. The excretory organs are the natural emunctories, through which effete matters, generated within the organism, are expelled from the blood. The product of the various excretions may be regarded, therefore, as the correct expression of the numerous changes that are taking place both in the healthy and diseased animal fabric. In febrile diseases these organs are generally suspended in the exercise of their healthy function, an increase of perspiration, or in the flow of urine, or a spontaneous diarrhœa, being generally accompanied with a subsidence of the febrile phenomena.

Relatively considered, the kidneys may be regarded as the most important emunctories through which morbid matter is expelled from the blood. The experiments of Orfila on this subject are highly satisfactory. He found that the pernicious effects of small and repeated doses of arsenic could be readily averted in animals, by giving them at the same time a diuretic medicine; and the converse fact has been frequently observed, namely, that persons who suffer from disease of the kidney, by which its function is impaired, very readily contract infectious disease, and are apt to suffer from their effects. It has also been observed that opium, arsenic, mercury, etc., operate with dangerous energy on such patients.

(*Peninsular Journal of Medicine, Jan.* 1854.)

II.—*The Uses of Galvanism in Obstetric Practice.*

BY ROBERT BARNES, M. D.

(From the London Lancet, Jan. 1854.)

I pass over all those means of rousing the uterus from a state of inertia which occur to the mind of the practitioner conversant with the physiology of parturition. An encouraging word, the restoration of hope and confidence, a timely stimulating draught, the pressure of the hand upon the uterus externally, the use of cold variously applied, all these and a thousand other means may, in numberless cases, supersede the resort to ergot. It is my object to direct attention to an agent which is, I believe, an absolute substitute for ergot, and one which may be resorted to with confidence in every form of labor marked by defective uterine action, and in every case where it is desired to excite contraction of the uterus. The agent which answers to these conditions is galvanism.

From time to time many valuable but isolated observations upon the use of galvanism in different cases of obstetric practice have been published. But no systematic attempt has been made to prove that in galvanism we possess an agent capable of universal application wherever we require a safe and effectual stimulus to the muscular structure of the uterus. I shall consider the

uses ot galvanism in the successive epochs of gestation and parturition, beginning with its use in the induction of premature labor.

The Use of Galvanism in the Induction of Premature Labor.

In 1803 Herder* suggested the use of electro-galvanism for the induction of premature labor. In August, 1844, Doctors Horninger and Jacobi† succeeded in bringing on labor by the electro-galvanic apparatus after other means had failed. The application was immediately followed by uterine action, and the child was born in an hour from the commencement of the operation. A successful case under the hands of Mr. Demsey is also referred to by Dr. Golding Bird. My researches into what has been written on the subject have not been sufficiently minute to enable me to say that no other similar cases have been recorded. In January, 1851, I myself had an opportunity of testing the efficacy of this agent.

Case 3.—I have already referred to this case for the purpose of illustrating the effect of contraction of the uterus upon the fœtal circulation. The result, although perfectly satisfactory, was by no means so speedily accomplished as in the case of Horninger and Jacobi. I had previously endeavored to bring on labor by puncturing the membranes, and inserting a sponge-plug in the cervix uteri. This proceeding was followed by no symptom of labor. On the 23d of Jauuary I applied the galvanic battery for half an hour, placing one pole on either side of the uterus. Immediately after commencing the shocks the bladder was irresistibly emptied, to the evident annoyance of the patient. The womb was felt to become hard, and the patient herself was sensible of contractions and increased movements of the fœtus. The contractions did not continue on the cessation of the galvanism, and I therefore repeated the applications on the 24th and 26th, for about an hour each time. On the 26th a "show" took place. On the evening of the 27th slight pains were felt; the cord was presenting, a small loop coming through the os uteri, which was now dilated to the size of a shilling, but feeling rigid. She had had rather copious flooding in the day time, but it had stopped. The head was felt lying on the pubes in front of the os uteri, the cord coming down in the free space behind it. On the morning of the 28th, the galvanism having been applied at intervals all night, the pains had increased. I have already mentioned how the galvanism increased or originated contraction. At nine A. M. the child was born. It was apparently not more than six months old. The patient had certainly reckoned falsely. The child's heart was pulsating; the chest made three or four convulsive heaves, at which the mouth opened, but no air seemed to enter; the lungs refused to expand; the walls of the chest were drawn in towards the spine. I endeavored to excite respiration by the galvanic apparatus, but although I could at will cause a respiratory effort, the child was evidently too immature to live. The womb contracted favorably, and the placenta being withdrawn was found healthy. The patient recovered without a bad symptom.

The excellent effect of galvanism in this case led me to recommend the use of the same agent to my friend, Mr. Mansford, who has favored me with the following account:

Case 4.—The lady whose case led me to attempt the induction of premature labor, was in the forty-first year of her age and the thirtieth week of her fifth pregnancy. On the 8th of November, 1852, having ruptured the membranes, I introduced one wire of the apparatns within the os uteri, and placed

* Diagnostische Praktische Beitrage zur Erweiterung der Geburtshulfe, Leipsig, 1803.

† Busch's Neue Zeitschrift fur Geburtskunde, vol. xvi.

the other in contact with the spine. From the one introduced in the uterus I had removed the brass handle, and twisted the wire upon itself so as to form a loop sufficiently curved to insure its remaining steadily in its proper place. I also carefully enveloped a considerable portion of this wire with lint, as well to protect the vagina from the twisted portion and extremity, as to prevent the galvanic current from being diverted from the uterus. I then increased the power until it produced "the most severe cutting pains in the loins," "great bearing down," and "a dreadful commotion in the womb." These were my patient's own expressions. This operation was repeated on the 9th and 10th, each morning for half an hour; the effect however, had not been as yet altogether satisfactory, as I had not been able to maintain a continuous action; but on the fourth morning, viz., the 11th, I remedied this defect, and kept up a continuous current for three quarters of an hour, when my patient begged me to desist, which I did, and determined to wait a few days to see if this might accomplish the desired effect. Happily on the 14th, without any further interference, labor commenced and terminated within four hours in the birth of a living child, and not a single untoward symptom occurred spontaneously. It was altogether a most satisfactory case.

The foregoing results are directly at variance with the opinion of Dr. Golding Bird, who says:

"The result I have arrived at is, that this agent, like the ergot of rye, and perhaps other *erbolic* remedies, generally fails to develop uterine action *de novo*. Hence, though I believe it will generally fail to produce premature labor, it will as generally succeed in stimulating the uterus to vigorous contraction after labor has actually commenced."

In weighing this negative opinion, it should however be observed, that the latitude of qualification implied in the word "generally," deprives it of all precision of meaning.

It would lead me beyond my present purpose to discuss the relative advantages of galvanism and the douche, recommended by Dr. Kiwisch, and other methods. I will simply remark, that whatever method be determined upon for the purpose of bringing on labor, the stimulating property of galvanism on the uterus will be a most useful adjuvant.

I will briefly refer to the great superiority of this method over the use of ergot of rye. An unexpected obstacle to the expulsion of the fœtus may arise after the administration of ergot; there is consequently danger of rupture of uterus. How, for example, can we foretell that the child will not be driven into the pelvis in a transverse position? Secondly, there is the great improbability that the child will be born within any reasonable period after the administration of ergot; many doses are required; there is the risk of ergotism to the mother; and the peril to the child rises in proportion to the amount of ergot given; moreover, it is extremely uncertain whether the ergot will act at all.

The use of Galvanism in Inertia during the first and second stages of Labor.

I will now illustrate the effect of Galvanism in lingering labor from uterine inertia. An interesting case of this nature is recorded by Mr. Cleveland,* which was brought to a close within fifteen minutes after the use of the electro-galvanic apparatus had commenced. Mr. Houghton† also relates four cases of arrested labor from atony of the uterus, brought to a successful termination by the agency of galvanism. In three of these ergot had previously failed.

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* Medical Gazette, June, 1845.

† Dublin Quarterly Journal, February, 1852.

In a similar case I had myself experienced the like good effect, but I prefer citing the following account supplied to me by my friend, Dr. McKenzie:

Case 5 —"I was sent for one morning to a young woman who had been admitted in labor at the Paddington Infirmary, and on examination I found that the head presented. Although she had been several hours in labor the os uteri was but little dilated. I saw her in the course of the same afternoon, but still found very little dilatation. At 10 P. M. but little progress had been made. I now determined to try the effects of galvanism, and applied one pole of a single current machine to the spine, and the other, by means of Radford's director, to the neck of the uterus. The current was from time to time intermitted, and uterine action of a vigorous character was excited. In about an hour a fine living child was born. So vigorous were the expulsive efforts during the passage of the head through the os externum, that I was obliged to take particular pains to prevent rupture of the perinæum. The impression left on my mind by this case was, that galvanism should not be employed except very cautiously in primaparæ, or in any other instance in which the perinæum is rigid or imperfectly developed."

Galvanism may also be usefully employed in many cases of hemorrhages before the birth of the child.

A judicious application of this agent may, in many cases of arrest of the head from inertia, obviate the necessity of resorting to the use of the forceps

The use of Galvanism in the third stage of Labor and in Hemorrhage.

We possess a greater amount of evidence of the value of galvanism in the third stage of labor. Dr. Radford has contributed many valuable observations, exemplifying the power of galvanism in exciting contraction of the uterus in cases of post-partum hemorrhage. These are too well known to require to be cited. Mr. Houghton has added other cases which occurred under his own observation. The only instance I will adduce here is one which occurred recently to Dr. McKenzie.

Case 6.—The patient had been upwards of forty-eight hours in labor, under the care of Dr. Keogh, who had called in Mr. Clark, by whom I was sent for. When I saw the patient uterine action had entirely ceased, and I found, on examination, that the head was impacted in the pelvis, the face presenting with the chin to the left cotyloid cavity. As the patient was exhausted, an opiate had been given, and as she was disposed to sleep, we agreed to meet again in some hours, and if uterine action did not return, to deliver by the forceps. At the appointed time no return of uterine action had taken place. I applied the forceps; the operation was accomplished with extreme difficulty, and the woman was delivered of a fine, large, living child. I left the patient shortly afterwards, but the next day on meeting Dr. Keogh and Mr. Clark, I learned that great apprehension had been felt throughout the night as to the occurrence of hemorrhage, inasmuch as the uterus had remained flaccid and uncontracted, and at the time of my visit it had reached above the umbilicus and was very soft and flabby. I proposed galvanism and applied one pole to the spine and the other to the neck of the uterus, occasionally intermitting the current. This was done for half an hour, and evident uterine action was excited, the uterus becoming harder and smaller, and on removing the poles two large coagula were expelled. The next day the uterus was more contracted and smaller, and no hemorrhage had occurred. Galvanism was again used for half an hour. The uterus certainly contracted under its influence. The following day no hemorrhage had occurred, and the condition of the uterus was such as not to require any further recourse to the agent. The woman from this time recovered in a most favorable manner." Dr. McKenzie adds the following remarks, in which I entirely concur: "It appears to me that the results of galvanism in this case were highly satisfactory, because coagula retained in

the uterus, from atony of the organ, are not only calculated to occasion hemorrhage, but by undergoing a species of putrefactive decay, to give rise to fever and all the consequences of vitiation of the blood. Under such circumstances, I have known the hand forcibly introduced into the uterus many days after labor for the removal of such coagula, with very disastrous results —results which this case shows may be obviated by having recourse to galvanism."*

Othes uses of Galvaniim in Obstetric Practice.

There is another case of not unfrequent occurrence in obstetric practice, in which galvanism may be of eminent service—temporary paralysis of the bladder following delivery. A case I have already related illustrates the power of galvanism in causing contraction of the bladder. Doctors Goodwin and Radford† describe an interesting case, in which the catheter was employed two or three times a day, and could not be dispensed with. On Dr. Goodwin's suggestion galvanism was tried, and the first application proved successful.

I would especially recommend the use of galvanism in those cases in which the action of the uterus has been unfortunately paralyzed under the influence of chloroform. In such cases I believe no other stimulus that can be applied will answer with equal certainty or efficacy.

I am also sanguine as to the value of galvanism in exciting respiration in asphyxiated children.

There is another class of cases in which galvanism promises to be of the greatest service. A most interesting case has been recorded, in which Doctor Tyler Smith‡ was enabled to produce expansion of the neck of the uterus, and to bring an intra-uterine polypus into view, so as to admit of the application of a ligature, by the application of galvanism after ergot had failed. I have also employed it with success for the purpose of causing the expulsion of hyatids. This case occurred in connexion with my colleague, Mr. Forbes, and I will relate so much of the account as bears upon the question before us.

Case 7.—Ann W., aged forty-two, had had eight children and three abortions. She applied to Mr. Forbes on the 17th of June last, having anasarca of the legs. Two months before she suffered a burning pain in the region of the womb. She had menstruated up to Christmas last. Since that date there had been a little hemorrhagic discharge at intervals. For the last month there had been a continual discharge of colored fluid. Her health is much impaired and her strength lowered. On the 18th, while in bed, she felt a vaginal discharge, and on getting up passed a large quantity of blood. The pulse was weak, thready, 108; face blanched; headache intense. No pain preceded the hemorrhage. There was a tumor in the seat of the pregnant womb, extending more to the right side, and reaching to the umbilicus; it was firm and elastic, tender on pressure, which did not bring on labor pains. The os uteri was the size of a shilling and rigid. No placental murmur or sounds of fœtal heart heard. The breasts were quite flaccid. The os slightly expanded towards the afternoon. A dead fœtus, or some diseased condition of the ovum was suggested. In consultation, Dr. Barnes suggested galvanism to cause contraction; this had the desired effect and Mr. Forbes was enabled to bring down a bunch of hydatids. The vagina was then plugged and the abdomen bandaged. The disposition to contraction thus given, more hydatids were

* Further observations are required in order to determine the action of galvanism upon the fœtus in utero. Present experience however does not indicate that it exerts any injurious effects.

† Provincial Medical Journal, December, 1844.

‡ The Lancet, 1852.

afterwards passed. Tincture of ergot of rye was then given in small doses. Early on the morning of the 19th the patient passed a large mass of hydatids, which were expelled suddenly with a pain like that of labor. She was quite exhausted with loss of blood and previous disease; symptoms of inflammation appeared, and she sank the same night. The post mortem examination revealed a large fibrous tumor in the walls of the uterus, and an advanced stage of granular degeneration of the kidney.

In such a condition of the uterus and the patient, none of the ordinary means of exciting contraction could have been employed with equal safety and advantage. The necessity of inducing contraction to expel the contents of the womb and arrest the hemorrhage was obvious, and the utility of galvanism in accomplishing this was manifest. I am disposed to regret that the galvanism was not more freely used. The expulsion of the hydatid placenta might have been hastened.

It is beyond the strict scope of this paper, but I may be permitted to refer to the advantages attending the use of galvanism in amenorrhœa, hysteria and other diseases of females, advantages which have been clearly established by Dr. Golding Bird, Dr. Gull, and others. The stimulating influence of galvanism is well worthy of trial for the purpose of exciting the lacteal secretion.

Mode of Apylying Galvanism.

I have now gone through a series of illustrations, affording evidence of the use and value of galvanism in most of the forms of labor characterized by defective uterine action, and in other cases where the indication is to excite the contractile property of the uterus. I will conclude this paper with a brief description of the mode in which this powerful agent should be applied, and a summary of the advantages it especially possesses in obstetrical practice over ergot of rye. The ordinary electro-magnetic apparatus in use for medical purposes is, I believe, the best form that can be employed. The principle of this apparatus consists in the induction of magnetic currents by a current of electricity, and the production of a rapid succession of feeble shocks by continual interruptions to the current. I have observed that the uterine contractions are always provoked at the break and renewal of the circuit. Repeated shocks act as a far more effectual and certain stimulus to uterine contractility than a continued current. It is probably through inattention to this fact that some practitioners have failed in effecting contraction of the uterus by means of galvanism. As to the mode of applying the poles, I do not think it necessary to apply one over the spine and the other to the neck of the uterus, as is usually done. I have found the application of the discs, covered with thin flannel moistened in water, one on either side of the abdomen over the uterus, much more convenient and quite as effectual. The practice of applying one pole over the spine and the other to the neck of the uterus, further seems to me to be based upon an erroneous view of the mode in which galvanism acts upon muscular fibre. When the poles are thus applied, one to the spine and the other to the cervix uteri, it is doubtful whether the ensuing contraction of the uterus is due to primary excitation of the spinal marrow. It is proved by the experiments of Matteucci, and it is confirmed by general observation, that galvanism acts directly upon the muscular fibre, stimulating it to contraction. It is clear that this direct action can be as effectually obtained by passing the shocks through the uterus, by placing the poles on either side of the abdomen. I would not be understood to affirm that this immediate action of galvanism upon the muscular fibre is its sole mode of action, but that it is the primary and essential one; this primary peristaltic action commenced, the secondary and

tertiary diastaltic emotional and voluntary reaction upon the uterus follow.* The duration of the application must depend upon the requirements of the case. It is often found that nothing but a primary excitation is wanted, and that this being supplied, the uterus will go on contracting spontaneously. In those cases where it is required to originate uterine contraction, as in the induction of premature labor, several applications of an hour's duration will be necessary. The uterus cannot be roused to perfect action before the appointed time without repeated stimulation.

The special advantage of Galvanism as an Agent for producing Uterine Contraction.

Among the advantages of galvanism, more especially worthy of attention are—

1st. The simplicity of the operation.

2d. The extensive range of cases in which it may be successfully employed, rendering the electro-magnetic apparatus a desirable addition to the armamentarium of the obstetric practitioner.

3d. The perfectly manageable character of the agent. Its action may be broken off and renewed at pleasure. The moment we think the uterus is acting too powerfully under its use, we may instantly withdraw the exciting agency, and leave the uterus to the ordinary physiological stimuli, which seldom impel the organ to undue activity. It moreover admits of easy regulation; both the strength and duration of this agent are completely under our command. We have it in our power to imitate in a remarkable manner the natural pains, both as to intensity and intermission. Ergot has neither measure nor certainty.

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* It will be seen from what has been said above, that I cannot concur implicitly in the views or conclusions of Dr. Golding Bird, expressed in the following quotation: "In the magneto-electric coil in which currents are excited by repeatedly breaking contact by a vibrating bar, we have two currents moving in opposite directions, to each of which the patient is submitted. Now these currents are of unequal strength, and if the most energetic—that on breaking contact—be passed in the direction of the vis nervosa, it will produce painful contractions, which, the moment it passes in the opposite direction, will become relaxed; for a direct current tends to produce contraction; an inverse current paralysis. Hence, I should urge the accoucheur not to employ the apparatus in which both these currents are produced, but simply the single current machine. In using this I would suggest the positive conductor to be placed over the lumbo-sacral region, and the other to be carried by gentle friction over the abdominal surface. In this way powerful uterine contractions may be easily produced."

Are we, when we place the positive conductor over the spine, and the negative one over the abdominal surface, warranted in assuming that we are passing a current in the direction of the vis nervosa? Are we so much as warranted in assuming that we are acting upon the contractility of the uterus through the medium of nerves at all? Conclusions such as these should be based upon demonstration. Has the uterus been isolated from all surrounding textures, preserving only its connexions with the spinal and sympathetic nervous systems; and have the effects of passing a "single current" along the nerves in a centripetal and centrifugal direction been observed? No. But has this been observed: that muscular fibre, both voluntary and involuntary, will contract under the galvanic stimulus when its relations with the nervous centres are severed altogether! The only legitimate conclusion from the known facts appears to be, that whether the shocks of the single or double current machines be passed through the uterus in one direction or in the other, whether through the lumbo-sacral region and the abdomen or cervix uteri, or from side to side of the abdominal walls, the uterine muscular fibre is stimulated and will contract. The observations of Matteucci, upon which the reasonings of Dr. Golding Bird appear to be built, apply more especially to the effects of very feeble currents upon the motor and sensitive nerves of the final system and the voluntary muscles.

4th. Its peculiar appropriateness and efficacy in cases of extreme exhaustion of the system, where deglutition is difficult or impossible, or where the stomach rejects every thing; where any other mechanical application to the uterus is dangerous or inconvenient, and especially where the introduction of the hand into the uterus would be likely to be attended by injury or even a fatal result. Indeed it may be truly said, that in cases of extreme exhaustion galvanism is the last resource left to us. The galvanic stimulus can be applied when every thing beside is out of the question. The uterine muscular fibre will respond to the stimulus when the nervous system is utterly prostrate, when the heart has ceased to beat, when the patient is moribund or even dead.

5th. Galvanism is less exhausting to the system than ergot or most other means of exciting contraction. It acts less directly upon the uterine muscular fibre and scarcely taxes at all the general powers of the system.

6th. It does not necessarily preclude or supersede the use of other remedies tending to fulfil the same indication.*

Devonshire Square, Oct. 1853.

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* An apparatus which combines to the greatest extent compactness, portability, and efficiency, is an especial desideratum to the obstetric practitioner. I know of no instrument that possesses these advantages to the same extent as that of Mr. Hearder, of Plymouth. This instrument is well made, it does not occupy one half the space of those usually sold, and while the maximum power is considerably greater, it admits, by the most simple means of accurate and minute graduation. This last quality, independent of its obvious utility in regulating the power according to the effect desired, is one which the scientific practitioner will appreciate, as affording the means of comparing his own observations with those of others. Further improvements in this machine are being effected, which will carry compactness to the furthest extent, and render it in every way admirably suited to the obstetric practitioner.

Part Third.

REVIEWS AND NOTICES OF NEW WORKS.

I.—*A Treatise on the Eclectic Southern Practice of Medicine.* By J. Cam. Massie, M. D. Philad. Thos. Cowperthwaite & Co. 1854.

It was the intention of the late lamented editor of this Journal to have given a review of the work before us, in the January number; had the design been executed, its excellencies would doubtless have been more forcibly presented to the reader, and its defects and shortcomings (if any it have) would have received more thorough exposition than from the pen of the present writer, into whose hands, at the eleventh hour, the volume has fallen; with just time enough, however, and just space enough, we trust, to do it ample justice—resting our claims to unconditional pardon, if in doctrine, arrangement, matter, our views may sometimes run counter to that urbanity which of right belongs to authors who give us books for edification, blended with physic for our bodily infirmities.

It is seldom that we meet with books of Southern birth; upon the practice of medicine two or three physicians of the South have written and published; at the solicitation of personal friends Dr. Massie has published a volume containing 716 pages, the title of which we have already given. It is, in its character and doctrine, essentially *eclectic*—of the school *eclectic*, which the author in his preface avows to be "the true school of medicine," and which, in his opinion, "may now be said to reign" triumphant! In this department Dr. Massie will doubtless be found to be an energetic and pains-taking laborer—an unpretending yet warm advocate; as such we are bound to regard the work under consideration as written more for the advocacy of some new theory in medicine than as relating to "the practice as especially adapted to the peculiar manner in which diseases manifest themselves in Texas," or out of Texas; for we have searched in vain the pages

before us for any *substantive* peculiarities of diseases of the South, aside from the fevers which impressed Dr. Massie with the importance and the necessity of placing before the profession his chosen practice. Had Dr. Massie informed us in setting forth the plan of his practice of medicine, that it was his intention to dive into the department of Surgery and Midwifery in order to secure "the consciousness of having endeavored to be useful" in his undertaking, we should at once have called the title a misnomer, and have been ready to suggest a separate volume upon each subject; but having no intimation of this kind, fancy our surprise upon finding Gonorrhœa and Syphilis following closely upon "Dropsy of the Cellular Membrane," and diseases of the Ear in turn being the antecedents of Amenorrhœa and Dysmenorrhœa! But so it is. Verily "truth is sometimes stranger than fiction."

In the reply of the author to the friends at whose suggestion the present work was undertaken, we find him expressing his opinion upon the manner in which it ought to be written, in these words, "In short, a book which would be a desirable addition to the library of the practitioner, and moreover a valuable aid to every father of a family"—our surprise then is vastly heightened as we proceed with the subject of Gonorrhœa and read the following passages: "The first consideration in regard to this disease is, to institute an inquiry, and if possible, ascertain how it can be prevented. The condom, if perfect, is of course an absolute preventive. To insure this, however, it cannot be used the second time without having been thoroughly washed in a soap of a very costly kind. Persons who use these instruments, which in special cases are recommended by surgeons, both to prevent conception and infection, purchase them by the package and run no risk from the second use."

And is it a book in which such subjects are treated, and upon the pages of which such a sentence as this last is to be found, that the author would think a valuable acquisition and guide to the head of every family—and upon which the sons and daughters might satiate a prurient imagination with baneful delight?

We should imagine that least of all would fathers of families desire to be instructed upon the subject of Gonorrhœa and Syphilis, (aside from the employment of a means which refinement and morals most sternly repudiates) when religion, modesty and honor preclude them from being the participants of such loathsome contagion! We can understand that heads of families should take pleasure in considering medical science chaste, whilst they would be scrupulously careful not to unveil every avenue of its mystic pathways.

Besides, the article referred to is not "an absolute preventive;" it

frequently serves only to drive the enemy to another quarter for attack. Ricord observes, "it fulfils the office of a bad umbrella, which the tempest may rend, and which, protecting badly enough from the storm, does not prevent the feet from getting wet." Or it may be, as a woman with great *esprit* has remarked, "a cuirass against pleasure and a cobweb against danger."

Thus much upon the doctrine, arrangement, matter—in which are embraced the principal novelties of the book. Upon the pathology of disease and the morbid changes which take place in the various fluids and solids of the body, we find a marked deficiency; upon the appropriateness of the treatment of given diseases, we are not competent to form an opinion, the agents employed for the most part being such as Eclectics, Thompsonians, &c., have made essentially their own.

The number of articles which enter into many of the prescriptions which Dr. Massie has given, being as many as eight or ten, if each have efficacy, we should say were vastly too numerous. A little philosophy, little medicine, based upon correct diagnosis, being far more likely to meet the indications of the case. That this is no fanciful allusion, we will give an instance or two, taken from the article on Consumption, which the author informs us he has used with advantage:

"℞ Elecampane.
Asclepias, aa 1 ℔. (Pleurisy Root.)
Macrotrys. (Black Cohosh.)
Comfrey.
Eupatorium Perfol. (Boneset.)
Hoarhound.
Sycamore Bark.
Iceland Moss, aa ½ ℔.
Sanguinaria, ¼ ℔. (Bloodroot.)"

Or—

"℞ Spikenard.
Hoarhound.
Elecampane.
Comfrey, aa ℥ iss
Caulophyllum. (Blue Berry.)
Macrotrys. (Black Cohosh.)
Ictodes, aa ℥ i. (Skunk Cabbage.)
Eupatorium Perfol. (Boneset.)
Lycopus. (L. Virginius.)
Ampelopsis, aa ℥ ij. (Bryonia.)"

Lest we should be chargeable with showing partiality for *round*

numbers in the art of prescribing, we will transcribe one other, which the author has employed with like benefit in the same disease :

" ℞	Hydrocyanic Acid,	gtt. xij.
	Syrup of Tolu,	℥ ss.
Then take	Powdered Gum Arabic	℥ ss.
	Water, fl.	℥ viij.

Dissolve the two last and add the former.

Mix. A table spoonful every three hours."

To this, as a palliative, we cheerfully say very good, with the correction of the acid, Hydroscy. Offic.; since, to have used Scheel's Prussic Acid pure, in the quantity and in the dose prescribed, would, in most cases, speedily have destroyed life.

Much of the treatment of this disease, as suggested by Dr. Massie, we must regard as peculiarly inappropriate ; for instance, it is stated that " Emetics, or remedies given to nauseate, are particularly recommended ;" and so is tincture of assafœtida for " inward pains," and Holland Gin for " dropsy of the head." Yet may we call in question the authority, nor tremble if we doubt the efficacy. The author of the Southern Practice should surely have stated that it was only in the *early* stage of Phthisis that emetics could be (if at any time) admissible, and then only theoretically, it being mere assumption that they can remove tubercle when formed. Those who have watched with the utmost solicitude the *rise and progress* of this insidious, yet too fatal disease—who amid hope and suspense, anxiety and dread, have seen the slow and withering decay of something that on earth was highly prized—may readily be pardoned if they turn in haste from that which must be designated—*puerile.*

In the treatment of the various types of fever, Dr. Massie is generally more judicious, and but for his *erratic* propensities, (which we are most willing to believe have their origin more in a proneness to *dissent* from established principles, than from an antipathy to the principles themselves) would, we think, meet with general approval. As illustrative of the wholesome opinion entertained by Dr. Massie, we cannot omit to quote from the 78th page, on " Fevers in General." "Our own impression is, whether in health or out of health, that in this climate the general system, from the effects of heat, is so much relaxed, that the least change of weather predisposes us to ague ; and it is under this head that, with feelings of some reluctance, I feel it to be an imperative duty to state, that in this climate our systems require a stimulant."

Farther comment upon Dr. Massie's book may be needless ; besides

our allotted space is well nigh consumed. How then shall we conclude It cannet be expected that we should

> "Next the great orators consult, and thence
> Draw all the moving turns of eloquence,"

in lavish praise of our author, who is even less known to us than to fame; but we may of right express an opinion boldly, fearlessly, pointedly (and in these reside, if we mistake not, the essence and embodiment of written thought that were worth the reading) upon that which is for the medical world—medical; or upon that which, purporting to be medical, is non-medical.

The theory and practice of Dr. Massie is evidently Eclectic; as such we are willing to believe that it is the best which has been published, and transfer it to the school which bears that name for its verdict. To the medical profession at large, we think the "Eclectic Southern Practice" will be made available when the pages of numerous standard works upon the practice of medicine shall be effaced by time and their prototypes mouldered away—but not *till* then. G. T. B.

New Orleans, Feb. 2d, 1853.

II.—*On the Etiology, Pathology and Treatment of Fibro-Bronchitis and Rheumatic Pneumunia.* By Thomas H. Buckler, M. D. Philadelphia, Blanchard & Lea. 1853. pp. 150. 8vo.

A new volume treating upon diseases of the chest, and particularly upoe one class which has been so carefully and scientifically investigated by M. Grisolle, when advancing new views should be received with great caution. From the writings of Bouillaud we first learn the coincidence of rheumatism with pleurisy and pneumonia; but in no writings do we find any positive mention of this coincidence or association with bronchitis. In this essay we see it classed with bronchitis, and the object of the work, in the author's words, "is to point out, as clearly as possible, the distinctive characters of fibrous or rheumatic inflammation of the bronchial tubes, and at the same time to show the differential diagnosis between it and ordinary catarrh." Its seat is in the fibrous coat of the bronchial tubes and not in the mucous lining, and for the purpose of marking this distinction the word rheumatic is used. Rheumatic pneumonia with the author "is never idiopathic, but occurs

as a secondary lesion, and is always symptomatic of, and directly dependent on, pre-existing bronchitis."

If rheumatism affects the serous, fibrous and cartilaginous tissues—if we find the fibrous coat of the bladder attacked by this disease, or the intestines the seat of the disease, as it was supposed by Stoll, we may clearly reason that the fibrous coat of the bronchial tubes would not be exempt. Most organs have had their different tissues separated and the diseases of each assigned to them. It is only of late years, however, that the diseases of the eye were known under any other name than the general one of opthalmia; now, each tissue has its diseases separately designated on the nosological table. Not so with the bronchial tubes; this is the first author who has associated inflammation with any other than the mucous membranes.

Consistent with these views, we here find bronchitis divided into three forms—mucous, fibrous or rheumatic, and fibrous or gouty—either of which may be acute, sub-acute and chronic.

Before showing the relation between fibro-bronchitis and rheumatic pneumonia, the "Vascular Mechanism of the Pulmonary Circulation," which is the heading of the second division of the work, is explained. The lungs have two distinct circulations; that which performs the oxidation of the blood, the depurative circulation; and that which supplies the nutrition of the parenchyma and to the bronchial tubes. The first returns the oxygenized blood to the heart by the pulmonary veins, while from the bronchial tubes and the pulmonary parenchyma the blood is returned to the general circulation by the corresponding venous circulation, as in other portions of the system.

In ordinary pneumonia, the capillaries becoming congested, there is an afflux of blood towards that part, which prevents, by the closure of many of the air cells from the increased flow of blood, the proper depuration of this fluid; the retained elements, especially the carbonic acid, adds to the difficulty, the terminal branches of the nutritive arteries pour out their plastic lymph, the serum is pressed out into the adjacent areolar tissue, according as the stasis becomes more complete and seriously established, and thus a portion or the whole of the parenchyma of the lung may become engorged.

Such being the explanations of ordinary pneumonia, the author, by analogous causes, attempts to explain the origin of rheumatic pneumonia, and shows the link of union between it and fibrous or rheumatic bronchitis. "Having explained the mode in which the vascular lesions take place and the order of their occurrence in ordinary pneumonia, it is well to remember that the fibrous tissue of the bronchi is tra-

versed solely by the minute branches of the nutritive arteries, and that where symptomatic pneumonia happens as a consequence of fibrous bronchitis, the order in which the vascular lesions take place is precisely the reverse of their occurrence, as already pointed out, in simple uncomplicated pneumonia. Insoluble uric acid, or its compounds, phosphates, or the extractive matters found in the urine, not being eliminated from the blood, are deposited in the meshes of the fibrous tissue, exciting nervous irritation, followed by vascular lesions, exudations, transfusions, and all the general phenomena of rheumatic inflammation. This process having set in, a symptomatic remora of blood takes place in the depuratory capillaries belonging to the inflamed bronchi, and a congestion, leading to an engorgement, reaching the first and second stage of pneumonia, and rarely going beyond it, often takes place. This form of inflammation is propagated from the fibrous tissue of the bronchi, both by contiguous and continous sympathy."

What is this element called rheumatic, is the issue upon which this essay turns.

It would seem from the next section of this monograph, that certain phrases in the works of writers from the earliest ages to the present time furnish indications that such an element was perceived by them, but none had well observed its influence in the production of pneumonia.

The rheumatic element is that which directly acts in producing the diseases under consideration, and rheumatic inflammation generally in any of the tissues affected by rheumatism. In discussing this subject, four forms of rheumatic inflammation are mentioned, which are induced by as many several causes.

"1st. It is believed that the most common producing cause of rheumatism is the presence in the blood of insoluble lithic acid and lithate of soda, which salts being arrested in the terminal blood-vessels supplying the white tissues, act as irritants, and thus become the primary link in the chain of morbid phenomena constituting, so far as this cause is concerned, one form of rheumatic inflammation.

"2d. It is believed that rheumatic inflammations of another class depend upon the retention in the blood of large quantities of nitrogenized matter, which is eliminated during the healthy performance of the various functions, almost exclusively through the excretory axhalants of the skin.

"3dly. There is a form of rheumatism depending on the abnormal presence of earthy phosphates in the blood; and under these circumstances, an excess of the triple phosphates of lime, soda and magnesia

will often be found in the urine, but not uniformly; the solubility of these salts, and consequent capacity of the kidneys to eliminate them, depending in a great measure on the proportion of phosphoric acid united with the earthy bases.

"There is still a *fourth* variety of rheumatism, depending, it would seem, upon the presence in the blood of those compounds which are found in the urine, and called extractive matters, the chemical composition of which is not yet ascertained."

The report of 11 cases follows this exposé of the author's views—cases which show the element called rheumatic to be predominant, and that the treatment essential to the elimination of this element was that best adapted. In all these cases rheumatism was either one of the antecedents, or else was developed during the course of the disease. In all, the peculiar salts common to the rheumatic diathesis were found in the urine, and in most the auscultatory sounds of the heart furnished additional evidence of the character of the disease. An analysis of 16 other cases is given, drawn from different sources, where the characteristic rheumatic element is shown to exist.

Nothing new is presented in relation to the treatment. The object is to elucidate the etiology and pathology of thoracic disease, the details of which we have attempted to present to our readers, as they are developed by the author.

It is seen by this brief analysis of this monograph, that starting with the proposition that ordinary idiopathic bronchitis never induces pneumonia, a cause is sought for its occasionally consecutive appearance. The rheumatic element, "or the presence of insoluble lithic acid or lythate of soda the most common cause of rheumatic inflammation, or the insoluble extractive matter of the blood" is the excitor. These detained in the capillaries become the irritants which produce the afflux of blood, from which follows that chain of phenomena we have shown as occurring in ordinary pneumonia. As long as these salts remain in the blood and are detained in the capillaries, so long will the disease persist in its course. A solvent for these salts is sought, and the alkaline treatment conjoined to the ordinary treatment, is that found best adapted to the occasion.

Several years ago the same author was an advocate for the exhibition of phosphate of ammonia in rheumatic diathesis, as being the best solvent for those salts, which he esteemed the morbid elements of the disease. This opinion, he says in this essay, has been confirmed by the testimony of Dr. Bird. It is this salt he especially recommends in the treatment of the disease now under consideration.

We would recommend this monograph to our readers. The views,

though novel, are yet interesting, and not without a foundation in pathology and animal chemistry. Many cases of troublesome bronchial disease can doubtless be relieved by the alkaline treatment, and much annoyance be saved to the physician.

"If these observations *should* be verified by others," then will this essay, which the author puts forward as a rejected address, be a valuable contribution to practical medicine, in having more closely established the pathology and treatment of that class of diseases which has already been so well revealed by pure scientific research and experience.

J. H. D.

III.—*History of the Epidemic Yellow Fever at New Orleans in* 1853. By E. D. FENNER, M. D. New York, 1854. Pamphlet. pp. 84.

Inasmuch as it was the intention of the late Dr. Hester to have added this entire work to the January No. of the Journal, which was prevented by the unexpected delay of its appearance, we have thought it advisable to give a brief summary of the chief points set forth by the author. This will furnish a satisfactory idea of its scope to those who may wish to have it sent to them by mail.

The work opens with a notice of the author's efforts for the last ten years to lay before the profession and the public full and faithful accounts of the Yellow Fever that has prevailed in New Orleans and the region around. He deplores the indifference to the subject displayed by the community, and deprecates, as altogether erroneous, the course pursued by the city authorities, newspaper press and influential citizens.

The history of the late Epidemic commences with a general account of the weather from the beginning of the year down to the 1st of October. Then follows an account of the *sanitary condition* of the city during this time, which is shown to have been very bad. Then comes a minute and circumstantial account of the first 26 cases that occurred in the various quarters of the city. The author now reviews the facts observed in *ten different localities*, and says—"if any one can trace any sort of connection or communication between the first cases of the disease as they appeared in the different localities I have pointed out, or any thing like the *gradual spread of an imported contagion or infection from one or more points to the region around*, I can only say it is more than I have been able to do." He then points out particularly the facts bearing on the supposed connection between this Yellow Fever and that

of Rio Janeiro. Then follows extracts from the author's Diary and the newspapers of the day showing the progress of the epidemic, the different types of fever prevailing and the mortality, &c., to its close. He says the Epidemic prevailed from 60 to 70 days, which is the customary period of Yellow Fever epidemics in this city. He notices particularly the fact that "*the Epidemic prevailed in the unpaved and of course, least improved parts of the city all around long before it did in the central and best improved parts.* We then have remarks on "*the general character of the disease*"—"*special observations,*" and the *Treatment.* Then we find *Statistics of Fever,* made up from the living and the dead, showing the various types of fever that prevailed at the time. We then have reports and statistics from the Hospitals, Asylums, Prisons and Benevolent Societies of the city.

The "*Mortality of the Epidemic*" is then given, with estimates of the probable *ratio* of deaths to cases, and cases to population. The author now again reviews all the facts he had presented, and concludes his account of the Epidemic with some remarks on Quarantine and the sanitary measures that promise the most effective protection against future Epidemics.

Addendum.—The author has added to his history some interesting extracts from "*The Second Report of the General Board of Health of England on Quarantine in Yellow Fever,*" which are worthy the profound consideration of legislators and city authorities, whose duty it is to guard and protect the lives and property of the people.

The author of this pamphlet has the gratification to learn just now that the Louisiana Legislature, at the instance of their *Joint Committee on Quarantine,* has ordered a large number of copies for the use of the members. Will not the Legislatures of Alabama and Mississippi do the same, as they are equally interested?

The price of the pamphlet has been reduced to 25 cents and that in muslin to 50 cents, with a view to extending its circulation as far as possible.

The New Orleans Medical and Surgical Journal.

Vol. X.] NEW-ORLEANS, MARCH 1, 1854. [No. 5.

CHANGE OF EDITORS.

With the present number the undersigned closes, for the second time, his editorial connexion with the New Orleans Medical and Surgical Journal. From the fact of his having been one of its original founders, he confesses to have ever felt a sort of *parental* interest in its successful progress. Indeed, in accordance with the wish expressed by many subscribers, he was willing again to become its Proprietor and Editor, if this could have been effected upon reasonable terms; but it has been ordered otherwise, and the undersigned can only express the hope that the Journal may continue to enjoy the favor and patronage that were extended to its late lamented Editor and Proprietor. As evidence of his interest and good-will in its behalf, he will continue to contribute to its pages as circumstances and his leisure will permit, and hopes all the former correspondents will do the same. He is gratified to know that the editorship is entrusted, for the future, to such able hands as Dr. Bennet Dowler, a name too well known to the medical profession of the United States to require either introduction or endorsement on the present occasion. There can be but little doubt that the Journal will be conducted with zeal and ability by Doctor Dowler, and, we would fain hope, with entire satisfaction to its patrons. The appeal to those indebted that was made in the last number, in behalf of Mrs. Hester, we trust will not be forgotten. Let all who are in arrears promptly pay up, and she will thus be enabled to employ men of talent to conduct her Journal.

After this number the Journal will pass into the hands of Dr. Dowler, as will appear from his salutatory address.

The undersigned takes this occasion to renew to the readers of the Journal the assurance of his grateful acknowledgments for the many marks of favor and esteem he has received at their hands.

E. D. FENNER.

TO THE FRIENDS OF THE NEW ORLEANS MEDICAL AND SURGICAL JOURNAL.

The unexpected death of the lamented Dr. Hester—which deprived the medical profession of an honored member—a young wife of an adoring husband—this Journal of a talented Editor, has, in the course of events, placed the undersigned in the perilous, responsible, and unsought position of conductor of a Journal hitherto deemed a reliable representative and able expositor of the

art of Healing, emanating from the great medical, not less than the geographical centre of the South-Western region of the Republic—a region of vast expansions wherein the climate, races, and diseases are scarcely less marked with peculiarities than its topography, geology, fauna and flora.

While the incoming Editor claims for himself no exemption from prejudice, bias, and error, he has the wish, the will, and the expectation to make this Journal, as far as in him lies, an independent, impersonal, disinterested, and catholic one in which the friends of science may meet as on a common platform —instructing and receiving instruction—conferring and receiving honors, by promoting the best interests of humanity, namely, the healing of the sick.

The friends of science who may honor this Journal with their contributions, will, doubtlessly see the propriety of avoiding whatsoever may tend to excite sectarian and partizan influences—personal, sectional, and national prejudices.

The science of Medicine, affiliated as it is with many collateral sciences, affords ample scope to its cultivators without the necessity of descending to attack quacks, cliques, and mercenary practitioners who sacrifice truth, honesty and science, without scruple, for the love of money, or, a love of that species of notoriety which is ephemeral even for its purpose, but lasting for its moral degradation.

New Orleans constitutes the key-stone city medically, not less than geographically, of the vast littoral arch washed by the Gulf of Mexico, which trends from the Rio Grande, deeply indenting the continent, resting on the Atlantic, presenting a varied climate.

Correspondents will favor this Journal by communicating with all possible brevity accurate accounts of the climatic and sanitary condition, the medical topography, the endemic and epidemic diseases and the modes of treatment prevalent in the towns and rural districts within the circle of their observation and practice, with special reference to natives, strangers, and acclimating processes, and medical police and jurisprudence—including the hygiene, the anatomical characteristics, diseases, medical treatment of the Indian and the black races, both pure and mixed—with the medical jurisprudence pertaining to the redhibitory vices and maladies of slaves, both absolute and relative, mental and corporeal, as defined by the Civil Code of Louisiana, by legislative enactments, and by judicial decisions. In this department of Forensic Medicine, it is hoped that some of our learned jurisconsults and physicians will confer a benefit upon millions of their compatriots in the Southern States by practical contributions to the pages of this Journal, tending to illustrate and determine questions of high import to the public and of no little difficulty to the medical witness.

In the mere business affairs of a Medical Journal the public can seldom feel much concern. All avowals of disinterestedness are received with a good de-

gree of incredulity as in the other affairs of life, in which self-interest is pretty sure to predominate. In the present instance, it may be acceptable to the friends of the late Dr. Hester to know that his accomplished widow is the sole proprietress of this Journal. A similar case does not probably exist. She has appointed as her Agent, Mr. W. P. Johnston, an able accountant and a gentleman of integrity, at the Picayune office, 66 Camp street, New Orleans, to whom all letters and remittances must be directed. Instructed to that effect by the succession of the late Dr. Hester, the undersigned on this occasion departs so far from the duties of editor as to call the attention of the delinquent subscribers to this Journal, to the necessity of settling up all arrears without delay. This is at the least one of "woman's rights" about which there can be no dispute, and which every man of honor will respect. The amount of outstanding debts due the Journal is great, increasing, and must be diminished. Let this appeal suffice. Justice is great. Conscience is omnipotent among true Æsculapians. Publishing is costly in New Orleans. Pay! And, having paid, let each obtain a new subscriber, as honest and as punctual as himself,—whereupon, he may soon expect to see the New Orleans Medical and Surgical Journal enlarged, and illustrated with engravings, and able to pay its contributors for their valuable writings. This proposal, so easy of execution, if carried into effect at once among the rich States washed by the Gulf of Mexico, would at the same time enlarge the sphere of the Journal's usefulness and enhance its value in a scientific point of view. A Doctor who half starves his Journal, or his horse, is unwise on the score of economy.

The acceptance of Articles for this Journal must not be construed by the reader as implying the approval or disapproval by the Editor; he will not accept any article which he has sufficient reason for believing to be false in fact, immoral in import, or injurious to the public and the profession. Articles valuable for their facts, though defective in orthography and style, will be considered, revised, and arranged for publication in strict accordance with the writer's views, without the editor's assuming the responsibility of either the facts or opinions which may be advanced on any occasion.

All articles provisionally accepted will be acknowledged, if not published immediately; others, may be considered as not accepted, and may be reclaimed within two months.

Papers of moderate length, having a practical bearing, will be preferred for publication in this Journal.

Editor's office, No. 30 Camp street, or residence, Tchoupitoulas street, between Louisa and Poyfarré streets, New Orleans.

BENNET DOWLER.

March 1, 1854.

YELLOW FEVER.

As this disease has attracted a great deal of attention of late and we had on hand a number of communications on the subject, we thought it best to insert them all in this number, for the convenience of future reference. This is done at the risk of tiring the reader and the sacrifice of many interesting extracts from our exchanges, but we trust the step will be approved. The next number will probably contain a greater variety of matter.

HEALTH OF THE CITY.

We stated in our last issue that our city was remarkably healthy at that time. The winter is now nearly over and we may say we have but seldom witnessed less sickness at this season. True, we have had some scarlatina, measles, chest-complaints and bowel-affections, but hardly so much as usual. There were 21 deaths from cholera during the month of January and only 23 deaths from fevers of all types. About the 4th of February there were seven or eight cases of cholera admitted into the Charity Hospital, mostly blacks, but some of them white. These cases did not originate in the city, but were brought in from different directions—some down the Red and Mississippi rivers, others were sailors from Boston and New York. The disease did not spread at all in the Hospital, though no precautions were taken to prevent it. Diarrhœa was quite common in the city at that time, but of very mild and tractable form. There has been but little sickness up to this date in February, (24th).

The weather since the 1st of January has been remarkably changeable; sometimes very warm, again very cold. There were several white frosts in January, and quite a freeze on the 22d. Icicles were seen hanging from the eves of the houses, but the ground was not frozen. There was rain on five days in January, and on six in February, with a good deal of damp, cloudy weather. On the 16th of February there was a light sleet for an hour or two.

The streets have been very muddy and filthy. There is no Board of Health and no special attention is paid to the sanitary condition of the city. With characteristic indifference this important matter will probably continue to be neglected till the city is visited by another epidemic of some kind, when our Aldermanic sages will again be roused from their lethargy and spend large sums of money in vain efforts to stay its progress. It is to be feared that our ill-fated city is yet to be scourged many times before her Councilmen become convinced of the importance of sanitary measures. They are now disposed to rest their chief hopes of safety upon the efficacy of *Quarantine* to prevent the introduction of disease from abroad, but if they would only examine their home-premises with discerning eyes, they would find sources of danger much

more to be feared. When they have removed these, if the city should still be visited by pestilence, they may then look abroad for its causes.

The subject of Qurantine at this time engages the serious attention of our Legislature. The experiment may be tried, but we confidently predict that it will never protect this city against Yellow Fever, whilst its streets, allies, vacant lots, &c., are suffered to remain in the filthy condition in which they are at present.

Mortality.—We here insert the Report from the Cemeteries for the entire month of January, collated by Dr. J. C. Simonds, Secretary to the late Board of Health. This is all that has been done, the weekly reports in the city papers having long since been discontinued.

Report of Interments in the Cemeteries of New Orleans during the month of January, 1854.

II.—Cholera, 21; diarrhœa, 11; do chronic, 3; dysentery, 9; do. chronic, 3; fever, 2; do intermittent, 1; do remittent, 1; do congestive, 1; do pernicious, 2; do typhoid, 9; do typhus, 6; do ship, 3; catarrh, 2; cholera infantum, 1; croup, 5—80.

III.—Whooping cough, 4; measles, 5; scarlatina, 11; small pox, 2; varioloid, 1.—23.

IV.—Marasmus, 2; teething, 5; debility, 5; decline, 5; anasarca, 1; dropsy, 7; hemorrhage, 2; gangrene 1; asphyxia, 1—29.

V.—Apoplexy, 8; congestion of brain, 5; cerebritis, 4; meningitis, 2; phenitis, 1; encephalitis, 2; cerebral fever, 1; hydrocephalus, 5; effusion of brain, 2; disease of brain, 2; softening of do 1; epilepsy, 2; convulsions 22; cramps, 3 tetanus, 3; lockjaw, 20; paralysis, 2—85.

VI.—Bronchitis, 5, pleurisy, 2; pneumonia, 8; consumption, 65; hydrothorax, 1; asthma, 3; angina, 1; pulmonary disease, 8—96.

VII.—Pericarditis, 1; aneurism of heart, 1; disease of heart, 1; dissolution of blood, 1—4.

VIII.—Gastritis, 2; gastro enteritis, 3; enteritis, 11; colic, bilious, 1; indigestion, 1; hematemisis, 1; congestion of bowels, 1; intestinal consumption, 1; tympanitis, 1; cancer of stomach, 1; peritonitis, 2; ascites, 7; worms, 1; jaundice, 1; liver, inflammation of, 1; do abscess of, 2; do hypertrophy of, 2—39.

IX.—Bright's disease, 1.

XI.—Cancer of breast, 1; metritis, 2; childbed, 4; puerperal fever, 1—8.

XII.—Rheumatism, 1.

XIII.—Scurvy, 1.

XV.—Old age, 9.

XVI.—Still-born, 15; premature 2—17.

XVII.—Fracture of scull, 2; contusion, 1; accident, 2; traumatic tetanus, 2; drowned, 4; burns and scalds, 5; cold, 1—17.

XVIII.—Want of care, 1; wound of head, 1; hydrophobia, 1—2.

XIX.—Delirium tremens, 6; intemperance, 2; suicide, 1—9.

Zymotic diseases, 103; sporadic do 284; external causes, 29—416. Not stated 79. Total, 487; of whom were reported as males, 295; females, 174; not stated, 18—487.

Whites, 384; colored, 88; not stated, 15—487.

Under 5 years of age, 183; from 5 to 15 years, 32; do 15 to 60 years, 202; over 60 years, 23; not stated, 47—487.

Interments on each day in January, 1854.

Jan.	Jan.	Jan.
1.....14	11.....7	21.....20
2.....19	12.....11	22.....14
3.....19	13.....17	23.....17
4.....21	14.....16	24.....18
5.....10	15.....13	25.....24
6.....16	16.....21	26.....18
7..... 8	17.....11	27.....14
8.....11	18.....11	28.....10
9.....15	19.....14	29.....21
10.....11	20.....16	30.....15
		31.....18

Total.....	470
Date not stated.....	17
Total.....	487

RECAPITULATION.

Interments in the cemeteries during the month of January:

Protestant, Girod street, 34; Hebrew, Metairie Ridge, 1; Odd Fellows' Rest, 8; Cypress Grove No. 1, 28; Cypress Grove No. 2, 58; Charity Hospital, 46; St. Patrick's, 84; Catholic No. 1, Second District, 8; Catholic No. 2, Second District, 45; St. Vincent de Paul, Third District, 78; Lafayette, Fourth District, 97. Total, 487.

J. C. Simonds, M. D.,
Secretary of the Board of Health.

LOUISIANA STATE MEDICAL SOCIETY.

We hope the physicians of Louisiana will bear in mind that the State Medical Society, instituted in 1850, will hold its regular annual session in this city on the second Monday of March. A large delegation from the country would be cordially welcomed by the physicians of the city. The reports of the standing committees are expected to possess increased interest this year. The recent visitations of pestilence have in some degree served to impress upon the people the importance of having an able medical faculty to protect them, and there can be no doubt that if the State Society were sustained with proper spirit and energy it would soon be productive of much benefit.

DR. MARSHALL HALL.

In the month of January we had a visit from this distinguished physiologist and London practitioner, which will long be remembered by those who had the pleasure of making his acquaintance. Dr. Hall is a man about 65 years of age, of short statue, but quite robust and active. If we mistake not, he has the

reputation of having an irritable temper, though this character is by no means indicated in his bright, cheerful and apparently amiable countenance. We found him remarkably sociable and unassuming in his manners. Whilst here he delivered two lectures on the nervous system, which were listened to with profound attention by a large audience of physicians and medical students. He also performed some experiments on the alligator, illustrative of his peculiar views of the physiology of the nervous system, which appeared to be conclusive and satisfactory to most of those who witnessed them, but not to *every one.* The animals were rather small, besides being somewhat torpid at this season of the year. We have full reports of Dr. Hall's two Lectures, which we had intended to publish in the present number, but are compelled to reserve them for want of space. They are highly interesting and of great practical value.

On the 18th January the physicians of New Orleans gave Dr. Hall a splendid dinner at the St. Charles Hotel, which was characterized by the utmost harmony, good feeling and vivacity. We had prepared a full report of this festival, which must be excluded.

Dr. Hall left us for Havana on the 23d January, expressing much gratification at his visit and carrying with him the good wishes of a host of new friends in this quarter.

THE MAYOR OF BOSTON, A PHYSICIAN.

We are gratified to learn that Dr. J. V. C. Smith, Editor of the *Boston Medical and Surgical Journal,* has been elected Mayor of this enlightened city. In this the people have displayed their wonted good sense as well as their just appreciation of private worth and professional merit. It would be well for all the cities of the Union if they had such men as Dr. Smith for Mayor. One other important office should always be confided to an intelligent physician, that is the *Coronership.*

We learn through a note from Dr. Smith that he has taken in Dr. Jones as assistant Editor of the Journal. This is the gentleman who conducted it whilst Dr. Smith was absent on his European and Eastern tour two or three years ago, and performed the duty very well indeed. We heartily wish continued success to this useful Journal.

ALABAMA STATE MEDICAL ASSOCIATION.

This Association met in the city of Montgomery on the second Tuesday of January, 1854, and after a pleasant and interesting session of three days, adjourned to meet in the city of Mobile on the second Monday in December next. Officers for the present year:

L. H. ANDERSON, President.
J. C. HARRIS, 1st Vice President.
G. E. REDWOOD, 2d " "
F. A. BATES, 3d " "
G. A. KETCHUM, Cor. Sec'y.
R. MILLER, 1st Rec. Sec'y.
J. C. MARKS, 2d " "
W. P. REESE, Treasurer.
W. TAYLOR, Orator.

The Association is in a prosperous condition, its course is onward. It is to be hoped that soon all the worthy members of the profession throughout the State will be numbered among its "Fellows."

MEETING OF THE AMERICAN MEDICAL ASSOCIATION.

(We insert with pleasure the following notice, and sincerely hope the Medicat Profession in the South and West will be extensively and ably represented on the occasion referred to. *Ed.*)

The seventh annual meeting of the American Medical Association will be held in the city of St. Louis on Tuesday, May 2d, 1854.

The Secretaries of all Societies and all other bodies entitled to representation in the Association, are requested to forward to the undersigned correct lists of their respective delegations as soon as they may be appointed, and it is earnestly desired by the Committee of Arrangements that the appointments be made at as early a period as possible.

The following are extracts from Art. 2d of the Constitution:

"Each local society shall have the privilege of sending to the Association one delegate for every ten of its regular resident members, and one for every additional fraction of more than half of this number. The faculty of every regularly constituted medical college or chartered school of medicine shall have the privilege of sending two delegates. The professional staff of every chartered or municipal hospital containing a hundred inmates or more, shall have the privilege of sending two delegates; and every other permanently or-

ganized medical institution of good standing shall have the privilege of sending one delegate."

"Delegates representing the medical staffs of the United States Army and Navy shall be appointed by the chiefs of the Army and Navy Medical Bureaux, The number of delegates so appointed shall be four from the Army Medical Officers and an equal number from the Navy Medical Officers."

The latter clause, in relation to delegates from the Army and Navy, was adopted as an amendment to article 2d of the constitution, at the last annual meeting of the Association, held in New York in May, 1853.

E. S. LEMOINE,
One of the Secretaries—Saint Louis.

The medical press of the United States is respectfully requested to copy the foregoing.

ERRATA,

In the second Original Article of this number.

The author of this article has pointed out the following errors, which we are glad to be able to correct so soon. In the first place the author's name is mis-spelt. It is Gorrie, instead of Garrie.

At page 585, the 5th and 6th lines should read—"They are considered as retarding the progress of philosophy and keeping it in the rear of all other sciences." At page 590, 20th line from the top, read *combination* for "*combustion.*" At page 592, 13th line, read *have*, instead of "*gave.* Same page, 14th line, add *afforded* after "*has.*" At page 596, 6th line from the bottom, read *vortical* for "*vertical.*" Page 597, 5th line from top, read *may* for "*must.*" Page 600, last line, read *process* for "*proofs.*"

ABSTRACT OF A METEOROLOGICAL JOURNAL FOR 1854.

BY D. T. LILLIE & Co., at the City of New Orleans.

Latitude, 29 deg. 57 min.; Longitude, 90 deg. 07 min. West of Greenwich

WEEKLY— 1853.	THERMOMETER.			BAROMETER.			COURSE OF THE WIND.	FORCE OF THE WIND, Ratio 1 to 10.	Number of Rainy Days	Quantity of RAIN — Inches.
	Max.	Min.	Range.	Max.	Min.	Range.				
January 7	65 .00	39 .00	26 .00	30 .40	30 .00	40. 0	E.	3 .½	0	0.000
" 13	64 .00	32 .00	31 .00	30 .24	29 .10	1. 14	SE.	3.	2	1.180
" 20	79 .00	44 .00	35 .00	30 .28	29 .95	33. 0	E.	2.	3	0.430
" 27	75 .00	30 .00	45 .00	30 .50	29 .90	60. 0	NE.	3.	2	0.895
Febry. 3	62. 00	43 .00	19 .00	30 .26	29 .99	27. 0	SE.	2.	1	0.250
" 10	64 .00	41 .00	23 00	30 .34	29 .90	44. 0	NW.	2 .½	1	0.500
" 17	87 .00	37 .00	56 .00	30 .20	29 .90	30. 0	NE.	4 .¼	4	0.965
21	67 .00	43 .00	24 .00	30 .19	29 .88	29 .0	E.	4 .¾	3	5 098

The Thermometer used for these observations is a self-registering one, placed in a fair exposure. Regular hours of observation: 8 A. M., 2 P. M., and 8 P. M.

REPORT OF THE CHARITY HOSPITAL,

(NEW-ORLEANS,)

For November, 1854.

	SEX.	JANUARY.	
Admissions -	Males	884	
Do. - -	Females	208	
			1092
Discharges -	Males	620	
Do. - -	Females	152	
			772
Deaths - -	Males	74	
Do. - -	Females	21	
			95
Births - -	Males	4	
Do. - -	Females	7	
Still-born -		4	
			15

Number remaining, February 1st, 638.

THE NEW ORLEANS MEDICAL AND SURGICAL JOURNAL.

VOL. X. MAY, 1854. NO. 6.

EDITED BY

BENNET DOWLER, M. D.,

Corresponding Member of the Academy of Natural Sciences of Philadelphia; Fellow and Honorary Vice President of the Medico-Chirurgical College of the same city; Fellow of the Medical Society of Virginia; Corresponding Member of the Society of Statistical Medicine of New York; Fellow and a Founder of the Royal Society of Northern Antiquaries of Copenhagen, &c. &c.

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Editor's Office—Notices.

MAY 1, 1854.

NEW MEDICAL JOURNALS.

New Orleans Medical News and Hospital Gazette—a semi-monthly Journal, edited by Drs. Choppin, Beard, Schlater and Boyer. Four numbers of the Journal have already been published, affording in matter, manner, appearance and promptness, satisfactory guarantees of success.

The Georgia Blister and Critic—a monthly Journal, devoted to Southern Medical Literature and the Exposition of the Diseases and Physical Peculiarities of the Negro Race: By H. A. Ramsey, M. D., Atlanta, Ga.

BOOKS RECEIVED.

A Treatise on Acute and Chronic Diseases of the Neck of the Uterus; illustrated with numerous Plates, colored and plain: By Chas. D. Meigs, M. D., Professor of Midwifery and the Diseases of Women and Children in Jefferson Medical College; Member of the American Medical Association; American Philosophical Society, and one of the Council; Vice President of the Philadelphia College of Physicians; Author of "Obstetrics, the Science and the Art;" "Woman, her Diseases and Remedies;" A Treatise on certain of the Diseases of Young Children, etc., etc. 8to. pp. 116, 22 plates. Philadelphia: Blanchard & Lea. 1854. From Mr. J. C. Morgan, bookseller, Exchange Place.

Pneumonia—its supposed connection, Pathological and Etiological, with Autumnal Fevers; including an inquiry into the existence and morbid agency of Malaria: By R. La Roche, M. D., Member of the American Philosophical Society; of the American Medical Association; Fellow of the College of Physicians of Philadelphia; Corresponding Member of the Imperial Academy of Medicine, and Foreign Associate of the Medical Society of Emulation of Paris; of the Academies of Sciences of Turin, Copenhagen, Stockholm and Nancy; of the Medical Society of

Marseilles, Lyons, etc., etc. 8to. pp. 502. Philadelphia: Blanchard & Lea. 1854. From Mr. J. C. Morgan, bookseller, Exchange Place. [A gentleman has promised a review of this work.]

Elementary Chemistry, Theoretical and Practical: By GEORGE FOWNES, F. R. S., late Professor of Practical Chemistry in University College, London; edited, with additions, by Robert Bridges, M. D., Professor of Chemistry in the Philadelphia College of Pharmacy, etc., etc.; a new American from the last and revised London edition, with numerous illustrations on wood. Philadelphia: Blanchard & Lea. 1853. Royal 12mo. pp. 555. From Mr. J. C. Morgan, bookseller, Exchange Place. [An estimable work.]

Homœopathy—its Tenets and Tendencies, Theoretical, Theological and Therapeutical: By JAMES Y. SIMPSON, M. D., F. R. S. E.; Professor of Midwifery, University of Edinburgh, and Physician Accoucheur to the Queen for Scotland; President of the Medico-Chirurgical Society; lately President of the Royal College of Physicians; formerly President of the Royal Medical Society and of the Physical Society, Edinburgh; Foreign Member of the Imperial Academy of Medicine of France; Member of the Society of Surgery of Paris, and of various Medical Societies in Stockholm, Copenhagen, Berlin, Ghent, etc. First American from the third Edinburgh edition. Philadelphia: Lindsay & Blakiston. 1854. 8to. pp. 304. From Mr. T. L. White, bookseller, 105 Canal street.

The Transactions of the American Medical Association: Vol. VI. Philadelphia: Printed for the Association by T. K. & P. G. Collins. 1853. 8to. pp. 869. Fine paper, plain and colored plates. From Mr. J. C. Morgan, bookseller, Exchange Place.

On Rheumatism, Rheumatic Gout and Sciatica, and their Pathology, Symptoms and Treatment: By HENRY WILLIAM FULLER, M. D., Cantab., Fellow of the Royal College of Physicians, London; Assistant Physician to St. George's Hospital, &c., &c. New York: Samuel S. & William Wood, 261 Pearl street. 1854. From the publishers, through Mr. T. L. White, bookseller, 105 Canal street.

A Treatise on Venereal Diseases: By A. Vidal, (De Cassis) Surgeon of the Venereal Hospital of Paris; Author of the Traité de Pathologie externe et de Médecine Opératiore, etc., etc., with colored plates; translated and edited by Geo. C. Blackman, M. D., Fellow of the Royal Medical and Chirurgical Society of London; formerly one of the Physicians to the Eastern and Northern Dispensaries, New York. New York: Samuel S. & William Wood, 261 Pearl street. 1854. 8to. pp. 499. From the publishers, through Mr. T. L. White, bookseller, 105 Canal street.

Homœopathy fairly represented—a reply to Professor Simpson's "Homœopathy" misrepresented: By William Henderson, M. D., Professor of General Pathology in the University of Edinburgh. First American, from the last Edinburgh edition. Philadelphia: Lindsay & Blakiston. 1854. 8to., pp. 302. From Mr. T. L. White, bookseller, 105 Canal street.

Letters on Syphilis—By Ph. Ricord, Chirurgien de l'Hopital du Midi, &c. &c.; translated by D. D. Slade, member of the Massachusetts Med. Society, &c.; formerly House Surgeon at the Mass. General Hospital, &c. Boston: David Clapp. 1853. pp. 404; paper cover. From Mr. J. B. Steel, bookseller, 60 Camp st.

Lectures on the Diseases of Infancy and Childhood—By Chas. West, M. D., F. R. C. P., &c. Second American, from the second and enlarged London edition. Philadelphia: Blanchard & Lea. 1854. 8to., pp. 487. From Mr. J. B. Steel, bookseller, 60 Camp st.

Types of Mankind, or Ethnological researches, based upon the ancient monuments, paintings, sculptures, and crania of Races, and upon their natural, geographical, philological, and biblical history; illustrated by selections from the inedited papers of Samuel Geo. Morton, M. D. (late President of the Academy of Natural Sciences of Philada.), and by additional contributions from Prof. L. Agassiz, LLD., W. Usher, M. D., and Prof. H. S. Patterson, M. D.: By J. C. Nott, M. D., Mobile, Ala., and Geo. R. Gliddon, formerly U. S. Consul at Cairo. Philadelphia: Lippincott, Grambo & Co. 1854. pp. 738.

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THE NEW ORLEANS
MEDICAL AND SURGICAL JOURNAL,
FOR MAY, 1854.

Part First.

ORIGINAL COMMUNICATIONS.

ART. I.—CASE OF OSSIFICATION OF THE PLACENTA.*

BY M. TROY, M. D.

On the 23d of January, 1854, I was requested by my friend, Dr. Ulmer, to assist him in the delivery of Mrs. A., æt. 19, who had been eighteen hours in labor with her first child.

In descending into the cavity of the pelvis, the occiput of the child had been arrested by the pubis, and though the uterus continued to contract powerfully, no progress had been made in the labor, for the eight or ten hours the head had been so situated.

Owing to the strong and almost continuous action of the womb, it was with some difficulty that a lever (the handle of a common table spoon) could be introduced, and the head dislodged, which being done, however, the lady was soon delivered of a fine daughter.

The placenta did not come away, and slight traction upon the cord produced no effect. After fifteen or twenty minutes, it was evident that hour-glass contraction of the womb was about to take place, and Dr. Ulmer introduced his hand for the purpose of delivering the placenta. He found it firmly adherent to the fundus of the uterus, and it was with the greatest difficulty that he could separate it from the womb—and during the separation, which had to be effected by

* The next article, which is supplemental to this, is submitted under the belief that an isolated case will be less likely than several cases to arrest and fix the practitioner's attention upon ossific adhesion as one cause of placental retention. Among the deplorable effects of retained placentæ may be reckoned hæmorrhages, purulent discharges of the most offensive kind, and a low fever, resembling typhoid and hectic.—[Editor.

almost *pinching the placenta off*, a distinct crepitation could be felt by him and heard by listening attentively.

The pain caused by these efforts was declared by the patient to be more severe than any she had experienced during her unusually severe labor. But this slow and painful operation was happily completed, and the separation of the entire organ effected, when a contraction soon forced both it and the hand from the uterus, which now contracted firmly and regularly.

There was, during all this time, no serious hæmorrhage (though we were terribly apprehensive of it,) and the patient retained her strength very well.

A large opiate was given immediately upon the delivery of the placenta, and she was soon quite comfortable.

Upon examining the after-birth, we discovered the source of the trouble. The placenta was not circular, but somewhat avoid in shape—one diameter being much longer than the other—and *wedge shaped*, that is, one margin of the longer diameter was abrupt and very thick, from which the organ gradually thinned off to a sharp edge at the other margin. In fact, the shape of the organ did not differ materially from that of the great lobe of the liver.

There was nothing peculiar in the attachment of the cord.

But the uterine surface of the placenta was the most remarkable and interesting portion. It was sprinkled more or less thickly throughout, with a calcareous deposit, which gave it a gritty feel when touched, and created a distinct noise when any hard substance was drawn across it, similar to the sound we perceived when its connection with the womb was broken up.

The deposit was white, and had an amorphous appearance, more like the incrustations upon the teeth than anything else. It was in masses or patches from the size of a dime to that of a millet seed, or much less—the whole uterine surface felt as if thickly studded with broken points, invisible in the substance of the placenta. The whole amount of earthy matter in the placenta was estimated, by guess, to be about 1 drachm, but of course it may have been more or less.

The deposition of bony matter in this situation is fortunately a very rare freak of Nature, and retention of the placenta from this cause, is hardly mentioned by obstetricians. But for the sake, both

of the practitioner and patient, the possibility of the occasional occurrence should be borne in mind. Dr. Garrison, of New Jersey, has reported in the American Journal of Medical Sciences, for January, 1852, the case of a lady where this condition was present in several successive pregnancies, and in each confinement it gave rise to the most alarming and dangerous hæmorrhage.

In Mrs. A.'s case there was fortunately nothing of the kind, and she has been doing very well ever since her delivery.

The service which the publication of Dr. Garrison's cases was to me in this instance, makes me deem the case more worthy of publication than I otherwise would. Such a case, if left to the powers of nature, would almost surely end fatally, as the womb is altogether unable to break up the bony attachment of the placenta.

I need not mention that neither Dr. Ulmer nor myself had ever before met with a similar case.

CAHABA, Ala., January 31st, 1854.

ART. II.—OSSIFICATION OF THE PLACENTA, WITH PHYSIOLOGICAL AND PATHOLOGICAL OBSERVATIONS.

[*Supplementary to the preceding article.*]

BY B. DOWLER, M. D.

MAY 19, 1838.—Mrs. * * *, aged 23; Magazine street, New Orleans; *primipara;* breach-case; child still-born. During prolonged, patient, yet unavailing attempts to deliver the placenta by traction with the cord, the latter broke; considerable but not alarming flooding; ergot failed; waited several hours; explored the uterus; found an hour-glass contraction; was unable to pass the stricture, and consequently failed to reach the placenta; after about six hours' delay (the retention having continued nearly all day,) and after having been urged most importunately to deliver the placenta without further delay, I succeeded in passing the constricted isthmus; found the uterus greatly elongated; its fundus had arisen to the inferior portion of the epigastrium; the hour-glass contraction around the wrist was strong, almost paralyzing, and unremitting; the maternal surface of

the placenta was everywhere found adherent to the uterus by bony and parenchymatous structure, which was gradually detached, portion by portion, by the nails and tips of the fingers, the motions of which could be distinctly seen through the clothes covering the lady; the separation was attended with moderate pain and flooding. As soon as the detachment was effected, the mass was brought away in the hand.

The recovery was as speedy as in the most favorable cases of parturition.

The bony matter intimately connected with the placental, consisted chiefly of plates with fine cancelli and reticulations, resisting pressure like the egg-shells of small birds, crepitating audibly on being crushed.

In such cases (which are more frequent than is generally supposed) an unskillful and violent force applied to a strong umbilical cord, might readily invert the uterus. The uterine contraction, though urged by ergot, can scarcely be relied on in such cases, while prolonged floodings, and, finally, slow putrefaction, would in most cases endanger life from anæmia, or fever of a typhoid character.

The placental mass in these cases being interspersed with bony plates, reticulations and cancelli, is comparatively incompressible, or virtually so as it respects the natural forces inherent in the uterus.

When the maternal disc of the placenta is thus adherent by osseous structure, the uterine action being strong, hour-glass contraction, from the configuration of the resisting mass, might be to some extent explicable, as such a body, after the expulsion of the child, would probably cause irritation and a more violent contraction in the fundus and central part of the womb, giving its upper portion the form of an inverted cone or hour-glass. Judging from several cases of hour-glass contraction where neither adhesion nor ossification was the cause of retention, it would appear that this curious phenomenon is not always attended with menacing hæmorrhage. For this there would appear to be in some cases a mechanical explanation, inasmuch as the organ closes so completely centrally, that it is difficult to find the strictured passage, particularly if the cord has been broken off at its placental extremity, so that with a complete stricture or closure, the upper portion of the uterus is a plenum, its walls closely investing and moulding themselves to the placental mass. If hour-glass con-

tractions were intermittent, or alternated with relaxations, this explanation would not be admissible—under the most favorable circumstances it is not reliable in these ossific implantations.

In retention of the placenta the judicious practitioner will be slow in distrusting Nature, and will be in no hurry to make a manual exploration of the uterine cavity. But in almost all cases of unusual delay of placental delivery, there is a moral force or pressure from without which can scarcely be resisted. Every feminine voice is lifted up in favor of complete deliverance, and against considerable delays. So far as the experience of the writer has extended, he must confess that he never has had cause to regret any bad result from conceding to the urgent requests of friends, on these occasions, though at the time, he may have often wished to avoid interference when danger was not imminent.

The following extract is so important, as showing the necessity of active interference to prevent impending death from a cause almost wholly overlooked by systematic writers, that its length will be readily excused, and the more so, as it may be the means of saving life when it is menaced by uterine hæmorrhage occurring in connection with ossified adhesions of the placenta to the uterus.

Dr. C. Garrison, of Swedesboro (N. J.,) in his interesting account of the ossification of the placenta occurring several times in the same individual, observes, that among obstetrical writers he had never found any satisfactory account of this phenomenon. Having been called (Oct. 25th, 1835,) to attend Mrs. J. H. in her second confinement, Dr. G. learned that great difficulty had previously taken place in her first labor, in delivering the placenta;—her attendant having been obliged to send to Philadelphia for Dr. Meigs, to assist him;—the latter with difficulty to himself and pain to her, finally succeeded, but not without considerable hæmorrhage. In her second accouchement, Dr. G. found no difficulty in delivering the child. "But," he adds,

"In about ten minutes there was a tremendous gush of blood, which was quickly followed by another quite as large as the first, the result of which was great and almost fatal prostration; the pulsations at the wrist were scarcely felt, the face became pale, the hands cold, and the respiration sighing; she grew sick and vomited, complained of being very cold, pushed down the bed clothes, gasped for breath, and in a very short time after the last discharge, was in a profound syncope. In the meantime, I had poured a pitcher of iced water, which was previously prepared, over the abdomen, and introduced my hand into the uterus, quickly and without any obstacle, as the hæmorrhage had produced complete relaxation of

both the uterus and external parts; but on reaching the placenta, and attempting to introduce my fingers between it and the uterus, for the purpose of effecting its detachment, I found it impossible to do so, as the union between them seemed perfectly firm and unyielding. After a moment's consideration, I determined to make a separation at all hazards, if it was possible, as death was inevitable if the placenta should be long retained. I began the operation slowly and cautiously, as the separation could only be accomplished by actually tearing or scratching the placenta from the surface of the womb, which could not be done except by the exertion of considerable force. more than it seemed possible that she could survive; and it was accompanied by a kind of crackling noise, which was heard distinctly both by myself and the attendants at the bedside. I soon found that I must hasten the delivery, or the patient would die before it was effected, and, letting go the cord, I placed my hand over the outside of the uterus, and as rapidly as I was able, proceeded to loosen the placenta from its attachments. In this manner I succeeded in separating the whole mass, though in detached portions, which, together with my hand, were soon expelled by a smart contraction of the uterus. But although I had succeeded in removing the placenta, I still felt extremely anxious about the patient; she was yet in a state of complete unconsciousness. I gave her a *large* dose of opium, applied ice to the abdomen, and kept up frictions over the outside of the uterus until it was firmly contracted and all hæmorrhage had ceased. She roused up for a moment, but the syncope immediately returned, and it was a considerable length of time before she showed any persistent signs of returning animation.

"On examining the placenta, I found all that surface of it which came in contact with the uterus, in a state of ossification; it presented, through a common pocket glass, a kind of cellular arrangement, similar in appearance to what might be produced by sticking the whole surface full of the husks of wheat which had been broken in two, leaving the broken edge of the husk turned outward. It was undoubtedly the separation of this bony matter which gave rise to the crackling noise which had been heard during the process of delivery. I could reproduce the same sound in the placenta after it was expelled; it crackled in my fingers like frozen grass or ground. I had never upon any previous occasion been compelled to use an equal degree of force in the delivery of the placenta, and I felt very doubtful as to what might be the issue of it; the utmost strength of my fingers was barely able to break up this bony union.

"Since this time I have attended Mrs. H. in *five confinements*, and in all but one there was a similar condition of the after-birth, accompanied by similar phenomena of excessive flooding and great difficulty of separating the bony surfaces of the uterus and placenta.

"Her last labor occurred about two years since, accompanied with the same circumstances—bony adhesions, and hæmorrhage; for many hours I altogether despaired of her recovery."—[*Am. Jour. Med. Sci.*—*West. Jour. Med.*, 1852.]

These osseous deposits appear to occupy a middle ground between physiological and pathological anatomy proper, with a bias towards the latter. In old age they are so frequent in certain tissues, particularly the cartilaginous, as to appear more like a normal law than a pathological alteration in the economy. Injurious they certainly are in some structures, as in the valves of the heart, by interfering with the functions of this organ, while, for example, in the tracheal rings, and cartilaginous portions of the ribs and in some other structures, no diseased action usually results from osseous degeneration. During an attendance for several years upon the slaves of a banking institution of this city, a very old negro, seldom sick, has been noticed whose arteries are extensively ossified.

In numerous examinations of subjects who had perished from fever and other maladies, often in the bloom of life, I have found in not a few instances ossifications in the cellular, parenchymatous, membraneous and muscular tissues. In one subject the serous membranes of the brain, chest, and abdomen, were universally studded with salient spiculæ or granular ossiform points, both isolated and in patches, presenting a rough, grater-like surface, originating apparently from the sub-serous tissue. Without wishing to multiply examples, I may allude to two cases of recent occurrence illustrative of non-pathological and the pathological conditions incidental to ossific degeneration: C. W.; born in Germany; aged 25; who died of yellow fever, Sept. 16, 1853; was entirely free from preternatural ossifications excepting the stylo-hyoid muscle, the whole of which consisted of bone except a slight cartilaginous portion of its upper end at its connection with the styloid process. In the lungs and bronchial glands, ossific and friable calcarious concretions often occur without producing symptoms of disease. In June, 1852, Mr. J. F., aged about 25—born in Philadelphia—clerk in a cotton press—was seized with an apparently sub-acute pneumonia in the first stage;—without entering into the details of his case, it is sufficient to mention that an abscess having formed in the lung, broke;—a vast amount of the most fœtid, thick, dusky colored pus was discharged, including an irregular mass of bone perforated in the center by an opening corresponding to a bronchial tube and its rings at one of its bifurcations; he also vomited a small mass of a chalky matter without cohesion, shapeless. This patient gradually recovered, and still retains his usual health.

Many of these concretions, particularly those in the lungs and bronchial glands, and about the articulations of gouty patients, are but amorphous, loose combinations of calcarious earth, containing but little animal matter, falling short of completely organized bone in cohesion, gelatine, and other animal matters.

Pathological ossification takes this character from its interference with the due performance of a physiological function, or from its causing inflammation as a foreign body in the soft tissues.

One occasional result of acute hyperæmia or inflammatory engorgement, is osseous transformation, which in its turn may finally become the cause of a new inflammation after the subsidence of the primary.

ART. III.—TWO CASES OF ORGANIC DISEASE OF THE HEART; WITH THE HISTORY, DIAGNOSIS, AND POST MORTEM OF EACH.

BY D. MACGIBBON, M. D.,

Visiting Physician, Charity Hospital, New Orleans.

The following two cases of organic disease of the heart were reported to the Physico-Medical Society at its session 25th February, 1854, and are now offered for insertion in the New Orleans Medical Journal. The history of these, together with the pathological conditions found at the *post mortem*, present some points of especial interest in each, which are deemed worthy of notice. The cases occurred under my care in the Charity Hospital:

The first is that of a Spaniard, a fisherman, forty-eight years of age, of stout muscular make, and bilio-sanguine temperament. He had been, as I understand since, frequently of late years, in the hospital; at one time treated for one disease, at another for something else. He entered towards the close of last year, suffering from an attack of acute bronchitis, and for the first time came under my charge. With appropriate treatment he soon got well of this complaint. On auscultating him a peculiar cardiac murmur was heard, which led me to diagnose organic disease of the heart as present in his case; and he was advised to remain in the house, but feeling himself relieved of the cough, the chief aliment for which he entered, he preferred being discharged, which was done. He returned soon after, on the 14th January, 1854, suffering much from an oppressive sensation, principally experienced about the region of the heart; which was aggravated by some distension of the abdomen, consequent on constipation, which was now, more or less, habitual with him. He would most frequently be found, at the visit hour, sitting up in bed, as he could not lie down well, from a sense of increased distress in the horizontal position, nor could he, as a matter of course, obtain proper sleep either.

The murmur, referred to above, was heard even more distinctly than when he was previously in the hospital. It was loud, and of a *cooing* character. It accompanied the systole of the heart, and could be heard best over mid-sternum; from whence it was transmitted up-

wards and to the right. It could also be heard distinct enough to the left of the hollow at the spine, near the third and fourth vertebræ. The heart's impulse was strong; but its beatings were regular. The pulse at the wrist was not noticed to have anything peculiar about it. No other sound, either normal or abnormal, could be said to be detected on auscultation. From all these circumstances—the character of the murmur—the time *when* and place *where* that was best heard, together with the fact elicited from the patient, that he had, at some former period, an attack of rehumatism, I had little hesitation in concluding that constriction of the aorta existed, with accompanying hypertrophy. The patient was seen at different times by several physicians and a number of the students, who listened to the *cooing*, and knew the diagnosis made. In the way of treatment, Croton oil was found the most useful in relieving the constipation; and tincture of aconite was mainly employed to relieve the præcordial distress and procure rest.

On the evening of the 9th February, the patient suddenly expired, and the next morning the body was examined, when the following pathological changes, as far as the heart is concerned, were found:

The organ, as a whole, was somewhat larger than natural; the walls of the left ventricle were thicker than usual, but there was no constriction of the aorta, as diagnosed. The caliber of that vessel at its commencement was of normal dimensions, but immediately beyond the sigmoid valves, and partially involving these, it bulged out to an abnormal degree on one side, forming, as it were, an incipient aneurism at this point. The cause of this bulging of the vessel was explained by the presence of an old cicatrix, rather larger than a five cent piece—showing that ulceration of the serous and fibrous tissues occurred at some former period at this point, and that the weakened tissues at the time partly gave way to the force of the blood current, as is known to take place in aneurism.

This dilatation in the aorta, in all probability, produced a change or whirling in the current, similar to that which is supposed to take place where the blood passes a constriction in the aorta; and thus, I suppose, gave rise to the peculiar musical murmur which was heard. Walshe, while he states that a murmur, as above heard, habitually signifies simple or rough constriction of the aortic orifice, also says

that in rare cases a cardiac murmur very closely simulating this type may arise from other causes, which he mentions, and the lesion in this case is indicated as one of these, as may be seen by a reference to page 214 of his work on the Heart and Lungs, American edition.

The integrity of two of the aortic valves was in some measure impaired by the ulceration above referred to, which extended to and destroyed a portion of the conjoined column formed by one of the horns of each. These valves, thus diminished, were no longer in a condition to perform their part in shutting up the aortic orifice. The third one, however, was intact; the three, with the exception referred to, were otherwise quite healthy. No doubt some degree of regurgitation took place here during life, though, as already mentioned, no abnormal murmur to indicate this was detected. Why a diastolic murmur was not detected, I cannot say, unless it be, for that best of all reasons, that it did not exist. And this view is strengthened by a reference to the authority already cited, where it will be found stated (page 216) that "mere incapacity of valves, healthy in themselves, but too small to fill the widened mouth of the aorta, has in rare instances produced this murmur;" so that, under the circumstance, its absence would seem rather to conform with the rule than the exception.

Supposing the case to have been as here represented, then the absence of constriction of the aortic orifice, with a systolic murmur heard as above—and, again, the occurrence of regurgitation where no diastolic murmur existed, are all points worthy of being remembered in making a diagnosis in any similar case.

Was the organic state of the heart the cause of the sudden death in this case? I have no doubt of it. Andral, who is a good authority in these matters, says that this is no uncommon occurrence in individuals affected with such complaints, even though but little advanced, and which do not as yet manifest their existence by any well marked symptom. It was, therefore, not unexpected.

The other case is one which had been a much longer time under observation, and is more interesting from the particular form met with being one much more rarely diagnosed, and from the opportunity of verifying the accuracy of the diagnosis by the *post mortem* examination of the case.

An Irish laborer, aged thirty-seven years, entered the hospital on the 29th July, 1853. When he entered he was suffering from remittent fever. There was some œdema of the lower extremities, and some fluid in the abdominal cavity. He was anæmic looking. There was, besides, blowing cardiac murmur heard on auscultation, which led me to conclude he had organic disease of the heart. He mentioned that he had been in good health up till recently, and that he had been working on some one of the neighboring railroad cuttings until attacked with the fever for which he entered.

By proper treatment he soon got clear of this latter, but was advised, in consequence of the other affection, to remain in the hospital. He took the advice. Here rest and a nourishing diet, which were what were mainly depended on, might do something to ward off, for a space, a fatal termination, which otherwise would be assuredly hastened by his being compelled to work in the swamps, as he had been.

The dropsical effusion gradually went on augmenting, notwithstanding that he was getting mild diuretics—as the muriated tincture of iron, with sweet spirits of nitre, and the infusion of juniper berries for drink. The anæmic condition of the system also increased.

To give relief to the tension and sense of suffocation, consequent on the greatly enlarged abdomen, it was deemed necessary, towards the close of the year, to discharge the fluid. Paracentesis* was therefore performed, when two large buckets full of fluid were withdrawn, with, for the time, manifest relief. The liver, which previous to this could not be felt, now showed itself through the flaccid parietes very distinctly. It seemed harder, and its free edge to be more obtuse than normal, and also to be larger.

At this time he lay in a bed close to that occupied by the preceding patient, and, like him, was seen by a number of medical gentlemen and students, who listened to the murmur and noticed the points of contrast. The murmur in both was systolic, but, unlike the former, this one was transmitted downwards; this, too, had nothing of the

*PARACENTESIS ABDOMINIS.—In New Orleans, where surgery is usually practiced with an ability which compares favorably with other cities, I have, nevertheless, the best reason (based on post mortem examinations) for believing that in several cases patients have perished from hæmorrhage following paracentesis, owing to the division of the epigastric artery—an accident which even the unskillful may easily avoid by operating in the course of the linea alba.—[ED.

musical tone of the other, but was an excellent specimen of the *bruit de soufflet* of the French. Before mentioning the diagnosis, let me state more fully the grounds upon which that was made. It is known, as a general rule, that nineteen out of every twenty cases of organic disease of the heart met with are situated in the left side of the heart, and that mitral regurgitation is the most frequent of these. Recollecting that the murmur in this case was systolic and transmitted downwards, the lesion of the mitral valves was the first to suggest itself. But a little more care, with frequent opportunities for auscultating the case, induced me rather to place the lesion in the tricuspid valves, and tricuspid regurgitation was accordingly diagnosed. Two circumstances especially guided me in forming this conclusion: First, the abnormal murmur was heard *best immediately to the left of the ensiform cartilage*—much more so, indeed, than under the left nipple; and in the second place, I satisfied myself that I could hear, a little above the left nipple, both a systolic and a diastolic click, the former of which I would not expect to hear were the mitral valve the seat of lesion. Again, tricuspid regurgitation accounted better for the condition of the liver, and the consequent ascites the patient suffered from—and which was now daily threatening his existence, for the fluid had again accumulated to as distressing a degree as before. In the beginning of the month of February he was again "tapped."—He had been for some time constantly bed-ridden. His general health had become greatly impaired. On both the upper and lower extremities were purpuræ hæmorrhagica patches, exhibiting this anæmic condition most markedly.—Before one bucket full of the fluid was withdrawn he had become so faint that further attempts were discontinued. No pain in the abdomen, following on this, was complained of; but diarrhœa supervened and great prostration; the countenance took on, too, a cadaveric expression, and he died on the night of the 16th of February. The next morning a *post mortem* examination was made, and the following results obtained:

On opening the abdomen and freeing it of the contained fluid, which was somewhat red in color, its whole peritoneal covering was found softened by recent inflammation, which, probably, took its rise in the recent operation. In subjects of this kind a low species of inflammatory action not unfrequently is set up, as is well known, without the

usual symptoms of inflammation having shown themselves, to awaken suspicion as to what was going on. This I have seen in some puerperal cases. The portion covering the intestines was of a darkish red color, and easily scraped off, when the other tissues were shown to be unaffected. A few slight shreds of lymph were also observed about the cavity.

The liver was granular on the surface; and, when cut into, the same diseased condition was found to pervade that organ throughout. The heart was a little larger than natural, and this was found to be occasioned, principally, by increased thickness in the walls of the right ventricle; which, when both ventricles were laid open, were seen to be as thick as those of the left. The right auricle was also found to be greatly distended. The radiating fleshy fasciculi in the *auricula*, like the *columnæ carneæ* in the corresponding ventricle, were hypertrophied: the whole showing most convincingly that tricuspid regurgitation had existed, as diagnosed. The *chordæ tendineæ* of one of the tricuspid valves were shortened, and the apex and border of this one were somewhat puckered, so that it could no longer fall back properly to assist in closing the auriculo-ventricular opening at the heart's systole. The other valves of the heart were all healthy. Nothing beyond of importance was noted.

The distended condition of the right auricle with blood, which must have existed here in life, prevented the blood from the ascending vena cava emptying itself in it as it should, and this continuing would necessarily obstruct the circulation in the liver, and produce in that organ a degree of congestion, ultimately leading to chronic inflammation and thickening of its tissues, with, we may well suppose, in the end, the Cirrhosis met with.

As this change in the liver advanced we can see how the vena portæ would become more and more obstructed; and thus give rise to the effusion of serum in the abdominal cavity, with the other train of symptoms which complicated, and added to the severity of the case: all which had their origin in the organic affection of the heart. This is the usual, and most rational, explanation of these cases.

Although medical art can do but comparatively little for the cure of organic diseases of the heart, yet it is well that they be carefully studied. When assured of their existence, by means of auscultation,

it is surely something to be able to advise the unfortunate patient of the best course to pursue, and otherwise to assist him, to render his life more comfortable; if not also to prolong that. Nor is the knowledge which a close study of these diseases affords devoid of value, should it in some of these, when death is lurking in the cup, only serve to prepare the doomed victim for the worst.

ART. IV.—EXPERIMENTS WITH PHOSPHORUS, AND REMARKS UPON ITS DOSE AND ACTION, WHEN GIVEN IN THE FORM OF ALCOHOLIC TINCTURE OR SOLUTION.

BY WM. M. BOLING, M. D.,

OF MONTGOMERY, ALA.

More especially since I have used in my practice the Veratrum Viride, the discovery of an article equally certain, prompt, powerful and reliable in its action as a cardiac sedative, but without some objections that have seemed to me to pertain to the operation of this remedy, I have thought a great desideratum. By an article presented to the Alabama State Medical Association, at its meeting in Selma, in December, 1852, with impressions that I had previously received in regard to it, my attention was directed to the Gelseminum Sempervirens, or Yellow Jessamine. I entered upon my experiments with this article, somewhat sanguine of success, but disappointment was the result.* After a number of experiments with it, in which certain effects said to result from its use, such as dimness of vision, trembling, &c., were produced in a marked degree, I could not discover that it at all reduced the frequency of the pulse. My experiments with it were upon healthy subjects.

In a well written and exceedingly plausible paper, which was published in the New Orleans Medical and Surgical Journal for January, 1854, on Pneumonia, by Doctor S. Ames, of Montgomery, *Phosphorus* is mentioned as a remedy of great value in this disease, and its curative influence, if I mistake not, ascribed to a cardiac sedative operation. I have been induced to make some experiments also with this article; the results of which I will give.

Two preparations of the article are mentioned by Doctor Ames, to wit: a saturated Tincture in Anhydrous Alcohol, which he supposes to be equal in strength to the Ethereal Tincture, or to contain four grains to the ounce; and a diluted tincture, made by adding nine parts of alcohol to one part of the saturated tincture. As a dose of the saturated tincture, from half a drop to two drops are spoken of, but it is stated that the medicine "cannot be continued in the smallest quantity just mentioned for any great length of time, without inducing considerable disturbance of the stomach, as shown by nausea or vomiting, burning heat, and a feeling of oppression at the epigastrium." It is also remarked that "its effects are cumulative; that is to say, a dose which singly is not large enough to produce any sensible effect, may become very troublesome, or even dangerous, after several repetitions at intervals of three or four hours;" and that "this quality was developed in one instance by repeating it in a dose of two drops of the strong alcoholic solution three times at intervals of twenty-four hours." In regard to its sedative action, Dr. Ames remarks, that "its sedative or contra-stimulant, is its medicinal or therapeutic action," and in a note, makes the following statement: "Two young gentlemen, my personal as well as professional friends, have been recently engaged in some experiments to test the effects of phosphorus on persons in health; they themselves being the subjects of the experiments. These gentlemen (Doctor Pollard and Doctor Oliver) found that a single dose of two drops of the saturated alcoholic solution invariably reduced the force and frequency of the pulse. The changes in frequency ranged in the number of pulsations to the minute, between eight and twelve beats. A change was perceptible in about twenty minutes, which reached its maximum in from an hour to an hour and a half."

Doctor Ames however, for reasons given in his paper, prefers the weaker solution, and his method of administration is, to add sixteen drops of it to four ounces of water, of which he gives a teaspoonful every "third or fourth hour—usually every fourth." As there are thirty-two teaspoonfuls in four fluid ounces, the quantity of the diluted tincture thus given at a dose, would be half a drop. This dose Doctor Ames prefers "after many trials with larger ones."

I have been thus particular in referring to the peculiar effects said by Doctor Ames to result from the use of these two preparations of phos-

phorus, but more especially to the dose and method of administration, that comparison may be conveniently instituted with the dose and manner of administration followed by myself, and any fault or deficiency in the latter, detected and pointed out. I feel inclined to pursue the subject further, if I have failed in any way to conduct my experiments satisfactorily.

The pulse—as most persons know, and as any one may satisfy himself of—will be found to vary in many persons, if not in every one, somewhat in frequency at different times in the twenty-four hours; and often, even when felt at short intervals, will be found so to vary from trivial or inappreciable causes. To prevent any fallacy from this circumstance, it will be noticed, that in my experiments on some days, the frequency of the pulse at different hours is given, though the phosphorus was omitted. A comparison may thus be made between the pulse on the days that the phosphorus was, and on the days that it was not, given.

My first subject was Sam, a healthy mulatto boy, seven years old. On the 25th of February, having kept him in the recumbent posture sometime, his pulse being 102, at 1 o'clock, P. M. I gave him seven drops of the saturated tincture. At 2 o'clock, P. M., pulse variable, from 90 to 108. 3 P. M. (subject asleep,) pulse 94. The dose of seven drops repeated. 4 P. M. (subject asleep,) pulse 104.

Feb'y. 26th—1 o'clock, P. M., pulse 100; 1½ o'clock, pulse 114; 2 o'clock, pulse 100. No phosphorus was given to-day. The subsequent experiments with Sam were with the diluted tincture. The experiments already given were conducted with the subject in the recumbent posture. In the following, he was generally, when a dose was to be given or the pulse counted, called in from play:

March 10th—11 A. M., pulse 100; half a drop of the diluted tincture given. 12 M., pulse 103; half a drop given. 1 P. M., pulse 100 to 106; one drop given. 2 P. M., pulse 104; two drops given. 3 P. M., pulse 108; two drops given. 4½ P. M., pulse 92; two drops given. 6 P. M., pulse 88, and five minutes later, 96.

March 11th—Half after twelve, pulse 96. 1 P. M., pulse 104. 2 P. M., pulse 100. 3 P. M., pulse 101. 5 P. M., pulse 86. 6 P. M., pulse 92. No phosphorus was given to-day.

March 12th—10 A. M., pulse 98; 50 drops given. 11 A. M., pulse 102. 1 P. M., pulse 96; 100 drops given. 2 P. M., pulse 104. 5 P. M., pulse 100; 200 drops given. 5½ P. M., pulse 100; 200 drops

given—making in all 550 drops in seven and a half hours. 6¼ P. M., pulse 107. 7½ P. M., pulse 94.

March 13th—8 A. M., pulse 92; 500 drops given. 9½ A. M., pulse 92.

March 17th—4 P. M., pulse 104; nine hundred and ten (910) drops, being exactly one ounce, given at a dose. Here a child seven years old took, at a single dose, one thousand eight hundred and twenty (1820) of Docter Ames' doses for the adult. Half after four P. M., pulse 116. 5 P. M., pulse 120. 7 P. M., pulse 104, and the subject "feels very well."

The two hours immediately succeeding each of the last two doses, Sam spent riding in my buggy with me, and attending to my horse at the different stopping places; and though I noticed him carefully, no appreciable effect was manifested—the variation in the pulse alone excepted—otherwise than that he seemed a little merry, which was fairly, I think it will be admitted, attributable to the alcoholic part of the dose—about half an ounce in one instance, and an ounce in the other. It would scarcely seem necessary, while the large quantity of alcohol would render it improper, to repeat, or to carry the —— joke any further with Sam.

My second subject was a negro man, about 25 years old, under treatment, by means of the bandage and the recumbent posture, for a small ulcer on the foot. Otherwise, he was healthy. He lay in my office during the experiments, which *were all conducted with the saturated tincture.*

Feb. 27th—1 P. M., pulse 78; ten drops given. 4 P. M., pulse 72.

Feb. 28th—12 M., pulse 78; ten drops given. 1 P. M., pulse 80. 3 P. M., pulse 72. 4 P. M., pulse 78.

March 1st—11 A. M., pulse 74. 2 P. M., pulse 74. 3 P. M., pulse 68. 4 P. M., pulse 80. No phosphorus was given to-day.

March 2d—12 M., pulse 72; 20 drops given. 1½ P. M., pulse 71. 3 P. M., pulse 67.

March 3d—12 M., pulse 74; 25 drops given. 2 P. M., pulse 74; repeat the dose. 3 P. M., pulse 67. 3½ P. M., pulse 72.

March 4th—11 A. M., pulse 80. 12½, pulse 72. 3 P. M., pulse 68. No phosphorus was given to-day.

March 5th—12 M., pulse 74; 40 drops given. 1 P. M., pulse 74. 3 P. M., pulse 66.

March 6th—9 A. M., pulse 70; 60 drops given. 10 A. M., pulse

71. 1 P. M., pulse 74; 70 drops given. 2 P. M., pulse 72. 3 P. M., pulse 80. 4 P. M., pulse 80.

March 7th—12 M., pulse 78. 12½, pulse 64; 80 drops given. 2½ P. M., pulse 76.

March 8th—Half after twelve o'clock, pulse 74; 100 drops given· 1½ P. M., pulse 76. 2½ P. M., pulse 74. 3 P. M., pulse 74; 10 drops given. 4½ P. M., pulse 84; 21 drops given. 6¼ P. M., pulse 84.

March 9th—8 A. M., pulse 74; 2 drops given. 9 A. M., pulse 74; 30 drops given. 10 A. M., pulse 76; 40 drops given. 10½ A. M., pulse 76. 2 P. M., pulse 70. 3 P. M., pulse 78; 50 drops given. 3½ P. M., pulse 75; 50 drops given. 4 P. M., pulse 84. 5½ P. M., pulse 84; 100 drops given—making in all 272 drops of the saturated tincture administered during the day. 6½ P. M., pulse 82.

March 10th—7½ A. M., pulse 76. 8½ A. M., pulse 70. 10 A. M., pulse 80. 11 A. M., pulse 80. 12 M., pulse 82. 1½ P. M., pulse 76. 2 P. M., pulse 68. 3 P. M., pulse 74. 4½ P. M., pulse 80. 6 P. M., pulse 82. No phosphorus was administered to-day.

March 11th—No phosphorus was given, but the pulse being counted at intervals, was found to vary about as it did yesterday.

March 12th—10 A. M., pulse 76; 100 drops given. 11 A. M., pulse 82. 1 P. M., pulse 74. 2 P. M., pulse 82.

March 13th—8 A. M., pulse 72; 2 drops given. 9½ A. M., pulse 80. 10½ A. M., pulse 84; 2 drops given. 1½ P. M., pulse 78; 2 drops given. 2½ P. M., pulse 80; 2 drops given. 3½ P. M., pulse 78; 2 drops given. 4½ P. M., pulse 80.

March 14th—9 A. M., pulse 74; 150 drops given. 9½ A. M., pulse 74. 12½, pulse 84. 1½ P. M., pulse 80.

March 15th—9 A. M., pulse 72; 200 drops of the saturated tincture given. 10 A. M., pulse 72. 11 A. M., pulse 74. The subject was questioned and examined particularly. There was no nausea or vomiting; no burning sensation, or feeling of oppression in the stomach or epigastric region; in fact, he says he feels "very well." 1 P. M., pulse 76. A short time before he took the two hundred drops he ate a hearty breakfast, and at 2 P. M., with decided relish, a substantial dinner of bacon, cabbage, potatoes and corn bread. 4 P. M., pulse 80.

How much further the dose might be augmented with safety and without *appreciable effect*, I am at present unprepared to say; but reasons I think will appear as we proceed, that will render it not very

improbable that the quantity of alcohol rather than any suppositious quantity of phosphorus, the preparations, as prescribed and given may contain, should form the only necessary limitation of the dose. To be ready for the press, I must here close my experiments so far as they are to be used in the present paper, though I shall probably continue them hereafter.

In my experiments, two methods of administration were principally pursued. In one, the tincture was dropped into a glass with water and given to the subject, sometimes before the entire disengagement of the whitish vapor that rises as the solution is mixed with water; and often a part of the vapor would escape with the first expiration after the dose was swallowed. In the other method, the solution was dropped into a vial with water; the vial was then corked and shaken, and the dose poured into a glass when given. These are the two plans pursued it would seem by those who have used the article here as a curative agent in pneumonia. The latter seems to be the one adopted in the prescription given by Dr. Ames in his paper. The preparation that I at first used in my experiments I obtained from the Apothecary from whom I am in the habit principally of procuring medicines for my own use, and who prepared it at my request; but though I had no reason to suppose that the article thus procured was not of good quality, I subsequently supplied myself with both the diluted and saturated tinctures from the Apothecary who prepared the tinctures used by Doctor Ames.

As to the effect of the article upon the pulse, others may judge from the experiments recorded as well as myself. To me as a sedative it appeared to be entirely null; and this I apprehend will be the ultimate verdict of the profession. From my previous reading I had been led to expect a stimulant action from the larger doses, but no appreciable effect of the kind was observed that was not fairly attributable to another and more obvious cause than the phosphorus, to wit, the alcohol. This I think, as the subject is further developed, will not seem at all strange. In not one single respect was any effect that I could discover produced by the phosphorus. In no instance was there either disturbance of the stomach, nausea, vomiting, a burning sensation or a feeling of oppression at the epigastrium, though from the quantity of alcohol alone in the last two doses taken by Sam, something of the kind might not unreasonably have been expected. In no instance was there any alteration in the appearance of the tongue, diminution of

appetite, or appreciable alteration or disturbance of the system in any way. My subjects ate with a good appetite, drank, slept, and in every respect deported themselves—while all their functions apparently were performed—as if nothing had been given; a temporary alcoholic excitement in the case of Sam after the last two doses alone excepted. When my subjects were asked how they felt after taking a dose, large or small, the answer generally given was, "very well," or "first rate."

It is stated in effect by Doctor Ames, that when the dose of phosphorus is enlarged beyond a certain point, the sedative influence is counteracted or prevented by a stimulant action, the result of the "local inflammation it excites" in the stomach and bowels. Now as nothing of this preventive or counteracting character occurred in my cases—the sedative action of the article therefore not being in the slightest degree interfered with, seeing that in the experiments mentioned by Doctor Ames the pulse was *invariably reduced in frequency* from eight to twelve beats in the minute in the course of from an hour to an hour and a half by a *single dose of two drops* of the saturated tincture—to what an extreme state of sedation should my subject have been depressed who took two hundred (200) drops of the same at a single dose, and again one hundred drops at one dose, and two doses of fifty drops each, besides several smaller doses, making in the aggregate two hundred and seventy-two (272) drops, all in nine hours and a half; while the degree of inflammation of the stomach and bowels that should have occurred, but did not, is awful, even to think of.

Further, in regard to the stimulant action of these tinctures. It will be observed, that after the administration of several of the doses, a slight *increase* in the frequency of the pulse took place. Still, the variation does not seem greater, omitting the doses of the 13th and 17th of March taken by Sam (in which so obvious a cause for the excitement was present, as in one instance an ounce, and in the other about half an ounce of alcohol,) than on other occasions where the smaller doses were given, or when it was omitted entirely. If, however, it should be supposed by any that a stimulant action from phosphorus was manifested on the occasion referred to, the absence of all symptoms of the local inflammation of the stomach and bowels, to which Doctor Ames regards the stimulant action as secondary and consequential, would show that it could not in my experiments be fairly attributed to any such cause. It would seem not improbable then, admitting that any stimulant effect really was produced by the phos-

phorus, that the received explanation of authors of a diffusive operation may be the correct one. It will not be doubted, however, that phosphorus may be so administered as to produce a local irritant action upon the stomach, consequent upon which there would be a febrile reaction.

Besides the experiments given, I have also taken a good deal of the article; indeed, had taken it sometime before Icommenced its administration to Sam. I took it at first for a while with the occasional omission of a dose on account of absence from my office on professional business; but in one instance I took for eight successive days as much as five drops of the saturated tincture three times a day without the omission of a single dose, and in no respect did I ever experience any appreciable effect from its use. Though in every instance I was careful to drop out the full number intended, of five drops, I did not regard it as a matter of any importance when a few drops, say a half a dozen or so, more than that number happened to escape, but took the dose as if but the intended number was contained in it.

In connection with this experimental practice upon myself, I will again call attention to the views of Dr. Ames in regard to the effects of the tinctures of phosphorus and their dose. Thus he says, speaking of the saturated tincture, that it "cannot be continued in the smallest quantity just mentioned,"—half a drop—"for any great length of time, without inducing considerable disturbance of the stomach, shown by nausea or vomiting, burning heat, and a feeling of oppression at the epigastrium." Though he admits that in the quantity of two drops, "a *single* dose, or *perhaps a few* doses may be given with impunity," he would evidently regard any lengthened use of it in such a dose, as a very grave and serious matter, and tells us of one instance in which dangerous effects resulted from the administration of three doses of two drops each, at intervals of twenty-four hours. It is most desirable that we should yet be able to discover and explain the cause of these discrepancies. While Doctor Ames tells us that doses of half a drop cannot be continued for any great length of time, without the most serious results, I have myself taken it in doses of five drops—being just ten times the quantity—a long time, and for eight days without omission of a single dose, without effect. While under his observation, from a cumulative action, dangerous effects resulted from three doses of two drops each, administered at intervals of twenty-four hours,

being in all six drops taken in the course of three days, yet I have taken for eight successive days three doses of five drops each, or fifteen drops per day, without effect. Indeed, unless I should discover something in its action, which has never as yet been manifested in any of my experiments, from my own experience with the article, and with all the lights at present before me, I should not hesitate, were it not for the mere trouble of the thing, to continue it in the same manner for years.

In the healthy subject at least, any effect of the article resulting in nausea and vomiting, could be easily appreciated, and not readily mistaken; yet not only did the subjects of my experiments take it in doses as mentioned, so immeasurably greater than the doses with which such effects are said by Doctor Ames to have been produced by it, but they took it under circumstances that were well calculated to favor the production of such an operation. Thus, while they sometimes took it in the middle of the intervals between the meals, they also took it at times immediately before eating, and at others immediately after eating. On several occasions I myself having forgotten my dose, which I usually took just before eating, until I had partly finished my meal, have called for my vial, taken the dose, and proceeded with my meal without disrelish or any subsequent manifest effect.

A word or two of explanation in reference to the dose recommended by Doctor Ames, that I may not be charged with rashness or temerity on account of the doses given to my subjects, may not be out of place. It will be seen that these took at different times doses of various sizes, from the favorite one of Doctor Ames—half a drop of the diluted tincture—up to two hundred drops of the saturated tincture.

It is supposed by Doctor Ames that the saturated alcoholic tincture contains four grains to the ounce. The diluted tincture would in that case contain four grains to ten ounces, or there would be one grain in two and a half fluid ounces. Doctor Ames' dose then of half a drop, supposing the drop to be equal to the minim, would be the one-twenty-four-hundredth ($\frac{1}{2400}$) part of a grain, there being twelve hundred minims in two and a half fluid ounces. But the drop is *not* equal to the minim. While there are but sixty minims in the fluidrachm—Professor Wood tells us that it takes one hundred and thirty-eight

drops of alcohol to make a fluidrachm—it would seem scarcely probable that the small quantity of phosphorus held in the diluted tincture would cause any material variation in the size of the drop. In sundry trials made by myself in which several vials were used, I found that the number of drops of the diluted tincture necessary to make a fluidrachm varied with the shape of the vial and the thickness of the lip, from 100 to 125. Let us take, however, the smaller number, though the drops in that case will be found above the average size. Of these there would be just two thousand in the two and a half ounces of the diluted tincture, and each drop therefore would contain the one-two-thousandth ($\frac{1}{2000}$) part of a grain, consequently the half drop dose would contain the one-four-thousandth ($\frac{1}{4000}$) part of a grain; or there are four thousand of Dr. Ames' doses in a single grain. The strength of the saturated tincture is ten times that of the diluted tincture, consequently my subject who swallowed two hundred drops of the former at a single dose, took what was equal to four thousand (4000) of Doctor Ames' doses of the diluted tincture, and this without any effect.

The above estimates are all based upon the supposition of the correctness of the assumption of Doctor Ames, that the saturated alcoholic tincture contains in solution four grains of phosphorus to the ounce, as the etherial tincture it is said does. We are not told by Doctor Ames whether he has any authority for this opinion, nor does he speak of any experiments performed by himself to test the solubility of phosphorus in alcohol. It would appear to be then a mere supposition, that an ounce of the saturated tincture contains four grains. Without some experimental tests we cannot feel certain that it may not contain more, or that it may not contain less, than the quantity mentioned. No author that I have consulted says anything as to the exact quantity of phosphorus that any given quantity of alcohol will dissolve. Many of our very highest authorities on the Materia Medica do not even speak of an alcoholic solution; and those who refer to it at all, at least all that I have consulted, speak of phosphorus as very insoluble in alcohol as compared with its solubility in ether.

In the absence then of any definite statements on the subject, I was led to institute myself a series of experiments with the view of ascertaining with greater certainty the solubility of phosphorus in alcohol. In a vial containing one ounce of anhydrous alcohol, I placed

four grains of phosphorus, in another two grains, and in another one grain. At the end of fourteen days—the time usually considered necessary for the preparation of tinctures by maceration, the time directed by a majority of the pharmacopœias—of the four grains, about one-fourth or less was dissolved; of the two grains, about one-half or less, and of the single grain, there still remained a portion undissolved. On testing these several tinctures by dropping them in water, they all gave off the white vapors as freely as any of the tinctures supplied to me by the apothecaries. It is fair I think to say then, that the saturated alcoholic tincture, instead of containing in solution four grains to the ounce, contains in reality but about one grain to the ounce. It may possibly be a little more; it would seem as likely to be less. The favorite preparation then of Dr. Ames—the diluted tincture—would contain one grain to ten ounces. Allowing, as heretofore, one hundred drops to be equal to sixty minims, or a fluidrachm—and this is a low estimate—we have in the ten ounces eight thousand (8000) drops. Eight thousand drops then of the diluted tincture will contain one grain of phosphorus, and one drop will contain the one-eight-thousandth ($\frac{1}{8000}$) part of a grain. The favorite dose of this tincture with Dr. Ames is half a drop, and the quantity of phosphorus consequently contained in it is the one-sixteen-thousandth ($\frac{1}{16000}$) part of a grain.

But it is very probable, even, that not all, indeed that but a small proportion of the almost inconceivably minute quantity of phosphorus contained in the dose is received by the patient. In using the saturated tincture or solution at the moment of contact between the drop and the water, a quantity of vapor is given off in the form either of phosphorus as such set free, or as one of its acids formed in consequence of chemical decomposition and combination. Possibly this may be the cause of the apparent nullity of effect in my experiments; and if so we are necessarily forced to the supposition of an equal nullity, from the same cause, in the hands of others, who use the alcoholic solution suspended in water; and this is the method pursued by Doctor Ames. Now it does not seem at all improbable, on the contrary extremely probable, that the action or chemical change alluded to, attended with a proportionate loss, also takes place when the diluted tincture is combined with the water, though the disengaged vapor, on account of the minute quantity of phosphorus contained, may not be appreciable

to the sight. The alcoholic solution or tincture, it is more than probable then, is not the best preparation of phosphorus, by which to secure with any certainty, its full and peculiar operation, whatever this may be, upon the system. Lobstein, who seems to have made the subject of phosphorus something of a hobby, prefers its administration in the form of etherial tincture, and tells us that such a preparation "is not decomposed by cold water," as seems to be the case with the alcoholic tincture.

I have heard it remarked by individuals that certain physicians* (naming them) must be in the habit of using *very strong* medicine, for that when they dropped it in water "it fairly smoked." Had the dropping been conducted in the dark, the conflagration from which the smoke was given off, might also have been seen. It would not, therefore, however it is probable, have been believed, that the strength of the medicine was all smoke—vapor—but rather that "'twas spirit Pandemonium," at the very least.

Lobstein—who from the case to be referred to, there is grounds to suspect, must have been decidedly something of a Charlatan—once, from his own account, took advantage of one of the singularities of phosphorus, to operate to an extent beyond the influence of mere smoke, upon the imaginations of the persons surrounding a patient that he had been called to see. A woman fainted. He gave her a few drops of phosphoric ether. "In two minutes after," he tells us, "a flame was observed to proceed from her mouth, which caused all present to cry out with astonishment. I observed, humorously," he goes on, "that she had a devil in her. Nevertheless, I gave her a second dose, and after a short time she opened her eyes and exclaimed 'ah, I feel very well!' The bystanders were so much surprised that they really believed that the devil had flown out of her mouth,"—exorcised, as a matter of course, by the medicine given.

I have the permission of my friend Doctor Baldwin to say, that he has performed a series of experiments somewhat similar to mine,—has even given larger doses—and with like success.

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*With *several* of the physicians of Montgomery, who are no doubt indebted principally to Doctor Ames for directing their attention to it, phosphorus, in the form spoken of in the text, has been for some three or four years past, quite a favorite remedy in pneumonia, and one to which a remarkable degree of success claimed, in the treatment of this disease, is, in a considerable degree, ascribed by them.

In conclusion, I would remark, that though prior to the experiments here recorded, I had taken and given, experimentally, a good deal of the alcoholic solutions of phosphorus, I have never, in any instance, given them in a case of acute or dangerous disease. It would seem to have been truly said, that "there are more false *facts* than false theories in medicine."

ART. V.—AN INQUIRY, ANALOGICAL AND EXPERIMENTAL, INTO THE DIFFERENT ELECTRICAL CONDITIONS OF ARTERIAL AND VENOUS BLOOD.

BY JOHN GORRIE, M. D.

[Continued from page 602.]

In the preceding number of this Journal I deduced, from the analogies afforded by the mechanical structure of the blood globules, the general physical properties of arterial and venous blood, the structure of the arterial coats, the comparative chemical composition of the two kinds of blood, and the probable dependence of some functions of the organic system upon electricity, the hypothetical conclusion that the normal electrical condition of arterial is one of higher tension than that of venous blood.

Before entering upon the second part of my inquiry, I deem it proper to remark, that in the physical sciences generally, and in physiology particularly, there are many propositions which have not been, and perhaps cannot be, proved by experiment, that, nevertheless, the most matter of fact mind does and must look upon as indisputably true. It is admitted as a sound principle of inductive philosophy, that where analogy accords rationally with the cause, adaptation and end of an object, it is not absolutely indispensable to its credibility that it should be tested by experiment. If this test of truth were always necessary to the progress of knowledge, its acquisition would often be seriously impeded, and sometimes wholly arrested. In the assumption that the blood is endowed with electrical action, I assume the existence of a force which, besides its support from analogy, the

wants of the animal system seem so imperatively to require, and which harmonizes so well with the phenomena of life that, it might appear, experiment was scarcely required to strengthen the position.

But however valuable logical deductions and the ingenious adaptation of means to ends may be in the pursuit of knowledge, they are not deemed sufficient to establish the existence of a physiological fact, if its nature admits of being proved by experiment or direct observation. Though furnishing principles towards elucidating every science upon which the learned and unlearned are compelled to act, they cannot be admitted as positive and certain sources of knowledge. Indispensable to the progress of physiological research, from the light they throw upon parts of the organic structure remote from observation, or beyond the reach of experiment, and imparting interest which urges to the investigation of doubtful questions, they are yet of more value as illustrations than as direct proofs.

In numerous instances of physiological research, where analogical reasoning establishes an affirmative position, it may not be within the bounds of experiment or observation to either confirm or demonstrate its falseness; the utmost it can do is to show a negative result. A simple experiment, referred to by Dunglison,* in relation to our theme, illustrates the position. "Muller," he says, "was unable to discover with the galvanometer any electric current in the blood, and he perceived no variation in the needle of the multiplicator when he inserted one wire into an artery of a living animal and the other into a vein." But in physiological, as in ethical science, the proof of a negative can never be more than presumptive. To deny a position founded upon positive deductions, whether furnished by analogical or moral evidence, cannot be deemed sufficient to set it aside. The experiment of Muller, though affording no confirmation, is yet incapable of confuting my hypothesis.

Throughout nature the operations of the imponderable forces are generally pursued in a manner so slow and silent that they are with difficulty perceived by the senses, even when aided by the most ingenious artificial apparatus. Their effects are commonly manifested in a way which the philosopher's art can neither imitate nor explain.

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*Physiology, Vol. 2, p. 122—7th Edition.

Even when producing the most stupendous results on the inorganic world, we often find it impossible to obtain direct evidences of their nature; and they have always such an energetic tendency to equilibrium that where the cause is satisfactorily known to our reflections, an excess of it is seldom maintained long enough to admit of a deliberate examination of its properties by our senses.

In the homogeneous and good conducting properties of the components of organic bodies, we perceive the evidences that any disturbance in the equilibrium of their imponderable forces must be feeble in character and of short duration; and, indeed, if it were not subject to rapid renewal, it would be wholly beyond the reach of observation. For these reasons the very existence of electricity in a form proper to animal organization was long looked upon as equivocal and even denied. Consisting of slight deviations from, or rapid revolutions around a neutral point, the phenomena were so obscure that under the influence of an hoypothesis supposed to be universal in its application, they were only considered as partaking of those properties of electricity common to all matter. Though fairly deducible from the earliest as well as all subsequent galvanic experiments with organic elements, the distinct existence of animal electricity was not admitted till Matteucci, more than half a century after its discovery, showed that the facts refused to submit to explanation on any principle.

It is important to a correct understanding of the subject before us, that clear and distinct conceptions should be entertained of the inherent difficulties attending its experimental elucidation. If the indications of electricity in organic solids have hitherto been found obscure, we can readily conceive that it must be difficult to show the presence of so subtle and diffused an agent in organic fluids. The experiment of Muller is apparently the most simple in plan and the most appropriate in principle that could be devised for ascertaining the comparative electrical states of arterial and venous blood. It presents the blood, to a very delicate measurer of electricity, when in its most natural condition; when it is in constant motion, and consequently in that state in which it develops the electricity of friction; when it is excluded from the air and the causes that tend to alter its natural properties, and when it is maintained at an equable temperature. But it is subject to the objection, among others, that it exposes

an exceedingly small surface of blood to the delicate collecting and conducting wires of a galvanometer, and in this circumstance alone may be found the cause of its failure.

Under all circumstances there are principles which stand in the way of an experimental development of the electrical tension of the blood, the chief among which are the following:

1st. The action of electricity itself upon the blood. It is found that when an electrical current traverses, or is set free from blood, albumen coagulates at and forms an insulating coating around its positive pole, while its most prominent source of electricity, oxygen, is set free from the surface of contact. Indeed, the mere presence of a metal in blood, (which can hardly be dispensed with in forming an electrical circle) is sufficient to cause a coagulum upon its surface,* the effect of which forms another objection to Muller's experiment; for acting upon objects so small as the wires of a galvanometer, it must almost instantly intercept the current or even evolution of electricity, and probably before a circuit with such an instrument could be completed.

2d. The physical properties of blood. Experiments with the present object in view, made on blood out of the body, are divested of some of the advantages of those made on it within its vessels, and are subjected to some peculiar disadvantages. In conducting them the blood must be at rest, and is thus prevented from evolving the electricity of friction; it is exposed to the air, and to a change of properties from the mechanical action of that fluid; and it is liable to be lowered in temperature, and from this cause, to lose a portion of its electrical tension. The extent of the obstruction, under these circumstances, to a successful experiment, may be judged of from the assertion of Liebig, that "no other component part of the organism can be compared with blood in respect to the feeble resistance which it offers to change from exterior influences." Though the energy of its vital principle enables it to offer, for a short period, a powerful resistance to the ordinary influence of material agents, yet its physical properties are held together with so weak an affinity that "any disturbance, however trifling, or from whatever cause it may

*Carpenter's Physiology, page 533.

proceed, effects an immediate change in them." Of course the state of the imponderable forces must differ with every change in the physical properties of blood, and any disturbance from that state which characterizes its normal condition must rapidly tend to equilibrium.

3d. The chemical properties of blood. The points of resemblance between the chemical properties of arterial and venous blood have been stated to be numerous and intimate. It is also found that each kind possesses so little power of maintaining its few distinguishing peculiarities that the moment it is withdrawn from the body it tends—the arterial by its affinity for carbonic acid, the venous by absorbing oxygen—to assume the characteristics of the other. It is obvious, from a consideration of general electro-chemical principles, that any difference of electrical excitement between the two varieties, must be one of exceedingly low intensity, and of very difficult measurement.

With these explanations of the action and reaction of electricity and blood, and the extreme liability of the latter to a change of physical and chemical properties, we can readily understand that it must be difficult to obtain a practical manifestation of its electrical tension; and that, other things being equal, the difficulty must be greater when withdrawn from than when contained within its vessels. But however contrary in appearance to Nature, it will be presently shown that all the disadvantages of the latter mode of experimenting are compensated by the large quantity of blood and unlimited extent of surface it admits of being operated upon.

On account of the difficulties attending the experimental investigation of this whole subject in the direct way, the electro-physiologist, in place of it, has frequently resorted to, and seems generally to have been satisfied with, the less difficult but inverted or indirect mode. Though inferior to that of direct experiment, and conducted for the most part with objects in view different from the present, it has yet tended to elucidate the electrical states of the blood. It is obvious, that if the blood by a change of properties develops electricity, the influence between them must be mutual, and the latter in its turn must affect the blood so as also to change the chemical character of its constituents. In this inverted action as applied to the solids of the animal system, is comprised the most familiar experiments in electro-physiology: no muscle is insensible to the stimulus

of electricity; the digestive function has been carried on by it; and experiments indicate that even the sensorial powers may be set in motion by galvanic and electro-magnetic forces. If, then, it can be shown that the properties of both arterial and venous blood may be changed so as to make each the other by electrical action, it must be evident that not only is there a difference between their electrical conditions, but, also, that their peculiar nature is intimately connected with this difference.

The most observable alterations in the physical properties of blood are the loss of the black and the acquisition of the red color in venous, and the change of the red to black in arterial. That these modifications of color do not depend, as commonly supposed, upon a mere alteration of affinity for oxygen or any chemical change of composition, is evident from the experiments of Fourcroy,* since repeated by Sherer.† The former found that arterial blood left in oxygen gradually darkens, and that after this change, oxygen will not redden it; and the latter has shown "that the bright red color is dependent on other causes than oxidation, and that the dark tint of the venous does not arise from carbonic acid or carbon." Early in the history of galvanism, experimenters‡ announced a discovery, which, though for a long time doubted, has been more recently confirmed, viz: that the fibrine of the blood immediately after it leaves its vessels, may be made to contract by the galvanic apparatus. Dr. Philip not only puts this discovery past doubt by new evidence, but at the same time makes the additional observation, that if the galvanic instrument be applied to fresh drawn arterial blood, an evolution of heat amounting to 3° or 4° F. takes place while the blood assumes the venous hue. He adduces these facts as favorable to his hypothesis of the identity of the nervous and galvanic energies;|| but whatever may be their value in this respect, they certainly lend some support to the doctrine of an electrical agency in the conversion of one kind of blood into another. It may be further mentioned in connection with this part of the subject, that Dr. Moran, in an inaugural thesis,§ asserts, as the result of the experiments instituted by himself, that recent venous

* Quoted by Thompson in Annals of Philosophy.
† Ranking's Abstract: vol. i, p. 286.
‡ Circaud, and subsequently Delamatherie.
|| Bostock's Physiology: vol. ii, page 225.
§ De Effectibus Electricitatis Quibusdam. 1820.

blood, acted on by galvanism, becomes at the positive pole, blacker and thicker, but at the negative pole, redder, thinner and spumous. On the other hand, Dr. Miller, in an Appendix* to Philip's "Treatise on Chronic Diseases," affirms that the positive pole of a galvanic battery, immersed in venous blood, removes the modena and restores the florid color; and, on the contrary, the negative pole deprives arterial blood of its florid hue, and substitutes the dark or modena. The discrepancy in the reported results of so simple an experiment may be owing to a careless or confused use of the terms negative and positive, or to a want of the requisite precautions in one or the other of the experimenters, to guard against deception in the observations. In repeating the experiment, with some modifications, the results appeared to me to conform to the representations of Dr. Miller. Upon a consideration of the whole of the experiments, they may be considered as establishing, so far as the inverse action of electricity can do so, that arterial and venous blood bear a positive and negative relation to each other; and, taken in connection with the facts stated by Fourcroy and Sherer, they indicate the probability, at least, that electricity is the efficient cause operating in the change from one kind to the other.

The results of indirect or inverse experiments, like deductions from analogy, are not to be received as conclusive evidences of a scientific fact, when those of direct experiments are of possible attainment. It must, therefore, be regarded as fortunate, for the important object we are striving after, that it admits of being verified by what may be considered experiments of a direct character.

It must be obvious from the considerations heretofore presented, that an experiment which shall determine correctly, or even with an approximation to accuracy, the electrical relations of arterial and venous blood, demands very great delicacy of means and extreme precaution in their management. Though these fluids comprise a greater diversity of chemical composition than muscle and its tendinous aponeurosis, from which Matteucci demonstrated so readily the existence of an electrical current, yet, being more diluted and diffused in water, the contrivance which shows it in the latter is unaffected in the

*Page 294.

former. To obtain a clear manifestation of an electrical relation between two animal fluids, it would, indeed, be desirable that some instrument, corresponding in delicacy with the balance of torsion for static, and the galvanometer for magnetic, and adapted to this peculiar electricity, should be invented. The prepared frog of Galvani furnishes this delicacy for indicating the apparently static form of the electricity of organic solids; but something even more delicate seems to be required to measure that developed by the contact of animal fluids. Besides sensitiveness of apparatus, it is essential to success that a large quantity of blood should be presented, over a very large surface, for the appropriate action of electricity. And, as it has been shown, that both arterial and venous blood approach very rapidly a common state, not only as regards their imponderable agencies, but also in the same striking degree their physical elements, the experiment should be made quickly, and, if possible, without exposure to the atmosphere.

To fulfill these indications as far as possible, and to insure accuracy of observation in the experiments about to be described, every precaution in my power was taken. I am aware that the precision which belongs to the importance, and should characterize the delicacy of electro-physiological experiments, has not been attained; and I fear the experiments will be contemned by those accustomed to the use of the nice appliances of a complete laboratory. But the idea involved, strongly impressed me with its truth and value, and I felt it a duty and pleasure to pursue it with such means as I could command to its practical development. Of the readiness of indication of some important portions of the apparatus I employed, I was unable to determine, from the want of a standard of comparison. Living in a frontier town, remote from cities, where the nice mechanical arts requisite for the construction of philosophical instruments are pursued as a business, I was compelled to supply, by my own ingenuity, any deficiency thence arising; and the apparatus I used was of domestic construction. In the course of the experiments a galvanometer was found indispensable; and as one of artistic construction was not to be obtained, the following was used as a substitute. Ten yards of fine copper wire covered with cotton thread were wound into the form of a parallelogram, having its sides equal in width to the length of the magnetic needle used,

and admitting of the motion of the latter within its area. This coil was placed upon a piece of varnished pine board as an insulating base. The needle was suspended by a silk thread from a support formed of copper wire, the ends of which were fastened to the piece of board and bent at right angles about six inches above the surface of the coil. The whole apparatus was covered by a bell glass to screen it from the action of the wind. It was not made a static, and, therefore, had not the delicacy of that form of the instrument. The delicacy of other portions of the apparatus was not a consideration of so great importance, and, on that account, I make no special reference to it.

Accuracy in physiological observation, like success in the experimental sciences generally, is seldom attained but after successive attempts and failures; and this truth was fully exemplified in the present investigation. It may subserve an interesting purpose to relate one among several experiments from which decided results were expected, but which failed totally, or produced either trivial or deceptive effects. Electricians tell us that a galvanic circle may be formed, and a current of electricity established, by the use of one metal and two liquids, as when a zinc plate is converted into a box, and acted upon on one side by diluted acid, and on the other by solution of common salt. Moreover, Aldini and many other electro-physiologists have obtained distinct signs of an electric current by touching the muscles and nerves of a frog with a single metal, and Matteucci by connecting two different parts of a muscle by galvanometer wires. For these reasons, I thought it possible similar evidences of electricity might be obtained by presenting large surfaces of venous and arterial blood to opposite sides of a single metalic conductor, or, what I considered the same thing, to two conductors of the same metal.

To make this experiment, I took two similar glass jars, each of about a pint capacity, and carefully increased their insulating properties by coating their outer surfaces with a varnish made of shellac dissolved in alcohol. Into each jar I introduced a sheet of zinc twenty-four inches long and four inches wide, cut from a larger sheet, and rolled into a form of a scroll. To the upper edge of each scroll a copper wire six inches long and the twentieth of an inch in diameter was soldered. The mouths of the jars were closed with corks covered with sealing wax, but each having an orifice of about one-fifth of an inch in diameter, to admit the passage of a flexible gum-elastic tube of the

same diameter; and another small hole through which the wire soldered to the scroll passed. These jars were placed upon a plate of window glass, and the whole put upon a pine table.

The apparatus being thus far ready for experiment, a galvanoscopic frog was next prepared after the manner described by Matteucci;* that is, by cutting through the middle of its spine, separating carefully all the integuments, muscles and bones of its thigh and pelvis, and dividing one of the lumbar plexuses of nerves as it passes out of the vertebral column. This, put into a glass tube covered with an insulating varnish, forms, with its long nervous filament (composed of the lumbar plexus and the crural nerve, extending beyond the end of the tube,) a most sensible electrometer for determining the presence of electricity in any two solid parts of an animal; and it was thought might be made also to measure that of animal fluids.

The blood experimented with was taken from sheep. Considerations connected with the properties of the globules have led me to infer that there is a relation between them and the electricity of the blood; and that in proportion as their number is greater or less the latter is greater or less. The blood of the sheep was selected because it contains in the normal state less globules than that of man, and hence it was concluded that if it manifested electrical tension, that of man must afford it in a higher state. This animal was also chosen for the experiment, because its timidity and passiveness rendered it easily manageable. Having laid bare and put a temporary ligature around the external jugular vein and carotid artery of a young and lively but full grown one, I introduced one of the flexible tubes before alluded to into an incision made in the former, and dividing the artery, I inserted about three-quarters of an inch of the section next to the heart into the other tube.† The other ends of the tubes were now passed through the perforations in the corks into the jars, and at the same time the ligatures were loosed from the vein and artery and slipped so as to include the tubes, and again tied.

Everything being considered ready for the experiment, the blood was allowed to flow into the jars; and its discharge was so regulated by the ligatures and pressure with the fingers, as to admit as nearly as possible an equal quantity into each jar. During the flow I applied

*Lectures on Physical Phenomena: page 177.

†It was necessary to insert the artery into the tube, because in the sheep this vessel is too small to admit the introduction of an inorganic tube through which blood would freely flow.

the delicate galvanoscope above described, holding it by its glass envelope so as to touch each of the wires connected with the zinc scrolls with different parts of the nerve; and though one of the two spectators present doubted whether he perceived any motion, the other and myself saw distinct but slight contractions of the leg, which were renewed two or three times, or as often as the contact was broken and the nerve reapplied. When about twenty cubic inches of blood had flown into each jar, the animal became convulsed, and the communication between the jars and the blood vessels was broken, and with the separation all galvanic influence ceased.

In a similar experiment, and as a further test of the supposed efficiency of this mode of experimenting, the conducting wires from the scrolls were connected with the wires of the galvanometer, and immediately a sensible agitation and a slight deflection of the needle, with an elevation of the north, and, of course, a depression of the south pole, were perceived.

This experiment may be considered the same in principle as that of Muller, before mentioned, and differing in execution only in the extent of surface of blood brought into action and in being withdrawn from the body. The scrolls of zinc acted merely as collectors and conductors of the electricity of the two kinds of blood, taking no share in the manifestation of force. But though the effects produced were consistent with what we may suppose the action of electricity from animal liquids would be, yet as they may have arisen from impurities in the zinc, or possibly from the shaking of the apparatus, and were altogether so minute and uncertain, they cannot be considered conclusive.

Unable to obtain sufficiently satisfactory evidence of an electrical current between arterial and venous blood by an instrument in which the circle was formed of a single metal and the two liquids, I resorted to the use of a battery constructed, on the general principles of galvanic machines, of two metals. This plan appeared to me to promise greater if not more accurate results. The modification of the instrument I employed is the simple circle made by rolling two metallic sheets, one of copper and the other of zinc, into the form of a double scroll. Each sheet was fifteen inches long and four inches wide, and was separated from the other by strips of gutta percha; it was furnished with a wire terminating in a cup to contain mercury for the convenience of easily making and breaking the circuit. The double

scroll was placed in one of the glass jars prepared as before mentioned.

This instrument and a galvanoscopic frog being ready, I laid bare the external jugular vein of another sheep destined for slaughter, and introduced into it one end of a flexible tube and inserted the other into the jar containing the battery. As soon as the blood was allowed to flow into the apparatus and the nerve of the frog was applied to the wire conductors, the limb was seen strongly convulsed, and the convulsion was repeated as often as the nerve was reapplied during the flow. When the quantity of blood reached a mark on the jar, which by previous measurement indicated twenty cubic inches, the tube was withdrawn from the vein, and the violence of the contractions of the muscles of the frog were immediately lessened, and soon ceased.

After freeing the battery of the venous blood, the carotid artery of the sheep was laid bare and divided, and, as in the former experiment, inserted into the flexible tube, the further end of which entered the glass jar. As with the venous blood so with the arterial; the flow, on the application of the nerve to the conductors, was instantly accompanied by a contraction of the muscles of the frog; and the contraction was apparently more violent than that from the venous blood, and increased as the quantity of blood increased.

These experiments, though neither so delicate, direct nor promising of accuracy as the former, prove to the satisfaction of the electrician the existence of an electric current; but as the physiologist knows that muscular contractions may arise from mechanical action and currents of heat, as well from volition or electricity, it is necessary to have recourse to the galvanometer, or some other unequivocal test of electricity, to place beyond doubt its existence in the blood. Moreover, such an instrument is indispensable to determine its relative quantity and its direction or character in each kind of blood.

The experiments just described were repeated on a vigorous young sheep, with the single change that instead of the nerve of a frog the wires of the galvanometer, inserted into the mercurial cups of the battery, were used to test the presence of electricity. As soon as the venous blood began to flow into the battery, the needle of the galvanometer was seen to be deflected, and with the increased flow the deflection was increased till the maximum divergence amounted to about

sixty-five degrees. When the battery was charged with arterial blood the same phenomena were presented with a maximum deflection of the needle of nearly seventy degrees.

The direction of the deflection, when the needle was suspended above the coil of wire, was in all the experiments towards the left hand, or west; but in one or two instances in which the needle was placed below the coil, it was towards the right hand, or east! The greater amount of it from the action of arterial than venous blood, shows that the electrical excitement was greater in the former, while it indicates a current flowing towards the latter; the former liquid bearing the relation of an anode or electro-negative body, and the latter that of the cathode or electro-positive body. Hence, the experiments may be considered as authorizing conclusions as to the kind, the comparative quantity, and even the absolute quantity of electricity in both species of blood.

It may be proper to remark that these experiments were not made consecutively within a short period of time; but have been carried on irregularly, and as opportunity or convenience would allow, over a series of years, and it is only after a long and earnest investigation of them in every practicable relation, that I have obtained my own consent to offer their results to the world.

The remarkable phenomena thus made manifest, though undoubtedly produced by the difference between the physical and chemical properties of arterial and venous blood, are not dependant on the saline matters contained in them, because the most careful analyses are incapable of detecting the slightest difference, in this respect, either in quantity or quality between one and the other. Nor are they to be considered as the mere effects of common electricity, produced by the mutual contact of three dissimilar bodies, but as the evidence of the different quantities of electricity in each kind of blood, because in that fluid only was there any difference of combination between the two electrical circles. In accordance with Galvani's hypothesis in relation to the solids, they show that a proper animal electricity is inherent in the blood, which, if we could devise a sufficiently delicate apparatus to render apparent, would not require the assistance of any external excitant for its development.

The cause of the phenomena is no doubt magnetism, induced by the electric current set free by the energy of the chemical changes of the

blood in the batteries. The difference in the intensity of the manifestation between the two kinds is owing to the arterial losing or lessening its distinctive chemical characteristics by combining with the zinc in a greater proportion than the venous. Every fresh accession of arterial blood by the battery caused it to lose more or less of its peculiar character and to set free a larger proportion of electricity, and in the same proportion the limb of the frog was convulsed, or the galvanometer needle was deflected. The process brings arterial blood so far as it is in contact with the zinc into the same state, in regard to electrical properties, as the venous. And these effects, insensible from a small collector of electricity, as from the wires of a galvanometer inserted into the blood vessels, and scarcely visible from a large surface in a simple circuit, formed of a single metal and the two liquids, are readily manifested by an equally large surface of a common form of galvanic battery.

Here then we may consider that through the attraction and repulsion manifested by the aid of the galvanoscopic frog and the galvanometer, in connection with the evidence afforded by analogy, we have ample proof of the existence in the blood of that primordeal force in an active state, which, in whatever form it may present itself to the understanding, we recognize as electricity. This proof, however, as a result of the investigations of one person, must be considered as adduced from a single experiment, and a single experiment on so novel, complex, intricate and obscure a subject, is of very little value. To throw a clear light upon electrical manifestations from organic fluids, the experiments ought to be many times repeated with better appliances than I possess, and by those more skilled in the use of electrical instruments and in making physiological vivisections than I can pretend to be; and thus, from the comparison of many and precise observations, deduce undoubted results.

I have inferred from both experiment and reflection on the nature of the electricity developed, that the other, and perhaps more marked and unequivocal evidences of an extraordinary presence of this universal energy, viz: the shock, the spark, chemical decomposition and change of temperature, cannot be rendered apparent; but the failure may be owing to the coarseness of the means employed or inefficiency in their application. Nor is this absence of effects or dissimilarity of

properties greater than between several other of the imponderables which are generally recognized as dependent on an identity of cause. Thus magnetism has never been seen with only one pole or one kind of magnetism; it is incapable of transferring its properties; it gives a permanent direction to an unobstructed needle; it cannot be insulated; in all these respects it finds no analogy in the effects arising from other forms of electricity, and yet no one doubts that it is other than a modification of this energy. It is not therefore indispensable to a proof of the existence of electricity in the blood, that it should manifest all and precisely the same phenomena that it does in inorganic, or even in other kinds of organic matter; yet I make no doubt their presence will be shown by future investigators.

Considering that we have proof of a disturbance of equilibrium and of the existence of electrical force in the blood, it will be proper to take a cursory but somewhat less general notice than has been heretofore made of the duties it may perform in the animal economy. I am fully aware of the prevailing tendency and error of the day to exaggerate the agency of electricity, and apply it to the explanation of every mystery in Nature. Like fire in the hands of the alchymists, we see philosophical speculators prone to regard it as a power without limit; one with which all things may be accomplished and without which nothing can be understood; and that, like fire, it has led to innumerable hypothesis and even experiments, which have been attended with no other result than the wisdom taught by painful failures. But the discovery of the electrical relation of arterial to venous blood, though new as a demonstrable fact, has been long expected; and as Nature makes nothing in vain, it is impossible to conceive of so energetic a force in the animal system without supposing it destined to fulfill important duties. Coming in aid of other physical discoveries of modern times, it meets the wants of philosophy, and will no doubt contribute to divest physiology of what remains of its speculative and metaphysical character.

The difference between the electricity of the two varieties of blood is seen to be slight, but accords alike with the physical and chemical difference in their properties; and, considered as a cause, is in perfect harmony with the physiological wants of the animal system. It is not without analogies showing that very great action may be pro-

duced in the vital economy by very slight physical causes. Setting aside the vaccine lymph as inapplicable, from its vital action, to an illustration of the subject, we know that strychnine introduced into the system in too small a quantity to be detected in the blood by chemical analysis, will produce very considerable convulsive action over the whole muscular system. To enter upon an inquiry concerning the manner in which the electrical energy acts upon the animal functions, would at present be premature, because, from the nature of the subject, it would require a long and elaborate train of previous investigation. At some future day, when the accuracy of the discovery shall be confirmed by renewed and extended experiments, it will be shown that in conjunction with other physical forces, it will readily and fully explain the function of nutrition, and, as already intimated, the systemic capillary circulation. On the present occasion I will simply remark, in exposition of the method I propose to pursue, that Liebig* and other physiologists assure us the composition of muscular fibre, so far as chemical analysis can demonstrate it, is identical with that of dried blood; and hence the only difference between them, in the normal state, is that the former does not contain the free gases found in the latter. Now, this very slight difference of chemical composition is sufficient to indicate, upon the principles we have endeavored throughout this essay to elucidate, that there must be a difference of electrical condition, and consequently in accordance with known electrical laws, an attraction between them. Moreover, Matteucci has shown that there is a current of electricity constantly flowing from the muscular fibre to its aponeurotic accompaniments, which must tend to diminish the electricity in it and increase the difference and attraction between it and blood. There cannot therefore be in the properties of electricity, or in the lowness of its tension, as detected by experiment in blood, any insuperable objection to its being considered the force through which the organic functions are made to operate.

Extended to pathology a solution of this interesting question may aid previous investigations, and tend to supercede the conjectural manner in which the nature of many diseases are still viewed. It

*Animal Chemistry, page 69.

may produce an epoch in this department of medical science analogous in importance to that formed in the last age by the discoveries of Bichat and his followers in general anatomy. Like them, it may increase the knowledge of the animal system, enlarge the domains of pathology, and give an impulse to the progress of every department of medicine. Here the parallel may end, for it is not derogating from the value of Bichat's labors to consider that they have accomplished all of importance that they are capable of effecting; and it may be said, that if we wish to give a higher perfection to medical science and advance it to that state which has been in all ages a great object of human toil, an exact science, we cannot look for aid to any other source of knowledge with a larger prospect of success than to the electricity of the blood.

In pursuit of this grand desideratum the modern physician has ceased to repose with the calm content that satisfied his predecessor upon the knowledge of disease in the living system which he may derive from an examination of the dead body. Equally unworthy of a blind reliance does he deem those ingenious but imaginary or improved laws of disease derived from the "existence and circulation of a nervous fluid, the presiding influence of the ganglionic system or the vital attractions and repulsions of the circulating fluids." Even the solid knowledge derived from the study of anatomy and physiology, though absolutely necessary, is alone deemed of little value towards establishing a complete pathology. There is a growing conviction that in addition to these studies, it is indispensable that all the methods to which the exact sciences owe their success should be adopted and energetically pursued if we wish to insure the continual progress of medicine.

As an example of recent improvement in medical science, from which high hopes of utility have been predicated, organic chemistry may be mentioned. Under the influence of this science the changes wrought upon the food by the solvent juices of the mouth and stomach, and the admixture of bile, pancreatic liquor and intestinal secretions; the new combinations which the blood forms by its assimilation and admixture with the chyle; the changes which it enters into in the glands, the membranes, the skin and the various tissues; and the mutual chemical action of this fluid and the atmosphere in respiration,

have been carefully examined. By these labors new lights have been thrown upon physiology; and new views of pathology, having an important bearing upon practical medicine, have been established. Applied to the blood, they have unfolded its composition in health and disease, and have enabled us to form accurate conceptions of the diagnosis, as well as aided us in the treatment of a numerous class of diseases. But the chemistry of the living system, unlike that of our laboratories, furnishes no clue to its modes of operation; we know of its existence only by its results. Hence, in its relation to diseases in general, it has by no means attained the practical value that might otherwise have been expected from it; and able men have spent much time and patient assiduity in pathologico-chemical researches without at all promoting the progress of medical science.

It may be regarded as an axiom of medical science, that there can be no physiological action without its influence over pathological states of the system; and, therefore, in the discovery of the electrical relation of arterial to venous blood, a new field has been opened which may supply all that experience has shown to be wanting in pathology. Modern researches in this science have deduced the clear conclusion that diseases may arise from the action of morbific matter which has been directly introduced into the current of the circulating fluid, and which has affected both its physical and vital properties;* and in the course of this essay, I have adduced sufficient reasons to warrant the probable conclusion that this morbific agent may be, at least sometimes, a redundancy or deficiency of electricity in the blood. But at present the subject is merely inchoate and suggestive; the first consideration to its investigation should be a determination of what is the usual proportion of electricity in the blood; and next, how far, in its free or combined state, it may vary from such a standard; to neither of which has more than an approach been made. All that we know with an approximation to certainty, is that it bears an equal relation to life with organic chemistry, and must exert a similar influence over the changes produced in the various portions of the organism. The relation points to a simple and rational view of the nature of disease, and through an intimate and accurate examination of it the physician

* Carpenter's Physiology: page 535.

may be enabled to find a more simple and certain method of treating the latter than he at present employs, and thus create a prouder destiny for his science.

In the administration of medicines the value of such a discovery may be readily conceived to be important. For slow as the progress of the other departments of medical science has been, and imperfect as their condition still is, there is much on which the physician may congratulate himself, in comparison with the knowledge he possesses of the action of medicines on the human system and their agency in curing disease. The proof of a comparative electricity in the blood asserts a liability to a pathological condition, requiring electro-therapeutical principles to remedy. That medicines act remedially through their electrical attractions and repulsions, has been vaguely guessed at by every recent projector in medical science, and can scarcely be doubted by any one who considers the present state of electro-physiology; and though this is not their only mode of operation, yet it may be presumed that an investigation of their electrical properties will often aid physicians in determining on the propriety of their administration. Modern chemistry has done much by separating the active principles of medicines from the inert matter which encumbers them in their natural state, to facilitate a knowledge of their electrical relations to other bodies, and it is within the range of a probable science that these relations to the blood, and through them to disease, will be accurately determined. When this is accomplished we shall be able to bring to light many obscure operations of medicines, obtain a base on which to construct new and grand principles in therapeutics, and place the power of alleviating human suffering on as secure a foundation as the complicated and peculiar nature of a living being will admit.

To the considerations already presented, recommending this subject to the attention of physicians, there are others which urge them to its examination. Physiology is the basis of all medical improvement, and in precise proportion as our survey of it becomes more accurate and extended, it is rendered more solid. The wonderful structure of the animal system will probably never permit us to look upon it as a merely physical apparatus, yet the demands of science require that the evidently magnified principles of vitality should be reduced to their

natural spheres, or if truth requires, wholly subverted in favor of those more cognizable by the human understanding. The spirit of the age will not tolerate in the devotee to science a quiet indifference, whether a conjectural cause of any of nature's phenomena be true or not, and still less will it sanction a satisfied unconcern as to the source whence the human functions spring. From the physician, as emphatically the student of nature, is expected not only an inquiry into cause, but an investigation of the whole empire of nature and a determination of the applicability of every species of knowledge to the improvement of his art. While the mode of action of a single disease to which his fellow man is liable continues beyond his comprehension, or he is unable to direct its course to a state of health, both his science and art are imperfect, and he is imperatively called upon to look diligently for their improvement. Enlarging his views and extending the boundaries of nature, offer the most obvious and simple means of effecting this improvement. Every accession to a knowledge of the electricity of the animal system opens a new field for his exploration, undoubtedly extends the boundary of nature, makes an approach to the knowledge of vitality, and holds out a promise of an increased ability to manage it for the benefit of his race.

In conclusion I may remark, that if the imperfect inquiry I have been enabled to make shall prove to be founded on ascertained facts, and my deductions shall be found legitimately drawn, a new electro-vital affinity will have been established. That such an affinity exists in the animal system, and that a knowledge of it may be considered to have a practical tendency to advance both the science and art of medicine, I have no doubt, but to what extent I have succeeded in demonstrating the former, or how far my suggestions are correct in regard to the latter, I am not the proper person, nor will it be left to me to say. I am pursuaded that difficulties will have to be overcome, and discoveries will have to be made before its connection with disease will assume a position according with the rigid demands of science; and though I believe I have taken an important step towards the attainment of this object, yet, from the intricacy and delicacy of the subject, and my limited knowledge of experimental electro-physiology, I am not without distrust in regard to the accuracy of some of my conclusions.

ART. VI.—IODIDE OF POTASSIUM IN LEAD POISONING.

BY SAMUEL L. GRIER, M. D.,

OF ADAMS COUNTY, MISS.

I beg leave to report, through the New Orleans Medical and Surgical Journal, a few cases of lead poisoning which recently occurred in my practice, the treatment of which, and the success attending it, may serve to confirm, in some measure, the theory of M. Melsens, in regard to the action of iodide of potassium as an antidote to lead and mercury. The views of M. Melsens, if sustained by practical testimony, will certainly establish a most important principle in toxicology, and must be considered among the most valuable of the many contributions which Chemistry has made to Medical Science.

On the 15th of November last, I was called to the "Anchorage" plantation, where I found a woman suffering violent abdominal pains, accompanied with bilious vomiting. There was no perceptible acceleration of the pulse, but apparently an increased volume of the same. No fever; the tongue natural, and an entire absence of any indications of inflammation of the abdominal viscera. Supposing it to be a case of bilious colic, I made use of venesection; prescribed a mercurial cathartic, combined with an anodyne, and directed a blister to be applied over the seat of pain. At my next visit, after an absence of twenty-four hours, I found the girl somewhat relieved, but was informed there were several other cases in the quarters, who were affected in a similar manner, but with less violent symptoms. I found in all, some eight cases that were suffering the same abdominal pain, with vomiting in almost every instance, and attended in all by a constipated condition of the bowels. This prevalence of the disorder at once suggested the idea of some poisonous substance taken into the stomach, as the common cause; but to find what this could be, was attended with some difficulty, and some time elapsed before the producing cause was discovered. I had the cisterns examined, expecting to find leaden pumps; but no such cause existed, and it was not until my third visit to the plantation that an explanation of the mystery was supplied. Some of the *meal* which the hands were eating was brought to me, in which I discovered particles of lead which had

evidently been triturated with the grain. On further investigation it was found that in repairing the mill some time previously, lead had been used to fix the spindle; this lump of metal, commonly called the "bushing" of the mill, had dropped from its position, and so became subject to the action of the stones in grinding. In examining the patients for further evidences of the effects of lead, I noticed, what I had before overlooked, the blue margin of the gums, more or less distinct in all of them, the number now amounting to twelve or more.

Finding the course of treatment which up to this time had been employed, had in most instances afforded but temporary relief, I determined to try the effect of the iodide of potassium, as recommended by M. Melsens, and which has been presented to the notice of the profession, through the medium of a translation, by most of our medical journals. I made a solution of the iodide, and placed *three* patients under its use, aiming to give about 30 or 40 grains daily in divided doses. Under its administration I observed a gradual but most decided improvement in these three cases, while the other cases under the ordinary palliative treatment, showed a greater tendency to a recurrence of the same symptoms, after a short period of relief. I then began its use with all the negroes under treatment, amounting by this time to more than twenty in number, and about two-thirds of the whole plantation force. In many of them a most marked and immediate improvement followed the change of treatment, and in the course of a few days I had the satisfaction of seeing that all of them were decidedly convalescent.

It is proper to state, however, that in many of these cases, the result was rendered less demonstrative from the fact that other remedial means were used simultaneously with the iodide. These were chiefly blisters to the abdomen, anodynes given internally when the paroxysms of pain were unusually severe, and cathartics with turpentine enemata to overcome the constipation, which was a prominent symptom. Some will perhaps pronounce these to have been the efficient curative agents, but such was not my conviction at the time, and I certainly had no other desire than to see the iodide fairly and impartially tested.

In further support of my opinion of the efficacy of the iodide of

potassium in these cases of saturnine poisoning, I will relate the history of an individual instance among them, in which a somewhat different plan was pursued. The *overseer* was attacked among the last on the place, and about the time the lead was discovered in the meal. His gums had the blue line around the margin very distinctly marked; he had very little vomiting, but entire anorexia, and suffered severe paroxysmal pain. The most obstinate constipation attended his attack, and he applied to me for something to relieve this particular symptom, which continued for several days with only temporary relief, obtained by the most persevering and active measures. His attack being prolonged some time after the negroes were convalescing, and finding at this time that the iodide had inadvertently been omitted in his case, I immediately placed him under the action of the same dose which had been administered to the others. Two days after, as I approached the place, I met him out riding on horseback, and was informed by him that the tendency to constipation was very much lessened, and that all his unpleasant symptoms were gradually disappearing.

I offer this hasty sketch of my trial of iodide of potassium in lead poisoning for what it may be worth. Others may deem it worth an experiment when occasion offers, and thus its value as an antidote will be finally and satisfactorily determined.

These people had been using the poisoned meal for three weeks or more, although some of them noticed it turned black when mixed with water. In no instance did it produce fatal effects, and in none was paralysis of the extremities induced, the ordinary remote effect of saturnine poisoning. In two cases dropsical effusions followed, and one suffered for a time from neuralgia of the chest and limbs. No other sendelæ were observed.

Before closing allow me to call the attention of country practitioners to the manner in which this poisoning was produced. Such an accident may occur at any time on plantations, and would very likely be overlooked until after it had worked extensive mischief. My friend, Dr. C. J. Stone, of Natchez, informs me that a similar instance of lead poisoning came under his observation in an adjoining county, in which very serious and fatal effects were produced before the cause was discovered and removed.

ART. VII—A CASE OF SHOULDER PRESENTATION—EVISCERATION.

BY W. TAYLOR, M. D.

OF TALLADEGA, ALA.

Although not an advocate for meddlesome interference in cases of labor, I beg leave to give the following example of a case in which it was necessary to eviscerate the fœtus in order to bring it away:

On the 18th of January last, I was called to see Sarah, property of E. McGee, Esq., living about five miles from town. She was a stout woman, of black complexion, æt. about 30, and a multipara, in the eighth month of pregnancy. Has had an attack of measles, which has induced premature labor.

Met the case in consultation with Dr. H. P. Graham, who had been with her some hours previously. Found the right arm presenting and protruding from the vulva. Head in the left iliac fossa, with the back looking anteriorly, and lying against the pelvis. We made every effort to bring down the feet, but could not do so, in consequence of the rigid and powerful contractions of the uterus. The doctor told me that the waters had been expelled before he reached the case. Finding it impossible to turn the fœtus (and it being dead,) I removed both arms at the shoulder joint with a common pocket-case bistoury (having no other instrument at hand,) and divided the spine between the second and third dorsal vertebra. It was necessary to use great caution in the operation, in order to avoid cutting the soft parts of the mother. After dividing the spine, I enlarged the incision with my fingers until I was enabled to introduce my hand into the cavity of the thorax, when I removed its contents; and introducing my hand still further, I lacerated the diaphragm and brought away the contents of the abdomen. This being effected, the fœtus was easily flexed upon itself, and was removed without further difficulty. The mother made a good recovery.

No one can deprecate the use of instruments in delivery more than the writer, and he only had recourse to them in this instance as a last resource. No one is more fully aware that the operation of eviscerating the fœtus, should in only very rare instances supersede the practice of turning under transverse presentations. But when he reached

the patient, he found that many hours had elapsed since the rupture of the membranes—the fœtal body was so firmly wedged within the pelvis that the feat of turning was rendered impossible, or would evidently be attended with the most imminent danger.

Under these exigencies, with the aid and advice of the attending physician, with no other instruments than those contained in a common pocket-case, and without any precise rules respecting the operation, we contented ourselves with endeavoring to get the fœtus away, piece-meal, in the best manner we could.

ADDENDUM BY THE EDITOR.

What has been termed spontaneous evolution or version in shoulder presentations, is but a forlorn hope; at least, it is a resource of Nature that cannot be safely relied on in all cases. Dr. William M. Boling, of Montgomery, Ala., in his late able essay on this subject, gives a more favorable view, together with examples from his own practice, from which he concludes that in the South a majority of cases of this kind terminate happily if not interfered with by art. A case where Nature had her own way is subjoined. An excellent lady, the mother of several children, removed with her family to a new remote neighborhood, between the Little Kanahway and Elk rivers, in Virginia. The nearest accoucheur lived fifty miles off. The route passed through a wilderness where no house had yet appeared for twenty-five miles. She was in labor; the arm presented; Nature relied on—from necessity, not choice—finally, when she was dying after two or three days' labor, she requested a friend to cut off the presenting arm of the child. Soon after Mrs. S. expired.

On the other hand Nature is great. In a case of this kind of malposition, having been called by a midwife of New Orleans, at a late period when turning was found impracticable, the lady—a *primipara* —young and vigorous, was bled freely; took an opiate; the natural forces caused the arm slowly to recede; the feet were brought down and the child delivered after a prolonged labor.

ART. VIII.—TWO CASES OF TRANSVERSE PRESENTATION.

BY JAMES S. DAVIS, M. D.,

OF SALEM, MISSISSIPPI.

Case 1st.—On the 4th of October, 1852, I was requested by Dr. R. S. Wily, of this place, to see with him a negro woman, the property of Capt. L———. Upon my arrival I was informed by the Doctor that the case was one of transverse presentation, with the right arm protruding; he further informed me that he and Dr. Ayres, who had also been called in, had made several ineffectual attempts to turn; at their solicitation I also made an attempt at turning, but from the extreme tenderness of the soft parts, as evinced by her piteous cries, I was compelled to abandon the operation, having met with no better success than my predecessors. Our patient had been in labor many hours, during which time the uterine contractions had been steady and severe, and being much fatigued ourselves, and thinking she needed repose, an anodyne was agreed upon and administered; after which it was agreed in consultation that embryotomy afforded her the only chance of delivery. The anodyne was repeated and fomentations ordered to be assiduously applied for the purpose of soothing the swollen and irritated parts. After waiting sufficient time, and being satisfied of the death of the fœtus, I made with Smellie's scissors a transverse incision below the axilla; after which, with a strong pocket-knife, having the blade wrapped up to within an inch of the point, several of the ribs were broken and divided; I then gave way to Dr. A., who with a hook proceeded to remove the thoracic viscera; just as he had drawn the lungs through the incision he remarked that an "evolution" was taking place, and immediately withdrew the hook. In a few minutes, to our surprise and gratification, the head ascended and the nates presented, and the fœtus was delivered without further manual aid. Immediate shampooing was used on the abdomen over the uterus to promote its contractions, and in due time the placenta was delivered. We remained with her until we were satisfied that permanent uterine contractions had taken place, when another anodyne was given, and we left her for the night. On the next day we all visited her again, and found her doing well.

From that time Dr. W. attended her. She made a rapid recovery, and in three weeks left with her master for Western Texas.

Case 2d.—On Sunday, 20th November, 1853, I was summoned to ride about eight miles in the country to see Mrs. M———, a large and robust woman, aged about twenty-five years, in labor with her first child. The messenger informed me that she had been in labor since the preceding Thursday night, during which time she had been attended by Mrs. Sledge, a very kind and intelligent midwife residing in the neighborhood. After having made some interrogatories of the lady and friends, I made a per vaginam examination, and found the os uteri dilated to the size of a five cent piece, and so high up as just to allow me to reach it with the point of the index finger; the lady informed me that she felt very much fatigued from the protracted labor and want of sleep; her pulse was over one hundred, full and strong; skin dry and hot; much thirst and restlessness. I at once abstracted eighteen or twenty ounces of blood and gave her a full dose of morphine, and left her to repose the balance of the night. About 5 o'clock, A. M., I was aroused and told that my services were needed. I found her having strong pains, which had commenced only a short time previously. On examination I found a bag of waters protruding, not of the usual wedge shape, but larger at the presenting part than at the os uteri. I was still unable to make out the presentation, owing to the extreme height of the os tincæl. The weather being very cool, I did not remain by the bedside of the patient all the time. While sitting at the fire I was admonished that the waters had been discharged. Again examining, I found with extreme regret the right arm presenting with the umbilical cord. I immediately made an attempt to turn, but owing to the powerful uterine contractions I was unable to reach the feet. I gave an opiate, waited some time and tried again, but with like success. I then frankly told the friends that an operation would have to be performed, and desired a consultation.

My partner not being at home, Dr. Whittow was sent for, and requested to bring the necessary instruments. When Dr. W. arrived (which was about noon,) he examined the case, and agreed with me that evisceration was necessary. Having satisfied ourselves by want of pulsation in the cord that life was extinct, we commenced the operation by making an intercostal opening under the arm, and having

fractured several ribs and made a crucial incision, we removed the thoracic and abdominal viscera, and then made an effort at delivery; but on account of the large size of the child, we did not succeed. Thinking that amputation of the presenting arm might facilitate the operation, it was taken off at the shoulder joint; but still, with the aid of hooks and other appliances, we could not succeed. Being worn out ourselves, and the patient complaining a great deal, we gave her an anodyne and laid down to rest awhile. It was not long before we were aroused by her cries and importunities. Again examining, we found the child had turned so far over as to present the left arm. We made some efforts by using the arm as a lever to move the head upwards; failing at this, traction was employed, with the hope that the child would double and come away; still our efforts proved abortive. Dr. W. then amputated the left arm. Having made some further incisions, and removing the remaining portions of viscera, the blunt hook was fastened around the spine, and strong efforts made to elevate the head and make the breach present. By using as much tractile force as we thought prudent, we at last, after sixteen hours hard work, doubled the child upon itself and delivered it; but with all our efforts we could not prevent the head from emerging first. The placenta was soon removed. We gave the mother a dose of morphia and placed her comfortably in bed, enjoining the strictest quietude. I visited the patient several times afterwards, and had the proud satisfaction of seeing her recover without the first untoward symptom.

In case 1st, I feel satisfied that spontaneous evolution would have taken place without an operation; in truth, it did occur before the viscera were extracted. The force that was used in removing the lungs, doubtless facilitated its occurrence. But in case 2d, I am assured from the length of time that intervened from the commencement of the operation to the termination of the labor (sixteen hours,) so fortunate a result could not have taken place. The large size of the child forbade our entertaining the least hope of so desirable a result. I should say that whenever evolution takes place, that the fœtus must either be not fully developed, or else it must be putrescent, or there must be on the part of the mother unusual pelvic capacity. And where a contrary state of things exists, delay, that would hazard the life of the mother in the least, would be exceedingly reprehensible.

ART. IX.—TWO CASES OF TETANUS.

The first by Dr. J. U. Ball, *of Louisiana; the second by* Dr. A. Poitevin, *of Mobile—Translated by the Editor from the Revue de Thérapeutique Médico-Chirurgicale.*

Dr. Ball says: On the 10th of October last I was called to see a sick boy at the plantation of Madame Woolfork, Parish of Iberville, La. Upon my arrival I found the boy rolling and twisting himself in every possible shape. Sometimes during the paroxysm he would rest only upon his head and heels, whilst his body would be raised into the shape of an arch. In a few instances the body would be bent forward so that the head and knees were in contact, and the patient rolled together like a ball; after which the patient would be bent to the side, &c. In this condition I found the patient; unable to articulate; eyes set, and writhing under pain of the most excruciating type. I inquired closely into the history of the case, when I was informed that the boy had been perfectly healthy up to the time of his present illness. Early in the morning I was informed that the patient complained of some slight headache; ate no breakfast, but did not feel sick enough to keep from work. He accordingly went to carting wood, and about 1 o'clock, P. M., he was found lying by the feet of his mules, rolling and twisting as above described. The manner in which the boy was found entangled with his mules, led me at once to suppose that he had received some local injury which gave rise to traumatic tetanus. I therefore examined the body minutely, without being able to discover any marks or bruises. I decided the case to be idiopathic tetanus, and treated him in the following manner: I administered chloroform in the outset to relax his system and relieve the intense pain under which he was at the time laboring. After the effects of the chloroform had passed off the exacerbation immediately returned. The patient being of a plethoric habit, I bled him freely, which was attended with a happy result. My next visit found the patient in great distress, bowels loose. I ordered the following: ℞. Ol. Ricini; Ol. Terebinthina, aa ℥ i. M.; to be given at a draught. Upon my next visit I found that the patient had passed a

great deal of indurated fæces; spasms somewhat lighter; interval longer. I continued the use of the chloroform. My next visit found him in great pain with spasm. I applied a blister along the course of his spine, and permitted him to drink freely of brandy, with sixty drops of laudanum three times a day, which was continued until the tenth day of his illness, when the patient was able to pursue his usual avocation.

Dr. POITEVIN says: During the two years in which I practiced medicine in Pensacola (Western Florida,) I have been called on to treat four cases of Traumatic Tetanus by the remedies usually employed: such as opium, mercurial frictions, cold and hot affusions, anti-spasmodics, etc. My four patients died.

A short time ago an English sailor, George Smith, a strong man, of a very sanguine temperament, had me called in. In working on board of his ship, he had through carelessness walked on the blade of an open knife which a companion had let fall; it cut the lower part of the big toe at its articulation with the metatarsus; the incision, which had little depth, might have been about half an inch long. At first he paid no attention to this slight cut, but continued at work, always taking care however to wash the wound twice a day to cleanse it, covering it afterwards with a piece of linen. Six days after this accident Smith sent for me; I found him with a strong trismus, which obliged me to use a knife as a lever to open his teeth sufficiently to enable him to drink. His body was as stiff as an iron bar, a spasmodic shudder threatened to suffocate him; the patient, who was perfectly conscious, showed me by a look that he suffered everywhere. His skin was hot, his face injected; a low plaintive cry succeeded each muscular shock. In this strongly characterized case of tetanus, anticipating as morally certain that Smith could not live in this state if I had recourse to the same remedies which failed in four similar cases, I prescribed accordingly 6 grains (30 centigram.) of tartar emetic (tartre stibié) in 180 grammes of liquid, with 40 drops of laudanum. The patient was to take a large spoonful every second hour. I came back seven hours after; no vomiting; two stools; skin

moist; same general state; I ordered a continuance of the potion. Twelve hours elapsed after my second visit, when I found my patient bathed in perspiration; I never saw so much in my life; no vomiting; two more stools. Smith spoke with a good deal of facility; the muscles of the jaw were hardly contracted at all; the patient sat up; his thighs were bent; the spasmodic shock had ceased four hours ago.

I stopped the potion, ordered soup and a little wine. The fourth day of his illness George Smith went on board of his ship entirely cured.

I shall certainly have recourse to the emetic on the first occasion. Tetanus is very common in this country; the heat is very excessive during the summer; the negroes, and a great number of white laborers are imprudent enough to walk barefoot, and sometimes get a nail in the sole of the foot, sometimes splinters of wood. The number of deaths each year from tetanus is truly frightful. We should, it appears to me, think more of the use of emetics; a more even temperature, as we have seen, is established immediately, and they produce relaxation of the muscular rigidity. I will add, that when the foot is wounded, it is imprudent to wet the wound. Those who abstain from it are rarely attacked by lockjaw.

Mobile, Aug. 20th, 1853.

ART. X.—A CASE OF TETANUS, WITH PATHOLOGICAL REMARKS.

BY B. DOWLER, M. D.

[*Supplementary to the preceding article.*]

Unsuccessful cases are seldom reported. I select one, because the treatment adopted was similar to that of Dr. Poitevin in the preceding article, and though the result was unfortunate, it is for this reason now submitted to the reader.

1848, August 2d, 8 A. M. Hellemenn, living on the river bank, in McDonogh, opposite New Orleans, resident seventeen years, aged nineteen; stout, muscular and of good constitution, was wounded with a splinter in the sole of the foot, at the anterior articulation of the second metatarsal bone. A portion of the splinter was extracted by himself without delay. He continued his avocations as usual for a

week, when finding that his foot was becoming very painful, he re-opened the wound, and, as he said, extracted another splinter an inch long. The following day, having been attacked with rigidity in his jaws, neck and back, he sent for me. The foot was free from swelling. The wound, which was small and open, was enlarged; no splinters were found; a slight trace of pus was noticed; the wound, now excessively painful, bled freely, and the bleeding was encouraged by the immersion of the foot in warm water.

His jaws were rigidly closed upon two wooden wedges half as thick as the finger, which his friends had with difficulty inserted between his teeth. The muscles of his neck, back and loins rigidly contracted, gave the body that backward bending called opisthotonos; shoulders retracted; stricture and pain in the chest, which increased upon attempting to make a deep inspiration; respiration very imperfect—twenty-four in a minute; occasional, not constant; rigidity in the limb injured—the left; believes he cannot swallow; dense mucosity in the mouth, which he discharges with difficulty; pulse regular—80; skin moist; axilla $100\frac{3}{4}^{\circ}$; eyes natural; lids incline to close; rather costive; urinates; intelligence unimpaired; temper irritable.

During forty minutes he reluctantly and imperfectly inhaled from a sponge two ounces of ether; imperfectly, because the stricture, rigidity and immobility of his chest and diaphragm restricted his breathing to such a degree that probably but little ether entered the lungs; the rigidity relaxed for a time; the wedges full loose; the mouth could be opened an inch or more. Advantage was taken of this to give a dose of castor oil, which, after many painful efforts, he swallowed; cups to the nape; frictions of sweet oil and mustard; flaxseed poultices to the foot; sulphate of morphia to be given after the operation of the oil.

Evening: 5 to $6\frac{1}{2}$ o'clock. Symptoms worse; opisthotonos extremely severe, curving the body violently backwards, being in paroxysms every two to four minutes; the muscles of the back and loins strongly contracted, having a wood-like hardness; jaws rigidly closed; senses regular; two defecations; the temperature of the skin natural; pulse small, variable—100; gave six or seven grains of tartar emetic in an ounce of water; but little water was used, and all was given at once, because of his difficulty and dread of swallowing; virtually he

had hydrophobic symptoms. Afterwards toast water was offered, but after many attempts, only one or two ounces were swallowed. Hearing from me that the medicine might vomit him, he became greatly alarmed, asserting that it was impossible for him to vomit without suffocating to death. His fears were nearly realized, for vomiting coming on, he appeared to be temporarily asphyxiated; face livid; pulse thready, irregular and quick; the lips and teeth closed so much that it was with difficulty that the mucosities and fluids ejected from the throat and stomach passed out at all. The paroxysms of strangulation having passed, in about one hour, great but temporary improvement took place; the paroxysms of muscular contraction abated in frequency and force; the pulse became regular; the external veins became fuller; copious sweats and some sleep followed. During the night he became much worse; refused to take morphia or other medicine; great dread of the pain in drinking.

August 3d, 6 A. M. Died, after a struggle of forty-eight hours. Two hours and a half after death I found the body laid out as usual, and free from the rigor mortis, being supple and preternaturally hot, though having forgot to carry my thermometer with me, I could not test this fact instrumentally. The absence of the death-stiffness, a significant fact, will not be now dwelt on; nor will any pathological investigation of this formidable disease be attempted in this place.

The spinal physiologists and pathologists, who, though unable to find morbid alterations in the spinal cord, which they regard as the exclusive seat of tetanus, seek to establish their theory by the abundance and steadfastness of their faith, ignoring the muscular system altogether. Whether tetanus be regarded as a purely dynamic or a purely structural disease, the obvious phenomena, particularly before death, display themselves prominently in the muscles. To ascribe the local knottings or muscular contractions called cramps to the cord, exclusively, where no changes can be seen, felt, or in any way be detected, is to reject facts for the sake of theory, and is no better a system of philosophy than that, now too prevalent, which teaches that there are four kinds of conducting nerves, each having a separate office; two for the brain, the sensational and volitional; two for the cord, the excitor and the motor—all being mere conductors to the centre; all of which are assumed; none of which can be shown as anatomical

facts, either to the naked eye or by the miscroscope! Yet they are spoken of as anatomical facts, arcs, tracts and conductors—drawn on the blackboard; engraved in maps and charts! When even normal anatomy is not based on physical materiality, pathological anatomy is not amenable to materializing tests.

Rhetoricians maintain that obscurity, "thick clouds and dark," "the moon's doubtful and malignant light," favor sublimity. This Æsthetic canon is not adapted to physiology, pathology, normal or morbid anatomy. It is not, however, wonderful that gentlemen who get all these from the imagination, should take from the same fruitful realm hydrophobia, epilepsy, spasms, cramps, tetanus and the like, as the types of the physiology, the diseases and pathological anatomy of the spinal marrow and what they call the true spinal system.

The pathological tenebrosity which envelops hydrophobia, epilepsy, spasms, wry-neck, cramps, tetanus and the like, makes these diseases the worst possible types for founding a new system; and yet they are taken as the standards of the spinal system, wherein no characteristic changes in the nerves have been ascertained; while the muscular system, in which many changes are known to take place, at least during the progress of these convulsive maladies, is ignored! This method of forming a system, whether it be based on function or structure, is the reverse of that adopted by naturalists, who seek not the obscure but the well known typical forms which characterize a group, family, class or genus, and which most truly represent the whole, and yet, at the same time, each of the species. Thus the hard-shelled, four-winged June-bug, beetle, or scarabæus, is the fundamental type of more than twenty-thousand known species in the class of insects called the Coleoptera; the common house-fly, or the mosquito, is the type of the Diptera, or two-winged class. Pathology has its types; fever has its intermittent, remittent and continued types; so has inflammation, the skin-diseases, &c.; pathological anatomy has its types, as cohesion, color, size, &c.

Spinal pathologists have, except in rare cases, signally failed in showing anatomical changes of the cord in tetanus. Hence, it is the worst possible type for founding a durable system. For whether tetanus be regarded as a purely dynamic or a purely structural disease, the muscular system seems to perform the principal, if not the perma-

nent rôle. That the spinal cord is the sole seat of convulsive diseases may be true, but the evidence to prove this weighty postulate has not been adduced.

Mr. Miller, in his excellent work on the Principles of Surgery, says of traumatic tetanus, "it is in the nerves of the part that inflammatory change is to be looked for, not in the spinal cord."

Baron Dupuytren, who, after treating a patient for tetanus unsuccessfully, begins his account of the post mortem examination thus: "Great rigidity of all those parts of the body which had not been affected by tetanus; but the muscles of the neck, shoulders, and in general all those regions in which the tetanic rigidity had existed, were completely relaxed."—(Clin. Lect.) The muscles have been sometimes found ruptured.

Sir A. Cooper says: "Nerves are very rarely inflamed. Wounds of the nerves, though dreadfully painful at the moment, are followed by little irritation. * * * The spasmodic and tetanic symptoms which follow punctured wounds, are the effects of injury to tendinous (and muscular) rather than nervous parts." He frequently cut out portions of large and important nerves without any marked constitutional irritation, or other unpleasant symptoms. "These instances," he remarks, "to which many more might be added, as well as the usual seat of the wound which produces tetanus, leads me to believe that it is rather the result of injury to tendinous than to nervous structures."—(Lect. 1, 245, iii. 125.)

Physical or anatomical alteration of the substance of the spinal cord would be more likely to cause palsy than tetanus; a loss rather than an increase of muscular tension. The rapidity with which tetanus sometimes runs its course, can be better explained as a muscular disease than as an alteration in the tissue of the cord. A preternatural development of the muscular force would, as analogy shows, cause death from exhaustion. A race horse from over-exertion; a wild bird from attempts to get through a glass window, sometimes die instantly. Besides this exhaustion of the muscular force, incidental to the most violent form of tetanus, the rigidity of the muscles would arrest or obstruct the circulation of the blood by compressing the vessels, and might arrest the action of the heart and diaphragm. The London Cyclopædia, of 1850, says that the late Professor Robison, of Edin-

burgh, had a negro patient who had scratched his thumb with a china plate, and died in a quarter of an hour afterward of tetanus.

Those physiologists who assign an host of dissimilar diseases (as epilepsy and tetanus) to the spinal cord as their primary and exclusive seat, regard strychnine as a fundamental therapeutic test of their postulates. It must be confessed that the action of this alkaloid of the nux vomica on the muscular system, approximates tetanus. But this in no degree favors the truth of the aforesaid theory, but goes indeed against it; for there is much more reason to think that as the muscles are richer in blood vessels than the cord, the strychnine would reach the former first, and most extensively, by means of the blood, and communicate to them the first shock tetanic. The blood, the muscle and the cord all take parts in the pathological drama.

ART. XI.—IMMOBILITY OF THE LOWER JAW.

BY J. J. MCELRATH, M. D.,

OF CAMDEN COUNTY, ARKANSAS.

Functional derangements of the "inferior maxillary bone" are occasionally observed as the result of structural changes of the soft parts within the mouth, in consequence of violent mercurial action. It may indeed be affirmed without exaggeration, that "immobility* of the lower jaw," whether partial or complete, is more frequently attributable to this than to any other cause, or, perhaps, than to all other causes combined. The present conservative mode, however, of using that powerful but valuable remedy, renders the occurrence of such accidents much less frequent now than was the case a dozen or twenty years ago. The only three cases that have come under my observation, in which surgical interference appeared to be imperiously demanded, were of several years standing. In two of these the affection had existed for more than twenty years; the immobility, however,

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* This word is not used in its most restricted sense generally in surgical nomenclature, but is, by common consent, made applicable, without qualification, to partial as well as complete loss of motion in different parts of the body.

was not entirely complete; yet the remaining action was so limited, that but little assistance was derived from this organ (lower jaw) in the masticating process. No treatment, so far as I know, has ever been instituted for the relief of either of them.

The subject of the present communication, Miss Margaret Watkins, aged twenty years, had mercury (calomel) administered to her for the cure of an intermittent fever, in the fall of 1846—then in her thirteenth year. The history of her case at that time, as detailed to me by her father, Col. Jacob Watkins, was simply this. To use his own language: She was badly salivated, but not worse than he had seen other cases. Under the use of stimulating, astringent, and other detergent washes, gargles, &c., the disease (salivation) gradually subsided, with the exception of the sores (ulcers) on the gums and contiguous lining of the cheek on the left side. These parts presented several large superficial ulcers (deep no doubt for this locality,) which did not seem to be benefitted by any of the applications that were used, until at the expiration of about six weeks, when a broad, thin, partially detached lamella of bone made its appearance, and was removed—(from the inferior maxillary); after which, the healing process advanced rapidly, so rapidly, indeed, that on the morning of the day following that on which this "osseous lamella" came away, the patient could but barely separate the jaws sufficiently to take nourishment; the effort causing considerable pain, too, she was permitted to use her pleasure. No further attention being given to the treatment at the time, supposing, as did the attending physician, that this latter symptom (pain) was the cause of the apparent inability to use the jaw. At the expiration of a very few days, however, the stiffness was found to have increased to such a degree as rendered the bone immovable. Some exertions were now made to overcome the difficulty, but no amount of force the medical attendant felt himself justified in using, was sufficient to produce the slightest effect. Similar attempts were repeated at intervals, and continued for several weeks with no better success, when they were finally abandoned as useless. In this unfortunate condition has she remained ever since—up to the month of October last—subsisting upon fluids, with only such solid food as she was enabled to introduce through a small space or opening between the superior and inferior bicuspids of the sound side (right side.)

I have been thus particular in reciting the previous history of this case, as indicating clearly by what simple means such accidents may

be avoided. Their occurrence, in fact, can result from nothing short of culpable neglect, or what is worse, culpable ignorance. As will be seen in the subsequent treatment, pledgets of lint carefully introduced between the contiguous diseased surfaces, and constantly retained there, or frequently replaced for a few days, together with moderate exercise of the parts during the day, and a forced open position of the jaws (if necessary) whilst sleeping, will be altogether sufficient to prevent adhesions, or excessive contractions, and, consequently, any material impairment of function in this organ (lower jaw) from such cause. This preventive treatment must be adopted immediately upon the slightest difficulty being experienced in extending the jaw, or in giving to it its proper healthy action; more especially should such a difficulty occur during the existence of a considerable degree of ulceration, or soon after some irritating substance has been removed or come away, as "spicula or lamella of bone, decayed teeth, sloughs," &c. The process of adhesion in these highly vascular structures is, when not interfered with by some disturbing cause, a very rapid one, requiring at most but a few days, and sometimes even but a few hours for the contiguous diseased surfaces to become so firmly agglutinated together as to resist the action of the depressor muscles of jaw. Should such accident, however, take place, and the bone become, either partially or completely, immovable before the attention has been directed to it—(I have known this to occur in a single night more than once in my own practice)—then, provided not more than twenty to thirty hours have transpired, a moderate degree of force properly applied upon the chin, the head being fixed, will be sufficient to break up and overcome the incipient adhesions and contractions, and restore to the jaw its normal action; when the above treatment persevered in will prevent their recurrence.

Although functional injuries of the lower jaw from mercurial action is a rare occurrence at the present day, yet it is, nevertheless, sufficiently frequent to justify our best directed efforts to prevent it, more especially when we reflect upon the fact, that a large majority at least of those who have been thus unfortunate, are suffered to remain so during life; as well from an unwillingness on the part of themselves to submit to the action of cutting instruments, or other painful operations for relief, as from a disinclination with most physicians (out of cities) to undertake any treatment in such cases beyond temporizing means.

As respects the appearance and condition of Miss Watkins at the time of the operation, it is somewhat remarkable, that notwithstanding she had been deprived the power of masticating, and forced to subsist, in a great measure, upon fluids, there was no apparent lack of physical development, nor was there any very marked deformity. The mouth was slightly retracted towards the affected side (left side,) which cheek was somewhat more prominent than the opposite one. When the lips were separated so as to display the teeth, the superior incisors presented an unnatural appearance—they having grown to an unusual length, projecting over, and reaching down in front of their inferior antagonists. The chin was also slightly retracted; indeed, the inferior maxillary in its whole contour, was somewhat imperfectly developed; yet when the features were composed, these irregularities were scarcely observed, unless the attention was specially directed to them. Excepting a barely perceptible lateral motion, the jaw was perfectly immovable—such force as I was able to apply upon the chin, producing no visible separation of the teeth; nor was such attempt, although frequently repeated, attended with the least pain. Immediately within the angle of the mouth, on the left side, was a firm, unyielding band of inodular tissue (cicatrix) embracing the gums above and below, and fastening the jaws firmly together. In attempting to explore the parts beyond this adventitious formation by means of a common probe, it was found impossible to pass this instrument more than a few lines along the surface of the teeth, so closely were they invested by the contracting tissues. By a careful examination and comparison of the opposite cheek, this hard, unyielding substance could be distinctly felt through the superimposed parts, extending back to near the anterior border of coronoid process. The right cheek was entirely free from morbid adhesions or contractions. The molar teeth on this side, however, were in a manner dove-tailed together, the depressions on the grinding surface of the superior set being occupied or filled up by corresponding elevations on those of the inferior set, and vice versa. So complete was this adaptation of their antagonizing surfaces that nothing could be passed between them, not even the point of the thinest spatula, or smallest probe. The lateral motion of the jaw being only sufficient to allow the sliding of these teeth upon each other through a mere fraction of their width.

The operation, which was performed on the 17th day of October

1853, and its results may be summed up in a few words. The patient being seated, the head resting against and supported upon the back of a chair prepared for the occasion, and the lips, near their left commissure, being separated and drawn outwards by an assistant; a straight, narrow pointed bistoury was forced, flatwise and with the edge downwards, along the external surface of the teeth, opposite their junction, to the extent of something more than an inch beyond the anterior stricture, and then with a thrusting or sawing motion carried downwards, parallel with and sufficiently near the bone to avoid wounding the facial artery, at the same time not so near as to divest this organ of its proper fibrous covering, dividing the tissues completely down to the base of the alveolar process. The edge of the knife being now reversed, the parts above were divided in the manner and to about the same extent. Blood was given out freely from a number of wounded arterial branches, and some little time was required to arrest the flow, which being accomplished, the finger was passed into the space thus gained, when the same unyielding substance was found still beyond, closely investing the teeth, occupying and filling up even the vertical spaces that would otherwise have existed between them in consequence of their imperfect development. With a little force the liberated parts were held sufficiently open to allow the dissection to be completed; which was here effected, both below and above, (regardless of Steno's Duct) by repeated strokes of a scalpel, and was continued back to the posterior surface of the last molar teeth. This latter part of the operation occupied much time, and was exceedingly painful; so great was the pain that it was with much difficulty I could induce the patient to suffer me to proceed. At almost every stroke of the knife it was necessary to constrict the bleeding vessels and sponge out the blood, in order to remove the temporary obscurity of the parts which its presence produced. These abnormal tissues had in many places acquired an almost cartilaginous hardness, rendering their division both difficult and tedious, retarding too, in some degree, no doubt, by their inelastic character, the closure of the divided vessels.

The separation having been thus completed, the fastenings and connecting bridles all divided, and the cheek freed from its attachments to the gums, an effort was made to depress the bone, by means of

force directed upon the chin, but to my great disappointment, as well as that of the patient and her friends, the jaw was as completely immovable as before the operation. These attempts being persevered in sufficiently to prove their total inadequacy, an opening was next made by forcing out the first inferior bicuspid tooth, and through this a strong piece of tape was passed and brought out between the antagonist bicuspids on the opposite side; upon the ends of this, carried below the chin, as much force was exerted as I could impart with the whole strength of my arms; which proved as ineffectual, however, as did the previous attempts upon the chin. Under these discouraging circumstances, having no mechanical apparatus, by which a greater degree of force might be applied, except a small, badly constructed screw dilator, and this unavailable for the want of space or surface to act upon, I could think of no means to effect a liberation of the jaw short of bisecting the masseter muscle. And even this expedient, after a careful examination of the muscle, with the finger introduced behind it, seemed to be unecessary, as the remaining rigidity was not sufficient to resist the force that had been used. Such was the anxiety of the patient, however, to be relieved, that she readily consented to any further operation that was thought advisable, but insisted upon the selection of a subsequent day for the proceedings; to this I saw no objection.

To prevent a renewal of the adhesions and contractions, pledgets of lint were introduced, so as to separate completely the raw surfaces. This dressing was renewed once every day, or oftener, as the convenience or comfort of the patient required. Smart reaction, with considerable swelling of the cheek, took place, but gradually subsided without treatment, in a few days. When the parts were examined on the 22d, (five days after the operation) the adventitious formation which had been separated from its connections with the gums above and below, and left as an irregularly circumscribed solid cicatrix, embracing the whole internal face of the cheek, appeared to have lost its vitality, and was becoming detached from the healthy structure beneath and sloughing away. No part of the molars were visible at this time, except that portion of their external surface which had been denuded by the knife; fleshy connections were observed extending from the superior to the inferior gums, and embracing the

last named teeth on their posterior and anterior aspects. Pressure upon the chin was again applied without benefit, the mouth being too painful to admit of further dissections. Tepid water ablutions were directed to be frequently used, and the lint dressings (spread with simple ointment) constantly renewed. During the succeeding two weeks the mouth had the appearance and fœtor of a severe mercurial ptyalism, affecting as well the sound as the diseased side, and extending even to the submaxillary and sublingual glands. In the meantime the whole of the abnormal tissues had come away, leaving the substance of the cheek on this side (left side) as soft and elastic as that of the sound side. By a very careful examination on the 29th, I was unable to detect any cause sufficient to prevent the jaws from being separated, unless possibly I was mistaken in my diagnosis, as to the condition of the articulation, and was preparing to make another effort by means of force directed upon the chin, when to my agreeable surprise, with but little pressure, the teeth separated, not freely nor widely, but sufficiently to admit of the easy protrusion of the tongue. Attempts to increase the separation, however, could not be borne, so great was the pain thereby induced in the neighborhood of the joints, and was therefore desisted from. A weak solution of zinci sulphas was now directed to be used once or twice every day until the pseudo-ptyalism should subside; the patient made to exercise the jaw in masticating as much as possible; the lint dressings to be retained, and whilst sleeping the jaws to be kept asunder with a little force, by a simple contrivance of wood, interposed between the ranges of teeth on either side, and secured by means of a bandage carried over the vertex. This treatment was persisted in without intermission until the 22d of November; at which time the bone had acquired about two-thirds of its natural action in the vertical direction, with a somewhat greater improvement in the action of the pterygoid muscles. That portion of the new surfaces which had been formed by the action of the knife upon the inferior gums and contiguous lining of the cheek, was still uncicatrized, having a granular appearance, and showing a disposition in many places to ulcerate. Fearing that the dressings might perhaps be the cause of this tardy recuperation, these were now discontinued, and a strong solution of zinci sulphas frequently applied; so great however was the tendency to a reproduction of the adhesions

in the absence of an interposing substance, that on the succeeding morning, the patient having neglected to apply the dilating apparatus as usual when retiring to bed the previous evening, the jaws were completely locked, as the lady herself expressed it, and could not be separated without assistance. Similar accidents occurred on two subsequent occasions, and from the same kind of neglect; yet, by timely interference and a reapplication and continuance of the dressings, no material obstruction to the progress of cure was caused by them. Upon a further and more careful examination some days afterwards, portions of the alveoler process were detected in a carious condition, and in order to facilitate the necrosis and separation of this source of irritation, it was found necessary to remove the remaining inferior bicuspid and molar teeth on this side; this however was no great misfortune, as their imperfect condition rendered their preservation, even for a few years, impossible. After the extraction of the teeth, the necrosed fragments of bone were soon spontaneously thrown off, and a few applications of the solid nitras argenti speedily effected cicatrization.

The tendency to a renewal of the contractions was strongly manifested for some weeks after the entire disappearance of all visible disease, rendering the dilating apparatus constantly necessary, and occasionally even the knife was required to destroy some newly formed sub-mucous bridle or bridles. It was not until the last of December, that all treatment could be safely dispensed with.

No further liberation of the jaw was gained after the 22d November, and the whole and only effect of the treatment after that time, was in securing permanently the advantages then obtained.

Would the division of the masseter muscle have been, or would it even now be sufficient to effect the complete liberation of the bone? Or, is it not more probable that the long-continued inaction of these muscles, at a time of life, too, when the physical system is being rapidly developed, has rendered them, as well as the temporals, permanently deficient (atrophied,) so that to effect anything by this means, it would be necessary to bisect the right as well as the left muscle?

In reflecting upon the various steps pursued in the treatment of this case, although entirely satisfactory, in the result at least, to the patient and her friends, it is not difficult to point out many errors, the avoid-

ance of which would, doubtless, have very much abbreviated the treatment, if not have secured a more perfect liberation of the jaw. In view of all the difficulties encountered in this, were a similar case to present itself for treatment, I should deem it best—whether the separation of the soft parts were accomplished in the same manner or by substituting for the latter (after) part of the dissections, horizontal sub-cutaneous division of the masseter muscle—to effect at once, and at any reasonable hazard, the separation of the jaws to as near the full extent as possible, which might be done after the removal of the teeth, if necessary, to procure surface to act upon, by means of some properly constructed dilating instrumen (perhaps a modification of the screw and lever dilator used by Professor Mott for a like purpose could be made efficient,) and the inhalation of chloroform when not contra-indicated, to prevent the sufferings.

ART. XII—CASE OF VESICO-VAGINAL FISTULA—CURE.

BY N. BOZEMAN, M. D.,

OF MONTGOMERY, ALA.

Emily, aged about 30, property of H. W. B. Price, of Daleville, was admitted into my Infirmary on the 26th of December last. She is large and stout; the mother of eight children; and had always enjoyed good health until the birth of her last one. This was in August, 1852; at which time she became the subject of the above disease. Labor lasted about sixty hours; craniotomy was then resorted to by the attending physician, and delivery speedily effected. A few days afterwards it was discovered that urine passed through the vagina—a circumstance first indicating the existence of vesico-vaginal fistula. In addition to this injury she had an attack of rheumatism, and from both of which she did not recover entirely for several months. Since then, however, her general health has been very good, menstruation regular, &c.

Upon examination of the parts, I found them as follows: uterus retroverted and slightly prolapsed; vagina quite capacious, but very much altered in appearance by the abnormal position of the womb; its anterior wall only two and a half inches, while its posterior seemed to be of the usual length. Running vertically through the anterior and a portion of the posterior lip of the os uteri, was to be seen a cleft an inch or more in length. At the anterior extremity of this cleft was the fistulous opening, partially filled by a herniated condition of the mucous coat of the bladder. It was triangular in shape; its base resting against the neck of the womb, and its apex presenting forward and sufficiently large to admit the index finger. The consequences of such a disease are too well known to require a notice in this short report.

Having satisfied myself as to the nature of the case, my attention was next directed to the mode I should pursue in an operation. Fully appreciating the difficulties which I had so often seen my friend, Dr. Sims,* encounter in similar cases, I of course had many misgivings as to the result of any proceeding. I determined, however, to make an effort.

Accordingly on the 11th of March, assisted by several of my medical friends, I proceeded to operate in the following manner: The patient was placed upon a suitable table on her knees and elbows. The parts were now brought into view by the lever speculum and a reflected sun-light. The edges of the fistula, formed partly by the anterior lip of the os uteri, were then freshened by means of a delicate tenaculum and small scalpel. This being done, the next, and what I considered the most difficult step was to be taken, namely: applying the sutures and clamps in such a way as to effect perfect apposition, and thereby obtain union by the first intention. Here I had the neck of the womb, an unyielding tissue, to deal with; and

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* J. Marion Sims, M. D., late of Montgomery, Ala., now of New York, of whom Dr. Bozeman makes such honorable mention in the above interesting case, is at home and abroad now justly regarded as the highest authority on this difficult branch of surgical practice. Dr. Sims himself says that "all we know upon this subject worth knowing, is due to America and France"—the two names which stand out in the boldest relief, being Professor Mettauer, of Virginia, and the indefatigable Jobert, of Paris. Dr. Sims attributes the first successful operation for Vesico-vaginal Fistula in this country to Professor Haywood, of Boston, who has operated with entire success nine times. Professor Pancoast, of Philadelphia, has operated several times with like results.—Editor.

to remedy this, it was necessary that the compensating parts should be so adjusted that the mechanical contrivance employed would not defeat the object sought to be obtained, by creating an undue amount of inflammation. From the nature of the parts, therefore, I determined to place the clamps longitudinally binding their upper extremities so as to be applied one on each side of the anterior lip of the os uteri, thus enabling me to secure the upper suture in the most advantageous way. The clamps were one inch in length, and contained four openings for the sutures. These were of silver wire, and were lodged one after another in their respective places, by being attached to the end of a silk thread carried through at a proper distance from the edges of the fistula, by a short, straight and spear-pointed needle. A clamp was now secured to the distal extremities of the wire and made to occupy its place. On the proximal ends the other clamp was passed down to its place. Traction then being made upon these ends of the wire, perfect apposition of the denuded edges was effected. This was now maintained by compressing with a pair of strong forceps a small, perforated shot previously slid down upon each wire to the clamp. After this, the wire was cut off close to the shot, and the patient put to bed. The self-retaining catheter was next introduced into the bladder and there allowed to remain, excepting when its removal became necessary to clear the mucous and earthy deposit from it. This was required twice a day. During the whole treatment the patient was kept upon her back, and the bowels prevented from acting by the free use of opiates. On the fifteenth day I removed the suture apparatus, and was most happy to find union of the parts perfect. The patient now has entire control over her urine, and says she feels as well in this respect as she ever did.

Whether the fistula in this case was caused by the use of instruments in effecting delivery, or by the sloughing process, I cannot positively say, but am inclined to the former belief. However produced, I think it affords several points of interest; showing in the first place to be a part of the same wound in the cervix uteri as indicated by the cleft or line of cicatrization; and in the second place, beautifully illustrates the extent of Nature's efforts to repair such injuries, as well as some of the valuable resources employed by art to overcome such obstacles as she herself fails to do.

I take great pleasure in saying, that the success I have had in this case, I attribute entirely to the advantages derived from those principles of treatment laid down by Dr. Sims. Every surgeon who has or may make an application of them in the treatment of such cases as the above, must feel a sense of pride in awarding to him the praise he so justly deserves.

April 7th, 1854.

Part Second.

EXCERPTA.

The *Excerpta department* of a Medical Journal is one of peculiar difficulty in reference to the expectations and wishes of different classes of subscribers; some of whom, taking no other Journal but this, would prefer to have this department so expanded as to contain a summary of the progress of Medicine, copious extracts, and the most interesting memoirs emanating from every point of the horizon; while others (a large class) who receive several Medical Journals, look with little favor, if not aversion, at articles copied from Journal to Journal which they are compelled to buy three or four times. A compromise between these extremes seems to be the only alternative, although the greater the amount of *Excerpta* the smaller is the editorial labor in conducting a periodical, seeing that a perusal of the Journals and a few marginal strokes of the pen are all that is necessary to guide the printer and make up a certain number of pages, and thus afford an Editor the pleasures of idleness. But even the *Excerpta* may afford the lover of work, work, if not distinction. "There is but one method," says Sidney Smith, "and that is labor; and a man that is not willing to pay that price for distinction had better at once dedicate himself to the pursuits of the fox. There are many modes of being frivolous; there is but one mode of being intellectually great."

Art. I.—*Dr.* John Davey's *views of the Nervous System.—Uses of the Ganglionic System.*

Dr. Davey read a paper on this subject before the Medical Society of London; the principal object of which was to prove the independency of the organic nervous system, and, what is more, the dependency of the integrity of the cerebro-spinal system, in common with all the organism, on it. To prove his position, Dr. Davey brought forward a variety of facts, more or less startling, and these selected with much apparent care, seemed to tell much in favor of the physiological views insisted on. After some preliminary remarks, intended to show the unsatisfactory and contradictory opinions expressed by our most popular writers on medicine (physiology,) viz: Wagner, Todd, Bowman, Carpenter and others, concerning the ganglionic system, he affirmed on the authority of many good names, that the ganglionic system of the sympathetic nerve are those parts first formed in the fœtus, and that this same fact obtains equally, it was premised, through the whole animal kingdom. The early organism of birds was referred to in confirmation of that opinion, which assigns to the solar ganglion and its dependencies an existence anterior to any other part of the animal fabric. Especial reference was made to the two monstrosities recorded by Mr. Lawrence and Dr. Marshall Hall. The first of these it is known was born without a brain, but with the spinal cord complete; but the second, still more wonderful, was born without either a brain or spinal cord.

Dr. Davey argued, that if in the latter instance the functions of secretion, absorption, &c., were duly and efficiently performed without any aid from the cerebro-spinal system, then was this latter in no instance either requisite or necessary in any way to the integrity of such functions in the animal economy. The ganglionic nervous system, said he, is perfect at birth, and its functions are also perfect. This is completely organized, whilst the brain is nothing more than a mere pulpy mass, without any kind of function or use to the individual in possession of the same; the one is in active and unceasing operation, the other is but a blank, doing nothing, useless; the ganglionic nervous system executes its functions instinctively, whereas the brain, if not the spinal cord, requires time and experience, and direction, ere it performs its functions, either for good or evil. Dr. Davey compared the monstrosity of Dr. Hall, organically considered, to the lower classes of animal life, the medusariæ; these, he said, performed the functions of an instinctive and preservative character; executed those functions only which are strictly vital, such as secretion, absorption, nutrition, and so on, and therefore was it to be inferred, and most fairly and logically, that the same animals were possessed of a sympathetic nervous system and none other; the nervous organism of both the amyencephalous monster and the zoophyte was precisely similar, and their animal functions were on a par. As a

further illustration of the opinions advocated, he cited some rare examples of vivisections performed by himself with the view of testing anatomically the relative importance of the ganglionic and cerebro-spinal nervous centres. It was shown that in frogs and fish, both the brain and spinal cord may be destroyed, and that, nevertheless, all the strictly vital functions will continue to be carried on as before.

These animals, it was observed, offered peculiar facilities for the performance of such experiments, from the peculiarity of their respiratory apparatus; but, added the author, you have only to establish an artificial respiratory process or function in the higher order of animals, *i. e.* those who breathe normally with the aid of a complex set of nerves and muscles, for that which is purely natural, and then may the spinal cord and brain be destroyed with an almost equal impunity, so far as the said vital acts are concerned in them. The phenomena of sleep and disease were referred to as offering strong and corroborative evidence of the physiological opinions insisted on; and, in so far as the latter is concerned, Dr. Davey mentioned that the wards of the lunatic asylum would afford many instances of individuals who were reduced to a mere vegetative or organic existence by disorder affecting the brain and spinal cord; such patients, he said, lived oftentimes many years with their cerebro-spinal organism so disorganized as to be perfectly useless to them; unconscious; without feeling, emotion or desire; void of thought; without hope, joy or passion; lost to all normal sensation, or perhaps without feeling of any kind, and incapable of only the most imperfect motive power; enfeebled; paralytic; they nevertheless digest, secrete, absorb, in a word, carry on, year by year, the strictly vital functions exactly as the malorganized fœtus does; exactly as the frog or fish deprived of the brain and spinal cord did; exactly as the polypus is in the habit of doing. He then referred the Society to the experiments of Sir B. Brodie, performed many years since, for the purpose of proving that those nerves having a cerebro-spinal origin had no kind of influence on the reparation of injuries, and so on. Frogs, guinea-pigs and dogs were the subjects of Sir B. Brodie's experiments; and it was found that the destruction even of the lumbar spinal cord, much less that of the crural and sciatic nerves, neither retarded nor impaired in any way the reparative process in the lower extremities; thus wounds and fractures made in the limbs so deprived of cerebro-spinal nervous influence, healed and united as readily and completely as under all ordinary circumstances. Dr. Davey explains the facts recorded by Sir B. Brodie, by saying that the ganglionic nervous power was necessarily left intact; and this it was which caused the wounds to heal and the fractures to unite; and insisted on it that although great and serious injuries to the brain and cord were borne with impunity, and for the reasons above stated, yet were such altogether impossible, in so far as the solar ganglion and its dependencies were concerned. It was very truly said, that a comparatively feeble blow on the epigastrium over the solar ganglion would destroy life; and

that it was a very common thing among boys to suffer greatly from slight accidental blows taking effect on the pit of the stomach, and the great danger of physical violence, even in a slight degree, to the epigastrium, was well known to the prize fighting gentry, who forbade the combatants to strike below the sternum; and if, as the author asserts, the solar ganglion be the seat of life, the *locale* of the *impetum faciens* of old writers, the irritability, the motions without force, of Haller, then can we easily account for the facts just cited. Instinct and animal heat were treated of by Dr. Davey as specific functions of the sympathetic nervous system; but our space will not allow us to do more than thus allude to this part of his paper.—*London Lancet.*

Art. II.—*Additional Experiments on the Excitability of Paralyzed and Healthy Limbs by the Galvanic Current:* By R. B. Todd, M.D. F.R.S.

[The Lancet, August 6, 1853.]

In the summer of 1847, Dr. Todd submitted to the Medical and Chirurgical Society of London the results of experiments tried with the view of testing the accuracy of Dr. M. Hall's dogma, that limbs paralyzed by lesion of the brain became more excitable than the healthy ones by the galvanic current, in consequence of an increased irritability of the paralyzed muscles. The present communication comprises the results of experiments to determine the difference on the influence of the current according to its direction, and also to ascertain whether any real difference of physiological effect exists when the galvanic trough, or the magnetico-electric or electro-dynamic machine is used. Thirteen healthy individuals were subjected to experiment, and with the following results:

1st. That the obvious physiological effect was produced only on completing or on interrupting the galvanic circuit.

2d. That more vigorous contractions were excited on the completion than on the interruption of the circuit.

3d. That the completion or the interruption of the direct current produced more vigorous contractions than the completion or interruption of the inverse current.

These experiments were made with a Cruikshank's battery, charged with very dilute sulphuric acid. The magneto-electric rotation instrument and the coil machine (electro-dynamic) were afterwards used, and it was found that the same effects precisely were produced, and the same variation in the intensity of the contractions according as the current was direct or inverse. Fifteen cases of hemiplegic paralysis, caused by lesion of the brain, are afterwards detailed. The results of the galvanic experiments on these cases were as follows:

1st. That of the fifteen cases, in only three was there any approach to a greater excitability of the paralyzed than of the sound limb, and that in two of these it was manifested only under the influence of the inverse current.

2d. That in three of the cases both the coil machine and the battery were used, and with precisely the same results; and that, in one of the cases, the coil machine alone was used, and with a result which corresponded with those obtained in similar cases by the galvanic battery.

3d. That in each of the three cases in which a greater excitability existed in the paralytic limbs, the paralyzing lesion in the brain was more or less of an irritative kind. In one case the irritation was probably connected with an incipient process of cicatrization.

4th. That in many of the experiments all degrees of galvanic power were used, and with no other difference than that of degree; the amount of physiological effect being exactly proportionate to the power of the galvanic stimulus.—*Ranking, Abst.*

Art. III.—*Of certain Pathological States of the Blood, and of their Treatment:* By James Copland, M.D. F.R.S., *President of the Royal Medical and Chirurgical Society.*

The author, after describing various symptoms and signs of irritation of the blood, and noting more particularly the changes observed in the excretions, &c., deduced a series of inferences as the bases upon which he founded his practice and treatment. He arranged the vitiations of the blood under certain heads or categories, according to the causes, extrinsic or pathological, producing them, with reference to the indications of treatment, and these comprehended the following seven orders:

1. Vitiations produced by imperfect assimilation or development of the blood-globules.

2. Vitiations occasioned by the increased action of the organs, which waste or decompose the hæmato-globulin—which increase the fibrine and augment the urea.

3. Contaminations arising from the absorption of purulent, sanious or other morbid matters into the circulation, or from the imbibition of any of these by the veins or cellular tissue.

4. Alterations sometimes supervening on the foregoing, or complicating the latter, such as fibrinous coagula or concretions, or inflammation of arteries, veins or lymphatics, puriform infiltrations, or fomentations.

5. Vitiations occasioned by the imperfect performance, or by the interruption or suppression of a depurating function.

6. Contamination produced by morbid miasms, or by specific seminia, as in malignant, pestilential and septic maladies.

7. The inoculation of poisonous secretions or fluids, as the fluids from erysipelatous inflammation, from asthenic or diffusive inflammation, from bodies recently dead from malignant diseases, or from putrid animal matters.

The treatment appropriate to each of these orders or categories of blood vitiation might be differently estimated by different observers; the author professing, however, to give only the results of his own observation and experience. His practice had been based upon a close observation, and upon rational inferences from such observation. The treatment adopted by the author in these various conditions was then detailed, illustrated here and there by some very instructive cases. The author dwelt at some length on the treatment of that morbid state of the blood which occurred in acute rheumatism, and which is characterized by the redundancy of the fibrinous and ureal constituents of the blood.

What medicines would counteract the disposition to fibrinous attraction in the blood, or such as might exist? Calomel, and calomel and opium, diaphoretics, emetics, purgatives, were doubtless excellent initiatory means to diminish excrementitious plethora; but to promote the depuratory functions he had found the greatest advantage from magnesia and its citrate, the carbonates and citrates of the fixed alkalies, the biborates of soda and potass, the nitrate and chlorate of potass, sublimed and precipitated sulphur, &c., &c., as well as the various preparations of cinchona and turpentine. For the treatment of the sixth category, the advantages derived from large doses of turpentine were detailed; and the author concluded by expressing his hopes that he should be excused for having made so frequent reference to his own writings, where many of the matters comprised in this extensive subject were more fully discussed; but he had his own originality in some topics to vindicate, as several authors who had recently written, had considered that opinions and ideas were fair objects of plunder, if they could be conveyed away without reference to their originators, and in a different array of words.—*Lancet.*

Art. IV.—*Cholera.*

In the *Revue de Thérap. Méd. Chir.* for March, 1854, Dr. Suley propounds a new theory as to the cause of cholera. The earth is, according to him, not an inorganic mass, but a living organized being, having as its parasites vegetables and animals which live at its expense. The water of this planet serves the purposes which the blood

does among animals; the air and electricity constitute its nervous fluid. The being called the earth is very liable to sickness, (un état pathologique—les conditions morbides.) The sickness of the earth affects its vegetable parasites, which become thereby poisonous, and cause the cholera. He refers to the diseases which appeared among the potatoes, (the potato rot) vines and other vegetables, simultaneously with the invasion of cholera, as proofs of his theory. Mother Earth suffers from local rather than from constitutional or general diseases. Hence epidemics appear only where these local diseases of the earth appear. The vegetables which grow upon these unsound spots of the earth constitute the principal diet of the poor, who, therefore, are the chief sufferers from cholera.

The Doctor's treatment, (a most delectable one) for which he claims great success, consists of carbonated waters, (eaux gazeuses) seltzer and various wines—particularly champagne, which is his favorite prescription—one bottle per hour. Champagne has a two-fold advantage; namely, it dissipates the patient's panic and cures his disease! Such is the substance, though not a literal translation of Dr. Suley's views of the cause and cure of cholera.

Art. V.—*Report upon the cases of Tetanus in the Jamjetjee Jejeebhoy Hospital, Bombay, from January*, 1845, *to December*, 1851: By J. Peet, Assistant Surgeon to the Hospital.

[Trans. of the Med. and Phys. Society of Bombay, No. 1, New Series, Bombay, 1853.]

Mr. Peet informs us, that during his seven years, 195 cases of tetanus were admitted into the hospital, exclusive of some 79 or 80 persons who were attacked subsequently to their admission, making the whole number treated during this period about 260. This is the largest number of cases of tetanus treated in any single hospital in the world. Mr. Peet has carefully analyzed this collection of cases, and the following are his conclusions:

1st. That the idiopathic form of tetanus is much more frequent in Bombay than in other parts of the world; and that, contrary to the experience of the disease in other places, it is more severe and fatal than the traumatic species.

2d. That it is often traceable to direct exposures at those seasons during which there are the greatest alterations of temperature.

3d. That of the traumatic form, many cases are ushered in by distinct febrile symptoms; but there is not sufficient reason to conclude that this constitutional disturbance is evidence of any more decided state of inflammatory action in the nervous centres than is present in cases where febrile symptoms are altogether absent. Nor would it appear that this febrile state is any indication of the severity of acute nature of the attack.

4th. That there is little doubt that there exists in tetanus, as in other diseases, a period of incubation, but there are no facts to determine the length of time over which that state may extend.

5th. That in many cases the more marked symptoms are preceded by a peculiar expression of face, to which sufficient prominence is not, perhaps, usually given, as a means of detecting the approach of the disease, and that this changed expression may exist for several hours before any other symptoms of tetanus is present.

6th. That no reliance can be placed upon the frequency of the pulse as evidence of the severity or otherwise of the attack; but the condition of the pulse in regard to strength is of much importance.

7th. That hurried respiration and dysphagia almost invariably indicate a fatal termination.

Mr. Peet remarks, in regard to the various modes of treatment pursued, "that the result has been nearly the same in each; namely, that of leading to the conviction that there is no one mode of treatment better than another in the management of this disease." He would, however, except from this general statement the assiduous use of nutritious food; this should be considered an essential element in whatever plan is adopted, for on this, rather than the medicines employed, will the favorable termination of the case depend. "The inhalation of ether and chloroform," he says, "has been well tested. That it affords great, and for a time, complete relief, there cannot be a question, but it has not appeared to have any influence in preventing, or even delaying, the fatal termination." The majority of cases were treated with preparations of the Indian hemp, but Mr. Peet does not consider it superior to other narcotics. The report closes with a table of all the cases treated during this period.—*Ranking, Abst., Dec.* 1853.

Art. VI.—*Lithotrity Performed on the same Patient forty-eight times.*

At a late meeting of the Medical Society of London, Mr. Coulson exhibited the bladder taken from a man aged eighty-three, on whom lithotrity had been performed forty-eight times during twenty years. Whether one or more fragments of the original calculus may have been left in the bladder, and became nuclei of secondary formations, or whether the bladder was at first completely freed, and the relapses depended on the same constitutional disposition which gave rise originally to the deposit of calculous matter from the urine, Mr. Coulson was unable to say. He did not see the case until the middle of last year. It cannot be denied that relapse occurs more frequently after lithotrity than lithotomy. The Norwich tables of Mr. Crosse show 12 cases of relapse after 704 operations of lithotomy, or 1 in 58. From

records of operations performed at the Hospital of La Charité, in Paris, between the years 1806 and 1831, it appears that 6 cases of relapse presented themselves after 70 cases of lithotomy, or about 1 in 11. M. Civiale states that the proportion indicated by returns which he received from Bavaria, is 5 in 162 operations (1 in 32); from Bohemia, 1 in 46 operations; from Dalmatia, 1 in 43 operations. At the Luneville Hospital, founded by Stanislaus, King of Poland, for the treatment of calculous disorders, the register shows 13 cases of relapse after 1492 operations of lithotomy, or 1 in 116 cases. The most correct registers are probably those kept at the Norwich and Luneville Hospitals, and from them it appears that relapse occurred after lithotomy once in 58 cases at Norwich, and once in 116 cases at Luneville. For lithotrity there are no other data than those furnished by M. Civiale from his own practice. After 548 operations, relapse followed in fifty-five cases, giving a proportion of nearly 1 in every ten cases. After lithotrity, every tenth patient may suffer relapse; after lithotomy the proportion seems reduced to 1 in 60. Mr. Coulson did not think sufficient attention was paid to the condition of the urine after lithotrity, as an indication of the existence of fragments in the bladder. It is not enough that the painful symptoms produced by the stone shall have subsided, and no portion can be detected by the sound or lithotrite. If the urine when first passed is at all turbid, and especially if it contains any exudation-corpuscles, first pointed out by Dr. Golding Bird, there can be little doubt that some fragment remains behind. This distinguished physician laid great stress on these bodies, as indicating the presence of stone, and Mr. Coulson knew two cases lately, in which, in consequence of the existence of these corpuscles, persevering efforts were successfully made in search of stone, although on previous examinations none could be detected.—*Lancet.*

Art. VII.—*Elimination of Lead by Iodide of Potassium.* Reported to the Biological Society by J. Outram, Jun., Lecturer on Chemistry in the New York Preparatory School of Medicine.

The value of Iodide of Potassium as a therapeutical agent in cases of Lead and Mercurial Poisoning, is now well known, many cases having been cured by this treatment alone in our hospitals. M. Melsens (the originator of this treatment) has given several cases of both lead and mercurial poisoning which he had treated successfully by this means. He was of opinion that the metal was acted upon by the iodide of potassium converted into a soluble salt, and eliminated through the kidneys. To prove this to be the case, the metal must

of course be found in the urine. This M. Melsens did not show, as he did not examine the urine chemically.

I have lately had an opportunity of examining the urine of several patients at the City Hospital, who have been under the above treatment for lead disease, and the experiments have entirely corroborated M. Melsens' theory, viz:

That the lead *is not* eliminated before treatment; and

That it is eliminated in the urine after treatment.

The following is the process to which I subjected the urine. I evaporated it to dryness, and burned the residue until all the organic matter was driven off. This residue I boiled in dilute nitric acid, filtered, and then precipitated the lead by a stream of sulphuretted hydrogen gas. Where the metal was present, it was thrown down as a black sulphuret.

I examined a number of samples of urine before the patients had been put on M. Melsens' treatment, and could not detect any trace of metal; while in those examined after treatment, the evidences of the metal were well marked. Sometimes, however, the lead could not be detected until the patient had been under treatment for some time. In most of the cases in which I detected the metal, the patients had been under treatment for at least four days; but in one case, which I examined every second or third day from the time of his admission into the hospital, it was about two weeks from the time of his being put on treatment until the time I first detected the metal in his urine. The dose of iodide of potassium that was given in those cases was ℥ i daily, in divided doses.

There was one patient to whom iodide of potassium was given for another case. After a few days, the characteristic blue line appeared on the gums, and in a day or two afterwards I detected lead in his urine.

The quantity of urine examined each time was not less than six ounces. I have examined twelve cases from the hospital, and three or four from private practice, and all of them with highly satisfactory results.—*Medical Times.*

Saturnine colic has been occasioned by the application of lead plasters (Emplast. Plumbi)—lead in the state of oxide—diachylum. Hence these should be avoided as applications to ulcers, unless no substitute is possible, which is seldom if ever the case.

It would be very desirable to ascertain whether any symptoms of lead poisoning, as colic, palsy, epilepsy, neuralgia, have been observed in the vast mining districts of the West, where many individuals have long been at work.—Editor.

Art. VIII.—*Valves in the Splenic Vein.*

Dr. Crisp, in addition to the preparations exhibited at the first meeting of the Physiological Society, showed the splenic vein of a giraffe, a dyker boc, and a Rocky Mountain deer, (which he had lately dissected) all containing valves. The splenic vein of the giraffe first exhibited was cut short, and only four valves were seen; but in the present specimen, from a male, which recently died at the Regent's-park Zoological Gardens, there were six valves within the space of three inches. Dr. Crisp thought that the discovery of these valves in a certain class of animals only was the first step towards the elucidation of one of the functions of the spleen.

Malpighian Corpuscles of the Spleen shown by the Application of Heat and Nitric Acid.

Dr. Crisp exhibited a portion of the fried spleen of a sheep, in which the Malpighian bodies were distinctly seen in the form of white spots, from the coagulation of the albumen. The same effect was produced in some other animals on the application of heat and nitric acid. He had made this discovery about twenty months since, and he thought it had likewise an important bearing upon another of the functions of the spleen.—*Lancet.*

Art. IX.—*On the Pathology and Treatment of Uterine Catarrh and Internal Metritis:* By E. J. Tilt, M. D., Senior Physician to the Farringdon General Dispensary and Lying-in Charity, and to the Paddington Free Dispensary for Women and Children.

This gentleman in a paper dated in January, 1854, and published in the Lancet, concludes by comparing these two maladies thus:

"Uterine catarrh is very frequent; internal metritis very rare. Uterine catarrh almost exclusively affects the neck of the womb; internal metritis its body. Uterine catarrh is as frequently observed in the single as in the married; internal metritis seems to affect almost exclusively those who have borne children. In uterine catarrh the discharge is viscous; in internal metritis, serous or sanguinous, and and very abundant. Uterine catarrh gives rise to no abnormal growths; internal metritis frequently does. In uterine catarrh life is never compromised; it is not unfrequently so in internal metritis. Injections have been found useless and often dangerous in uterine catarrh, but are sometimes serviceable in internal metritis."

Dr. T. defines internal metritis as a sub-acute or chronic inflammation of the mucous membrane lining the body of the womb. He

applies the tincture of iodine, or the iodide of iron with a *sable* paint brush, introducing it as far as possible into the neck of the womb, having first cleared away the uterine mucosity; on withdrawing the brush, he paints the vaginal portion of the neck of the womb. A drachm of acetate of lead in a pint of decoction of poppy-heads, he considers as forming the best injection in such cases. The incidental menorrhagia, he treats not only by the usual remedies, justly attributing much virtue to the influence of opium, but he recommends what few females will submit to, an issue or a seton above the pubis; and, what still fewer will allow, namely, "the introduction of a brush as far as possible into the neck of the uterus," and, what the fewest of all, if any will permit, that is, the introduction of a *curette* or sound wherewith to scrape the cavity of the womb, under the mere hypothesis that possibly vegetations or morbid grows may lurk within; a threefold combination of tools—speculum, brush, *curette*—a method which Dr. T. follows, even in menorrhagia; one which, however, he says "is almost new."

That the *élite* of the profession should, on so many occasions, find it necessary in the treatment of female maladies, to use numerous instruments, for example, pessaries, whether of wood, glass, or metal, for virgins or matrons, is marvelous; and, at the same time, too, well adapted to dilate, inflame and denaturalize the structure, functions and physiological finalities of the vaginal canal and its affiliated organs. That the speculum, curette, brush, probe, bougie, sound and pessary are sometimes necessary, is indisputable, and, doubtlessly, Dr. Tilt is skillful in their application.—[Editor.

Art. X.—*Pharmacy—New Process for Coating Pills.*

M. Calloud, (de Chambéry) in the *Journal de Pharmacie*, xxiii, 301, treats of the subject of enveloping medicinal substances in a covering to prevent unpleasant taste. After having tried gum, starch and sugar without satisfaction, owing to the hygroscopic tendency of the sugar and gum in moist air or with a moist mass, and their tendency to crack when very dry, he had recourse to the dried mucilage of flax-seed prepared with sugar, with success.

The following is his method:

Take of Flaxseed,	one part.
White sugar,	three parts.
Spring water,	a sufficient quantity.

A thick mucilage is obtained by carefully boiling the seeds, the sugar is added, and the whole of the moisture evaporated by careful desiccation.

This mixture is but very slightly hygroscopic, may be reduced to fine powder, and employed for covering pills. This operation is effected extemporaneously with great facility. The pills slightly moistened, are rolled in the mucilaginous powder, by which they are coated with a layer of the compound.

M. Calloud has used this chiefly for carbonate of iron pills, but it may be applied to other kinds.

Garot's process of coating pills with gelatine is most applicable to disagreeably odorous substances, as assafœtida, castor, valerian, etc., which are completely masked by it. When the gelantine is previously colored with carmine the pills bore the appearance of hawthorn berries.

M. Calloud suggests another process applicable in certain cases, which is the use of the butter of cacao as a covering for pills, where, owing to the gastric irritation, the unmasked pills will cause disagreeable symptoms. The process is very simple. The prepared pills are thrown into melted butter of cacao, then removed with a perforated skimmer, and finally rolled in finely powdered sugar, or what is better, sugar of milk.—*American Journal of Pharmacy, Jan.*, 1854.

Art. XI.—*Negligence in Writing and Putting up Prescriptions.*

The following case which occurred near London, quoted in the American Journal of Pharmacy, by Prof. Wm. Porter, Jr., of Philadelphia, for September 1853, taken from the Pharmaceutical Journal, of London, for July, 1853, "shows the importance of caution in the mode of ordering strong medicines in prescriptions; also the necessity of attention to the dose in dispensing. It is not necessary to mention the names of the parties concerned. The presciption was as follows:

℞. Strychnos,
Nucis Vomici, ℈ij.
Bismuth Trisnit., ʒ iss.
M. Ft. Pulv., xxiv.

The young man put in strychnia and nux vomica, *of each*, ℈ij. The patient took one dose and almost immediately expired. The accident arose from the fact that the word *strychnos*, being written on a separate line, was considered by the dispenser to be another ingredient, instead of being, as it was, the generic title of nux vomica. This would not have misled a young man qualified for his business, who considered for a moment the strength of the dose."

Art. XII.—*Malignant Diseases.*

Professor Gross, of Louisville, Ky., was appointed by the American Medical Association in 1851, to report on the results of Surgical Operations in Malignant Diseases—which report he submitted as instructed at the meeting in 1853. This report, which occupies 158 pages of the transactions, is exuberant with historical lore, analytic research, statistical tables, and the personal experiences of the author and his medical compatriots. The conclusions of a surgeon and author of world-wide reputation, are thus summed up by himself:

GENERAL CONCLUSIONS.

From the facts and statements which have now been presented, embracing the opinions of many of the most intelligent, experienced, and distinguished practitioners in different ages and in different parts of the world, the following conclusions may be legitimately deduced:

First. That cancerous affections, particularly those of the mammary gland, have always, with a few rare exceptions, been regarded by practitioners as incurable by the knife and escharotics. This opinion, commencing with Hippocrates, the father of medicine, has prevailed from the earliest records of the profession to the present moment. Nature never cures a disease of this kind; nor can this be effected by any medicine or internal remedies known to the profession.

Second. That excision, however early and thoroughly executed, is nearly always, in genuine cancer, followed by a relapse at a period varying from a few weeks to several months from the time of the operation.

Third. That nearly all practitioners, from the time of Hippocrates to the present day, have been and are still averse to any operation for the removal of cancerous tumors after the establishment of ulceration, rapid growth, firm adhesions, organic change in the skin, lymphatic invasion, the cancerous dyscrasy, or serious constitutional derangement; on the ground that, if had recourse to under these circumstances, the malady almost inevitably recurs in a very short time, and frequently destroys the patient more rapidly than when it is permitted to pursue its own course.

Fourth. That in all cases of *acute* carcinoma, or, in other words, in all cases of this disease attended with very rapid development and great bulk of the tumor, extirpation is improper and unjustifiable, inasmuch as it will only tend to expedite the fatal result, which, under such circumstances, always takes place in a very short time.

Fifth. That all operations performed for the removal of encephaloid cancer and its different varieties, are more certainly followed by rapid relapse than operations performed upon scirrhous or hard cancer.

Sixth. That in nearly all operations for cancerous diseases hitherto reported, the history has been imperfectly presented, being deficient in the details which are necessary to a complete and thorough understanding of the subject in each case. This remark is particularly true in reference to the diagnosis of the malady, the minute examination of the morbid structure, and the history of the case after the operation, as to the period of relapse, the time and nature of the patient's death, and the result of the *post-mortem* examination.

Seventh. That cancerous affections of the lip and skin, now usually described under the name of cancroid diseases, are less liable to relapse after extirpation than genuine cancerous maladies, or those which are characterized by the existence of the true cancer-cell and cancer-juice.

Eighth. That although practitioners have always been aware, from the earliest professional records, of the great liability of cancer to relapse after extirpation, a great majority of them have always been, and still are, in favor of operation in the early stage of the disease, especially in schirrus, before the tumor has made much progress, or before there is any disease of the lymphatic ganglions, or evidence of the cancerous cachexy.

Ninth. That many cases of tumors, especially tumors of the breast and testicle, supposed to be cancerous, are in reality not cancerous, but of a benign character, and, consequently, readily curable by ablation, whether effected by the knife or by escharotics. It is to this circumstance that we must ascribe the astonishing success which is said to have attended the practice of Hill, of Scotland, Nooth, of England, and Flajani, of Italy.

Tenth. That all operators insist upon the most thorough excision possible; removing not merely the diseased mass, but also a portion of the surrounding and apparently healthy tissues, as well as all enlarged and indurated ganglions.

Eleventh. That the practice has always prevailed, and still obtains, to save if possible a sufficient amount of healthy integument to cover the wound, and to unite if possible the wound by the first intention, on the ground that these precautions will tend much to retard, if not to prevent, a recurrence of the disease.

Twelfth. That much stress is laid by writers upon a properly regulated diet, and attention to the bowels and secretions after operation, as means of retarding and preventing relapse.

Thirteenth. That there is no remedy, medicine, or method of treatment which has the power, so far as we are enabled to judge of its virtues, of preventing the reproduction of the morbid action after operation, no matter how early or how thoroughly it may be performed.

Fourteenth. That life has occasionally been prolonged, and even saved, by operation after relapse, as in some of the remarkable cases mentioned in a previous part of this report; but that, as a general rule, such a procedure is as incompetent to effect a permanent cure as a first extirpation.

The following points may be considered as of an unsettled character; at all events, opinion respecting them is much divided, and farther observation is necessary before they can be positively determined, either affirmatively or negatively.

First. Excision is of doubtful propriety in all cases in which the disease is of hereditary origin, or where it occurs in several members of the same family.

Second. It is doubtful whether an operation should be performed when the patient is very young and the disease is of rapid growth. There is reason to believe that surgical interference, in such a case, will only expedite the fatal issue, which is generally inevitable.

Third. It is problematical whether an operation should be performed when the disease is attended by suppression of the menses, or by great irregularity of this discharge.

Fourth. Not a few surgeons regard a resort to the knife as of questionable efficacy when there is a quickened state of the pulse, occasioned by the local irritation.

Fifth. There appears to be no general agreement among surgeons as to whether extirpation is proper when there are two or more coexistent and accessible cancerous tumors.

Sixth. It is supposed, but the fact is not established, that excision of carcinomatous tumors only tends to hasten the patient's death.

Seventh. It is doubtful whether, as has been asserted by different surgeons, the prospect of a permanent cure is greater, all other things being equal, after an operation on an old cancer, than after an operation on a cancer of recent standing.

Eighth. It has been stated by writers of great respectability, among others by Dr. Macfarlane, of Glasgow, that in robust women of sanguine temperament, the reappearance of cancerous disease, and its subsequent progress, are more rapid after operation than in nervous or lymphatic persons; an assumption demanding verification.

Ninth. It requires to be proved whether excision ought to be performed in the ulcerated stage of malignant disease, as a means of prolonging life and of procuring comparative relief from suffering.

In bringing my labors to a close, I feel conscious that I have added nothing whatever to our previous information of malignant diseases. Nor, in truth, could this have been reasonably expected. My sole aim has been to sum up our knowledge upon the subject; to construct, as it were, a mirror which should reflect the practice and opinions of our predecessors and contemporaries, and thus serve as a guide to future travelers in the same path. In a word, my object has been to show, not only what has been done, but what remains to be done. The facts which I have collected, if useful at all, are so rather in a negative than in a positive point of view. Our knowledge of the results of surgical operations in malignant affections is in a state of transition, which has much to hope from the future, but can gain little, if anything, from the past. What the microscope and animal chemistry, guided by the hand of modern science, may do for the subject, it would perhaps be

premature to predict; already they have rendered the cause essential service, and it would certainly be unphilosophical to suppose that they are incapable of affording farther light.

Finally, in drawing up this report, I have rarely appealed to my own experience, believing that, even if it were fully exposed, it could add nothing of real value to the general stock of our knowledge upon a subject respecting which so much has been written by others. In point of number, indeed, my own cases could not be put in competition with the extensive scale of facts brought forward by some of the surgeons, the results of whose labors it has been my duty to bring before the Association. I have little, indeed I might say, no confidence in any operation for malignant diseases, except the cancroid varieties; and I have for years past, as Professor of Surgery in the University of Louisville, deemed it my duty to discourage a resort to the knife in all cases of the kind, especially in scirrhus and encephaloid of the mammary glands, ample experience having satisfied me of the utter futility of such an expedient, however early and efficiently employed. In cutaneous cancer, on the other hand, my rule has always been to operate, provided the disorder has not advanced so far as to preclude the possibility of removing the whole of the morbid growth; and provided, also, that there is no evidence of constitutional infection. To this rule I shall rigidly adhere, unless my own experience, or the experience of others, shall show me the impropriety of it, when I shall most cheerfully abandon it.

LOUISVILLE, April 20, 1853. S. D. GROSS, *Ch'n.*

Art. XIII.—*The Relations of the Pulse to certain states of Respiration* ;* By S. WEIR MITCHELL, M.D., of Philadelphia.

The mere numerical relations of respiration to the pulse, and conversely, of the pulse to respiration, are so well known as to demand no further notice here. Increased activity in the one commonly entails a like state in the other. To this general law there is a very remarkable exception, whose insertion at this point is justified by its singular novelty and interest.

The substance of the following observations is taken from an unpublished essay upon the state usually known as the "mesmeric sleep," an abstract of which was read by its author, Prof. J. K. Mitchell, before the Philadelphia College of Physicians, in the year 1839.

Thirty-seven cases of "animal induction," or induced somnambulism,

*The numerical tableaux illustrative of the fundamental postulates of this essay are omitted for want of space. This paper, printed in April, 1854, cannot be credited with certainty to any particular journal, because the sheet received does not designate any one.—[EDITOR.

were studied with reference to the relations of the pulse and respiration. The result is briefly stated in the following summary of a very elaborate table:

Pulse before sleep.	Pulse of sleep.	Respiration before sleep.	Respiration during sleep.
Average: 81.7	Average: 105	Average: 19.04	Average: 19.68
Difference: 23.3		Difference: 0.64	

The pulse of the mesmeric state was in every instance greater than that of the waking condition; the least excess being 8, the greatest 48. If one extraordinary case be omitted from the table, the average proportion of the numbers of respirations would be as 18.9 in the waking condition to 18 in the sleep.

While, therefore, the pulse always quickens, the respiration either falls absolutely below the normal waking standard, or undergoes no change. The proportion of the pulse to the respiratory movements was four to one in the waking condition, and nearly five to one in the sleep; an excess of twenty-five per cent. for the mesmeric slumber.

The facts to which I would now desire to call attention are easily brought within range of the reader's personal experience.

Let a finger be placed on the pulse, and let the chest be held fixed in full inspiration, the pulse will be observed to become less frequent.

Numerous individual exceptions occur, and, in a few cases, negative or exceptional results are obtained. On the other hand, the observer is sometimes alarmed by the long pause between successive pulsations. In one or two cases I have been forced, on this account, to suspend altogether any further research. Again: let the chest be fixed in a state of extreme expiration, and the pulse will almost invariably quicken.

Persons who are susceptible of these changes will thus observe that there is a point, midway between these extreme conditions of respiration, at which fixation of the chest fails to affect the pulse. If, starting from this point as the zero of influence, the lungs are slowly inflated to fullness, the pulse will usually beat less and less rapidly. In like manner, during the movement of expiration, the heart quickens its pulsations, and sometimes they suddenly increase to a still greater extent when a state of complete expiration is attained. In a few rare cases, these changes may be studied in the thoracic movements of those whose natural respiration is excessively long and laborious.

For brevity and convenience I shall term the two pulses induced by extreme respiratory conditions, respectively, the inspiratory and the expiratory pulses.

In forty persons the average normal pulse was 80.5. In fixed inspiration it fell to 70.15; in fixed expiration it rose to 93; giving, as the average of extreme difference, 23 per minute.

After very careful study of all the phenomena, in many experiments and vivisections, I feel disposed to refer the induced pulses of extreme respiratory states, to causes chiefly physical.

I confess to some pleasure in thus placing these interesting facts within the domain of the laws of dynamics. This pleasure is at least not lessened by the knowledge that I am indicating a new region for research, and that many of the points in question have hitherto escaped the eager reach of physiological induction.

The present state of knowledge in this direction, is best illustrated by the following quotation from a standard text book of the highest character:

"The pulmonary circulation is unaffected by atmospheric pressure, and is not exposed to the influence of the pressure of muscles. The force by which it is accomplished, and the course of the blood, are alike simple."—*Kirke's and Paget's Handbook of Physiology, p.* 132, 2*d Am. ed.*

I have repeatedly observed in rabbits the phenomena recorded by Reid, the pulse becoming slow during inspiration, and fast during expiration, when those movements were long and laborious. Animals may be forced to breathe thus, and in fact, I have sometimes noticed similar phenomena in man. They have been in part explained by Reid. He believed that, during dilatation of the lung, the flow of blood from its spongy tissue was necessarily lessened. The left heart would thus receive a smaller share of blood, and would therefore contract less often.

During expiration, a portion of blood, as well as the air of the lung, is expelled; this blood, moving in the tide of circulation, enters the left heart and compels it thus to contract often enough to get rid of the overplus of fluid. Hence a more rapid pulse.

Fixed Inspiration.—When the lung is fixed in the state of complete expansion, new dynamical relations arise, and the pulse becomes slow.

After several fruitless efforts to apply the hæmadynamometer within the tide of the pulmonary circulation, I attained the desired ends in a very simple and novel manner.

I observed that the exposed lung of an animal, kept alive by artificial respiration, flushed as it shrunk in expiration. This is possibly due to the closer aggregation of the capillaries. It might also be caused, in part at least, by actual increase in the amount of blood, which the most minute vessels contain during complete expiration. Indeed, the extreme pallor of the surface of the lung, in deep inspiration, gave some weight to this idea.

To test these inferences, I opened a rabbit, and while artificial respiration was made, I carefully scratched through the pleura with a cataract needle, thus wounding the capillary vessels of the living lung. The lungs were fully dilated while I effected this, and, to my sur-

prise, scarcely a drop of blood followed the instrument. As the lungs fell in expiration, the wound began to bleed—always bleeding most freely in complete expiration. This experiment was repeated many times, in the presence of Drs. Brinton, Morehouse and Da Costa, and always with the same result, so long as we took care to injure no large vessel. The inference seems to me a fair one, that, in full inspiration the capillary circulation of the lungs is somewhat impeded. If this be so, then in fixed and complete inspiration the left heart will, for a time, receive less blood than usual, and the excess will exist in the right heart and pulmonary artery. Is this the cause of the slow pulse of full inspiratory fixation? Some light is cast upon the subject by a study of the phenomena of asphyxia. In that condition the heart beats slowly, as soon as the circulation begins to be arrested in the minuter vessels of the lung. The conditions are then to some degree alike—too much blood on the right side, too little on the left. Without doubt, the slow pulse of asphyxia is also due, in part, to other causes, but the circumstances I have named have always been admitted to exert a large share in the phenomena in question. While offering this as a partial, and, I admit, not a wholly satisfactory account of the pulse change in inspiration, I do not mean to exclude the element of reflex nervous impression.

Finally, in sudden and violent muscular exertion, the right heart receives for a moment an excess of blood; yet, in this case, it beats more rapidly. In inspiratory fixation there is also a surplus of blood in the right heart; but, in this instance, it is a *reflux* of fluid which congests the pulmonary arteries. We have also, in inspiratory fixation, the other additional element of deficient supply to the left heart. We have no further means of estimating the relative effect of these several sets of conditions to which the two hearts may be subjected.

1. It seems to me clear enough that the pulmonary circulation is modified by the various conditions of respiration in which the lungs may be placed. Do not relatively similar effects attend every respiratory movement, however simple? We cannot demonstrate this upon the healthy living lung, yet the inference would seem to be a fair one.

2. During complete inspiration the tide of blood is momentarily retarded in the capillaries of the lung. Aeration of the vital fluid then takes place with the greatest facility; and during expiration, and more especially in complete expiration, the blood thus fully aerated is expelled from the lungs by the rapidly acting heart. In other words, the circulation in the lungs is slower when these organs contain most air, and becomes most easy and rapid during the movement of expiration.

The effect of respiration, in mechanically diverting the blood from the course of the fœtal circulation, is also of interest in this connection.

At this point I shall leave this interesting subject. If I have failed to observe correctly, or to reason justly, I shall at least have called to the task more careful observers and more able thinkers.

In the course of this paper I have avoided detailed accounts of individual vivisections, prefering rather to state results than to overload my

pages with useless matter. Many of the vivisections alluded to were made by the skillful hands of my friends, Drs. Morehouse and Brinton, aided by our private pupils, whom I desire to thank for their kind and ready aid.

Art. XIV—*Fevers—their Pathology, Treatment, &c.* Digested from Braithwaite's Retrospect, Jan. 1854; taken from the Med. Times and Gaz., The London Lancet, &c.

On the Relation between Therapeutics and Pathology.

[Dr. Johnson truly observes that on few subjects is there such diversity of opinion as upon the effects of remedies in disease, their modes of action, and the best methods of administering them. This, in a great measure, he believes to be owing to the carelessness manifested as to the natural history of the disease. If we had a specific for most diseases, as quinine for ague, the importance of diagnosis and scientific pathology would not be so great. They are the more important, however, since, as we do not possess many of this class of remedies, we are obliged to treat diseases on what are termed *general principles;* and it is improbable that for many diseases specific remedies will ever be found.]

There is, probably, no better test of the truth or soundness of any doctrine, when applied to practice, than the fact of its being consistent with the dictates of well-informed common sense; and, perhaps, I could give no better illustration of the beneficial influence which has been exerted by this kind of appeal, than is to be afforded by the modified views now prevalent with regard to the nature and treatment of inflammation. It certainly does appear contrary to common sense to suppose that a disease, whatever may be its name or its nature, which has been induced by exhausting and depressing influences—by excessive labor, by loss of rest, by abstinence, by hæmorrhage, or by some other drain upon the bodily strength—that a disease so originating can be removed by a recourse to measures which have the same exhausting and depressing tendency—by the free abstraction of blood, by a rigidly low diet, and by active purgatives. Science has listened to the voice of common sense in this matter, and in spite of the dogma *similia similibus curantur*, a method of practice which was originally suggested by imperfect and erroneous views respecting the nature of inflammation, has been greatly modified, and with the happiest results. It is now becoming more and more apparent, that the *quantity* of blood in the vessels of an inflamed part is not the primary and most important fact in the phenomena of inflammation; that the so-called increased afflux, or determination of blood, is in reality produced by impeded flow of blood through the capillary vessels, resulting from an altered relation between the blood and the tissues with which it is

brought into contact. This altered relation between the blood and particular tissues, may depend upon an excess of nutrient materials in the circulating fluid, or it may depend upon a directly opposite condition, or upon other changes in the *quality* of the blood. So that, however great may be the resemblance between the local phenomena of inflammation in different cases, the treatment must be conducted with a constant reference to the causes which have given rise to the disease.

The importance of ascertaining the remote originating cause of a disease is great in proportion to the degree of doubt which there may be as to the precise nature of the local changes upon which the symptoms in any given case are supposed to depend.—*George Johnson.*

Eighteen cases of Typhus Fever treated by the free exhibition of Brandy—one death.

[In all these eighteen cases the type of the disease was of the well-marked typhus character—presenting the following symptoms previous to the commencement of the treatment:]

A copious eruption of scattered measle-like spots (mulberry or typhus rash); bowels either confined or but slightly relaxed; great prostration of strength; delirium (in six cases coma was present); a small and very rapid pulse. It may be well to premise, that they were treated as is done in almost all general hospitals in the open wards, their beds being purposely arranged so as to occur at some distance from each other, in order to prevent the accumulation of contagious emanations. The treatment pursued consisted in administering, either every hour or every half hour, day and night, from half an ounce to an ounce of brandy, with a draught every second hour, containing sp. æth. chlorici min. x., ammoniæ carbonatis gr. v., aq. pur. ℥ j. The patients were induced to drink as much strong beef-tea as possible; the head was always shaved; and, in most, a blister was applied to the scalp. We are indebted to the careful observations, noted daily by Mr. Macnamara, the clinical assistant in charge of the cases, for the whole of the particulars respecting them. The first to which we shall allude was a very severe attack, and happened to an elderly and unfavorable subject; the beneficial effects of the alcoholic stimulant is strikingly shown, and there even appears some cause to infer the superiority of brandy over wine.

On the day that the administration of brandy, etc., was commenced, the pulse had, in five cases, a frequency of 136 per minute; in three, of 126; in seven, of from 120 to 126; and in one, of 116. After the measures above specified had been pursued for four days, the pulse had, in eight cases, fallen to 92; in five others it had fallen below 92 on the fifth day, and in the remaining four to below 90 on the sixth. Again, taking the day on which the treatment was commenced as our starting point, the skin, previously hot and dry, relaxed and became moist and perspirable on the fifth day, in nine case; on

the sixth day, in five cases; on the twelfth day, in one case, and in the remaining two the date of this crisis was not recorded.

The degree of success exhibited by the above facts is, we suspect, very considerably beyond that usually obtained in cases of so severe a type as those under consideration, and is very encouraging to a pursuance of a similar plan of treatment in future. That the success did really depend on the treatment, appeared to be conclusively evidenced in several cases, in which the pulse progressively increased in frequency up to the time that the brandy was ordered, steadily fell from that day forwards. The relapses of one or two, in consequence of the accidentally inefficient administration of the remedy, also afford important support to the same conclusion. In respect to the numerical age of the fever at which the brandy treatment was commenced, it varied so much in the different cases that there does not appear to be any practical advantage in attempting to state it. In all, however, the first stage had passed, and low "typhus" had become fully developed.—*Under the care of Dr. Todd.*

Quinine in Fever.

[Cinchona bark has been used in continued fever since the period of its introduction into England. Sydenham did not use it in continued fever, but Morton, his contemporary, gave it boldly, ʒ j. every three or four hours, with great success.]

Dr. Sims expressed himself thus: "I solemnly declare that I never saw a case of nervous, low, putrid, or malignant fever, when the patient could be brought to take this medicine in sufficient quantity, turn out unfortunately." He did not rely on less than six or seven ounces given in little more than two days. Drs. Fordyce, Armstrong and others, were of an opposite opinion.

[Our readers are aware, that under the form of sulphate of quinine, Dr. Dundas has recently introduced it into this country as a remedy for the ordinary forms of continued fever. The character of the disease is the ordinary one, presenting no peculiarities.]

The most frequent complication was sub-acute bronchitis; a few cases of pneumonia and a few of pleurisy occurred. Bowel complication of a serious nature was extremely rare.

Before administering the quinine it is well that the patient should have an emetic, for of itself it is beneficial, and after its operation the quinine rarely disagrees with the stomach. It may be given at all stages of the disease, unless there be great prostration. The dose of the quinine is from three to ten grains every two or three hours, given in the form of powder. From the commencement the patient is allowed three pints of milk and one of beef-tea daily, and, if at a late stage of the disease, or the patient has been a drunkard, wine or whiskey is given. Purgatives do not appear to be necessary, for the emetic usually relieves the intestines as well as the stomach;

and if constipation should occur, a simple enema is generally sufficient to remove it.

[A severe case of typhus—a rare disease in New Orleans, except among recent immigrants—occurred in the editor's practice, which terminated successfully in a young lady of Mr. Levi's family in Annunciation street, while these sheets were passing through the press. The treatment consisted of small doses of opium, morphia, ferro-cyanate of quinine, pil. hydr. sinapisms, a large abdominal blister, wine, a persistent horizontal position for two weeks, and an occasional enema. The skin, abdomen, chest, throat and head were all implicated; the debility extreme. How British typhus can agree with the digestion "of three pints of milk and one of strong beef tea" per day, is marvelous. The patient in this climate is apt to loathe and vomit food.]

In those cases where the quinine treatment was commenced during the first seven days of the fever, the average duration of the disease was 4.4 days after the first administration of the remedy; when during the second week 5.8 days. From cases treated during or after the third week no inference can be drawn, the disease having then nearly run its course.

Of sixty-one uncomplicated cases, eleven were under the treatment during the first seven days; of these, two were free from fever on the second day after treatment; one on the third day; two on the fourth day; three on the fifth day; one on the sixth day; one on the seventh day; one on the eleventh day. In twenty-four, the treatment was commenced during the second week; of these, one was free from fever on the second day after treatment; two on the third day; five on the fourth day; seven on the fifth day; three on the sixth day; two on the seventh day; one on the eighth day; two on the ninth day; one on the twelfth day. In twenty-six, the length of time that the fever had previously existed was not ascertained; of these, five were free from fever on the second day after treatment; six on the third day; three on the fourth day; four on the fifth day; three on the seventh; one on the eighth day; three on the ninth day; one on the twelfth day.

The quinine was also given in twenty-seven complicated cases, the chest being involved in twenty-two, the abdomen in two, and in three both the abdomen and the chest were affected. Of the twenty-two cases where the chest was involved (the affection being, as before remarked, generally sub-acute bronchitis), fifteen were treated during the first week; of these, one was free from fever on the third day after treatment; one on the fourth day; two on the fifth day; four on the sixth day; two on the eighth day; one on the ninth day; two on the tenth day; one on the eleventh day, and one on the twelfth day. In seven the treatment was commenced during the second week; of

these, two were free from fever on the third day; two on the fourth day; one on the sixth day; one on the seventh day; one on the tenth day.

The remedy employed in the above complication, in conjunction with the quinine, was a large mustard jacket placed round the whole of the chest, both back and front, applied twice or thrice daily, and left on as long as the patient could bear it. It possesses the advantage of not weakening the patient, at the same time being an excellent counter-irritant. Great care should be taken, more particularly in winter, to prevent the patient taking fresh cold while at the night chair; in hospitals we think the use of the bed-pan the best preventive.

Two cases occurred with abdominal complication; of these one was admitted on the ninth day of the fever, and recovered on the sixth of treatment; the other was admitted on the fifth day of fever, and recovered on the seventh day of treatment.

Three cases occurred with abdominal and chest affections; of these two were admitted on the fifth day of the disease, and were free from fever on the fifth day of treatment; one admitted on the eighth day of the fever was free from it on the tenth.

Thirteen cases in which the quinine was given proved fatal; of these five died within forty-eight hours after admission. Of the remaining eight, one was admitted on the eighth day of fever, and died on the eighth of treatment. One was admitted on the fifth day of fever, and died on the sixth of treatment; this man had persistent hiccough, congestion of lungs, and had had a severe attack of fever seven months before. One was admitted on the sevenh day of fever, and died on the tenth of treatment; this patient had been allowed nothing but barley water previous to admission; tubercles in the right lung. One was admitted on the fourteenth day of fever and died on the fifth of treatment; the quinine seemed to produce no effects. One was admitted on the ninth day of fever and died on the sixth of treatment; this patient had two kinds of spots, the one disappearing, the other unaffected by pressure; had been a hard drinker. One was admitted on the fifth day of fever and died on the eighth of treatment; a woman seventy-six years old; this was the only fatal case where the pulse was reduced below 100. One was admitted on the fourteenth day of fever and died on the fourth of treatment; the quinine did no good. One was admitted on the eighth day of fever and died on the third of treatment; had involuntary motions when admitted.

The effects of the quinine on the pulse is, in favorable cases, sometimes very remarkable; in one case it fell from 104 to 72 in twenty-four hours; in another from 140 to 84 in seventy-six hours; but generally speaking the reduction is gradual and steady. In cases which terminated fatally, the quinine, with one exception, never reduced the pulse below 100; it sometimes fell from 148 to 124, from 112 to 108, from 140 to 114, but not below 100. None of the fatal cases

were under treatment before the fifth day. In complicated cases the effect on the pulse is less marked than in those which are free from complication.—*Dr. Robert Gee and William Eddowes.*

Clinical Remarks on Yellow Fever.

[Dr. Cummins observes that the small doses of quinine, recommended in our Pharmacopœias, have no effect in yellow fever, and that many cases are lost from the dread of pushing the remedy to the necessary extent.]

Most persons believe that when deafness is produced there is no further indication for its use; but this is a fatal error, as in most cases it is necessary to push it for a much longer period. It is difficult to lay down any fixed rules as to the quantity that should be given in any individual case; but if a well-founded and long established rule of medicine is borne in mind, that in certain diseases ten times or more of the ordinary dose of medicine is required to produce its effect, and that yellow fever is one of those diseases, it will be found that the best guide for the administration of quinine is the *amount of nervous depression present.*

Another general rule of medical science must also regulate the administration of this medicine—that depression follows the excitement of all stimuli. Now it is certain that quinine acts as a stimulus to the nervous system, and that its action as such is temporary, and invariably followed by depression. Now as long as the yellow fever poison is in the system, anything which promotes depression of nervous action must increase the disease; it is therefore obvious that the primary or stimulant action of this medicine must be kept up by continuing its administration until the poison has passed off the nervous system, which it is certain to do in the third stage; for, as I have mentioned, it has its limit of operation like other poisons, and wears itself out in three or four days, leaving only its effects behind. It is true that the more quinine given, the greater will be the depression when its use is discontinued; but if this occurs after the poison passes away, it will be a depression *sui generis*, and not an aggravation of the specific depression induced by the poison. I do not mean for a moment to make light of the depression which follows the use of large quantities of quinine; on the contrary, I consider it as the probable source of the tedious convalescence which *occasionally* is observed after yellow fever, and which renders removal from the tropics necessary. But I would say that by comparison with the specific depression of yellow fever it becomes trivial. We have a choice of two evils before us, either by pushing quinine to its full extent, to increase the depression of convalescence, which can be cured by change of climate, &c., or to increase the specific depression of the poison, by allowing the quinine depression to exist along with it; and who that knows the awfully destructive nature of the yellow fever depression would hesitate in choosing the first? It may

be supposed from my remarks upon the duration of the poison that I consider it a disease which, when once established, must go on for a certain number of days, but this is far from being my opinion; for I have frequently seen a most violent attack cut short by thirty or forty grains of quinine administered *early*, and perhaps repeated in an hour. Now, the possibility of such an occurrence as this seems to contradict my remarks upon the danger of leaving off quinine too early; but there is a vast difference between cutting short a disease and only suspending its action, and the danger of the after depression of quinine is not to be apprehended in the former. If a patient is seen *very early*, and we determine upon attempting to cut short the disease, it is better to give thirty grains of quinine and twenty grains of calomel immediately, and another similar dose conjoined with five grains of James' powder an hour afterwards. This should be followed in two hours by an ounce and a half of castor oil. It is extremely probable diaphoresis will then be established, the pain in the head and back will have subsided, the congestion have been removed, the bowels well opened, and all the functions perfectly restored to health. This is cutting short the disease, and we need not fear the depression which follows the quinine in such a case; but it is no easy matter to determine exactly whether the disease is stopped or suspended; and we must watch the case closely for at least twenty-four hours, and if the symptoms are not completely removed, we have lost time and injured our patient by permitting the secondary effect of the quinine to become established, so that an attempt to cut short the disease is not without a certain degree of risk; at the same time, if tried *early*, it very frequently succeeds. It must therefore remain a question for the physician to decide whether the symptoms are sufficiently recent to admit of their cause being rooted out of the system, and my own experience limits the attempt to five or six hours. We must not allow an idiosyncracy to interfere with the full use of quinine.—*Dr. W. J. Cummins.*

Practical Observations on Yellow Fever and its Treatment with Spirits of Turpentine.

[To Dr. Gilbert King is due the credit of having first used this remedy in bilious remittent fever. Mr. Laird is of opinion that this disease is not of an inflammatory diathesis, but a blood poison from external influences—the organic lesions being secondary. Hemorrhages from the nose, mouth and bowels of a passive character, proved that the blood was in a dissolved and deteriorated condition.]

It appears natural to conclude that the blood, after its primary impregnation, or rather at the commencement of the attack, becomes afterwards further contaminated by the non-elimination of the different excretions, as is known to take place in certain forms of jaundice, in milk fever and disease of the kidneys.

With this view, therefore, in regard to the pathology of yellow fever, and there is good reason to believe also of cholera, supported as it is by the analogy of the action of certain poisons on the system, I will now endeavor to prove the consistency and reasonableness of the turpentine mode of cure. During the epidemic it was first prescribed by Dr. King, in five very bad cases of this disease, with the view solely, I believe, of restraining passive hemorrhage, when its peculiar salutary influence was so strikingly observed. This happened on the 27th of August, about a month after the fever had broken out; a revulsive plan of treatment, including general and local bleeding, to a large extent, having up to this period been practiced with very unsatisfactory results. After these experiments on the five cases alluded to, all of whom remarkably recovered (and two were reported as having had black vomit), the turpentine was afterwards given in every case and in every stage of the disease, with what success documents then sent into office will show. When I joined the hospital on the 19th of September, the epidemic may be said to have been at its height, and there were then 103 cases of fever under treatment. The general mode pursued was a moderate bleeding at the commencement (from 12 to 20 ounces), followed by cupping or leeching, if it seemed necessary.

If the patient had received no medicine prior to admission, the primæ viæ were cleared out by a purgative dose of calomel or blue pill, along with the solution of Epsom salts, and the turpentine then given in doses of twenty minims in a little camphorated water three times a day. In consequence of strangury, it was occasionally administered in combination of sweet spirits of nitre; but this troublesome symptom was happily of but rare occurrence, and then generally consequent on the application of blisters. Small doses of tincture of opium, and also castor oil, were occasionally combined with it, in cases attended with frequent bloody and otherwise vitiated dejections, or in an opposite state of the bowels. The auxiliary treatment comprised sinapisms and blisters to the epigastrium, emollient enemata, and during the stage of debility, wine negus, beef tea, &c., cinchona injections. The principal remedy being, therefore, the turpentine, since in the generality of the cases nothing else was given, I consider it, without any prejudice whatever, as a remedy in bilious remittent fever, not only perfectly consistent with, but particularly indicated by, the symptoms during life, and also the appearances after death, for the following reasons: First, the hemorrhagic character of the disease, depending, doubtless, on some change either in the chemical or vital properties of the circulating fluid; secondly, from the speedy embarrassment of the different excretory functions, particularly that of the kidneys, a diminished or suppressed excretion of urine having been particularly observed here as the most unfavorable symptom, and proved by the frequently contracted and empty condition of the bladder in fatal cases. As regards the first proposition, since this medicine is universally acknowledged as producing certain

styptic and other salutary effects, melæna and ordinary passive hemorrhage, is it not reasonable to infer that it will be equally effectual in fevers, not recognizing inflammation as their proximate cause and attended with a somewhat similar pathological condition? In regard to the second proposition, its well known physiological influence on the renal and cutaneous vessels proves it to be, as a diuretic and sudorific, the very remedy which Nature points out in the means she herself adopts in the spontaneous cure of malignant fever. Besides these long and well known properties, and also its acknowledged action on the nervous system, I believe that turpentine possesses antiseptic or antiscorbutic qualities, as I have in several instances observed in scurvy, and particularly in scorbutic ulcers of the leg, indicated by their florid, healthy appearance, after a few doses of this medicine. That it is not in the doses above mentioned a stimulant, but, on the contrary, an indirect sedative, and therefore not counter-indicated in the first stage of yellow fever, our experience here most satisfactorily proved in hundreds of cases, as it has also since done in the treatment of dysentery. Unprejudiced as I hope I am, and being anxious to bring this really valuable remedy more into notice by an honest appeal to plain ascertained facts, I beg respectfully to state, in the most earnest and unqualified manner, that instead of exciting the circulation it quieted it, and allayed the urgency of the primary pyrexia by quickly (and in some cases almost immediately) restoring the pent-up secretions. The moderate bleeding, which was generally practiced at the commencement, and frequently found essentially necessary to divert local determinations, no doubt materially assisted it in its action on the skin and kidneys, and on this account must be considered as a very important step in this mode of treatment.—*James Laird.*

Art. XV.—*Professor* J. L. Riddell's *Opinion on the Causes of Yellow Fever, &c., &c.*

To the Sanitary Commission of New Orleans:

Gentlemen—In compliance with your resolution of Dec. 21, I have the honor to present you the accompanying records of testimony, respecting the origin and spread of Yellow Fever this year, in some of the Southern towns, as high up the Mississippi as Lake Providence.

Deep interest is universally manifested in the labors of our commission; and all possible facilities were tendered me in the prosecution of my inquiries. My constant regret has been, that want of time would not permit me to prosecute them further.

Of course I found conflicting opinions, and now and then statements more or less contradictory; yet from all, as well as from data previously in possession, it appears to me the following inferences are deducible:

1st. That our yellow fever of 1853 has not been personally contagious; that the poison, virus or material cause producing it, does not emanate in an active condition from the person of the patient laboring under the disease.

2d. That the disease has been marked by characters of infection and infectious communicability, the poisonous matter (doubtless some species of living organism) maturing its germ or spores on the surface of solids devoid of life, surrounded by confined or impure air; which germs become diffused in the impure atmosphere.

3d. Three peculiar conditions seem to favor the development of the infection. 1st. The absence of ozone, the great chemical promoter of oxidation, which absence permits the undue development of obscure cryptogamic life. 2d. Abundant emanations from decomposing and disintegrating organized matters, complex products, gaseous, liquid and solid, the pabulum or blastema of cryptogamic growths. 3d. The presence of the specific organism, whose perfected spores constitute the material cause of yellow fever.

4th. That the towns and plantations of the Southwest have this year derived their yellow fever from New Orleans.

5th. That although black vomit fevers or types of yellow fever may perhaps originate in this region, yet, that the germs of our epidemic of 1853, have probably been derived from countries further South.

6th. That the mixture of equal parts by weight, of black oxide of manganese, sulphuric acid and water, which in the cold will continue for many days to develop ozone, promises to be the most convenient, most economical and most efficient disinfectant ever used; and therefore deserves hereafter a fair trial.

7th. It is proper and feasible for New Orleans to have some kind of quarantine in certain months of the year, which will exclude filthy persons, filthy clothing and filthy ships, until they are fumigated; and goods from West Indian, South American and Mexican ports, until they are fumigated.

8th. The city should be kept cleaner than heretofore, by efficient drainage, and sanitary regulations carried into effect.

9th. Legal ordinances should be framed and carried into effect, to prevent the undue huddling together of human beings within the limits of the city.

Respectfully, J. L. RIDDELL,
Member of the Sanitary Commission of New Orleans.

New Orleans, January 1, 1854.

Part Third.

REVIEWS AND BIBLIOGRAPHICAL NOTICES.

Prelusory Remarks.

"Curious conjectures he may always make,
And either side of dubious questions take."
CRABBE.

ÆSTHIC science affords no contrast more striking than that between the Ideal and the Actual in the important department of literature devoted to critical and analytic Reviews. Practically, what is and what ought to be, must be often regarded as belonging to differential categories. The causes which operate—the impelling motives, all powerful in the rendition of a critical verdict, damnatory or eulogistic—do not always appear—are not always avowed—cannot always be divined; and will not always bear the light of day. Silence, "faint praise," and open condemnation, may have the same warp and woof, the same purpose, the same result.

Praise and censure, awarded by chance, in stereotyped phrases, without analysis and without just appreciation, are powerless for good, mighty for evil. But who will venture upon innovation and reformation? What are the causes that lead to the evils intimated? A few of these may be enumerated. A Medical Review, like a College, ought to be distributed among different persons with a reference to their qualifications to pronounce upon what they actually know in the particular branches which they have studied thoroughly. They should read the book as well as review it. A reviewer should not write anonymously, but sign his name to his communications. The author reviewed and his book, not the publisher, should be named and considered. The gift of a book from the publisher, how desirable soever

it may be to the donee, is not without effect; the praise almost constantly awarded, aids the sale more, far more, than an ordinary advertisement. The publisher's name, and the bookseller's, too, is usually mentioned with great commendation, and the public are urged to buy the work of that particular bookseller who bestows the book, while, perhaps, a dozen of others equally worthy of patronage are passed by.

"Silence is great." The New Orleans Medical and Surgical Journal does not affect to be over-righteous, and proposes at present no radical reform, except that of not preferring, in print, one bookseller to all others, and that of recommending the signing of the name to the critical verdicts given for or against authors and their works. This latter doctrine, advocated some years ago by the present Editor, and since adopted by that most able work, "The British and Foreign Medico-Chirurgical Review," was expressed as follows in a Review of the work of the late Dr. Drake; and it is hoped that the reader will for this once, now that the writer is entering upon the career of Editor, pardon the liberty of quoting one's self:

An author expects commendation; if he be vain, it gratifies his ostentation; if disinterested, it diffuses throughout his entire being an inexpressible satisfaction, which arises from the hope that his labors will promote the well being of society. But in no case can a well balanced mind derive real pleasure from the approval of others, when he at the same time feels conscious that he does not deserve it. Whatever may be the aims of an author, they must fail if his work be not read and appreciated. An unread book is, in effect, no book—a nonentity. "The misfortune of a book," says Boileau, "is not the being ill spoken of, but the not being spoken of at all."

One of the ends aimed at by the modern system of periodical reviews—a self-imposed one—is that of preluding the true value of the books which appear from time to time. The self-appointed forerunner and critical informer-general, whose vocation consists in giving opinions about other people and their works, is, in the present organization of society, a necessary evil, but still a happy invention for those who cannot or will not examine for themselves. It has been supposed that reviewers have a predilection for bad books, because "bad books make good reviews, as bad wine makes good vinegar."

The writer of a good book would probably be, in most cases, the

best reviewer of it, were he willing to express his own estimate of its value. After all, a review is but one man's opinion, perhaps an impertinent one, concerning another. Seeing that this is the case, why should the reviewer, as is the custom with critics who write in the English language, assume the ponderous editorial WE, and launch forth his anonymous, perhaps sectarian dicta, masked by impersonality. The French, who invented reviews as early as 1665, affix their proper names to contributions of this kind. It is natural for the dove to wish to know whether the hawk is the reviewer. The wolf and the lamb, the fox and the hound, must have a similar curiosity. But so unlimited is the assumed jurisdiction of a reviewer, in many cases, that it is not necessary to speak of the work reviewed at all; for its title will serve as the point of departure for an independent essay. Reviews, however, proceed in general upon the principle of the division of labor. "People do not think for themselves; their reviewer thinks for them." A grievous evil this is. Gœthe says:

"Books are now written, not to be read for the sake of information and instruction, but to be reviewed, to the end that people may talk and descant upon them *ad infinitum*. Since it has been the custom to review books, no soul reads them but the reviewer, and he too but so-so. To be sure it rarely happens now that any one has something new to say, and communicate something proper to himself, the growth of his own brain, instructive and worked out with love and industry; so it is all one in the end."

An analytic Medical Review is one of the needs of the South, whither many valuable books never come, or come tardily; a Review which would give an exhaustive analysis of all that is new and useful in foreign and native works; works which are not the mere handbooks republished on the commercial principle. A few hundred dollars per annum spent in the purchase and speedy transmission to the South of the most able works appearing in insular and continental Europe would afford, upon careful analysis in a Medical Review, much practical matter of great value which is inaccessible to the practitioner in the South, thereby saving money and time as well.

Rev. I.—*Homœopathy : its Tenets and Tendencies, Theoretical, Theological, and Therapeutical:* By James Y. Simpson, M.D., F.R.S.E., Professor of Midwifery, University of Edinburgh; and Physician-accoucheur to the Queen for Scotland; President of the Medico-Chirurgical Society, and of the Obstetric Society: lately President of the Royal College of Physicians; formerly President of the Royal Medical Society, and of the Royal Physical Society, Edinburgh; Foreign Member of the Imperial Academy of Medicine of France; Member of the Society of Surgery of Paris; and of various Medical Societies in Stockholm, Copenhagen, Berlin, Ghent, etc. First American, from the third Edinburgh edition. Philadelphia: Lindsay & Blakiston. 1854. Pp. 302, 8to.

Homœopathy fairly represented; a reply to Professor Simpson's 'Homœopathy' misrepresented: By William Henderson, M. D., professor of general pathology in the University of Edinburgh. First American edition, from the last Edinburgh edition. Philadelphia: Lindsay & Blakiston. 1854. 8to., pp. 302.

Although the material interests of property, trade, commerce, as well as social rights, may, in an enlightened nation, be safely entrusted to majorities, yet many scientific truths in astronomy, metaphysics, anatomy, physiology, surgery, and therapeutics, which require years of profound study for their comprehension by even the greatest intellects, could not be submitted to vote with any reasonable expectation of a just verdict from the mass of the community. In medicine, majorities are not always on the right side, nor minorities always on the wrong; and the profitable side may be with the few as well as with the many—with the false as well as with the true—with the dishonest as well as with the honest; while neutrality for the sake of gain is little better than open warfare against the truth. A slightly differing friend is not far removed from an avowed enemy.

To the discerners of the spirit of our era, it must be evident that extremes form its characteristic features; witness its forward and retrograde marches—its brilliant discoveries on the one hand, and its ineffable delusions on the other. If the dark ages made less progress in the physical and medical sciences than the present, they were less disfigured by palpable fanaticisms and pretended revelations. The mythic in antiquity embodied the ideal in its most beautiful forms: our age, reversing the classical models, offers the most hideous instead, among which is quackery.

All of the quackeries which in every age have deformed the face of society, claim a common origin—claim to be the true interpreters of the eternal, immutable, paramount laws of Nature—claim to be truth itself, which is one and indivisible—claim to be founded on experience, which, when properly appreciated and translated, is but another word

for the truth. Appealing to their experience, the apostles of deception often deceive without being themselves deceived; they proclaim belief without believing—publicly solving the problems of health and disease, of life and death, of which inborn conscience tells them they know little if anything.

Homœopathists may boast of the intelligence of their followers, including all sorts of celebrities. Perhaps they can find in equal numbers of their clients as much general intelligence, if not more, than is found adhering to the regular profession. History shows that in physic this logic is invalid. All charlatanries have a good proportion of philosophers on their side—philosophers in everything but in medical science. The learned have always patronised innumerable delusions, as they now do spiritual knockings. These, however, meteor-like, pass away, to be replaced by others equally erratic and illusory; while the fundamental principles of medical science, like the fixed stars, have twinkled with more or less brightness, though often obscured by clouds of error, from the days of Hippocrates to the present era. The march of legitimate medicine is onward;—its pathway, illuminated ever and anon by new gushes of light, preludes for the future a more glorious day. No science can boast of discoveries so interesting to humanity. It is sufficient to name the discovery of the circulation—of vaccination—new remedies—new surgical processes—anæsthesia, or pain-destroying agents, and so on—all comparatively recent. It cannot boast, it is true, of a panacea adapted to the ever-varying condition of a single disease;—Charlatans of every era and of every land boast of infallible panaceas for all diseases. The fears and the credulity of the multitude supply the place of authentic evidence.

> "Tincture or syrup, lotion, drop or pill,
> All tempt the sick to trust the lying bill;
> With cruel avarice still they recommend
> More draughts, more syrups, to the journey's end;
> 'I feel it not'—'Then take it every hour:'
> 'It makes me worse;—'Why, then, it shows its power,'
> 'I fear to die;'—'Let not your spirits sink,'
> 'You're always safe while you believe and drink.'"
>
> CRABBE.

It may be doubted whether well informed Homœopathists believe the fundamental, peculiar, and differential principles which distingush Homœopathy from what is reproachfully styled Allopathy. If the homœopathists cured, and the allopathists killed all their patients, the

fundamental characteristics of the former would not be thereby established; for example, the efficacy or potency of infinitesimal divisions of a dose would not be proven, but simply this result—that non-medication is better than any medication. The expectant method of many French pathologists, albeit not homœopathists, would be confirmed—nothing more; least of all, the potency of smallness, and the omnipotency of negation.

On the score of general intelligence, the homœopathists are not, as before admitted, so vastly inferior to the regular profession as many imagine.* On all sides charlatanry is epidemic, and the more it is characterized by talents misapplied, by intelligence misdirected, and by sagacity which never deviates from the lures of self-interest, the more does its treason against humanity acquire a damning potency not found in any of its dynamizations, how much soever they be shaken.

Beyond the confines of the mathematics, the logician could scarcely find topics on which he could found propositions more evidently impossible and contradictory than those which constitute some of the fundamental principles of homœopathy. It may be doubted, as before intimated, whether the professed believers in this system believe in it at all; for sincere belief is not tested so satisfactorily by words as by actions. Many erroneous systems of belief are wholly ignored in practice. Thus it is with the science of phrenology. The lawyers, judges, and juries, never consult the bumps of benevolence and destructiveness, nor amativeness, in the prisoner's cranium, in a case of a suspected murder or infanticide. Victoria, Napoleon, Nicholas, Abdul Medjid, have at this moment armed navies ploughing the Baltic, the Mediterranean, the Ægean and the Black seas—mailed legions glittering in sunbeams and moonlight, converging rapidly upon the Danube, Bulgaria, Wallachia, Moldavia, and the confines of Western Asia, without having examined whether the bump of war or peace is developed under half a million of plumed helmets—whether these countless hosts are hastening to embrace and kiss, or mow each other down like grass. Were phrenology a practical science, it would be of infi-

* Professor Henderson of the University of Edinburgh, the most distinguished homœopathist of the day, in his recent work in answer to his colleague, Professor Simpsom, has the following as a part of the caption of one of his chapters: "*Homœopathy countenanced by great folks and by the clergy.*"

nite use in all the walks of life, and particularly in the science of killing.

Thus the philosophers who, with the learned Bishop Berkeley, believe in the non-existence of matter, and who contend that ideas alone have a positive existence, show that they do believe in fire, water, great guns, grape, cannister, swords, bayonets, smoke, lamp-posts, women, wine and potatoes—they call things by wrong names—belief, unbelief—certainty, doubt—demonstration, deceptive negation—a therapeutical nothingness—an intensified entity—a flat no—a consenting yes; all of which being admitted, it follows most logically that a grain of quinine, morphia, strychnia, or arsenic, becomes more powerful and dangerous the more it is divided, until

> ——————— "Nothing is
> But what is not."

Homœopathy, if legitimately carried out in practice, amounts to this, namely: it has no influence upon the sick except through the imagination. Nature, as all know, will often effect a cure where no drug, or even a hurtful one, is given. That homœopathists often cure their patients, is doubtlessly true. Instead of doses of infinitesimal nothingness, their patients swallow allopathic ones under a wrong name and in a most concentrated form. Pious frauds!

It has been shown, for example, in the cholera of 1849, at Cincinnati—a city in which homœopathy is somewhat endemic—that homœopathists gave "the strongest tincture of camphor in doses of one or two drops every five minutes; and from three to five drops every three minutes—equal to fifteen or twenty grains of camphor every hour."* Now although this may be called infinitesimal dosing, it exceeds the doses directed by the pharmacopœia of the regular profession. Cases nearer home might be mentioned. A letter is at hand, stating that in a Southern State, not far from Louisiana, one of the regulars, lately of high standing, now asks the sick, "will you be treated homœopathically or allopathically?" A gentleman well acquainted with a celebrated homœopathist in an adjoining State, says that the latter puts the same question; but has the goodness to say that homœopathy is his favorite method. Diplomacy can go no farther.

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* "The Cholera in Cincinnati," by the Rev. S. A. Latta, M. D., 1850.

Mr. Lee, cited by Dr. Simpson, says that

"A professor of homœopathy in Leipsic, was accustomed to ask his patients by which method they would be treated, as both were equally good."

"The fate of the Duke di Cannizzarro, well known some years ago in London as the Count St. Antonio, having to take homœopathic globules at intervals for some slight ailment, in order to save trouble he took three doses at once, and died two hours afterwards; the supposed globules having been a concentrated preparation of nux vomica." "Dr. Taylor, the distinguished toxicologist and chemist, of Guy's Hospital, found one-third of a grain of morphia in a powder *professedly* homœopathic and infinitesimal."

The following extract from Dr. Simpson's work can scarcely be called amusing: impending death and the unutterable griefs of the sick room, are little adapted to excite merriment:

"The author of the 'Confessions of an Homœopathist,' in referring to the due and adequate drugging of the Hahnemannic doses, amusingly remarks: 'Patients who are skeptical of the truths of homœopathy, from a love of variety, or a hundred other reasons, will consult you. As these persons are inclined to ridicule infinitesimal doses, it is sometimes highly useful to give them powerful doses of various highly concentrated medicines, in globules similar in appearance to all the rest, but consisting of morphia, strychnine, arsenic, corrosive sublimate, and such like: a few of these mingled with your sugar and starch globules, will cause effects to be felt by the skeptic which will quickly overcome his disbelief: he generally makes an excellent patient, and often a good decoy-duck. Never scruple in paralytic cases to give strychnine largely, but never allow it to be supposed that you are giving more at a dose than the one-hundred-thousandth of a grain. This rule may be followed in other complaints with other very active drugs, such as croton oil; but this is one of our profoundest secrets, and must be kept so. Were it known, our wonder-working powers would be reduced in the estimation of the public and the regulars.' "

Quackery is one of the social evils. Indeed, in a free government, it is an inalienable right. Attack quacks, they will make their fortunes—they will cry out, persecution! The public straightway sympathises with them. The law may fix fines and direct imprisonment as punishments in order to deter them from practicing, but in vain, as experience in Louisiana and elsewhere proves. Legislation against quackery is as powerless as logic, learning, and the decalogue.

The optimist, if not Professor Simpson, might find something to admire in homœopathy, inasmuch as it is the most decent, if not the most rational system of quackery extant. Were it honestly carried

out in practice, it would fullfil one of the golden rules of therapeutics often violated, namely: *do no harm;* or, as Moses hath it—"Thou shalt not kill;" though in a violent disease requiring active medication, infinitesimals would *do no good.* Homœopathy ought to be encouraged, provided there is reason to think that it will displace or prevent worse systems. Homœopathy will have its day. It is fashionable; and being fashionable, profitable. It was Byron who said' "cash is virtue." It was Burke who said of the beautiful, it must be small, without angles and so forth. What novelty, charms, philters, and fascinations corruscate from globulistic forms! During the last epidemic in New Orleans, a certain Æsculapian never gave less, but often more than four ounces of castor oil—a dose for an ogre. Some years ago another declared that the way he cured yellow fever, was this: he bled enough to float a 74 gun-ship, and gave calomel enough to load her. Homœopathy is doing good service by opposing such Allopathy as this.

Dr. Simpson comes forth mailed in logic; his good sword gleaming with syllogisms, to chop up homœopathy, little and beautiful as it is. He gives a full exposition of "its tenets—quotes chapter and verse of Hahnemann—homœopathic oaths—doctrines medical and theological—doses, potencies, triturations, shakes, dilutions, attenuations, and globules—showing the enormous potency of an infinitesimal dose taken by simply smelling—detailing how non-medicated masses take from the medicated globules by contact or infection, like small pox or itch, medical potencies of the highest kind—how "a single dry globule imbibed with a high medicinal dynamization, communicates, according to Hahnemann, to 13,500 unmedicated globules, with which it is shaken for five minutes, *medicinal power fully equal to what it possesses itself,* without suffering any diminution of power itself;" and, finally, the Professor offers the calculations made by four or five of the most eminent mathematicians of Edinburgh, based on the statements of the homœopathists, showing that the thirtieth attenuation or decillionth is as one followed by seventy-two ciphers to the primary drop—that the proper dose contended for by the homœopathists for any drug weighing one grain requires, in the fifteenth trituration or potency, a mass equal to sixty-two times the size of the earth. Hahnemann directs a higher potency or division, namely: the thirtieth; a grain

thus attenuated would require a mass as many times larger than the earth as are expressed by the following figures:

61,000,000,000,000,000,000,000,000,000,000.

One grain of a drug dissolved in one and a half tea-spoonfuls of alcohol would, at its thirtieth attenuation, require a mass of alcohol filling up the entire space within the orbit of Neptune—of this, a drop or globule forms a dose.

"Medical jurists and lawyers know of no poison, however strong and powerful, the billionth or decillionth of which would in the least degree affect a man or harm a fly. They tell us that the mere division of their drugs invests them with "tremendous" power, but they could not injure or affect the frailest insect with one of their "tremendous" billionths or decillionths. For what, in fact, is a billionth of a grain? Why, if a simple grain of sulphur were divided, as the homœopathists use it and other drugs, into billionths, and if our common parent, Adam, when called into existence some 6,000 years ago, had then begun swallowing a billionth every second, and if he had been permitted to live up to the present time, doing nothing but swallowing night and day sixty billionths every minute, he would as yet have completed only a small part of his task. It would require him to work and swallow at the same rate for 24,000 years yet to come, in order to finish one single grain of a drug, which has little effect on his present descendants in doses of 25 or 50 grains. Yet the homœopathists believe that a few of the sulphur billionths which Adam would have swallowed during these 30,000 years would cure, forsooth, an attack of jaundice. If we appeal to our standard—*the standard of common sense*—should not men holding principles of such unmitigated ridiculousness be requested to withdraw from this and other medical societies? But a billionth was, after all, only the sixth dilution of a grain. Homœopathists have dilutions far more refined, and doses far more infinitesimal than that. Some amongst them allege that drugs should always be used in their thirtieth dilution, or in decillionths, and Hahnemann averred that even the smell of such a dose is enough. When they speak of the decillionth of a grain they seem little to reckon what a decillionth amounts to. For it is a sum the mere figures of which can scarcely give us any conception of its infinitesimal amount, (viz:

1,000,000,000,000,000,000,000,000,000,000,000,000,000,000,000,000,000,000,000,000,000)

The world is computed to contain at the present time some nine hundred millions of human beings. If all these nine hundred millions of human beings had lived during the past six thousand years, and each of them had swallowed, every moment of their existence, a decillionth of a grain, such as the homœopaths use, they would not, during these six thousand years, have finished one single grain."

These absurdities have been exposed a hundred times in the medi-

cal journals and in the newspapers, not only without retarding, but probably with the effect of accelerating the march of homœopathy. Professor Simpson calls the bad, *bad.* But will his colleague, the professor of the Practice of Physic in the University of Edinburgh, Dr. Henderson, the homœopathist, be converted to the Simpsonian platform under the torture of arithmetical pincers, saws, wedges, racks, wheels, pullies and weights.

Science has its optimists—believers, that all things are for the best—that the millennial glory of perfect knowledge is at hand. But can they account for the prevalence of Mormonism, clairvoyant mesmerism, Silesian hydropathy, infinitesimal homœopathy, spiritual knockings, and ghostly saturnalia, as being indubitable proofs of the superior intelligence and profound researches of this progressive era? Is pure reason as yet popular in any one of the moral, physical and medical sciences? Has it ever been—will it ever be paramount? The Kants and the Newtons, the Bichâts and the Franklins, still belong to the minority.

Quackery always was, is now, and will be prevalent in company with ignorance, credulity, and illusory expectations, inviting arrogance, deception and cupidity to profit by the same, as the great fortunes thus made testify.

Wm. Henderson, M. D., probably the ablest defender of homœopathy that has appeared, in a work just from the press, in reply to his professional colleague, quotes and adopts Hahnemann's grand test of homœopathic treatment, namely, that the medicinal agent which causes a particular disease in the healthy, is the remedy which cures the same disease in the sick. Hahnemann* says:

"Those violent tertian and quotidian fevers which I cured four and six weeks ago by means of a few drops of cinchona tincture, I observed in myself yesterday and to-day almost exactly, after gradually taking, while in perfect health, four drachms of good cinchona bark, by way of experiment."

In the tropical margin of this Republic upon which the wave of homœopathy is now surging, the Jesuit's bark (cinchona) has been used in doses indefinitely large and small, for morning bitters, where

* Hahnemann—born in Upper Saxony, in 1755—died at Paris, in 1843; aged 88.

no disease existed, and for real and imaginary diseases—from debility, sore legs, and anæmia, up to typhoid and yellow fever—the human stomach and intestines have been filled with its active principle, quinine, at the rate of from one to two ounces in twenty-four hours, and yet not a single case of tertian or quotidian ague has resulted which could be attributed to this agent. Now as this is the *experimentum crucis* of homœopathists, it is useless to allude to any weaker test. The friends of Hahnemann declare that he, by

"Trying the peruvian bark upon his own person, was led to the discovery of the homœopathic law—Lond. Cyc., 1849, vii, 270—believing that the mode of operation of all remedial agents was in perfect harmony with this principle. It should be stated that, although these experiments have been often repeated, no uniform results have taken place. And thus the first law of *similia similibus* gleamed upon him."—*Ib.*, vi, 972.

In this, their strongest proof, they fail. The homœopathic world united cannot create in the sick or the healthful a well-defined ague by cinchona, though the latter may be as good as that which was swallowed by the Spanish Viceroy's lady, the Countess de Cinchon, who was cured of a fever by it, at Lima, more than two centuries since. Dr. Henderson seeks to qualify this great test: he says, with *naiveté*,

"That cinchona *does* produce symptoms of fever in man (though not in all) when taken largely in health, is attested by the experience of some dozen of persons on whom Hahnemann proved the drug." (235.)

The Edinburgh professor could easily find many systems which he deems erroneous, and which are attested by more than twelve experiments or witnesses. He might count them by millions. Verily, too much faith is as bad as none in physic. Dr. H. hunts up some northern authorities to prove his faith, namely, that chills and fevers are produced by quinine; of which hallucination he might cure himself by dropping down into the low latitude of New Orleans, where the use as well as the abuse of that medicine is remarkable, and where, during the long inundation, the quantity of water is great, whereby the lower dilutions, as is supposed, might be effectuated conformably to homœopathicity. He could then try the potency of his similars—whether the smell or the swallowing of an infinitesimal dose would cure congestive chills, algid fevers, and pernicious remittents and inter-

mittents. "The smaller doses are more powerful than the larger," as he affirms. One might as well be killed, therefore, by "the larger."

Dr. Henderson calls in requisition his ten years' experiences, from which he furnishes the four meagre examples which follow:

1. "A married woman took the one-hundredth of a drop of the spirit of turpentine for a dose, every four hours; it took five days to remove the morbid state—hematuria. 2. A middle aged lady, long subject to constipation, got the third attenuation of nux vomica—half a drop twice a day—in four days she was suffering from over doses of strychnine; every drop contained only the millionth of a drop of the mother tincture of nux vomica. 3. An old lady was salivated by the sixth attenuation of mercury. 4. One dose of sepia of the thirtieth attenuation produced dangerous effects."

These four cases, making a little over a page, are as unsatisfactory, futile and imperfect as can be imagined. In neither of the last cases does he mention how many drops were given or smelt. In the second case the millionth of a drop of the tincture of nux vomica (the active agent in which is strychnine) proved an overdose and poisonous; dangerous, because so small; regulars give from five to twenty drops for a dose; safe, because so large; equal to one-twelfth or one-sixth of a grain of strychnine. Dr. Henderson's four test-cases are the worst reported ones that have yet appeared in print.

Of this favorite remedy of homœopathists, strychnia, not one of the affiliated fraternity would for the universe take four grains, while no regular physician would hesitate to take himself, or give to his infant child, the awfully potentized dose of the millionth of a drop of the lowest or highest attenuation of the tincture. A few grains of the active principle will kill man, dog or alligator. Medicines are dangerous in large doses, and yet acquire increased potency by infinitesimal subdivision! If this be science, what is absurdity? Latin does not solve this mystery—Similia similibus curantur!

Hahnemann's provings prove, according to Dr. Simpson and others, that in a healthy person lime produces 1090 symptoms of disease—a statement which Dr. Henderson thinks is *probably* too high (270)—taking the lowest number of symptoms, according to his statement, they amount to 181,666; this list is formidable enough. Every individual ought to have at the least all these symptoms who drinks the ordinary hard (lime) waters of the springs, pumps and wells which

are used by more than half of this nation. The carbonate of lime is held in solution in every degree of dilution in the drinking water of the multitude, very generally, in almost all lands, as well as at the most fashionable watering places. The Sans Souci Spring, at Ballston, contains in 231 cubic inches of water, 247 grains of solid matter, nearly one-fifth being carbonate of lime, which, according to Dr. Simpson's reckonings of Hahnemann's provings, produce 1090 distinct symptoms!

But Dr. Henderson boldly assumes that the lime in these waters and in other substances is not absorbed. But if it is not absorbable how can the homœopathists make out their provings; that is, 1090 symptoms? Now, man being "of the earth, earthy," is little more than an animated column of lime, dressed up for a time in "the flesh," which itself contains lime, while his skeleton contains little else. "Lime," says Prof. Carpenter, "is one of the most universally diffused of all mineral bodies, for there are very few animal or vegetable substances in which it does not exist. The principal forms in which it is an element of animal nutrition, are the carbonate and phosphate.

A hen, be it known Dr. *Hen*-derson, that gets lime in no form, lays eggs without hard shells. Prof. Carpenter says:

"That the degree of development of the soft tissues depends in a great degree upon the supply of the carbonate of lime which the animals receive. Thus the mollusca, which inhabit the sea, find in its waters the proportion of that substance which they require; but those dwelling in streams and fresh water lakes, which contain but a small quantity of lime, form very thin shells; whilst the very same species inhabiting lakes, which, from peculiar local causes, contain a large impregnation of calcareous matter, form shells of remarkable thickness. The crustacea, which periodically throw off their calcareous envelope, are enabled to renew it with rapidity by a very curious provision. There is laid up in the walls of their stomachs a considerable supply of calcareous matter, in the form of little concretions, which are commonly known as crabs' eyes. When the shell is thrown off, this matter is taken up by the circulating current, and is thrown out to the surface, mingled with the animal matter of which the shell is composed. This hardens in a day or two, and the new covering is complete. The concretions in the stomach are then found to have disappeared; but they are gradually replaced before the supply of lime they contain is again drawn upon."—*Phys.* § 440, 441.

Prof. Henderson's labored "provings" that lime is not absorbable,

lacks proof. Common table salt, the non-homœopathic world will be astonished to learn, is little better than lime, causing just 450 symptoms in the healthy individual, and consequently cures the same number in a sick one. A list of these, as given by homœopathists, is the craziest that ever emanated from a mad house.

Crabbe (called by Byron "Nature's sternest poet, yet the best") speaks on this wise to charlatans of every grade, and his animadversions are none the less valuable for being "married to immortal verse." Hippocratic prose cannot be truer:

> "Can all the real knowledge ye possess,
> Or those—if such there are—who more than guess,
> Atone for each impostor's wild mistakes,
> And mend the blunders pride or folly makes?
> What thought so wild, what airy dream so light
> That will not prompt a theorist to write?
> What case so prevalent, what proof so strong,
> That will convince him his attempt is wrong?"

[EDITOR.

Rev. II.—*A Treatise on the Diseases of the Eye:* By W. LAWRENCE, F. R. S., Surgeon Extraordinary to the Queen, &c., &c., &c. A New Edition, edited with numerous additions, 243 Illustrations, by Isaac Hayes, M. D., Surgeon to Wills' Hospital, Fellow of the Philadelphia College of Physicians, Member of the American Medical Association, of the Philosophical Society, of the Academy of Natural Sciences of Philadelphia, etc., etc., etc. Philadelphia: Blanchard & Lea. 1854. 8to. pp. 948.

Although several excellent treatises in the English language appeared cotemporaneously with that of Mr. Lawrence, none have surpassed it in popularity, ability and usefulness. Founded on the authors's course of lectures in the London Opthalmic Infirmary and published first in 1833, and republished in Washington the next year—enlarged and revised in a second edition in 1840, it has grown so as to be nearly twice its original size. Its qualitative equals its quantitative contents. The typography, paper and illustrations are excellent, while the editor's name is a guarantee that the contents of the work fullfil the highest behests of opthalmic pathology and practice as they now exist. Although this book is a magnificent monograph; a Cheops among the pyramids of eye literature, it is not good for a review; being too massive; too long known; too much esteemed, and withal, too impregnable. As a material mass, its *vis inertia*, or static force, is too great for the dynamic force of the biceps and other muscles of the arm when

the book is placed at the wrong end of the lever, that is, in the hand. The work would make two volumes without being deficient in ponderosity. The putting of the volumnious works of an author, as Scott, Byron, Addison and others, into one volume, is a violation of the laws of finality or adaptation, and deserves the attention of the Bridgewater authors who adopted this fundamental principle in their speculations on Nature's works.

As the hanging of a man is often carried into effect upon circumstantial testimony, so may the apotheosis of an unread edition of a book like this, seeing the number, character, knowledge and competency of the witnesses who have pronounced favorably upon it. Although time and space are unlimited, a bi-monthly journal is neither; but is the antithesis, or, as Kunt would say, the antinomy of both. Even the title pages of the books which reach this flat margin of the Republic are almost enough to fill the Review department of the New Orleans Medical and Surgical Journal. In Nature (as the experimental philosophers will have finally to admit), the validity of synthetical judgments will be at last recognized, though in reviewing, without reading the work reviewed, such judgments are liable to be erroneous.

In the next edition of this eye-encyclopœdia it is hoped that a life of Mr. Lawrence, that is, his literary life, will be given, so that certain Æsculapians may know how it has happened that this gentleman, whose works on the Natural and Surgical Sciences, of which the eye forms but a part, have been written amid the hurry of a large practice. Indolence and ignorance are often masked under the hurry of business and the immensity of practice, so that there is no time to think, much less to write. Indeed the latter are sought to be placed as incompatible with the actualities of practice. Warren, Gross, Mott, Dunglison, Meigs, Dickson, Stokes, Graves, Watson, Andral, Louis, Velpean, and many more, have been as great in practice as in writing. If the number of physicians in New Orleans be nearly two to every thousand inhabitants; if there be but little sickness except during epidemics, which strike down for a time poor strangers—all of which may be maintained—surely there must be time enough to rival Philadelphia itself in making books, not to mention Medical Journals. At all events, the pretended antagonism between literary labor, logic and learning and practice, is a stroke of policy in which sciolists take

great delight, to the great danger of the unfortunates who swallow their blundering prescriptions.

Passing from this digression back to Lawrence and Hays on the Eye, the preface, by the latter, will close this notice:

"A new edition of Mr. Lawrence's Treatise being demanded," says the editor, Dr. Hays, "and the author having declined to revise it, we have, with his approbation, complied with the request of the publishers to prepare the work for the press. In the performance of this task, we have not felt that we should be justified in omitting any portion of the original, but have endeavored to make such additions as will render it a faithful exponent of the present state of opthalmic pathology and practice. In relation to one subject, however, the theory of inflammation, it has not been deemed necessary to carry out this plan. * * * Among the additions which have been made may be noticed a full account of the recent microscopical investigation into the structure and pathology of the eye; the descriptions of several affections not treated of in the original; an account of the catoptric examination of the eye, and of its employment as a means of diagnosis; a description of recently invented instruments for illuminating the retina, and of some new methods of examing the interior structure of the eye; two hundred and forty-three illustrations, some of them from original drawings, and a very full index.

There have also been introduced in the several chapters on the more important diseases, the results of our experience in regard to their treatment, derived from more than a third of a century's devotion to the subject, during all of which period we have been attached to some public institution for the treatment of the diseases of the eye."—[Editor.

Rev. III.—*A Practical Treatise on the Diseases of Children:* By D. Francis Condie, M. D.; Secretary of the College of Physicians; Member of the American Medical Association; of the American Philosophical Society, etc. Fourth edition revised and augmented. Philadelphia: Blanchard & Lee. 1853.

"Books, like children," says Crabbe, "when established, have doubtless our parental affection and good wishes; we rejoice to hear that they are doing well, and are received and respected in good company; but it is to manuscripts in the study, as to children in the nursery, that our care, our anxiety, and our tenderness, are principally directed. "Dr. Condie's "*children*" "are doing well"—"are received"—"are respected, and are in good company," and can take care of themselves. "It is with poems" (and books on children as well) "as it is with actions," says Goethe, "it is bad when they have

to be justified," or reviewed after reaching the fourth edition. Let Dr. Condie, therefore, look after his "manuscripts in the study," content with the verdict of the profession which renders a formal review a willful supererogation, and the more so as this last edition is, in its exteriorations and internalities (forgive "the un-English"), much improved—exceeding the first by nearly 100 pages. Wherefore, if the book be a bad one, Dr. Condie must be a great sinner, and cannot justify himself as did Miss * * *, who, having been charged with maternity, excused herself by saying *it was only a very little one.* Dr. Condie is responsible for 732 pages, octavo.—[EDITOR.

Since writing the above the editor's attention was accidentally directed to the following unfinished review of the works of Drs. Meigs and Condie on the diseases of children. It is, without doubt, the last lines penned by a hand that has "lost its cunning," palsied by the touch of death; the last lines traced by lustrous eyes, now shorn of their brightness; the last thoughts transferred to paper from a brain probably nearly as large as Cuvier's, now shrouded in the impenetrable darkness of the tomb, unagitated by the molten waves of passion and those strugglings after the great truths of science which seem always near, but still recede like the rainbow which the youth vainly tries to overtake.

This memorial is copied word for word, omitting only the titles of the books. It ends like his life, in the middle of a period, and apart from its literary merit, will be read with a mournful interest by his friends, with whom he has communed so often through the pages of this journal, now that he has passed to the realms of death:

"These two volumes embrace over fourteen hundred pages on the nature and treatment of the diseases of children; they were written by men of high standing in the profession, and emanated from the same book-making community. We think it a great pity that the two authors, after the French fashion of book-making, did not unite their talents and experience and give us the fruits of their combined labors in one medium sized volume. Howbeit, we must take them as we find them, and compare the one with the other on particular points of practice.

With the "*Hygienic Management of Children*," begins Dr. Con-

die, and he proceeds to point out the external agencies, both climatic and dietetic, which may tend to modify the sanitary condition of the infantile organism. Of air, temperature, cleanliness, bathing, clothing, food, sleep, exercise, moral treatment, &c., Dr. Condie treats at considerable length, and gives us some excellent rules for the government and preservation of the health of infants.

In childhood, the brain, lungs and bowels are the organs most liable to functional and organic disease; the first, because of its tender structure, its relative bulk and extreme impressibility; the second, because of their extreme susceptibility to external impressions; to climatic changes and the paramount importance of the functions which they are constantly required to fullfil in the work of hæmatosis; the third

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Rev. IV.—*The Transactions of the American Medical Association.* Vol. vi. Philadelphia: Printed for the Association by T. K. and P. G. Collins. 1853. 8to., pp. 869. Illustrated with plain and colored plates.

The Annual Meeting of the American Medical Association took place at New York on the 3d of May, 1853. Thirty States, Territories, the District of Columbia, the Army, Navy, and the American Medical Society of Paris, were duly represented by delegates.

Dr. Beverly R. Wellford, of Virginia, President of the Association, having delivered his address, received the thanks of the Association for the same, with a request that a copy of it be furnished for publication, which was conceded. Chaste in style, elevated in sentiment, this address aims at a grand finality necessary to the well-being of society, namely, the thorough education of medical men in all that pertains to the art and science of healing; Dr. Wellford wishes to invoke the power of the Legislature as a protection against quackery.

Although it is a long time since the editor of this journal has visited his native State, Virginia, he believes his knowledge of public opinion in that State warrants him in saying that the laws which Dr. Wellford desires, if passed by the Legislature, would soon or late prove a dead letter and a nuisance, as much so as the medical laws of Louisiana have proved; the laws of the latter were all that could be desired; yet in every legal attempt to enforce them in New Orleans, both the law and the regular faculty were defeated, and charlatanry chanted its *Io pæan* in the face of both. Such is public opinion—

and whether right or wrong the effect is all the same. So long as public opinion remains unchanged, all legislation for the purpose of protecting the regular profession and society against charlatans is useless, and to the profession injurious, giving to the latter the odious appearances without any of the benefits of a monopoly. The positive evils of this sort of legislation are not the less real because masked.

The following Reports, exceeding 800 pages, are without exception creditable to their authors, and are published in the order and with the titles following: Medical Education, by Dr. L. Pitcher—14 pages; Medical Literature, by Dr. N. S. Davis—38 pages; The Agency of Refrigeration, produced by upward radiation heat as an exciting cause of disease, by Drs. G. Emerson, I. Hays, and W. S. W. Ruschenberger—14 pages; Results of Surgical Operations in Malignant Diseases, by Dr. S. D. Gross—158 pages; Epidemics of Tennessee and Kentucky, by Drs. W. L. Sutton, T. Lipscomb, E. B. Haskins, F. A. Ramsey, and A. Evans—45 pages; Acute and Chronic Diseases of the neck of the Uterus, by Dr. C. D. Meigs—147 pages;* Typhoidal Fevers, by Drs. H. F. Campbell, and C. F. Quintard; Coxalgia or Hip Disease, by Dr. A. March—64 pages—a monograph of extraordinary merit, founded on an analysis of two hundred morbid specimens from all points of the compass,—whence he concludes, contrary to the common opinion, that spontaneous dislocation of the hip seldom or never takes place as the result of absorption, ulceration, or destruction of the ligaments of the joint and of the acetabulum, and of the contraction of the muscles surrounding the joint. Dr. G. Buck, Surgeon to the New York Hospital, submitted a report "on the Surgical Treatment of Morbid Growths within the Larynx," 26 pages, rich in facts of gloomy import, illustrative of a neglected branch of medical and surgical pathology—including a deplorable case, ultimately fatal, in which the author performed tracheotomy three times, for the removal of intra-tracheal vegetations and polypoid tumors. Dr. H. F. Campbell's paper on the Sympathetic Nerve in Reflex Phenomena, consists of 5 pages. Dr. W. L. Atlee, of Philadelphia, received one of the two prizes offered by the Association. His essay entitled "The Surgical Treatment of certain Fibrous Tumours of the Uterus," (95 pages) is of high prac-

* Prof. Meig's contribution, republished in a book, has been reviewed in this journal by a gentleman of New Orleans.

tical significance with but little speculative tendency; surgical eventualities here replace etiological generalizations—personal experience, historico-pathological induction; it is therefore an unanalyzable essay, but not the less valuable for that reason.

The Prize Essay of Dr. J. Burnett, of Boston, "The Cell—its Physiology, Pathology, and Philosophy, as deduced from original investigations—to which is added its History and Criticism"—a broad title—but an elaborate, though not an analyzable treatise, in which the past and the present, the transcendental and the experimental physiological anatomy, histology, and teleological anatomy—the finalities of development and microscopic provings are converged into a focus of 187 pages. Altogether it appears to be a very able monograph upon the microscopic phenomena illustrative of the higher generalizations of cell-physiology and histological anatomy; but inasmuch as it rises so far above the usual range of professional thought and education, its postulates if erroneous will seldom be detected—if just seldom tested and appreciated, and the more so as at the bed-side of the sick, little good comparatively has yet been achieved; and even in the study many conflicting and unsatisfactory findings must bewilder an understanding addicted to rigid logic unbiased by exaggeration and unseduced by microscopic illusions, although what is already known, preludes a glorious future for scientific histology and pathology.—Editor.

Rev. V.—*A Treatise on the Acute and Chronic Diseases of the Neck* of the Uterus—Illustrated with numerous plates, colored and plain: By Charles D. Meigs, M. D., Professor of Midwifery and the Diseases of Women and Children in Jefferson Medical College; Member of the American Medical Association, &c., &c. Philadelphia: 1854.

It is a very ungracious task to review a medical book and to do justice to all parties. There are three parties interested, each claiming impartial justice. 1st. The author of the work. 2d. The profession to whom it is to be a guide, and 3d. The public, who are to profit or to suffer, as its doctrines may be good or bad.

When the author is unknown, or without influence, everything he writes is severely scanned by the profession before it is made the bases of action, and the public are in no danger of suffering from any

erroneous doctrines he may advance, or from any mal-practice he may recommend. Not so, however, when the author, like Dr. Meigs, occupies the high position of a Professor in a popular medical school in a great city, renowned for its medical learning; when he has a large practice and a fame so far spread that patients from different States flock to him for medical advice; when he has age and experience on his side, and an unexceptionable character to recommend him; the opinions, doctrines and practice of such an author, whether right or wrong, so far from being scrutinized or examined into, are too apt to be taken upon trust, and to be adopted by that large portion of the profession who bow to authority and do not take the trouble of thinking and examining for themselves. But when a work, written by such an author, has also been *ostensively* approved by the American Medical Association and published in the transactions of that learned body, as Dr. Meigs' treatise on the diseases of the os uteri has been, it becomes doubly potent for good or for evil, because the unthinking class regard the *ostensive* as a *real* approval of the entire Association, or a majority thereof, in all the leading doctrines contained in the book. Whereas, in truth, it should be regarded only in the light of a communication sent to the Association, which was not read or discussed by the Association, but only by a committee appointed to draw it up. The Association could scarcely do otherwise than receive the report of the committee and publish it, as a matter of course, in the transactions of that body. By so doing none of the members had any obligation imposed on them to approve of the doctrines or leading views contained in the report, except those on the committee, consisting in all of but three members, Dr. Yardley, of Philadelphia, and Prof. Channing, of Boston, being the other two. But as our author, in the dedication of the book, takes occasion to deprive them of any credit in writing it, they cannot bear any blame for the thing written. So far, therefore, from the work being, as it might at first sight appear, an embodiment of American medical knowledge on the subject of the diseases of the os uteri, it is truly nothing but the essay of a single individual, our author himself, who alone is responsible for its doctrines and practical teaching. There is no evidence that the American Medical Association, or a single member of that body, has adopted it as a rule of faith and practice.

Having thus unlocked the defences thrown around the work by its connection with the Association, it lies open, like any other individual production, to the shafts of criticism. Yet, after all, so high is the position of our author, that it was difficult to find a person willing to risk his reputation *pro bono publico* by playing the critic or reviewer. The editor of this journal knows how urgently he asked for a review and how positively he was refused, time and again, by the present reviewer, on the ground that he had very serious objections to Prof. Meigs' work; that its author was among his friends, and he would not willingly wound his feelings by stating those objections; that few men have climbed so high in that better philosophy which can bear the friendly probe to be thrust into those tender places that human weakness and vanity may, by chance, have made. At length a circumstance, soon to be mentioned, occurred at the eleventh hour, when the Journal was going to the press, which produced the conviction that the interest of the profession and the public good imperiously demanded a review of our author's work by some one or other, and the same circumstance just alluded to, indicated that the unpleasant duty was allotted to him, and as a duty he performs it. He has played with a good many other subjects, but he has studied the diseases of the uterus.

Before entering on the merits of the work, it is proper to observe, that there is one feature of it very remarkable, considering that it was addressed to a learned body of physicians. A good portion of the book, including most of the pictures, would hardly have been appropriate if addressed to students. Indeed, most students, who have paid any attention to anatomy, or ever dissected a subject, would have their pride wounded in being suspected of ignorance on these anatomical points, which it seems to be the intention of a good portion of the work to teach and to illustrate by plates. Such, for instance, as the natural size, situation and appearance of the uterus and its anatomical relations. If the work was intended for the general reader, or to give popular instruction to those unacquainted with anatomy and physiology, the meaning of those anatomical plates and drawings of the old fashioned speculum would be apparent. But what possible use can they be to a physician or even to a student in the profession of an anatomy? At page 30, our author apologizes for "the want of artistic excellence" in these drawings, but they are

too pretty for the shop, and an apology is necessay for introducing them at all, if the work was not designed for the boudoir.

But to come to the merits of the book. At page 89, our author says: "*No one can deny that prolapsus uteri is a disorder or weakness of the vagina and its ligaments, and that to cure the prolapsus we must cure the vagina first.*" Almost every enlightened physician of Europe and America denies it. It is believed that our author stands almost alone in attributing prolapsus uteri, *as a general rule*, to such a cause. But what are the means he recommends to cure the vagina, accused so unjustly of being the cause of that evil heretofore attributed to the uterus and its ligaments, the laxity of the abdominal muscles and the impaction of the bowels? A hoop or ring pessary. We look in vain through our author's work for any other remedy, and find nothing but an allusion to astringent injections, which are recommended in certain cases. The ring pessary has been long before the profession, and has been found to be useful only in a very limited number of cases, principally such forms of prolapsus as occur in elderly women. At page 93, our author informs us that his attention was first called to the advantages of a ring pessary in the case of "*an aged woman*" by "*a nurse*," who constructed it of whale-bone. Whale-bone, perhaps, has some advantages over wood, ivory, &c., as being easier introduced, but any other merits are not perceived. But our author claims an improvement in pessaries—"*Made*," he says, "*of absolutely pure silver cylinders, bent into a circle and gilt with fire gilding.*" He further says, that "*this is perhaps the most perfect of these instruments, yet difficult to use and to obtain*"—(page 97.) Not having told his medical brethren exactly how he applies this instrument, or where it can be obtained, they will be compelled to send their patients to him to profit by its advantages, if it has any over other instruments of the kind. Indeed, one of the reviewer's patients, in whose hands he lately met with the work now under review (and which led him to consent to review it), was seriously contemplating a journey from New Orleans to Philadelphia to obtain one of those rings and have it applied by the Professor himself. She had been told that she had ulceration and hypertrophy of the uterus, causing the pains she complained of and the inability to walk. She quoted the book under review to prove that ulceration of the uterus was one of the rarest complaints imaginable, and not of frequent occurrence, as her physi-

cian said it was. She was satisfied in some measure on being informed that the thing called *moluscum* in Prof. Meigs' work, and mentioned therein as being very common, was the very thing which Bennet and other authors have almost universally agreed to call "*ulceration.*"

The reviewer deems it needless to tell the profession that a great authority of the last century, John Hunter, had had his attention called to this *moluscum* long before its discovery by the Philadelphia Professor. What the latter took to be something like a "*soft moluscum*" and left unexplained for the world to wonder at, John Hunter—great enough without a title—accurately described and explained as a phenomenon attending *ulceration*, viz: "That ulceration consists in a removal of parts back into the system by the action of the absorbents," and "it is evident," says Hunter, "that nature, in order to effect this object, must throw the part to be absorbed into a state which yields to this operation." It was this *softening* under the ulceration process that led the Professor into the error of supposing that he had made a discovery in medicine, and strightway called into requisition his knowledge of the fine arts to illustrate it. He rides the crayon hobby well; but it is plain he let it run away with him, otherwise he would not have given us a book of pictures not needed in the shop and dissecting room, but rather too attractive to the eye of public curiosity.

Our author seems to have but one remedy for all the acute and chronic diseases of the neck of the uterus, and that consists in what he calls "*antiphlogistic contacts with nitrate of silver.*" Strange to say, he gives no very special directions how those contacts are made. He tells how they are *not made.* "*They are not made by destroying the part,*" or "*by producing no effect upon the part.*" The distance between the limits he sets up is so great, that the nearest way to find what the true antiphlogistic contact is, would be to go to Philadelphia. The case represented by a drawing in plate the 15th, we are told at page 74, was treated for months "*by means of nitrate cauterizations destructive as to the hæmatome, but antiphlogistic as to the cervical ring.*" But he immediately afterwards, on the same page, confuses all the ideas his readers may have formed of what he means by "destructive and antiphlogistic contacts," by candidly confessing that the hæmatome or fungus, so far from being destroyed, was not reduced at all, except "perhaps some positive reduction." How the contact with

the crayon, as he calls it, could be destructive to the hæmatome and for the hæmatome to be but little reduced, if reduced at all, is a question he has left unexplained.

Next we have our author's "*nacent polypi*," as he calls them, introduced to our notice in the text, at pages 75, 76 and 77, and further illustrated among the pictures which make up nearly half the book. He describes them as "*small vivaces or bunches of red cellular tissue, peeping out from the os tincæ, or jutting beyond it, and attached by a delicate pedicle or foot stalk.*" He winds up the history of those cases with some very good remarks on the importance of a correct diagnosis in such affections, yet he fails to come to any diagnosis at all of the cause of these nacent polypi. The assertion that they resemble nacent polypi is no diagnosis. Duparcque, a French writer of high authority, says: "It is not uncommon to find the neck of the uterus studded with vegetations of various forms, volume and consistence. In the greatest number of cases these vegetations are soft and more or less bloody." But Duparcque shrinks from a positive diagnosis, and is rather inclined to the opinion that they arise from some remote venereal taint, and gives several cases, the 45th among the rest, in support of that idea. Hunter regards such excrescences on other parts of the mucous surfaces in the same light. The speculum was not in use in his day. The reviewer has had a longer experience with the speculum than the author under review—it having been introduced in this section of country anterior to its being adopted in Philadelphia. While he and those of the Northern cities, educated in the Puritanical school, were raising their hands in holy horror at the bare mention of such a means of exploring disease, the reviewer had already seized upon the speculum to investigate a variety of diseases; among the rest, the yaws, which appeared on several plantations in the circle of his practice. He long ago satisfied himself that the uterine vegetations the author speaks of, required the deuto-chloride of mercury, guaiacum and diaphoretics to effect a cure. Twisting them off and cauterizing the part would not prevent their return. Mr. Abernethy suspected they originated from distinct poisons. The constitutional treatment would effect a cure, in process of time, without the local, but united the cure was more speedy. That such affections are produced and propagated, aggravated, arrested, and also cured, in obedience to

the laws governing frambœsial affections more than syphilis, there is good reason to believe. Our author's 18th plate exhibits the same disease, but he does not seem to recognize it. Exactly a similar frambœsoid case was sent by the reviewer to Dr. Foster of this city, an expert surgeon, who excised it and applied caustic. It soon began to reappear, but was entirely dissipated, without a resort to caustic or the knife again, by the constitutional treatment for yaws.

In conclusion, it may be mentioned, that in one thing, very remarkable, our author is not singular. It seems to be the besetting sin, not only of him, but of the medical writers generally in our large cities, to have their eyes so steadily fixed *Eastward* on every petty compiler, book-maker and medical journal of London and Paris, as to be perfectly blind to what is going on in this great Continent where their lots have been cast. The idea never seems to have occurred to them that the circumstances which mould diseases into the forms met with in cramped-up London and Paris and their hospitals, do not obtain in wide, open, free America, and consequently the treatment, imported into this country, does not fit our diseases, like imported gloves, shoes and hats fit our hands, feet and heads. Yet no dandy is prouder in sporting his attire of the latest European fashion, or looks with more contempt on his countrymen in homespun, than the class of physicians just mentioned, in straining every new and fashionable plan of treatment, just imported from Europe, on their patients, and live or die, fit or no fit, making them wear it; scorning everything more fitting and appropriate discovered by their own countrymen. Hence our author has made no mention of American physicians West of the Schuylkill, or their method of treating uterine diseases, yet often quoting an old Madame Boivin of Paris. He does not seem to know there lived a man—he lived so far from Philadelphia, in the back woods—away down South in Virginia, and almost as far West as the Blue Ridge Mountain (Dr. John E. Cooke), who not only cured, in a few days, uterine hypertrophy, menorrhagia, prolapsus and leuchorrhœa, arising from visceral obstructions and engorgements of the liver, by a bold use of purgatives that touched the bile and brought away the atra-bilious matter, but taught others the same practice, ex-cathedra, in two medical schools in the Mississippi valley.

He does not seem to be aware of the superior virtues of iodine

over nitrate of silver, directly applied to the hypertrophied cervix in many cases. Nor has he made any mention of that other mode of reducing hypertrophy of the womb, as enlarged spleens are reduced by the *sang-dragon orientale.* He knows how to use Lallemand's port-caustic in making applications to the cervical cavity, but he does not know how to use a better thing—long, strong, fine staple cotton, wound round a flexible probang or gum-elastic male catheter of small size, with a strong wire in it, for the same purpose. His Lallemand's port-caustic tells him nothing of the morbid condition of the part to which it is applied; whereas the cotton gives accurate information of the condition of the parts beyond the sight—being stained with pus, mucous or blood, according to the morbid affection of the canal into which it is introduced. On prosoposcopia, or the art of being able to tell, by the countenance, the diseases of the womb, our author has not given a word of information. It is no very great stretch of medical knowledge for a doctor to be able, in many cases, to tell a woman what ails her, and to give her a minute detail of her symptoms, without her speaking a word—a great saving to her modesty and a key to her confidence. Good overseers can read negroes by their countenance. Hippocrates introduced the art of reading diseases that way—a way which our author has neglected to show. The great Luzenberg of New Orleans had some fine portraits of the expression of countenance of yellow fever in its various stages. Good drawings of the expression of countenance in the various forms of uterine diseases, would be a work which our author is well qualified to perform, and such would be its great utility, that it could not fail to immortalize his name. Nature, as if to spare female delicacy, has kindly stamped their peculiar diseases on their countenance in characters so faint, that none but the practiced eye of the physician can read or even perceive.

Our author uses the old fashioned cylinder, in place of the improved speculum with two or more blades. He objects to the bi-valve, because the folds of the vagina fall in between the blades, obstructing the view. But he surely knows that this objection only applies to elderly persons, or to those whose vital powers have been greatly exhausted by disease, or by the unphilosophical practice of too long confining patients, afflicted with uterine diseases, to a recumbent pos-

ture. In young women, unless the health has been greatly impaired, the walls of the vagina never obtrude themselves between the blades of the instrument. Our author tells us nothing of the use of the speculum in pregnant women, although he doubtless knows that abortion, in many cases, can alone be prevented by applications, made directly to the cervix, through that instrument. He raises no warning voice against ruining the health and spoiling the complexion by tanning the vagina with astringent injections—but sanctions the practice at page 36, in the very cases in which it does the most harm to check the discharge—viz : when there is no apparent disease, and "*the discharge proceeds from the muciparous apparatus*"—omitting to remind his readers that in such cases the discharge is vicarious—a kind effort of Nature to eliminate from the system some effete organized matter which the inactivity of some other organ has failed to throw off. To arrest a vicarious discharge by tanning the vagina into a kind of leather with astringents, must, from necessity, be ruinous to the organ, and to the general health. Besides the leucorrhœas, menorrhagias, prolapsuses, ulcerations and hypertrophies, caused by the disordered states of the hepatic and digestive systems, of which our author said nothing, there is a much greater number of such affections radicated in a defective hæmatosis and a deficient pulmonary exhalation, from an ascetic life, the depressing passions and sedentary habits. Our author does not seem to have dreamed, that when arising from such a cause, they vanish before the improved Willardian theory, or American doctrine of the motive power of the blood, called hæmatokinety, reduced to practice. These are, no doubt, the cases which he has found to be so rebellious to his crayon.

There was method in Dr. Cooke's madness in proposing to cure all female complaints by keeping up an artificial diarrhœa of atrabilious matter; there was method in the madness of Samuel Thompson, that prince of empirics, who tried to accomplish the same thing by a course of steam, lobelia emetics and red pepper. Both astonished the regulars in effecting many remarkable cures, in cases where their treatment had failed—but they were cases wherein the ground work of the uterine ailments rested upon those hepatic derangements and visceral congestions, so rife in hot climates and paludal districts, which European writers on the diseases of the uterus had never taken into account,

because they never saw them. But where is the method in that madness, imported from Europe, which proposes to cure nearly all such complaints by burning the womb with lunar caustic? If our author would visit the Choctaw nation of Indians, he would find that they are ahead of the French and English in a liberal use of the cautery—preferring the *actual* to the *potential.* They apply it outside on the pubis and back and not to the womb itself. European authors are beginning to recommend this practice, without giving the Choctaws any credit, who have used it from time immemorial. They make all kind of *touches* with the remedy, from those "*which destroy the part to those which produce little or no effect upon the part,*" including, of course, between the two extremes, our author's "*antiphlogistic touches.*" Alas! how long will the imported madness of treating all, or nearly all, the diseases of the os uteri with caustic, be epidemic among the shining lights of the American Medical profession? Can flashy pictures of the Rubens school, drawn from Nature as large as life, and ornamented with a surplusage of red drapery that nature disowns, spin out the day of such a delusion beyond the brief span of delusions in general?

SAMUEL A. CARTWRIGHT, M. D.

New Orleans, April 24, 1854.

Rev. VI.—*On the Subject of Priority in the Medication of the Larynx* and Trachea.—By HORACE GREEN, M. D. New York, 1854.—p. 17.

Professor Horace Green, M. D., of New York, has published a pamphlet asserting his claim of priority in the direct local Medication of the Larynx and Trachea by means of a sponge-probang saturated with the solution of the nitrate of silver, for the treatment of chronic and acute affections of these structures—a postulate which with good reason he desires to warrant and defend against all persons and claims whatsoever, as well on his own account as on the account of his friends, at home and abroad, who have privately acquiesced in, or publicly contended for his priority in this behalf. A great scientific association in Insular Europe takes for its motto—"To discover, not to apply." The opposite extreme, that of rejecting or neglecting discoveries, the application of which cannot be at once ascertained, is no small error. All discoveries have their preludes, which tend to prepare men's minds for their reception. But he who proves, discovers. The critics in both hemispheres, after some delay, having taken the parallax of Dr. Horace Green, agreed with few exceptions, very well in assigning his place among discoverers. But dissenters have lately discovered in the heavens, as they report, Dr. David Green as the fixed star, in place of Dr. Horace Green, who is nothing more than erratic nebulosity.

Dr. H. Green clearly shows how much others had done before him—how little they had left him to do—and how important his further advances have been; while at the same time he shows with what intensity he can hate his opponents—an intensity which in a calmer hour a magnanimous spirit will think of with regret.

It is not intended in this place to reproduce the authority of the competent critics who have given verdicts in his favor, but to give Dr. H. Green's summary of his claims in his own candid words. He says—

"This, then, constitutes a brief history of what has been done in Europe, by those who have employed the local application of caustics, in the treatment of diseases of the air-passages. By this, it will be seen, that no one had succeeded, or claimed to have succeeded, in

passing the sponge-probang, wet with the caustic solution, into the larynx, until after the announcement in my work, published in 1846, that 'it is an operation which, in the treatment of laryngeal disease, I have been in the practice of performing every day for several years.

"Previous to that time, the medication of the larynx and trachea by cauterizations, in the numerous forms of disease of these organs, had only been ventured upon by a few individuals in Europe; and in the practice of these, it was limited to the 'sponging of the back of the throat,' or, at the most, to the application of the solution to the apperture of the glottis, or, by pressure of the sponge, to the discharge of the fluid into the larynx. In this country, so far as I am aware, previous to that time, the employment of caustic solutions to the interior of the larynx and trachea, was 'entirely neglected.' Now, this treatment receives the sanction of, and is employed by, the most eminent men of our profession, not only in my own but in almost every country in Europe. It has not only proved successful in the treatment of follicular disease of the air tubes, and in the ordinary forms of angina, but eminently so in the management of many cases of whooping-cough, and of membranous croup. If there is any honor in the revival and introduction of this practice, *that honor I claim.*"

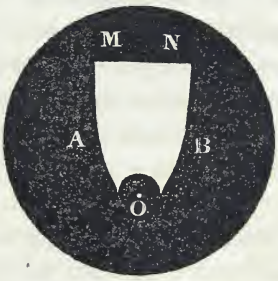
EDITOR.

Part Fourth.

MEDICAL INTELLIGENCE.

Art. I.—*New kind of Objective for the Microscope: By* Professor J. L. Riddell, M. D., of the University of Louisiana.

At a meeting of the Physico-Medical Society, April 15th, Prof. J. L. Riddell announced that, in accordance with a plan which he had formed several years ago, he was now engaged in the construction of a new kind of objective for the microscope, which seemed to promise superior results. It is simply an elipsoidal or paraboloidal reflector, made of homogenous glass. The following figure was shown as representing a vertical section:



The dot (.) at O shows the position of the focus of a parabola, or one of the foci of a very long elipse. At or near this point the object to be viewed is to be placed. Concentric with this point the lower end of the glass paraboloid is removed by a hemispherical concavity to an extent sufficient to give working focal distance. A and B are sides of the paraboloid. M N is the upper limit of the glass, a

transverse section of the paraboloid, which is either plane or slightly concave.

Rays of light emanating from points near O enter the paraboloid without sensible refraction; they impinge upon A and B and suffer total internal reflection, and then emerge without disturbance at M and N, to be ultimately received by the field glass of the ocular.

Prof. Riddell acknowledges that the mechanical execution of such objectives, would be considered as beset with difficulties. He thought, however, that the methods he had devised, and the means he had carefully provided, would enable him completely to overcome them.

For use in his binocular miscroscope, Prof. R. proposes to make this objective in two vertical halves. With ordinary (not erecting) eye-pieces, the binocular image produced by it will be both erect and orthoscopic.

Art. II.—*Medico-Legal Jurisprudence—Ethnological, Physiological* and Sanitary Observations. (Suggested by a recent trial indicated in a foot note.*)

Sociology,† whether it be considered as a natural or an artificial science, presents no question of deeper import in the South than the paramount one of *status*. The rights of property and the abstractions of philosophy "pale their ineffectual fires" before the stern and inexorable actualities of Race. The right to exist alone takes precedence over that of blood, even in this most democratic Republic. Physiologists may not be able to explain how African blood differs from the Caucasian and the Indian, contaminating both from generation to generation. The great world careth not for the physiologist. The bravest of the sentimental philosophers who ignore this distinction in

*Fourth District Court of New Orleans—George Pandelly vs. Victor Wiltz, embracing all the testimony adduced and taken during the trial of this interesting case, together with an Appendix, containing testimony taken in a former suit and not presented to the court and jury in this case. Published by John M. Burk, Book-binder and Stationer, No. 113 Chartres street, New Orleans: J. L. Sollée, 137 Chartres street. 1854.

†M. Comte, author of *Philosophie Positive*, the ablest critic, analyst and historian of the Sciences of the present age, uses this term, *Sociology*, fearless of the Academy.

theory, are the first to adopt it in practice, guarding with increasing care against contacts and fusions with an inferior race, setting at naught all its attempts to obtain social and political equality. Whether inferiority of race necessarily carries with it inferiority of social rights, is a question which transcends the legitimate limits of physiology. Inasmuch, however, as Race is a fundamental principle pervading forensic medicine, it becomes one of high import in this relation, while apart from this, it deserves the utmost attention of the physiologist, the natural historian, the comparative anatomist and the ethnologist.

The following remarks have been suggested by the late unparalleled excitement caused by a judicial inquiry instituted in the Fourth District Court of New Orleans, in relation to the status of a prominent and an esteemed citizen. The verdict, generally acquiesced in, not to say desiderated, was very anomalous in character, though favorable to this citizen.*

The testimony, historical, documentary, ecclesiastical, legal, social, traditional and oral, is fraught with physiological, sanitary, medico-legal and ethnological interest, covering a period of one hundred and fifty years. The knowledge which these witnesses had of the Indians is of the greatest value, because intimate and authentic. The number, character, agreement and motives of these affiants will not be considered, only so far as their testimony on a few of its interesting points, incidentally mentioned, relate to the physiological, ethnological and sanitary aspects of humanity for a century and a half upon the shores of the Mississippi. Among these witnesses are generals who "have set squadrons on the field"—statesmen and jurisconsults who have electrified Senates and Courts with their eloquence, and fair ones, who have done still more.

Sanitary Developments.—The ancient population, as the Creoles are usually termed, must strike the eye of the observer as thickly sprinkled with gray heads, indicating advanced age among both whites and blacks. Pass from the streets to the grave yards, and it will appear that the former generation lived, as the inscriptions will prove, to ages scarcely paralleled in modern times. The Pandely trial exhibits inci-

* "We, the Jury, find a verdict in favor of plaintiff, without damages. This verdict indirectly affected an extensive family connection among the oldest, most talented and worthy in the city, and was looked to perhaps as a portentous one to others, and the boldest "held his breath for a time."

dentally the longevity of the ancient population in a still more favorable light, for although the numerous witnesses may be regarded as selected ages in a certain sense, yet they prove, orally and by documents, that their ancestors were, like themselves, long lived, for a century and a half back. Some of these witnesses had never been beyond the limits of Louisiana, others seldom beyond those of the city. Feminine ages, rarely revealed to even the census-taker, were unveiled by the potency of judicial oaths; one confessed to 110 years; twelve children; pronounced "a curse on all laws but such as love hath made," or, to use her own language, she would "not marry her only husband, nor put herself in the yoke for anybody." So spoke the faithful * * *, now well advanced in her second century.

Of twenty witnesses whose ages are deposed to, there was one of 110; one of 93; one of 85; one of 80; one of 77; two of 74; one of 72; one of 70; one of 69; one of 68; one of 66; one of 64; one of 63; two of 62; one of 59; one of 56; two of 54; which afford an average life of 70:6 years—more than "three score and ten." Can the present effeminate, dissipated and fashionable generation promise itself the same patriarchal longevity?

A Creole lady, aged 59, deposed that her grandfather "could read the finest print without spectacles at the age of 98, at which age he died."

Another lady, aged 56, says: "My father at the time of his demise, eight or nine years ago, was more than 110 years of age." One of the most youthful of the witnesses testified that his maternal ancestor conquered an entire century before she departed this life. The documentary evidence, as the church records of burial, indicate great longevity.

The sanitary import of these facts is most interesting and deserving of elucidation. Sanitarians do not seem to be aware, much less do they attempt to explain the great longevity which probably obtains very generally in, and bordering upon the tropics, and which is susceptible of an easy explanation upon a well known physiological principle, namely, that the heat-producing process is impaired in advanced life, and the more so where the winter is bleak, cold and prolonged. Hence the snow of winter is the winding sheet of the hoary head, as the mortuary records of perhaps all countries testify; and it is believed,

even in New Orleans where the winters are mild, that this season proves the most dangerous to the old. In a warm climate where summer is eternal, the demands upon the vital powers for the continued creation of animal heat are less imperious and are less interfered with by external agents. Indeed the latter, in a mild climate, are auxiliaries, not the enemies to the calorifacient function of the old and feeble. New Orleans, often called the Crescent City, from its geographical configuration, deserves a more flattering *sobriquet*, namely, the Centenarian City—the City of the Ancients.

Ethnological Developments—The Hair—Color—Fecundity.—What has become of the populous tribes of Indians (an ethnologist might ask) which not long since swarmed in Louisiana? Mr. Marigny says that the Choctaws, from 1699 to 1785, mustered 8000 warriors. The French were under the necessity of temporizing with them. This interesting ethnological question will be waived in order to take up the testimony which more directly falls within the range of this inquiry.

The short woolly hair of the negro, the long, straight, coarse hair of the Indian, for a century back, as well as at present, was a subject of judicial inquiry, although its microscopic anatomy was not referred to. The terms used were numerous — ondée, crépus, gratiné, &c.

M. Bouligny deposed: Witness has seen a great many mulattoes and mulatresses, issue of negroes and white men, that have many of them flat, straight hair. He notices one instance: if the Court and Jury could see a girl at Mr. Daquin's of this city (her mother is a negress), they would see a perfect image of an Indian girl, she has the hair of a sauvagesse.

The same witness says of another woman: *She has the hair of a negress.* Has not examined the hair of ——— as an amateur, but saw it; she had *negro hair. She had the hair of a mulatresse* (les cheveux d'une mulatresse sont crêpus.) There is but one species of hair of a mulatresse, and this crêpu *(kinky* or *woolly)*, with but few exceptions. *Her hair was that of a mulatresse, very* KINKY *or* WOOLLY.

Joaquim Courcelle, aged 77, deposed: Has seen the hair of these people, he could hardly look at them without seeing their hair. The hair of * * * was the hair "d'une femme de sa classe," *(of a woman of her cast,)* that of a mulatresse. Deponent says that her hair was undulating, *(ondée)* that sometimes when she was well combed and dressed, the hair seemed to be more flat, without being more straight on that account; but, when she had her hair in *négligé*, her hair was more undulating, and was raised [higher.] Saw her head *en négligé*, and has seen her sometimes having her hair well combed, but cannot say often.

The following deposition is a puzzling one for the ethnologist who takes his postulates and proofs from book worms:

Pierre Deverges sworn: Says he was born in 1806 in this city. They had in their family two negroes imported from Africa. Their hair was as black as their skin, and straight, a little curled *(ondée.)* They were brother and sister. The brother had his mind deranged, and became notorious from this fact. Witness'

family is an old family of this country. The name of the black African referred to by witness was Célestin. That boy came from the Foular nation, on the coast of Africa. The brother and sister both came from the coast, and had the same features, they both had *noses* such as *negroes* have, to-wit: flat. They had piercing eyes, they were first very intelligent, until one of them, the boy, became deranged. They looked like all negroes, with the solitary exception of the shape of the hair.

A female descendant of the Pawnee Indians, born in New Orleans, aged 110, deposed that Mme. ——— "had very *straight* hair; the last time witness saw her, her hair was *white* and as *straight* as witnesses; (witness exhibits her hair.) When she was young she heard M. called by the name of "*la belle sauvagesse*" (the beautiful Indian); but in those days, when Indians were low in public estimation, those of Indian blood did not relish the name of *Indian*, but preferred to be called and considered as mulattresses or quarteroons. * * * * F. when young had the most beautiful hair witness ever saw; they formed a roll as thick as witness' arm; when undressed, they fell below her waist; they were jet black and straight as a line. * * * Witness was a slave, as Indians were then slaves; Indians being caught in their own country or tribes, were brought here and compelled to labor. Witness served as a slave, because she knew no better, and submitted to power. Witness never left this State; never went further than the Red Church."

A gentleman deposed concerning this female witness, to whom he paid an official visit, "that she opened her dress to let witness see her breast; let down her hair for witness to examine; her hair was very straight and gray; her color was that of a half-breed by a white man; witness *saw her daughter, who was of a brighter complexion*, and was *more like an Indian than her mother*."

These significant facts, confirmed by the Right Rev. Bishop Portier, Gen. Waul and others, here and elsewhere, cannot fail to strike the attention of ethnological physiologists of the different schools.

"Mme. had the color of a *sauvagesse;* she was redder than a mulattress, or a yellow person."

"Great distinction was made about fifty years ago between the Bohemians, Mulattoes, Quarteroons and Negroes."

"She was of a tawny (*basanée*) complexion, like all old mulattresses and Indian women."

A free woman of color, born in New Orleans, aged 72, swore that her father was white, her mother was black and "had a straight

nose." She deposed to a lady's hair, which was "very black, straight, coarse, not curly, not *ondées*." Another she characterized as having high cheek bones, a straight nose—*la sauvagesse*—both regarded as mixed with the Indian blood.

This witness lived with a gentleman fifty years—had seven children, all now living—facts not favorable to hybridity, sterility, and short life among mulattoes.

In the South, whatever infertility may be noticed in mulattresses is owing, not to their whiteness or hybridity, but to their dissoluteness. The same truth applies to the white race of like immorality.

The Right Reverend Bishop Portier deposed that Mme. * * * "had real Indian features, high cheek-bones and a peculiar shape of the forehead; besides, there was something serious, and grave, and an air of melancholy about the face, which is characteristic of the Indian race. The impression was corroborated in witness' mind by the differing complexions of the children in the colored or African race; as the family descend, the first descent is half African and half white; the quarteroon contains one-fourth of the African and three-fourths of the white, and so on. The African blood *disappears*, as it were, in succession of time. It is not so in the mixed white and Indian blood; for the writer observes, and he has known facts to the same effect, that the Indian blood and features will reappear in the third and fourth generation. Witness thinks that the same observation applies to * * *, some being more brown than others, and some having the distinctive Indian features.

It was among the children of * * * that witness observed this distinction. The color of * * * and her children was a clear white color, and Mrs. * * * and her other sisters had the Indian mark.

Witness had known the family of * * * here. The father was a Mexican and had Indian blood, and was keeper of the king of Spain's store in Mobile. The father married a Miss * * * in Mobile, a white person: the son married a Miss * * * of New Orleans, also a white lady: and there is a considerable difference in the color and features of their children; some look like squaws and are very dark, and others are of white color. This family occupied a respectable position in Mobile, and no question ever arose as to their status and position in life. There is nothing more mysterious than the products or offspring of mixed races."

General Plauché: In his younger days had many occasions to see Indians, they were much more numerous then, and the complexion of the Indian races differed very much in the different tribes, some were darker than others. Has not noticed that when Indian females get old they become more tawny.

General T. N. Waul, sworn, says that he has had occasion to see the complexion, features, and appearance of various tribes of Indians, particularly of the Southern Indians.

There is a great variety of complexion among them. So far as he has seen the Indians of the South, the Choctaws and Chickasaws are very nearly of the same complexion. The Cherokees are lighter, the Creeks are darker. The Seminoles become much darker, and the color of the skin increases as they approach the sea board. The Karanquois of Texas are the darkest of all. Some of the Mexican Indians, of whom he has seen a few specimens, were as black as African negroes although they had no African blood in them, and their hair was perfectly straight, and their features more prominent than those of the North American Indians—sharp features, high brows. They resemble in appearance as nearly as witness can compare them, a Spaniard or Castilian painted black.

Witness observed the old people in the tribes to be generally darker than the young people.

Has seen officers and soldiers of the Mexican army and the Mexican population generally.

So far as the army is concerned there is every kind of feature and color among the soldiers. So far as the families are concerned, he remembers the family of Castillo, a very respectable one. Some of its members were as black as negroes, others were brighter. They enjoyed all the rights of citizens in the State of Texas, civil and political. The first time witness saw one of the black brothers come to vote, he inquired and found that the question of legitimacy had never been contested.

Deponent heard Bishop Portier's cross-examination with respect to the question submitted to him about the effect produced from the mixture of the Greek race with any other race in this country, and his answer that he never thought of it, and that there is nothing more mysterious than the product or offspring of mixed races.

Answers: Witness also says that he knows of nothing more mysterious than the product or offspring of mixed races; but from his observation of the Indian race the most mysterious thing of all is, that in families said to be descendants from a mixed ancestry, a portion of which is Indian, no matter how remote, children are frequently born, showing in complexion, hair and appearance, the Indian ancestry to a great or greater extent, than the appearance in the ordinary half-breed.

Witness says the families he has known most remote of the Indian race to which he has alluded, claim to be descended from Pocahontas.

Being requested to say how many instances occurred under his own observation, of the Indian blood being returned or shown for a number of generations,

Answers: I have known three or four instances where families that were said to be descended from Indians, and in all of whom there was occasionally some member of the family plainly and distinctly marked with Indian lineaments and complexion. There was invariably a great distinction between the members of the family, and there were always some members that did not retain their resemblance of Indian origin.

Colonel Hamilton Smith, a great admirer of the *morale* of the negro, quotes several authorities to show that some tribes in Dongola and Sennaar have one lumbar vertebra more than the white race! (Nat. Hist. Man. 101.) This statement is not given as one entitled to belief, but to show how deplorably uncertain the anatomy of the black race is. It is but little creditable to the scientific character of the southern portion of this Republic, where Providence has cast the lot of nearly four millions of blacks, that almost nothing indisputably reliable has been published concerning the anatomical, physiological, and pathological peculiarities of a population so extensive. One class of *savans* contend that the negro blood when mixed with the white, soon produces a set of sickly hybrids; another that this intermixture improves the race—making it more intellectual, vigorous and fertile, and long lived. One says that the Indian blood never can be washed out—the Indian conformation reappearing in all its essential features, in the descendants of the mixed red and white races forever, while the negro blood fuses with the white, all its primary elements becoming merged, is completely lost! These enigmas, hypotheses and con-

tradictions might be solved, verified, and reconciled by hosts of witnesses and observations. Men having the wish, the will, and the energy to undertake these investigations, might put these questions, now doubtful, at rest forever; and while thus earning immortality for themselves, confer benefits upon others. The field is American; its cultivation *ought* to be by Americans. Let contagion and quarantine and miasma, and the causes of yellow fever, cholera and the like, rest, being unfruitful; let them be replaced with inquiries more hopeful of satisfactory results.

A private letter which recently reached the latitude of New Orleans from an American functionary to a distant foreign court, expresses a wish to get further proof of the anatomical facts which have been advanced purporting to show that mulattresses have few or no ovarian cells. For if they have been thus spayed by nature, they must be as unproductive as so many animals, spayed by art, and consequently do not belong to the human species at all! Hence, in the same vein, others say they are hybrids! It is surely time that anatomists should look into these matters, and not allow the imagination to run riot, while the facts lie at their door. Although the New Orleans Medical and Surgical Journal is not tinctured with these new-fangled beliefs, it is open to conviction upon evidence of a reliable character.

John Bachman, D. D., of Charleston, one of the ablest of living naturalists, in his controversy with the late eminent Dr. Morton, of Philadelphia, thus addresses the latter—*

And now I will endeavor to answer the question you have put me—"how long would the mixed breed of mulatto offspring last, were they compelled to marry among themselves?" You answer "not beyond the third or fourth generation." My reply is, they would last till the day of judgment. I have resided in situations where I have possessed the amplest opportunities of observing the fertility of these mulattoes. The males and females are equally prolific. Among individuals with loose morals, they are in this respect characterized by the same tendency that exists in the whites similarly degraded; but, even here, the fertility of the mulatto female is decidedly greater than that of the white woman under the same circumstances. At the moment I am writing, my eye is from time to time directed to a free mulatto carpenter, superintending the building of an adjoining house. I knew his respectable parents before him, and am acquainted with this man's children—they are in color what are usually called light mulattoes—they have for generations past married with those of their own color and grade. This man weighs about one hundred pounds more than either of us—all the brothers, sisters and relatives, have reared large families of children; I doubt, indeed, whether among any of our white inhabitants instances of greater fertility can be produced. Could you favor me with a visit here, and examine some fifty families that I would

* Charleston Medical Journal, September, 1850.

be prepared to point out to you, I am confident you would greatly modify your statement of their dying out after three or four generations, if you did not entirely abandon the ground you have assumed. Although I have seen mulattoes that have arrived at a great age, I am not prepared to say that as a general rule they attain to the age of either of their predecessors. Still they cannot be said to be short lived—they raise large families of children, and I have often supposed that they were even more prolific than the whites. I have seen on an average a greater number of cases of sterility among white females than I have discovered among the mulattoes. Our records are so imperfectly kept, and your theory of repugnance is so little carried into practice, that it is not easy to trace the mulattoes who have regularly intermarried beyond five or six generations, but as there is no greater tendency to sterility in the sixth generation than there was in the first, and as sterility is even rarer among them than in the whites, we are warranted in believing that in this particular they partake of the characteristics of the admixture of the Caucasian with the ancient Huns, who for ages and centuries have continued to increase and multiply as rapidly, and are as healthy and long lived as either of the unmixed races from whom they have originated. Indeed, I have seen the descendants of an admixture of all the five varieties of Blumenbach, and probably one-fourth of the world is now composed of individuals of mixed blood, yet I have never seen any races that evidenced a tendency to sterility. The American Indians, as far as I have been able to ascertain, are less prolific than any others, but the causes of this must be traced not to any intermixture, for such products I have always found more fertile than the native Indians, but to the slavish drudgery of the females, and to the irregular wandering lives of deprivation and suffering to which the tribes are exposed. In the lower animals it is the same—all varieties are prolific with any other variety of their own species."

Without venturing to take ground as to which of these gigantic intellects should have precedence as authorities in the natural history of man, it may be said that he who has descended to the tomb, mourned wherever science is loved, had fewer opportunities of observing the black race than Dr. Bachman, whose home has always been in the hive of the American Africa.*

Dr. Bachman's views of the fertility of the mulattoes are confirmed in New Orleans, where, if anywhere in the Republic, they can be fully tested; because the free colored population of this class is here the most numerous, wealthy, intelligent, and *white.* In fact the *white,* after a few fusions, has, in not a few cases, almost completely, perhaps quite effaced the *black element,* so that the past rests on a few vague expiring rumors, and not on ethnological characteristics. The colored element, thus merged and lost, can boast of a fecundity, and probably health and longevity equal to the pure races of like habits and morals.

---

* The sun has its spots. Intellects cast in a gigantic mould sometimes assume an imperial attitude as well as altitude. The following Bachmanian corruscation upon the darker masses of humanity below, is what it is:

"The naturalists of America are composed of a very small band. You (Morton) and another eminently distinguished naturalist and friend, who welcomed me with cordiality, and honored me with his confidence when I was an invalid and a stranger in a foreign land, and whom I love as a brother, *are the only two individuals in our country with whom I would be willing to discuss this subject even in a single line.*"

The disputed doctrine of the unity of the human race is not now under consideration. But, if the doctrine of mulatto-hybridity be an unwarrantable assumption, and, if, on the contrary, the anatomical and physiological characteristics of the black race soon merge themselves in the white, then it follows that the white and black races are less remote from each other; less separated by indestructible barriers, than the Indian and white, provided that the statements made by the witnesses in this trial, and by others, shall be found to be true upon further and more complete investigation.

The *morale* of this judicial drama, the *dénouement* of which was of unutterable import to a large family connection of elevated position, including indirectly one of the most eminent orators of modern times, awakens associations which it were useless to amplify. Let the following picture suffice: "Mme. was now led into court by her son and brought before the jury. She was requested to unloose her hair for the inspection of the court; all agreed in describing the appearance of Mme. as an elderly lady, small in stature, with prominent Indian features, of a copper color, with deep set eyes, a high forehead, high cheek bones and with long straight hair." But it is said justice is blind, and saw not this scene—the unveiled face, the disheveled hair—for forensic examination.

Take another, the gifted gentleman alluded to, the father of nine children, *with an increasing family*, was, as the testimony disclosed, recently at the head of a college having fifty pupils, and in prosperous circumstances; but the *rumor* reduced his pupils to five; his family to distress, and his library to seizure for debt; whereupon the son of genius returned to New Orleans, his native city, and simultaneously with the prolonged trial alluded to in this paper, delivered a course of lectures upon Ancient Literature, in a strain of surpassing eloquence to brilliant auditories! Here is an example of the moral sublime in action that beggars fiction, and is honorable to humanity.

The importance of this trial, as the possible forerunner of others, has been scarcely appreciated. It will be seen by the reader how carefully the present writer has sought to avoid names and intrusions upon the sanctity of the fireside.—[Editor.

Art. III.—*Amputation at the Shoulder Joint.*

THIBODAUX, La., March, 1854.

DR. DOWLER, *Editor New Orleans Medical Journal:*

Sir—It is usual for publication to be made of all important operations in surgery, more especially if the operation be performed in a city, by an aspiring surgeon. And while we are, from the force of obvious circumstances, compelled to admit that a city combines more scientific men than the country, we do not admit that the city has all the science, to the exclusion of the country.

And in proof of this, sir, permit me to say to the lovers of surgical science, through the medium of your valuable journal, that quite recently, in the town of Thibodaux, an operation in surgery was performed which demanded for its accomplishment the highest degree of skill that falls to the lot of any one man to possess. The operation to which we allude is the shoulder joint operation. It was performed with great skill, and equal expedition, by Dr. F. C. Ewing, in the presence of several other physicians. It is an old, and no less true saying, that the highest degree of human accomplishment is only found in the possession of the truly modest. This, sir, holds good in reference to Dr. Ewing. His extreme modesty forbade him to give publicity to his own skill; but neither his scientific attainments nor his modesty have been lost sight of by your friend,

S. E. MCKINLEY.

The above letter is published with the hope that a circumstantial history of the case alluded to may be elicited, and the more so, because rumors have been more or less current of a contradictory character. Dr. McKinley's letter, how accurate soever it may be, gives no particulars, and does not expressly mention whether he was present at the operation.

There can be no danger of damaging any one's "modesty" by doing good in the cause of humanity and truth. Both the patient and the surgeon are interested in knowing what are the chances of success from an operation so formidable, and which has seldom been performed, even on the field of battle, except by Baron Larrey. Dr. Peaslee, of the New York Medical College, who recently performed this amputation, says that only twenty-six cases in all have been

reported in this country. Dr. S. Smith, junior, editor of the New York Journal of Medicine, whose statistical researches on this operation have been extensive and elaborate, says that Dr. John Warren, of Boston, first performed this operation in America in 1781, and that the hospital reports of France show 18 cases and 13 deaths; the British, 28 cases and 11 deaths; the American, 15 cases and 8 deaths.—(N. Y. Jour. Med., Jan. 1853.)

Professor Ferguson remarks—"The celebrated instance recorded by Cheselden, of Wood, the miller, who had his right arm and scapula torn off by machinery, and several of a similar kind which have been seen and recorded by Carmichael, Dorsey and others, have exhibited the wonderful powers of Nature in sustaining life, even after such frightful and extensive mutilations." "It is only within the last half century," says Professor Pancoast, "that amputation at the shoulder joint has been admitted as a regular process of art."—[Ed.

Art. IV.—*Medical Legislation.*

It appears from the last number of the American Journal of Pharmacy, that during the recent session of the Legislature of Virginia a bill was introduced, requiring that the box, bottle, or envelope, containing any nostrum or quack medicine exposed for sale, shall have connected therewith a label, in which the ingredients of such nostrum or quack medicine shall be printed in English. "Five of the Richmond papers were entirely in opposition to the bill, but whether their sympathy with quackery had any connection with their income from quack advertisements," or whether the druggists, who at a regular meeting opposed the bill, as an interference with the profits of their trade, are questions of little importance, because the Ancient Dominion has always allowed quackery unlimited ingress, egress and progress. Such a law, unexceptionable though it be, could not be enforced.—[Editor.

Art. V.—*Legislative Reports on Public Health :* 1854.

A Majority Report and a Minority Report were submitted to the Legislature on this subject; both being characterised by ability, commendable research and zeal, though somewhat different in their conclusions. The limits of this Journal will not allow of a fair analysis of the one or the other. Only portions will be given at present:

That the yellow fever *never* takes its origin or propagates in a pure atmosphere.

That epidemic yellow fever, cholera, influenza, &c., depending on atmospheric constituents for their origin and propagation, *are not contagious*, as the term is generally understood, viz: a disease depending upon a specific poison, generated by, and emanating from a person, and producing its like in another.

That the yellow fever *can be* and *has been* of domestic origin in hundreds of instances, in different localities and periods, during the last hundred years.

That we are convinced that *no quarantine* regulations will have any protecting effect in preserving the health of the city of New Orleans or the State from the spread of epidemic diseases.

That the only possible good to be derived from any sanitary regulations, would be by the appointment of a health officer, to be stationed at the Balize, to go out with the pilots, and while the vessels are coming into the river, to examine as to the existence of ship fever and small pox, and these diseases only. In case of ship fever and small pox, clean the ship, wash the bodies and clothes of the well, and send them on their way rejoicing; the sick to be detained under some cheap, airy shelter until well enough to travel.

We believe no possible benefit can be derived from the simplest or most complete system of quarantine at any other locality, and the process above suggested at the Balize is not put forth to operate as a *system* of quarantine, but for the sole purpose of preventing the herding of the sick and filthy immigrants into the already crowded and ill-ventilated tenements of the poorer classes in New Orleans.

We would suggest—That every street in the city and its suburbs should be paved.

That every lot in the city and suburbs should be graded to a height *above* that of the surrounding side-walks, &c.

Thoroughly drain, by ditching or grading, all the swamp land in the rear of the city, exposing no more swamp land to the action of the sun's rays, by felling the timber (its natural protection), thereby creating a source of malaria equal to the famous Pontine marshes near Rome.

Erect a sufficient number of water works on the river to throw a constant stream of fresh water down every gutter in the city and su-

burbs, at all times when the river is too low to flow the water without the works.

A better construction of the dwellings of the poorer classes, viz: airy and well floored, high and dry above the surface of the earth.

E. E. KITTREDGE,
F. A. WILLIAMSON,
J. A. BRAUD,
CHAS. SEUZENEAU.

From the Majority Report on Public Health, by Messrs. A. Trudeau, R. W. James, B. Olney, R. M. Kearney, and J. S. Williams, of the Legislature of Louisiana (1854), the following statement is taken:

Yellow fever proper cannot be imported and spread *without the existence of an atmospheric predisposition to that disease.* If we cannot prevent yellow fever here—if it be a disease of a strictly local origin—if the cleaning of our streets, the draining of our swamps, and all sanitary measures must fail, we may at all events contract the sphere of its action by depriving it of that infectious character which it took in 1853.

Art. VI.—*Statistics of the Charity Hospital for* 1853.

From an official Legislative document in the State Journal (the Louisiana Courier) of the 29th of April, 1854, the following Statistics of the Charity Hospital for the year 1853, have been translated from the French and condensed:

Total admissions, 13,759—of which number 10,126 were males, and 3,633 females; discharged, 10,733—of which 7,971 were males, and 2,762 females; deaths, 3,164—males 2,426—females 738. In 1852 the admissions were much more numerous, amounting to 18,031, while the deaths (2,098) were about one-third less. The nationalities of 1853 were distributed among these admissions thus: United States 1,306; Foreign countries 12,338; unknown 120. Of foreigners 7,217 were Irish; 2,260 Germans; 843 French; 519 English; 415 Prussians; 195 Swiss; 144 Scotch; 74 Swedes. 68 Danes, &c.

The principal causes of death were, of yellow fever 1,890; (cured of this disease 1,427); diarrhœa 117; dysentery 110; cholera infantum 115; typhus 59; typhoid 57; pernicious intermittent 20; remittent 12; consumption 199; coup de soleil 12—leaving only 573 deaths from all other causes.

The ratio of admissions declined from January to April, and augmented from May to July, then slowly declined to the end of the year.

It is very remarkable, that the ratio of admissions of natives of the United States was not affected by the epidemic, having been, from January to May, 551 for these five months. The next five months gave precisely the same number, 551! During the first-named period of five months, the Irish admissions amounted to 2,750; during the next five months to 3,541.

Births in the Hospital for 1853: boys 61; girls 74; still-born 24: total—159.

Art. VII.—*Sanitary Remarks.*

The salubrity of the cities of the South, including New Orleans, is satisfactory, and demands no special notice. While the New Orleans Medical Journal will conceal nothing, suppress nothing as to the true sanitary condition of the city, how unfavorable soever that may be, it will indulge in no dismal predictions calculated to excite terrorism, for the simple reason that medical science has discovered no data, no precursors upon which to found a reliable calculation whether an epidemic will or will not appear during the current or any other year.

Art. VIII.—*Medical Memoranda.*

Necrology.—M. Roux, the celebrated Surgeon of the Hôtel-Dieu, at Paris, died on the 25th of March last, from the effusion of blood in the brain; aged 75.—(*Revue de Thér.*, April, 1854.)

Fistula in Ano, has been successfully treated by injections of the per chloride of iron, by Dr. A. Miergues.—(*Ib.*)

Headache.—Dr. Murphy thus classifies Headache:

The Anæemic.	The Congestive.
The Rheumatic.	The Periosteal.—(*Lancet.*)

Treatment of Nævus.—Dr. Cumming has treated 8 cases of nævi; of which number 7 have been completely cured, by tartar-emetic plaster; the cicatrices which followed, though large, were not unsightly.—(*Ib.*)

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THE NEW ORLEANS MEDICAL AND SURGICAL JOURNAL.

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EDITED BY

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Editor's Office—Notices.

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BOOKS AND PAMPHLETS RECEIVED.

I.—*Woman: her Diseases and Remedies.* A series of Letters to his Class: By CHAS. D. MEIGS, M. D., Professor of Midwifery, and the diseases of women and children, in the Jefferson Medical College at Philadelphia; Member of the American Medical Association; of the American Philosophical Society, and one of the Council; Vice President of the College of Physicians of Philadelphia; late one of the Physicians to the Lying-in department of the Pennsylvania Hospital, &c. &c. Third edition revised and enlarged. Philadelphia: Blanchard & Lea, 1854. pp. 672, 8vo. From the publishers through Mr. J. B. Steel, bookseller, 60 Camp street.

II.—*Handbook of Chemistry, Theoretical, Practical and Technical:* By F. A. ABEL, Professor of Chemistry at the Royal Military Academy, Woolwich; and Assistant Teacher of Chemistry at St. Bartholomew's Hospital; and C. L. BLOXAM, formerly First Assistant to the Royal College of Chemistry; with a preface by Dr. HOFMANN: and numerous illustrations on wood. Philadelphia: Blanchard & Lea, 1854. Pp. 681, 8vo. From the publishers through Mr. J. B. Steel, bookseller, 60 Camp street.

III.—*The Science and Art of Surgery; being a Treatise on Surgical* Injuries, Diseases, and Operations: By JOHN ERICHSEN, Professor of Surgery in University College, and Surgeon of University College Hospital: Edited by JOHN H. BRINTON, M. D. Illustrated by 311 engravings on wood. Philadelphia: Blanchard & Lea, 1854. Pp. 908, 8vo. From the publishers through Mr. J. C. Morgan, bookseller, Exchange Place.

IV.—*Transactions of the Medical Association of the State of Alabama* at its Seventh Annual Session, held in the City of Montgomery, Jan'ry, 1854. Mobile: Dade, Thompson & Co.—Pp. 190, 8vo.

V.—*Quarterly Summary of the Transactions of the College of* Physicians of Philadelphia: From February 1, 1854, to April 5, 1854, inclusive.—Pp. 98.

VI.—*Principles and Practice adopted in the Orthopædic Institution of* Brooklyn: By Louis Bauer, M. D., and Rich'd Barthelmess, M. D. New York: L. W. Schmidt, 1854.—Pp. 39.

VII.—*Remarks on Croup and its Treatment:* By Horace Green, M. D. New York, 1854.—Pp. 21.

VIII.—*An Essay on Follicular Diseases of the Throat and Air* Passages: By J. A. Wilson, M. D., of Memphis, Tenn.—Pp. 11.

IX.—*Essay on the Mechanism and Management of Parturition in* the Shoulder Presentation: By Wm. M. Boling, M. D., of Montgomery, Ala. Charleston, 1853.—Pp. 91.

X.—*Eleventh Annual Report of the Managers of the State Lunatic* Asylum, Albany, 1854.—Pp. 55.

XI.—*On Spiritual Manifestations:* By the Rev. Jerome Twichell, A. M., New Orleans, 1854.—Pp. 15.

XII.—*Causes of Contagious and Epidemic Diseases, with Hints for* their Prevention, and Reform in Medical Police: By M. M. Rodgers, M. D. Rochester, 1854.—Pp. 31.

XIII.—*The Transactions of the Iowa State Medical and Chirurgical* Society: Third and Fourth Sessions. Held at Fairfield, May, 1852, and Davenport, June, 1853. Burlington, 1854.—Pp. 48.

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THE NEW ORLEANS

MEDICAL AND SURGICAL JOURNAL,

FOR JULY, 1854.

Part First.

ORIGINAL COMMUNICATIONS.

ART. I.—CATARACT IN THE AGED; WITH PHYSIOLOGICAL AND PATHOLOGICAL REMARKS.

BY BENNET DOWLER, M. D.

Suffusio quoque, quam Græci ὑπόχυσιν nominant, interdum oculi potentiæ, qua cernit, se opponit.—Celsus de Medicina, Lib. vi. § 35.

1828, Jan. 15th.—Mrs. Ancram, aged 85, of good constitution, had been blind from cataracts in both eyes, for seven years before I saw and advised her to submit to the only means of relief, namely, surgical operations. This advice was given incidentally during a visit made to a gentleman ill of typhoid fever, in Mrs. A.'s family, in Tyler county, Virginia. Mrs. A. was much rejoiced to learn that there was a probability of regaining her sight, and was anxious for me to operate without delay. As Tyler county was destitute of the necessary surgical apparatus, a messenger was dispatched for mine, a distance of about forty-five miles.

Assisted by my brother, then one of my pupils, now a practitioner of this city, namely, M. M. Dowler, M. D., I proceeded to break up the cataracts in both eyes. It was my wish to operate at first on only one eye, (the left) at the same sitting. During the operation, Mrs. A. exclaimed, "I see a glimmer of light!" and as soon as the almost fluid lens and its capsule of this eye had been disentegrated, she surprised me with an urgent request to operate on the other. To this request I consented the more readily, as it would save me a journey in future of ninety miles should the operation upon the right eye have been deferred.

The operation upon the right eye was a little painful; the pain increased subsequently, and radiated through the head, accompanied with nausea, vomiting, and sleeplessness; symptoms which, however, during the night were relieved by opiates.

Jan. 16th.—Right eye inflamed, though not very painful. Mrs. A. was permitted for an instant to try the visual powers of the left eye; saw objects plainly, and recognized the countenances of her friends instantly.

Jan. 17th.—Could read large print in an almanac, without spectacles.

April 3d.—The pupil of the right eye, irregular, and partly occupied by opaque shreds of the capsule of the lens. The vision of the left eye is as perfect, or rather is more perfect, than is usual with persons of her advanced age, since, without spectacles, she can distinguish printed letters.

1829, March 10th.—The left eye is in the best possible condition for a person of her age; the right is somewhat inflamed, in connection with a recent catarrhal fever. In 1833, and again in 1836, I heard that this lady still could see well with one eye; a period of eight years after the operations above described.

1828, Jan. 16th.—In the same neighborhood, within twenty-four hours after having performed the above operation, I was called to operate on Mr. Smith, a robust farmer, aged 72, who resided in the family of his son. As the final result of this case was learned chiefly by means of letters from the family, it is proper to state that the parties were highly respectable and altogether reliable for veracity.

Mr. Smith has been blind from cataracts for five years. The pupil of the left eye greatly dilated; permanently fixed, and not perfectly circular owing to adhesions between the iris and lens; the latter bulges forward prominently into the pupil. A strong light produces slight oscillations, at intervals, along the iris, some portions of which are free or have but slight adhesions to the capsule.*

In the right eye the lenticular opacity is apparently incomplete—the pupil large—the iris movable, responsive to the light. Although

* Adhesions are not necessarily fatal to the success of an operation. Sir C. Bell says, (Anat. 1834,) "I should refuse to operate when the pupil is rugged and irregular, because the disease may be more extensive than it appears to be;" this dictum is contravened by the same author, (see his Op. Surg., vol. 2, p. 44,) who says, with truth, that "irregularity of the pupil is an unfavorable circumstance, but not a bar to the operation."

Mr. S. can distinguish between day and night as is usual in cataractous patients, yet useful vision is wholly lost, a guide being necessary when he attempts to walk the plainest road.

While operating on the left eye, the numerous adhesions between the iris and the lenticular capsule suddenly gave way, the former contracting strongly after having been, as it is believed, permanently dilated and fixed for several years.

Immediately after withdrawing the cataract needle, the patient could discern objects, though it was deemed unsafe to attempt to test the accuracy of vision fully by any particular examination of minute objects; the eye was therefore almost instantly closed.

The capsule and lens of the right eye were tough and hard. During the operation, the iris or pupil dilated enormously, and then suddenly contracted. In this eye pain was felt during the operation. Directed sulph. magn. for aperients. Rested imperfectly—a little pain in the right eye.*

Jan. 17th.—Right eye painful.

April 4th.—Mr. Smith sees a little laterally with the left eye; in the right, the cataractous opacity is increased. On dilating the pupils with an infusion of stramonium, the ires were found to be adherent to the lenses or their capsules in sundry portions, giving the pupils serrated or angular margins. The lens of the left eye had almost wholly disappeared or dissolved; the greater portion of the capsule remained, constituting membranous cataract; this was lacerated and removed from the axis of vision, but owing to some remaining minute threads of attachment to the iris, rose again. During the operation upon the right eye, which contained the hard cataract, an extravasation of blood into the posterior chamber of the aqueous humor occurred, embarrassing to the operator from the obscuration of the aqueous humor and pupil.

Immediately after the operation upon the left eye, vision was in a good degree accurate, although much of the pupil was obscured by shreds of the capsule having filiform attachments to the iris.

The patient was bled. Sulphate of magnesia, as occasion required, with a low diet, was directed.

---

* Unfortunately, my notes of this case do not mention whether the cataract in the left eye was, at this first operation, soft or not. These notes, taken from my first MS. volume of researches and experiences, now grown to 22 vols. folio, are like most beginnings, defective.

April 30th.—Learn by letter that his sight is restored.

1829, Jan. 1st.—Learn by letter that he can travel, work, and discern small objects without spectacles.

I subsequently learned by letter and the testimony of individuals, that his sight continued good until his death in 1833.

I have operated with various success on several other persons, whose ages ranged from 72 to 79, but it is not necessary to detail their cases for the purpose of illustrating the fundamental aims of this paper—among which are these: the physiological necessity of the lens—the expediency of operating on the aged, and other considerations which the sequel will disclose.

"No one has been known who could read an ordinarily printed book without the aid of a convex glass, after the removal of the crystalline; while such instances as that of the postillion, related by Sir William Adams, confirms the belief, that one eye, if young and otherwise uninjured, may accommodate itself in a very considerable degree to the loss of the crystalline, so far as distant objects are concerned." —*Brit. and For. Med. Chir. Rev., Oct.* 1847.

Mr. M'Kenzie, a very high authority, it must be admitted, says, in his elaborate Treatise on the Eye, "it is an error to suppose that the eye has the power to change its focal distance, after being deprived of its lens. It is necessary to use cataract glasses, double convex or plano-convex." This statement appears to agree with the mechanism of vision, or rather with the functions of the artificial lenses of optical instruments, yet my own experience does not coincide with it even in the very cases wherein the necessity of cataract-glasses would seem most obvious, namely—among the very old. If in a single case it can be proved that the lens is not essential to vision, this theory must fall, even if it be proved that glasses are commonly necessary.

It is probable that the site of a displaced or absorbed lens, is speedily replaced by the vitreous humor, which adapts itself in such a form as to compensate in refractive power the loss of the crystalline humor. The reproductive and adaptative power of the vitreous humor is remarkable, as is often seen in traumatic lesions, particularly in unsuccessful attempts to extract cataracts. I have seen the protrusion and loss of a large portion of the vitreous humor immediately after the division of the cornea, before the cataract could be extracted; yet, experience shows that such losses in healthy individuals are of temporary duration only.

Whether the lens itself may not be regenerated, is a question that experimenters might investigate in comparative physiology with a fair prospect of arriving at satisfactory results, by the destruction or removal of this organ in the inferior animals, and, then after a lapse of time, ascertaining the condition of the eye. I am not aware that an extended and satisfactory series of experiments have yet been made, illustrative of the reproductive powers of the living economy in relation to the crystalline humor. The following meagre account is taken from Muller's Physiology:

"It would appear that, in certain cases where the lens has been removed, it is reproduced by the capsule—its matrix. Leroy d'Etiole has observed this. In the first case, thirteen days had elapsed since the extraction of the lens when the eye was examined; in the second case, thirty-three days; in the third, ninety days; in the fourth, thirty-one days; in the fifth, forty-six days; in the sixth, one hundred and sixty-five days. The experiments were made on rabbits, cats, and dogs. The contents of the capsule were either a crumbly mass, as in the second case; or a small lenticular body, as in most of the other cases; but in the sixth case a full-sized lens was found."—(120.)

Were the physiological theory of Lamarck well founded, nothing would seem more natural than the reproduction of the lens. According to that great naturalist, whenever an animal wants an organ, nature goes to work and supplies the organ needed, and that such organ so supplied, is never permanently lost, but is invariably transmitted in all its integrity to all succeeding generations. Yet, dreamy as this theory appears, there are some facts reported which, if true, would seem to favor the Lamarckian doctrine. If, for example, an animal be placed in a condition where the eye (of all organs the most complex) is not wanted, as in perfect darkness, and, if, as a consequence of this, the annihilation of the organ, now useless, should occur, this fact would seem scarcely more extraordinary than the creation of an organ or any important part of it, as the consequence of a prëexisting want. "This high argument," though apparently a negation, is in a certain sense an affirmation of the theory of Lamarck. Now, in the Mammoth Cave of Kentucky, this theory becomes (if the expression will be allowed) a concrete fact. For, in that case, as some naturalists affirm from actual dissection, certain animals, as fishes, insects, &c., are completely eyeless. The affirmative of this argument might possibly be illustrated by placing some

of these eyeless animals in the light. In such a medium eyeless fishes might need eyes to escape from enemies, &c., and it would then be seen whether conditions of exteriority or internality—whether exigencies and wants, can create the most complicated and noble of all organs—the Eye!—that point of departure for the physiological anatomist, the priest of natural theology, and the teleologist who finds here the doctrine of adaptation—the means and the ends—the purpose, the grand finality.

But to return from this digression: it is worthy of remark that most writers upon the operations for cataract, are silent as to the necessity of cataract-glasses. Doubtlessly some person require glasses while others do not.

If the lens be invariably necessary to vision, an artificial, to replace the natural lens would be necessary for a double reason where one eye only is cataractous—necessary for the eye operated on—necessary to the sound eye in order that both should have the same focal distances.

Physiology can scarcely go so far as Dr. Arnott, who asserts, "that the eye is nothing but a simple Camera Obscura, formed of the parts essential to the Camera Obscura." *(Elements of Physics.)*

Mr. Samuel Cooper *(Dict. Surg.)* says: "That the patient would never be able to see distinctly after the operation, (upon the cataractous eye,) by reason of the difference of the focus (foci) in the eyes, is (I have grounds for believing) only a gratuitous supposition, transmitted from one writer to another."

The density of the lens has been supposed to exceed that of the vitreous humor by one-eleventh; the specific gravity of the former being 1.079; of the latter 1.0009. Now, it is probable that the greater power of refraction incidental to the superior density of the lens, may, at least, in some cases be compensated by the altered configuration of the vitreous humor and the adaptative powers of the living economy. Mr. R. Owen, an eminent naturalist, in his Notes upon Hunter's Animal Economy, says, "it has been shown in the well-attested case of Henry Miles, that the eye may retain its power of adjustment after the removal of the lens." *(296 Am. Edit.)*

Artificial dilitations of the Pupil:

As all know, strammonium, belladonna, and the like possess the singular property of causing a relaxation of the circular or sphincte-

roid fibres of the iris, leaving the transverse or radiated fibres which converge upon the circular, active, thereby dilating the pupil, which latter, is on the other hand, diminished by the contraction of the circular. These agents are often very useful in breaking up recent adhesions between the uvea or posterior surface and margin of the iris and the capsule of the lens; they are advantageous in expanding the pupil where the operation of breaking up the lens is performed through the cornea; but they are not called for—indeed they have proved inconvenient in their action when the operation is performed through sclerotica, as I have found—because as soon as the lens becomes detached it is apt to slip forward through the pupil into the anterior chamber of the aqueous humor where it can neither be depressed nor divided, and where, if it be hard, it causes inflammation before it dissolves and disappears. Any manipulation of the dislocated lens from behind the iris, may injure the latter and bring on traumatic iritis which is the chief danger in this operation.

Can cataract be cured by medication without a surgical operation?

Recently, M. J. Leport, oculist, at Rouen, in France, obtained the prize proposed by the Medical Institute of Valencia, in Spain, for the best essay upon the cause and cure of cataract. M. Leport maintains that medical treatment never cures cataract without an operation:

Mon opinion est que jamais jusqu'à présent on n'a guéri un seul cas, même au début, de cataracte vraie, à l'aide de moyens médicaux; qu'une grande partie des gens soidisant guéris de la cataracte sans opération ont été guéris d'une toute autre affection par des médecins exploitant le charlatanisme, ou par des praticiens qui avaient commis une erreur de diagnostic.—*De la cataracte mémoire couronné*, 11.

In the Virginia Medical and Surgical Journal, for April, 1854, an article digested from the researches of Dr. Garcia Lopez and others, gives a more hopeful view of the medical treatment of cataract:

The majority of surgeons consider the cure of cataract without an operation a chimera. And yet it is impossible to deny that cataract is sometimes spontaneously cured. Is it then impossible that therapeutics may some day realize what nature occasionally accomplishes by herself? This question is often asked by unprofessional persons. Medical men have uniformly denied the possibility of the cure of complete lenticular cataract by medical treatment, while impudent and shameless charlatans have industriously propagated the opposite opinion among all classes of society. In the clinical reports of Dr.

Beauvais,* are recorded five *homœopathic* cures of cataract, one of which, treated by Dr. Caspari, was evidently a case of corneal opacity depending on trichiasis. The offending cilia were removed *surgically*, the patient took one drop of *cannabis* daily, and recovered entirely.

In the work of Tavignot the reader will find an account of the experiments of Professor Pugliatti, of Messina, on the curability of capsular or capsulo-lenticular cataracts without an operation. This surgeon supposes that a strong solution of ammonia, applied at the internal orbital angle, penetrates the tissues of the eye, and exerts a curative effect upon the opaque crystalline system. He combines with this treatment the administration of five grains of iodide of potassium daily. He reports several cases which were benefitted, and one or two which were supposed to be cured by this means. In every case the treatment lasted many months. In the majority of cases no amelioration was perceived. In the Spanish journal *El Porvenir Medico*, for December, 1853, we find four cases in support of the efficacy of the treatment of Pugliatti, reported by the author, whose name we have placed at the head of this article, Dr. Garcia Lopez:

1.—A man of fifty years; nearly complete capsular cataract had existed for three months: vesication with ammonia, without any internal treatment, procured resolution in two months. 2.—A man of thirty; soft, striated, lenticular cataract, two years old; capsule healthy. After six weeks of treatment the patient demanded an operation. Dr. Lopez found the lens diffluent and lactescent. 3.—A woman of forty; hard, incomplete capsulo-lenticular cataract in both eyes, developed within a year. She could only distinguish very large objects. After seven months of treatment the patient recovered her vision, and could sew and follow her usual avocations; the opacity had disappeared except in a limited central point. 4.—A woman of fifty; capsulo-lenticular cataract had existed for three years on the right side; a similar opacity was forming in the left eye, complicated with amblyopia. In six months, after the constant employment of ammoniacal vesication and iodide of potassium, a decided amelioration took place in the left eye; the right was unaffected.

Dr. Lopez reports these cases without any desire to attach undue weight to them, and can hardly be supposed to be prejudiced in behalf of a method which he did not originate.

There is another remedy to which much consequence is attached, to which Dr. Lopez does not allude, we refer to the iodine ointment, allowed to dissolve on the conjunctiva. The editor of the Paris *Gazette hebdomidaire*, (Dec. 1853,) reports a case of double capsulo-lenticular cataract cured by this method.

In a succeeding article we have given a full account of the mode of treatment of cataract depending on capsulitis. The reports of the

* Annales d'Oculistique, tom. ii., p. 218.

Medico-Chirurgical Society of Richmond, contained in the present number of the Journal, contain the histories of two cases of traumatic cataract, and one of supposed congenital cataract, in which the advantages of a free use of mercury in opacities of the crystalline system connected with inflammatory action are strikingly exemplified.

In all such cases, and immature cataracts, no harm can arise from instituting medical treatment during a period which would be otherwise lost in awaiting the opportune moment for an operation. It must be confessed, however, that Lebert's researches into the structure of cataract, demonstrate that in the great majority of cases medical treatment must be unavailing. Rognetta was of the opinion that an absolute denial of the efficacy of remedies in cataract was unwise; we concur in this sentiment, and trust that farther researches will be made in this interesting and obscure subject.

Is Cataract Hereditary?

Mr. W. W. Cooper relates the following interesting facts: His late Royal Highness, the Duke of Sussex, informed me that cataract was brought into the present Royal Family by the marriage of one of his ancestors with a princess of Saxe-Coburg Gotha. She became blind from cataract, and the following members of the Royal Family have since been afflicted with that disease: The Duke of Cumberland; George the Third; George the Fourth; the Duke of Gloucester, the Duke of Sussex assured me that such was the case, and that it was one cause of his seclusion. Three years ago a young woman came under my notice with congenital cataract, and she stated that her grand-father, father, uncle, aunt, and three sisters, had all been born with cataracts.—*Lond. Jour. Med.*, 1849—*cited by Dr. Hays in Lawrence on the Eye.*

Do Concussions, Wounds, and Inflammatory Affections of the Eye, produce Cataract?

Although in a great majority of cases of cataract, the causes producing it remain unknown, yet, inasmuch as the cause of one variety of this disease, that is, the traumatic is indisputably traceable to inflammation, the latter might upon the principle of analogy afford presumptive proof that all cataracts may have a similar origin when no orther cause can be suggested as probable. The ideas usually associated with the word inflammation are vague and often erroneous: thus, neither redness nor vascularity is an essential element of inflammation; for example—the arachnoid membrane of the brain, when inflamed, is erroneously described by nearly all writers as being red,

vascular and injected; whereas, inflammation of this tissue causes opacity, thickening, increased tenacity in, and fibrinous and watery exudations from, this membrane; the vascularity, redness, and injection being seated in the sub-arachnoid tissue and pia mater and seen through the semi-transparent arachnoid. Even pain is sometimes absent in slow changes of an inflammatory origin and character. Thus, it may happen in chronic capsulitis, lentitis, and crystallino-capsulitis. In fact these known inflammatory products, in the absence of all other ætiological knowledge of cataract in general, should be viewed by the pathologist as the possible or provisional types of even congenital and non-traumatic opacities; instead of concluding dogmatically with M. Leport, (already quoted) that inflammation must be wholly excluded from the causation of cataract, he may take the known cause as a guide to the unknown. M. Leport says:

"L'opacité de la capsule antérieure survenant pendant une capsulite ou une irido-capsulite, ou bien à la suite d'une lésion traumatique, ne peut jamais constituer une cataracte. Ce genre d'opacité est très souvent curable, et on ne doit lui donner le nom de cataracte que lorsqu'elle persiste longtemps après que tous les symptômes inflammatoires se sont dissipés."

On the other side of this question, the following extracts taken from "The Virginia Medical and Surgical Journal, for April 1854," will be read with interest:

Extracts from the Records of the Medico-Chirurgical Society of Richmond, Va. By the Secretary.

Jan. 3, 1854.—*Two cases of Traumatic Cataract.*—Dr. Parker related the following case: A lad of nine or ten years received a blow on the eye from a small pebble, which inflicted a slight wound of the cornea. There was not much vascular excitement of the conjunctiva; the pupil was clear; the movements of the iris were natural, and there was not much pain. Under these circumstances, Dr. P. was satisfied with directing the application of cold compresses to the eye-ball, and the use of a mild astringent collyrium.

One week afterwards, he saw the child again, and was astonished to find a well marked opacity behind the pupil. The capsule, in fact, was of a milky white color throughout its whole extent. The wound of the cornea had been converted into a circular ulcer, but it did not penetrate the interior chamber. The aqueous humor had not escaped; there was no anterior or posterior synechia, nor deformity of the pupil or other symptom of iritis. Vision was greatly impaired, though not entirely lost. There had been considerable pain in the orbit.

The patient was put upon small doses of calomel, and cupping and counter-irritation employed. Under this treatment, the opacity gradually diminished, until, at the end of four or five weeks, the capsule had completely regained its transparency, and vision was entirely restored.

Dr. Gibson reported a somewhat similar case: A boy of twelve years of age fell into a bush, and, in so doing, received a severe concussion of the right eye. When he was seen by Dr. G. several days after the occurrence of this accident, the eye presented the following appearances: The cornea was perfectly clear and free from injury; the iris was inflamed, and the uvea had already contracted adhesions with the anterior surface of the capsule; the pupil was deformed, and the dull opacity characteristic of capsulitis could be perceived behind it. The vessels of the conjunctiva were highly injected. The next day the capsule was completely opaque: its surface was manifestly covered with fibrinous deposits.

Active treatment was at once instituted. The patient was cupped, and mercurials and belladonna were administered internally, while revulsives were applied to the temple and neck. Resolution gradually took place. Absorption was complete at the end of five weeks; the iris was liberated, and resumed its normal movements, and vision was entirely restored.

In these cases inflammation was prominent; but it may in other cases, be masked, local, and attended by whiteness, rather than redness, &c.; and thus cause many, if not all cataracts. At all events if inflammation be not the cause of cataracts the cause is unknown.

In the case of Smith, above mentioned, the cataract in the right eye at the first operation had but a slight opacity, which, however, prevented vision, and which at the second operation was perfectly opaque, doubtlessly owing to the consecutive inflammation from the first. In this case the lens at the first operation was but slightly opaque in the central portion, not the capsule; the cataract at the next operation proved to be altogether opaque and completely membraneous, perforated with several holes by the needle. There can be but little doubt that cataract could be artificially produced, thereby illustrating its ætiology by a series of experiments upon the inferior animals. The puncturing of the capsule and its enclosed lens, with the cataract needle, would probably produce cataract, differing from non-traumatic cataract only in this, namely—the inflammation of the numerous other tissues necessarily wounded in the artificial process. It would appear from actual experiment that the disintegration of the parts involved in ordinary cataract gives no pain. Thus, if this element—pain—be

deducted from the non-traumatic cataractous process, it may analogically speaking be reduced to the vague category called inflammation—so that the cataractous alteration, a peculiar, local, painless, white one—the analogues of which exist in other structures, would be established on a basis more or less satisfactory; whereas, it has now none whatever.

Are there any satisfactory creteria whereby hard and soft milky cataracts can be distinguished anterior to the actual operation, of extraction, depression, or keratonyxis?

This is a fundamental question lost sight of by the partizans who dogmatize upon the superiority of the particular mode of operation to which they adhere; the advocate for extraction admitting that a soft cataract should be couched or broken up, and the coucher that a hard one ought to be extracted; but neither give any satisfactory criterion anterior to the actual operation; and then it is too late to be of any use! This kind of logic affords a good example of that called the *petitio principii* and that termed a bull—as the learning how to swim before venturing into the water.

Ought the operation for cataract be performed on both eyes simultaneously?

The rule laid down by authors answers this question negatively with few exceptions, but the rule may be controverted morally, physiologically, and pathologically. The moral tone of the patient's mind when once made up to undergo the operation in one eye will suffice for the operation on both. When I was going to operate on *one* of his eyes, according to the rule, J. M. said in a firm tone, "I will not make two bites of a cherry."

If only one eye be operated on, and if this first operation should result unsuccessfully, the patient having not only the fear of another unsuccessful operation but the dread of a second course of medical treatment to deter him, will probably refuse to permit the second attempt. The double operation which is pathologically but one, does not materially enhance the dangers of consecutive contingencies.

Therapeutic Retrospection.

The reader will have seen in Smith's case, as above mentioned, that a blood-letting was performed in anticipation of consecutive inflammation which the writer does not now regard as a sound rule of prac-

tice, though twenty years ago it was the fashion to bleed a great deal more than at present. Even at this day systematic writers do not sufficiently guard their readers against the danger of depletion in some cases of pure opthalmic inflammation in which bleeding, purging, and antimonial nauseants weaken the patient and strengthen the disease—the eyes running into rapid disorganization. Cases of this deplorable character might be given as well as cases in which rapid cures have resulted from the administration of opium, morphia, Dover's powders, quinine, blisters, together with small doses of mercurials, warm baths, &c. Even in idiopathic or spontaneous iritis, these remedies subdue inflammations, in some cases, where the so called intiphlogistics aggravate it. Happy is the patient who has a physician who can make the distinction!

ART. II.—OBSTETRICAL CASES AND PHYSIOLOGICAL REMARKS, &c.

BY BENNET DOWLER, M. D.

Monstrosity.—1838, June 28th, 4 P. M.—Mrs. Keefe, born in Ireland, aged twenty-eight, mother of four children, resident in New Orleans three months—of robust constitution—sick for a week last past—in severe labor for twenty-four hours, under the care of an apparently demented midwife—tongue furred—intense thirst—restlessness—vomiting—skin cold and clammy—hiccuping—almost pulseless—vital powers failing—vagina hot and painful on being touched—two feet and legs of a monster protruded—a third leg of double size completely solid and round having two feet, was brought down—the amniotic waters, completely discharged—the uterus contracted closely on the presenting immovable mass—the three legs proceeded from an enormous unyeilding body consisting of two consolidated pelves—a single common anus was discovered with indications of female organs of generation.

Death impended; the friends were apprized of this—the husband was absent up the river. A clergyman was called, while I went for instruments to dissect the monster, to gratify the wishes of the patient, though without the expectation that she could be saved by these means or by the Cæsarian section, as she was evidently sinking from

exhaustion and constitutional irritation. Unavailing parturient pains still continued.

The clerical visit proved the great value which psychological medicine often betows upon the patient, thereby paving the way for the easy execution of the materializing processes of the surgeon and the physician. Before the last rites of the Church were administered, her mind was all perturbation—she was restless—impatient both in conduct and conversation, and unmanageable—after this for twelve or thirteen hours, she presented an example of moral courage, of unmurmuring quietness, of calm, of deliberate and even cheerful conversation alike free from enthusiasm on the one hand, and sullen indifference on the other. The clearness, serenity, and energy of her mind even under the exhibition of opiates which seemed to produce no effect, were to the last hour like the setting sun undiminished, unclouded.

Lest the term psychological medicine should appear to some readers as a neologism, it may be remarked, that several periodicals bearing this name have recently appeared in different languages and countries. A knowledge of the mental frame, including its physiological and pathological laws and reactionary effects upon the body, is at once the highest and most useful kind of knowledge, not excepting that of drugs, in practice, whether the primary disease be mental or corporeal.

But to return: Mrs. K., though aware of her danger, still desired the removal of the monster. This occupied nearly five hours. The legs having been removed—the pelves or hips strongly fused together, were broken and extracted, as were the apparently single viscera, the double spines and the single or rather double continous ribs. The anterior spine corresponding with the symphysis pubis and anterior abdomen of the mother was particularly in its upper half, as the anatomist must perceive, in an inaccessible position not being in the axis of the inferior outlet of the pelvis, but rising vertically with and above the pubis, and thus beyond reach. Of the vertebræ of the anterior spine of the monster only about half could be extracted; the residue, were beyond the range of instruments. Of the posterior spine corresponding with that of the mother, all was removed but the cervical portion, together with nearly all of the ribs. The operations had now extended as high as possible; but the head or heads could not be reached;

one arm, however, was at length brought down. Towards midnight all further dissection was abandoned. Mrs. K. was now pulseless. An hour after midnight, on taking my leave, Mrs. K. full of gratitude for my unavailing but well meant attempts, said "that she knew that I had given her up to die," and her manner and language showed that she had the same opinion herself. These facts are alluded to merely to show the great effect of moral or psychological influences in sustaining the mind in the midst of the greatest conceivable trials—the final agony. In an incurable case psychological medicine is the only remedy—in a curable case often paramount.

At 5 A. M., Mrs. K. died. An hour after, the Cæsarian section was performed. The abdomen was already convex from gaseous distention. The uterine walls were injected, dark, and nearly gangrenous in places where the pressure of the monster appeared to have been greatest. The uterus contained two or three pounds of black, grumous, and almost putrid blood; one enormous placenta; one large umbilical cord; and a two headed monster with four fully developed arms. The upper part of the chest still entire had no mesial line showing the place of fusion. In fact it was difficult to say whether it did not partake more of the single than of the duplex form of monstrosity. The chest appeared single though double-ribbed, double-spined, double-necked, double-headed, and furnished with four large symmetrical arms. The necks were long, yet well proportioned; the heads were large and beautiful; the tracheæ and foreheads were directly opposite, facing each other. The arms, except the one that had been brought down, were raised and laid along the sides of the heads and faces.

From the configuration of the monster, it presented an unyielding mass of a wedge-like or conical shape, the apex of which corresponded to the outlet of the maternal pelvis; for, the opposing foreheads and necks with the upraised arms and shoulders, gave the cranial or upper end of the mass in its shortest diameter, a greater diameter than the maternal outlet afforded. Indeed the descent of the monster was arrested by the brim or superior strait of the maternal pelvis; so that delivery was impossible by the natural passage. The continuity of the ribs from spine to spine, together with the double spines, and the two pelves fused into one, gave the body a rigidity throughout incompatible with its descent through the curving outlet of the maternal

pelvis, as all acquainted with the mechanism of labor must admit. Had the existence of the monstrosity been ascertained in time, the Cæsarian section would have afforded the only chance of life for the mother and the monster.

The mutilated monster was taken to my office, and in a few hours after, several hundred people having seen it, the relatives demanded and received it for burial. The late Mr. Shaw, apothecary, who saw its remains, estimated its weight at seventeen or eighteen pounds, independent of the parts that had been removed, However this may be, its weight may safely be supposed to equal that of two large children at birth.

A gentleman of undoubted veracity who saw this monster, informed me that in the same neighborhood where Mrs. K. lived, he had seen a few months before her death, a wax figure of a double-headed monster which some strolling musicians carried about for exhibition, which resembled very exactly the one he saw in my office above described, so far as the unmutilated parts were concerned. His attention had been strongly directed to the wax figure which was brought to his door and which he ordered away, because he was afraid his wife, then *enceinte*, might see it. He inferred that Mrs. K. must have seen the wax figure. Hence the cause of her misfortune, according to his reasoning.

If fright, sorrow, jealousy, and anger, kill the fœtus and cause abortion, as must be admitted, why may not these same moral causes during its early state of growth, disfigure, modify or alter its form?

The following case of monstrosity reported by Mr. W. H. Popham, in the London Lancet, for June, 1852, (Am. Ed.) though very different from that above mentioned, is both curious and interesting. The lady, ten or twelve hours before delivery, (which occurred at the end of the sixth month,) expressed to Mr. P. her fears that she carried twins or a monstrosity. It will be seen that the monster was small and weighed but four pounds. Mr. P. says:

"After three hours more in strong labor, she was delivered, and the first question she asked, was, 'Is the child all right?' On examining the child, I found it most strangely malformed. There were two heads attached to the neck, and united by integuments all up the side to the vertex. The mouths, noses, and eyes were all perfect, the neck was single, and evidently contained but a single vertebral column, and

which continued single as far as the eighth or tenth dorsal vertebra, where it bifurcated. From this downwards, the body was double, there being two separate and distinct pelves, detached from each other, and containing the organs of generation (female) perfect, as well as the ani. There were four legs, and perfectly formed; in fact, the body was double from the naval down. The navel cord was single, entering the bodies at the point of their junction. The chest presented nothing abnormal, and the arms were well made.

"This extraordinary monster was born alive, and lived as long as the circulation was carried on in the cord; it gasped several times simultaneously with both mouths, and passed meconium from the ani at the same time. It weighed about four pounds.

"From the commencement of Mrs. M——'s pregnancy to the time of her delivery, she had a most morbid desire to see anything that was unnatural, and wherever she heard of a 'lusus naturæ,' there she would go and see it. For two months previous to her confinement, she continually said she should either be brought to bed with twins or a deformed child. Her recovery was quick, not having had a bad symptom. Of course she was not aware that she was delivered of a monster."

Monsters are not, as was formerly supposed, mere sports of nature, amenable to no laws, nor beyond the pale of scientific classification.

Human monstrosities limit themselves to, and form themselves after, the normal standard so far as never to lose altogether the unity of the human type. They never degenerate into the inferior forms of the classes of their proper sub-kingdom; that is, they never identify themselves with the other classes of vertebrata, as monkeys, birds, fishes, &c.; much less do they sink into the other sub-kingdoms, as the articulata, mollusca, radiata.

Some malformations become such from deficiency or redundancy of development in a particular organ; others, by supernumerary or supplemental formations which, still, do not violate the oneness of the individual. Compound monsters—in which two individuals are fused or blended into one are rare, and present physiological, psychological, and medico-legal questions not easily answered. A more common variety of duplex monstrosity is characterized by a more or less extensive union of two individuals by means of cutaneous, ligamentous,

fleshy or bony cohesions, as in the Siamese Twins, the Hungarian Twins, (Helen and Judith,) and more recently in the Carolinian Twins, exhibited in New Orleans. The union of these black female twins of North Carolina is very like that of the Hungarian girls, who lived twenty-two years. Judith fell sick and died. Helen died three minutes after. Such is the fate in reserve for the black girls.

Helen and Judith were exhibited for seven years in the principal capitals of Europe. They were distinct persons except where they were united back to back from the loins to the common anus and common vulva, as in the Carolinian Twins. In the Hungarian Twins it was ascertained that "the aorta and venæ cavæ communicated in their lower parts, and thus established a large and direct communication between the two hearts, producing an intimate relation of life and functions between the two beings. Whenever one was ill the other felt so too, and participated in her sister's disease; it was therefore predicted that the death of one would necessarily destroy the other, which proved to be true. Judith, at the age of twenty-two years, was attacked with disease of the lungs and the brain, of which she died. Helen, who, at the commencement of her sister's attack, was in perfect health, soon became ill, and both expired at almost the same instant."—*Penny Cyc.*

The doctrine of final causes or purposes in physiological anatomy is a fruitful principle of research, but is hard to reconcile with great aberrations of structure which interfere with the essential conditions and functions of life and individual well-being. The normal form of organization comprehends both the means and the ends of existence—the abnormal fall short in both and must be regarded as deviations from the general plan of Nature, being no more adapted to the ends of existence than the oyster is to flying.

It is a curious but incontestable fact that a great majority of monstrosities are females. These vices of conformation probably take place from some disturbing cause or accident in the early stage of fœtal life, as certain diseases do, such as dropsy of the brain, nævi materni, and probably traumatic alterations.

"Direct evidence has been afforded, by experiment, that the natural stages of formation may be altered in the embryo of the chick, during incubation, by external injury. Geoffroy St. Hilaire

injured several eggs, in which the process of incubation had commenced, and had been going on naturally for several days. He shook some of them violently, he perforated the shell of others in different places with a sharp instrument, or kept them in a vertical position, upon either the large or small end, during the whole term of hatching; again, in some he covered part of the shell with wax, or varnish impervious to air. The constant effect of these injuries, was the production of a very considerable nnmber of anomalies, either simple or complicated, among which may be mentioned *cyclopia* and other malformations of the face and head, eventration, and spinal fissure. In no instance was any case of double monstrosity met with, which might, *à priori*, have been supposed."—*Penny Cyc.*

The Cæsarian section successfully performed by a negress while drunk.

The late eminent Judge Waggaman, many years ago a member of the Senate of the United States, informed me that an old drunken negress, who acted as midwife on his plantation, above New Orleans, on being called to a black girl during her first labor, which was natural, took a sharp case-knife, and without any reason to justify her conduct, laid open the abdomen and womb, and took therefrom a living child! The girl speedily recovered, with no other inconvenience except a slight incontinence of urine. The judge vouched for the accuracy of these facts, and pressed me to visit, with him, his plantation that I might examine the girl, which, however, I had not then an opportunity of doing. He fell in a duel soon after.

According to the estimates of Merryman and Blundell, published a few years ago, it appeared that the Cæsarian section had been performed in only twenty-six cases in the British Isles, all of which, two excepted, proved fatal to the mother.

Midwifery in New Orleans.

In his work on the Hospitals of Paris, in 1843, Dr. Stewart, of New York, says "Midwives in France after two years' study at the School of Delivery, and submitting to two satisfactory examinations, receive a diploma to practice, always, however, under certain restrictions, one of which is, that in no case, and under no circumstances whatever, shall they resort to delivery with instruments without the attendance of a physician. They amount, in all France to the number of 450, and practice almost exclusively among the lower classes of the community." This number would give in France, one midwife to

every 75,000 inhabitants. New Orleans has probably one for every fifteen hundred or two thousand people, while, perhaps, not one in twenty of these midwives have received even an elementary anatomical education.

That such uneducated persons should be generally successful is owing to the fact that in a great majority of cases no scientific skill is required, and thus a lucky negress becomes the rival of the most learned obstetrician.

The whole argument which the uneducated midwife offers and the credulous public receives is better expressed by the poet Crabbe than by the professor of midwifery himself; the midwife's opinion of the accoucheur is represented on this wise—

> "By proud and learned words his powers are known,
> By healthy boys and handsome girls my own;
> That *I have luck* must friend and foe confess,
> And what's good judgment but a *lucky* guess?
> Can this proud leech, with all his boasted skill,
> Amend the soul or body, wit or will?
> * * * This stranger from your door repel,
> And be content to *be* and to be well.
> So long *successful* in my art she cried,
> And this proud man, so young and so untried!"

An example or two will best illustrate the evils alluded to, and of which society is the victim: on the 27th of November, 1832, I was called to see Mrs. R., fifteen miles distant, whom I had attended in a former labor. Mr. R., the husband, before he came for me had called in the nearest midwife in whose care he left his wife. On arriving at the house, I learned that the child had been delivered. I asked to see it. The midwife with great reluctance retired to the corner of the room and brought forth the child. I found a large lacerated wound of the vertex exposing the brain, from which the blood issued freely. The wound was dressed. The child lingered in great misery for two days before it died. The following is the explanation of the cause of this murder: the midwife offended that a *doctor* should be thought necessary when *she* had been called in, went, as soon as Mr. R. left the house, and got a pair of shoemaker's pincers, and seizing the scalp of the child, which judging from the wound must have presented in the most favorable manner, she tore open the brain—some hours afterwards the child was born.

A midwife of large practice for a quarter of a century, had, as she had often told me, "a falling of the womb" for thirty years. It was, as she asserted, completely external, and gave her much uneasiness. Finally she was confined to bed, whereupon she sent to town for my assistance. I found the womb high up and instead of its prolapsus, a pudenal hernia of large size which had descended within the pelvis opposite the acetabulum, and which pushed forward the right labium and a part of the vagina.* A lucky midwife ignorant of the position, size, structure, and properties of the womb, and her own womb too! Oh most wise public! think of this. But the women are not alone in these blunders. A very amiable young woman, an only child, the wife of a merchant, during her first labor had a convulsion during which she bit her tongue. Her father who was present, held her jaws assunder to prevent further mischief during her struggles, while an old male practitioner of reputation, though not a regular M. D., delivered her. This practitioner and another of the same gender and like reputation, though not an M. D., "doctored" this lady thirty-seven days, as I was assured, because her mouth was widely open and could not be shut—her chin projected forward—her strength and bloom were gone—she was unable to take anything but liquids—whereupon, I was called in—I found ulcers within the cheeks and a complete dislocation of the under jaw; which I at once reduced, on the thirty-seventh day. I will not add to this catalogue by examples nearer home.

There is probably no city of equal population in Christendom where the male practitioner is less employed in child-birth than in New Orleans—none in which the female practitioners are less educated, being chiefly negresses or mulattresses, or foreigners without anatomical, physiological, and obstetrical education, and consequently unacquainted with the mechanics of parturition.

Whatever science, art, skill or manual manœuvres the exigencies of child-birth may demand, physiology and anatomy, not mere luckiness, must guide the accoucheur, just as they guide the surgeon and physician.

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* I have met with a similar case of hernia in this city, although writers rarely allude to its existence.

ART. III.—OSTEO SARCOMA OF LOWER JAW.

BY C. S. FENNER, M. D.,

OF MEMPHIS, TENN.

A little girl, eight years of age, was brought to me in July, 1852, at Aberdeen, Miss., with an Osteo Sarcomatous tumor, occupying all the anterior portion of the lower jaw; extending from the bicuspid tooth, on the right side, to the first molar on the left. The tumor had an elastic feel, was about three inches in diameter, distending considerably the under lip, and encroaching on the base of the tongue. Its top presented a circular aspect, with flat ulcerated surface; both incisors, and left cuspid teeth gone; the right cuspid, and left bi-cuspid loosely hanging to the surface of the tumor, and apparently ready to drop out. When the jaws were closed the upper incisors were nearly buried in the tumor below. I advised the immediate removal of all the diseased portion of the bone; to which the child's mother readily assented. After the administration of chloroform, assisted by Drs. Pope, Randle, Haughton, and others, I made an incision through the mesial line of the lower lip, down to a point just below the base of the jaw, continuing the incision on each side along the base of the bone, to the points where the facial arteries cross. The flaps thus formed, were dissected back, leaving the anterior portion of the tumor exposed. The attachments on the inside were divided to admit the forceps; the saw applied to notch the bone, and the separation completed with the bone cutting forceps. The attachments to the tongue were then divided and the entire tumor removed, including a sound tooth on each side. But little hemorrhage followed. The divided parts were brought together and secured by interrupted sutures. The little patient awoke entirely unconscious of having undergone a serious operation. No unpleasant symptoms followed. The pins were removed the second, and the entire dressings on the third day. The external incisions, were entirely healed leaving scarcely a trace of the knife. The patient was removed to her home, on the fourth day, since which time I have not seen her. I heard from her however not long since. There has been no return of the disease and the deform-

ity is scarcely perceptible. On dividing the tumor the bone was found to be completely degenerated to a soft, elastic, grayish substance, of the consistency of the albuminous portion of a hard boiled egg—several small spiculæ of bone were interspersed through this substance—a very thin shell of bone, with the periosteum entire, remained, giving attachment to the hyoid muscles.

ART. IV.—WOUND OF THE SMALL INTESTINE SUCCESSFULLY TREATED BY THE INTERRUPTED SUTURE.

BY J. C. McGEE, M. D.,

OF CADDO PARISH, LA.—Reported by C. G. Young, M. D.

Merriman, a stout, middle aged negro man, belonging to the plantation of Thomas Holt, in attempting self-destruction, stabbed himself in the right iliac region. The knife entered the abdomen just above poupart's ligament and about half an inch outside of the iliac artery, making a gash an inch and a half long, and wounding the small intestine in two places. The first opening in the gut was about fourteen lines in length and the second about four; the direction of both wounds was longitudinal, and both were upon the same side of the intestine. This occurred on the 11th of April instant, and the man was seen in half an hour after by Dr. McGee, who informs me, he found a considerable length of the intestine protruding, and wounded as described. Four stitches of the interrupted suture were taken in the longer wound and one stitch in the shorter with fine sewing silk. The ends of the thread were cut short, and the intestine returned within the cavity of the abdomen without difficulty. The external wound was secured by stitches—an anodyne given, warm fomentations applied and strict rest enjoined. After thirty-six hours the belly was moved by small doses of castor oil, and the man recovered without an unfavorable symptom. I have known several instances where farmers have wounded the intestines of their pigs in spaying them, and they have pursued the practice adopted by Dr. McGee. The pigs have been turned loose in a pen and have done as well as if nothing had happened.

ART. V.—ENCEPHALITIS IN A CHILD.

BY R. L. GRAVES, M. D.,

OF SAN ANTONIO, TEXAS.

On April, the 20th, 1854, I was called to see a child two years of age, laboring under encephalitis, with violent convulsions; the pupils of the eyes were greatly contracted with great drowsiness and tendency to coma.

On my arrival, I learned from the family that the patient had been in good health except that it had shown great restlessness a day or two previous to the attack from convulsion which had already taken place before I saw the child. It still showed symptoms of another spasm. I ordered chloroform and antispasmodics, and the feet to be bathed in a warm mustard bath, and cold water to the head until the patient became quite composed, at which time, I prescribed calomel, and James's powders, which were given every two hours until the bowels were freely moved. On the next morning, I found the patient composed and in a mild perspiration. The head continued hot. I then prescribed the following mixture: distilled water, four ounces; tartar emetic, two grains; spirits of nitre, two drachms; a teaspoonfull to be given every two hours, and directed the patient's head to be held over a basin, then poured water on the head from a pitcher in a small continued stream, and at the same time kept the feet in a warm mustard bath. The next morning after I saw the child and found it doing well, having had no convulsion for twenty-four hours previous; the head was cool and the extremities warm, and a gentle perspiration over the body. I neglected to say the child was cupped on the nape of the neck during the above treatment. I then prescribed ten grains of quinine, ten drops of Fowler's solution of arsenic mixed with a little magnesia to be divided into five powders; gave one every three hours during the day. The child continued to improve. On the morning of the 23d, I found the child doing so well that I discontinued my visits.

Four days afterwards I was called to see the child again. On my arrival, I found it quite drowsy and with a tendency to coma; the

head was quite hot. I again applied cold water to the head, and cupped it on the nape of the neck; gave a teaspoonful of castor oil and turpentine, every four hours. The patient being quite thirsty, I gave gum arabic water and applied a poultice to the stomach. The next morning, I found the patient much improved, having had two operations of the bowels. I then directed quinine to be given every two hours; but before it was given, the parents imagined the child worse, and a consulting physician was called in, who approved of the course that had been adopted; we then prescribed quinine, one grain; calomel, one-sixth of a grain; to be given every three hours, and cold water to the head. The child continued to improve, and is now (May, 15th) convalescent.

ART. VI.—AN ESSAY ON THE MEDICAL PROPERTIES OF QUININE.

BY RICHARD H. DAY, M. D.

Read before the Baton Rouge Physico-Medical Society on the 13th of May, 1854; and, by order of the Society, furnished the "New Orleans Medical and Surgical Journal" for publication.

Mr. President and Fellows:

The subject, as stated to you at our last meeting, upon which, at this time, I intend to make a few observations, is the therapeutical properties of the sulphate of quinine.

My fellows will excuse me if, in this essay, I studiously avoid to appear learned, and decline noticing all the dogmas and dicta of the different authors, who have, from time, written upon this highly interesting subject. My purpose, gentlemen, is to be practical—to deduce from my own experience, and the observations of others, the true medical properties of the sulphate of quinine. That this subject is of the utmost importance, you will all readily admit; and, to us of the South, where its use has become so general and common, and I might add extravagantly large, it surrounds itself with the most absorbing interest.

To prescribe any medicine, intelligently and usefully, it is indispensably necessary that the therapeutic properties of that medicine be definitely and accurately known.

To prescribe it, under the vague impression of its being inherently stimulent or refrigerant, tonic or sedative, must give instability and want of precision to our practice, and, at least, reduce our efforts in the healing art to the humiliating character of *legal empiricism.*

What, then, I would inquire are the real medical properties of the sulphate of quinine? Is it a tonic or sedative? Narcotic; anti-spasmodic, or anti-febrifugic?—if you will allow the latter classification. It is well known that physicians hold a contrariety of opinions upon this matter; and, as a necessary consequence, differ much in their practice. I believe, however, there is no difference of opinion, among the medical faculty, in regard to the tonic qualities of this agent in small doses. The experimental researches of physiologists, and the universal experience of physicians, attesting its virtues in anæmic conditions of the system, in which there is no doubt of the impairment of vitality and a state of positive debility, have long since placed this drug at the head of tonics in the Materia Medica. That it is also a powerful anti-febrifuge is a settled fact, and gathers confirmation from every day's experience of its use. But, how it operates to produce this result? whether, by giving *tone* to the system and *increasing inervation*? or, acting as a direct and powerful sedative? is a question yet mooted in the medical profession. I take the position that it is a *tonic,* and that its power in controlling and arresting fevers, subduing arterial excitement and nervous irritation, is in virtue of its *tonic action, conjoined,* as I believe, with *peculiar* and *strong anodyne qualities.*

In order to settle this question we must go back to first principles. Those principles we must hold as land-marks and the light to guide and direct us in our observation of the phenomena of nature, that we may properly arrange the facts which we observe, and deduce correct conclusions therefrom.

I maintain, gentlemen, that no medicinal or physical agent can, *per se* and inherently, possess antagonistic properties. It may have a diversity of operation, but the power by which it operates, or modus operandi of operation, cannot be inconsistent and antagonistic with itself. That which is stimulant, is *stimulant;* that which is tonic, is *tonic;* that which is sedative, is *sedative:* except as a difference in special pathology and the mode of appeal may make a seeming difference in operation.

I will illustrate, to endeavor to make my position clear and tangible. No one will deny the stimulating properties of cayenne or brandy. And, yet how many cases of conjunctivitis and laryngitis have we seen cured, and cured rapidly under the local application of these powerful stimulants? The experience of every housekeeper confirms this truth, and makes the practice common in all civilized countries. Now, here is the seeming absurdity of stimulants curing inflammations, and the apparent contradiction of stimulants acting as sedatives. But, if we bear in mind the important *fact* that inflammation may be connected with and actually dependent upon a *debilitated* and *dilated* condition of the capillaries of the affected part, those seeming contradictions immediately vanish, and the modus operandi of stimulating collyria in the cure of inflamed eyes, and of stimulating gargles in the cure of sore throat, is at once plain and rational to the intelligent pathologist, without resorting to the absurdity of asserting that the same remedy is both a stimulant and sedative.

No one questions the exciting property of electricity—that subtile fluid which pervades all nature and flies upon the airy breath of every atom of atmosphere. Our elasticity of frame and vivacity of intellect depend, no doubt, in a great measure upon its presence in a greater or less abundance in the atmosphere around us. More concentrated, but still controlled, as in the different electric and galvanic batteries, the most unquestioned evidences are given of its stimulant and tonic properties upon the human system.

But let the current be stronger still, as when the thunder cloud lets loose its pent up store and tracks the heavens by its zig-zag course, instead of stimulating, should the animal frame be placed within its track, a complete overwhelming of the energies or instant dissolution is the result. The unphilosophical beholder of these different results would say that electricity, in a small quantity, is stimulant and tonic—in larger doses, sedative. But, to the attentive and philosophic interrogator of nature's laws, another and a different conclusion would become manifest.

It is well known, and universally acknowledged by all physiologists, that every impression made and action had upon the living frame is through the excitability of the nervous system. It is well known, also, that any impression too long continued or too powerfully applied to this excitability, exhausts the principle itself and fails in its wonted

effects, either partially or completely, as the application has been more or less protracted or intense.

In the instance of stunning, or death by lightning, the appeal has been made so powerfully to the excitability of the nervous system as partially or completely to exhaust that principle, and with it, as a necessary consequence, the impairment or the extraction of every vital function. Again, no one will deny that venesection is constantly, powerfully, and from the beginning, sedative in its effects. And, yet, what physician has not seen its employment act as a stimulant when the vital and animal functions have been oppressed by an active plethora or congestion of the vital organs? But, because blood-letting has taken off this oppression of the vital organs, loosed their trammelled energies, permitted the free exercise of their functions, quickened the circulation, and, as a necessary consequence, given manifest and decided increase to all the operations of life, will any one maintain that this potent sedative agent is a stimulant or tonic?

These observations are made to show the necessity and importance of studying general and special pathology, in order to understand the application and difference of action of remedial agents, as constituting a truer, more reliable and rational basis for the practice of medicine, than the absurd notion of attaching to the same medicine opposite and antagonistic properties.

Now, in reference to the action of quinine upon the human system, much disputation has been had in consequence of the disputants losing sight of, or overlooking, that peculiar and special pathological condition of the system, upon which depends the different and various actions of this agent.

Those who regard it as a sedative, in large doses, have been led into that error simply from observing its apparent effects in cases of vascular excitement, without regarding or detecting the peculiar condition of the system in which that febrile excitement originated. They lose sight of the important and settled fact, that the highest arterial excitement and perturbation of the system, and even inflammatory action itself, may and often does, supervene upon a depraved and debilitated habit of the consititution. And, such, I regard, to be the real condition of things in all those cases of fever, with or without inflammatory complaints, in which quinine has been so successfully given. If we look to the localities, either in this or other countries, in the treatment

of whose diseases Quinine has been so eminently successful, we shall see they are such as are supposed to abound in malarious influences, or of such climatic and atmospheric peculiarities as to *debilitate* and derange the whole nervous system. I simply refer to the fact, that in every section of country, where this agent is extensively used, intermittent, remittent and continued fevers, are the prevailing and endemic diseases, to establish my position; for, I believe all authors of late agree that this whole family of diseases originates in some cause acting upon and *debilitating* the nervous system.

The highest and most unquestioned authority, which I can quote, to support my views is the language of Dr. Watson, page 451 of his unequalled work on the "Principles and Practice of Physic." I might adduce other authority, and much of it; but when I have told you what Dr. Watson says, you have the best medical authority known. He says, "When I have told you that *debility*, any how produced, constitutes a predisposition to intermittent fever, I need scarcely add that all the multiform causes of debility may also be regarded as predisposing causes of this same disease, as they are of so many others." His language is still stronger when speaking of continued fevers, bearing upon the same point, but too long to quote, and I refer those who are not familiar with his views to his work.

But we need not written authority to establish this; every physician who has practiced his profession in a marked malarious district, as I have done, has seen in the inelasticity of frame, laxity of fibre, sallow complexion, erratic pains, and general malaise, evidences of the most unquestioned operation of some debilitating agent or influence on the nervous centres.

Now, it is in these localities and latitudes, where *debilitating* causes are known to exist, that quinine exerts its most herculean powers. As you go to more elevated regions, where climate and local causes give buoyancy and a high degree of vitality to the animal system, and develope diseases of a more acute and highly inflammatory character, *quinine* ceases to be used, and those remedial agents resorted to which are acknowledged to be ab initio depletive and sedative in their operation.

How comes it, if quinine be a sedative in large doses, that its use is limited to those diseases connected with or dependent upon *debility*,

or restricted to the localities where *debilitating* causes are known to operate? Why is it not given in large doses in open, undisguised, high inflammatory affections, occuring in vigorous, athletic constitutions, if it be a sedative? Sedatives, proper, are not so dealt with. It avails nothing to say that quinine is given, either in large or small doses, in the same diseases and in connection with blood-letting and other depletants. It only shows the mixed and compound nature of diseases, and the imperative necessity resting on physicians to analyze every disease they are called to treat, and to understand its seat and nature, whether mixed or otherwise, before they undertake to prescribe. This may be irksome and seem foolish to the indolent and unobserving practitioner; but not so with the intelligent, practical physician; for he sees many cases in which it becomes necessary to combat local inflammations by depletants, general and local, and at the same time husband the resources and nourish and support the failing strength by the judicious administration of nutriment, stimulants and tonics.

If, then, I am correct in my position that in all those cases of high febrile excitement, with or without an inflammatory complication, in which quinine is successfully administered, the vital energies though lashed into excitement, are really laboring under the *oppression* and *debilitating* influence of some morbific agent, corrupting and vitiating the life-blood, or working some profound lesion of the *nervous centres*, the conclusion is inevitable that the "methodus medendi" of quinine is not sedative, but tonic and anodyne.

If we look to the elementary constituents of this potent alkaloid, as given by the learned Prof. Liebig, in his animal and vegetable chemistry, these views will be still further strengthened. I quote from the New Orleans Medical Journal of November, 1845, as reported by Dr. McCormick. He says, "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 motion, 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 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. We must not forget that in whatever light we may view the vital operations, the production of nervous matter from the blood presupposes a change in the composition and qualities of the constituents of the blood. That such a change occurs is as certain as that the existence of the nervous matter cannot be denied. 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 respectfully so nearly alike in composition, if they do not absolutely produce 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, 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. 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 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, membranes, of the saliva, of the seminal fluid, &c., &c. Then, in a chemical sense, there is no objection to the opinion that substances of a composition analagous to that of nervous and cerebral matter, may be employed instead of the substances produced from the blood, either to furnish the necessary *resistance* or to *restore* the *normal condition.*"

Adopting Dr. McCormick's language, I would say: "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 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."

With these views, how is it possible to regard quinine as a sedative? If fever result from a depressed and debilitated condition of the nervous system, and quinine in its elementary composition is shown to be food for that system, how, in the name of common sense, can it be regarded as a sedative in the cure of fever? Or, if it be a sedative in large doses, how can it be reconciled with consistency and sound rational treatment to give an agent in such quantities as engenders that identical state of the organism which is believed to be the foundation of diseased action? Regarding quinine as a *tonic* possessed of *peculiar anodyne* properties, a position which seems to be sustained by every view we can take of it, all the effects resulting from its administration, at once become plain and rational. If high febrile excitement, resulting from and depending on a peculiar depressed condition of the nervous system, be relieved by the free administration of quinine, it is plain that quinine has accomplished it by controlling or removing the cause which kept up that excitement, by supplying the food or material needed by the nervous system to enable it to assume a normal condition.

The effects of general relaxation, tremors and prostration, which are always noticed when it is given in excess, (and which have given rise to the doctrine of its sedative action,) have resulted from the appeal having been too powerfully made upon the nervous centres, and in-

stead of responding to the appeal, a stunned, confused, nervous agitation is the result, and an impairment of the functions corresponding to the degree of stunning produced. Given in proper cases, and in doses to meet their exigencies, it is a medicine whose virtues cannot be too loudly extolled; but, because it is thus potent for good and applicable to such a numerous family of diseases, I would not have its reputation blasted by its indiscriminate use in all cases, under the erroneous impression that it is both a tonic and a sedative, according to the quantum given, or the volition of the prescriber.

All will see the certainty of medical prescription when this notion lies at the foundation of practice, and the blunders and serious mistakes that must grow thereout. General and special pathology, that nice distinction in the nature and seat of morbid action, which distinguishes diseases and determines their appropriate treatment, is overlooked and leaves the practitioner to prescribe at random, and fight symptoms or an unknown disease, as best he may, in the dark.

While, then, I object not to the free and liberal use of quinine, and admit the wide range of its application in the diseases of the Southern and Southwestern portions of our country, still I maintain that it is important to entertain correct opinions in regard to its medicinal qualities, and the peculiar morbid actions of the system requiring its administration. If we entertain the opinion that in large doses it is sedative, regardless of the seat and intimate nature of diseases we are called to treat, much serious injury may be done, as already has been. I know that physicians have written much in our medical journals in regard to the safety and utility of quinine in certain forms of inflammation, and the disposition on the part of some to class all agents as sedatives that are capable of reducing febrile excitement, lessening nervous irritation, and under some circumstances admissible and useful in inflammation.

Knowing the sources of imperfect and fallacious observations in the field of medicine, and the strength of that ambition which prompts men to desire the applause of their fellows, we should receive all reports of the kind with caution and scrutiny. As is the case with other *patent* medicines, so with *quinine;* the good it has done, or seemed to do, has been widely circulated; while the injury which has resulted from

its improper and excessive use is hid in the dark and undivulging bosom of oblivion.

Our own experience and careful observations, had in accordance with the settled and established principles of medical science and the laws of vital action, must be the criterion by which we are to judge of the truth or fallacy of the observations and deductions of others.

Let us then look back over the field of our experience and call to mind the nature of the diseases, the quantity and mode of prescribing *quinine* and the phenomena attending its administration, and see if the views here advanced are not fully sustained.

In all those cases of high febrile excitement in which it has lessened the action and force of the heart and arteries, subdued pain, released the skin and thrown out upon its surface a free and genial prespiration, will not the pathology of fever, as herein stated and sustained by the highest medical authority, and the *tonic* and *anodyne* properties of quinine, as deduced from long experience and corroborated by the learned Liebig, in its chemical composition, fully explain the phenomena observed? And will not the same views rationally explain its salutary effects in certain forms of inflammation, engrafted upon a debilitated constitution? I think so; fully and clearly.

In regard, however, to its use in inflammation of any of the viscera or vital organs, whether supervening upon a broken down constitution or occuring during the progress of fever, my experience teaches me to be cautious. I have always found local inflammations, in whatever organs occuring, either at the beginning or during the progress of fever, materially to complicate the treatment and interfere sensibly with the administration and salutary effects of quinine. And, I hazard the opinion that in *active congestion* or *inflammation* of any of the viscera, except when preceded by free *depletion* or existing in a *sub-acute form*, connected with *diminished innervation*, that its use in large or full doses will always be injurious and perilous.

P. S.—Since this essay was written, I have had the pleasure of forming the acquaintance of Dr. W. J. Hord, a young man of intelligence, and a graduate of the University of Pennsylvania. I learned from him that Dr. Wood, the distinguished professor of the Theory and Practice of Medicine in that celebrated institution, holds opinions

in reference to the medical properties of the sulphate of quinine which strengthen and confirm the views which I have here advanced to-day. Upon referring to the last edition of the U. S. Dispensary, page 1175, I find Dr. Wood expresses himself in the following language: "Given largely, in diseased states, it has been the obvious cause of fatal results, not so much, however, by its peculiar action as by coöperating with the disease in establishing *intense local irritation* or *inflammation*, especially in the brain. From its occasional effect in diminishing the frequency of the pulse and the general strength, it has been supposed to be essentially sedative in large doses. Such an opinion, unless well founded, might lead to *hazardous practice.* The probability is that the apparently sedative effect upon the circulation arises from an *overwhelming stimulant* influence upon *cerebral centres*, whereby the system is deprived of the support of these centres, and the heart's action is depressed with other organic functions. Similar effects may be obtained from excessive doses of most of the cerebral stimulants. Examination of the brain in the lower animals, after death from quinine, has shown great *congestion* of that organ and its membranes, and even *meningitis.* In the present state of our knowledge, therefore, it is safest to consider the sulphate of quinine as a direct and powerful *stimulant* to the brain."

ART. VII.—HYSTERIA—TÆNIA—HYDROPHOBIA.

BY ALLEN DEDRICK, M. D., A. M.,

OF NEW ORLEANS.

History of the Case.—Patient a female, aged 45 years, a native of Alsace, France, married to a third husband, the mother of several children, living in comfortable circumstances and resident here several years.

Temperament, bilious inclining to the nervous; stature under the medium height; form slender; previous health none of the best, being subject to rheumatic pains; never received any great injury of either head or spine.

Diet preferred and habitually used, farinaceous substances and

crude vegetables preserved in vinegar; most common beverage, coffee.

April 28th, 1854.—Found her suffering from an attack of subacute metritis with a muco-sanguinolent discharge from the womb. Symptoms of hysteria were also present—moreover she had passed, the previous morning, about thirty pieces of tape-worm. By the use of the aloes and myrrh pill with belladonna and the hip-bath, she was speedily relieved of her pains. During the operation of this medicine, she passed another piece of tænia, measuring fifteen and a half feet. Feeling now very well, the administration of a vermifuge was postponed until other symptoms should indicate it.

May, 8th.—Her husband stated that her right arm had become partially paralyzed. He was directed to use frictions with some stimulating liniment.

May, 10th.—Found her again ill; complaining of nothing but a slight pain in the right arm, which was probably due to the action of the remedy. Observed no fault in the use of the arm.

On examination, there appeared to be no disorder of any of the organs contained in the thorax or abdomen; but peculiar spasms, very much like those of hysteria, affected the muscles generally, more especially those of the throat. She complained of thirst and an inability to satisfy it,—nay the bare sight of any fluid threw her into these spasms. This was noticed first, this morning. The eye did not betoken hysteria nor was any pelvic pains complained of. Pulse, slightly accelerated; skin warm and dry; speech animated; defæcation normal. She could swallow only pieces of moistened bread, the juice of oranges or lemons and small pieces of ice. Spasmodic efforts accompanied the act of deglutition. Antispasmodics were given every hour, and frequent enemata administered. Under this treatment, an abatement of symptoms was observable.

On making inquiries, I learned that she had been bitten in the hand, by a strange dog, about six months before: moreover the man, who subsequently killed the dog, averred that he was rabid.

May, 11th.—Learned that she had passed a bad night—the medicine having had no further effect. Deprived of sleep; tormented by a craving for water; incessant nausea; retching and vomiting, at first of bilious matter, afterwards of the contents of the stomach;

profuse flow of saliva, which immediately excited spasms in the throat, unless spit out; unable to swallow anything but moistened solids, she exhibited a state of suffering which may be imagined but not easily described.

She was sitting in bed, and complained of fatigue and weakness. The vomiting became more frequent and painful and deglutition more difficult. Her eyes sparkled; hands cold and shrunken; pulse scarcely perceptible; speech coherent, except when speaking of drinks or of the dog, whom she seemed inclined to defend against any accusations of madness. Evacuation of urine and fæces as in health. She manifested no disposition to bite or to do any injury. About 1 o'clock, P. M., she became tranquil and asked for a drink of vinegar and water. She now drank apparently without difficulty. Her face was bathed with the same, when she expressed herself much relieved, feeling only a slight pain in the head, and desired to be left to sleep. She remained quiet, apparently sleeping; the warm perspiration starting forth; breathing regular. A few minutes after she was observed lying with her eyes turned upward, and was then bathed with the spirit-wash. Reviving, she expressed her hope of soon being well, and again desired to sleep. Shortly after this she breathed her last as gently as happens in death from any other cause.

This took place thirty-three hours after the appearance of the symptoms of hydrophobia.

Saw the body two hours after death. Countenance placed; cicatrix on the hand, scarcely perceptible; body spotted with ecchymoses, particularly in the course of the nervous trunks. No autopsy.

Observations.—The above is a succinct history of a case which rarely presents itself to the observation of the general practitioner. Viewed simply with regard to the various obscure symptoms therein revealed, it becomes interesting; but studied with an endeavor to discriminate between them and to refer each to its cause, it becomes exceedingly difficult and yields much food for thought. A cursory review of the case may not prove unprofitable. A female, after passing through a laborious and eventful career, arrives at that period, universally acknowledged to be the most critical era in woman's life, and finds herself, in addition to the other ills incident to that stage,

affected with one of the worst of internal parasites, and that too, while her system is under the silent influence of one of the most dreadful poisons known. For the previous three weeks, a change had been going on in her system; the womb was about ceasing from its function, and a white and red discharge was taking the place of this her last monthly evacuation. Symptoms of hysteria, then first manifested themselves; but yielded readily to simple treatment. The first appearance of tænia was noted on the evacuation of the pieces above mentioned. I could not learn that she had ever before been troubled by this kind of parasite. A few days before death she experienced a sensation, as if a worm was advancing up the throat; but a draught of water removed this feeling. Was this an hysterical symptom, or was it the prelude to the more frightful spasms which followed in the course of the next thirty hours? A pain, succeeded by numbness and partial paralysis, was felt in the wounded arm some three days before spasms manifested themselves, or an inability to swallow liquids was observed. Death ensued some thirty-three hours after, during a calm, when no signs of disease were noticeable, and only appearances of utter prostration, marked the close of the scene.

Now several questions, not devoid of interest, present themselves to the inquiring mind. A few of these I shall state, and venture thereon such comments as the case seems to bear out.

1st.—Was there anything in the constitution or habits of the individual, that would conduce to the generation of these parasites?

This query gives rise to several others, and until we know what relation there is between the existence of tænia and any of the hereditary or acquired diseases, it will be impossible to answer the first satisfactorily. It will, however, be borne in mind, that the patient was of the bilious temperement, was subject to rheumatic affections and preferred a crude diet. As to the second query, one would suppose, on first thought, that it could be readily answered in the affirmative. Yet, who can decide, whether her morbid tastes were the cause or the effects of the presence of tape-worm. This is a subject which will be more fully treated by the Editor of the Journal.

2d.—What caused the expulsion of the fragments of tænia, as above recorded?

The portions first voided, were in small pieces, and, nothing but as

common cathartic pills had been taken, it is presumable that, having become broken by the peristaltic action of the intestines, they descended into the rectum and were evacuated with the contents of that bowel.

Will the same explanation apply to the expulsion of the last piece, measuring fifteen and a half feet; or shall it be attributed to the action of the aloes, myrrh and belladonna pill, on the worm itself? Taking into consideration the tenacity with which this entozoon clings to the human entrails—resisting often every means employed against it—I am inclined to believe that its death is due to some other cause, and that its expulsion merely was effected by the cathartic operation of the medicine.

3d.—What relation had the tænia to the uterine disturbance?

Reasoning from known pathological conditions and therapeutical effects, one can affirm, that such an irritation in the bowels, could not be without some influence, tending to aggravate the disorders, incident to that period of life. Moreover those symptoms ceased, on the expulsion of the worm, much too soon to be wholly attributed to the action of the medicine on the system. Whether the entire worm was expelled or not, I am unable to say; yet enough had come away to materially lessen the intestinal troubles.

4th.—Had the tænia any influence in calling into action the poison of rabies, so long dormant in the system?

The known effect of intestinal irritation, in exciting and aggravating all nervous affections, (I do not like to say *reflex* actions of the nervous system) particularly those of a spasmodic nature, would lead one to answer in the affirmative. Yet the case is obscure and the relationship doubtful. That the presence of a worm of such a size, could aggravate the malady, can easily be conceived; but to aver, that rabies would not have manifested itself, had tænia been absent, is more than one can do with any show of reason.

5th.—Did the belladonna have any influence in postponing the attack of rabies?

The purely nervous symptoms were very much alike in the first and second attack. In the first they were removed by the medicine and by the expulsion of the worm, and did not again appear, until a day or more after the pain in the wounded arm. Now belladonna has been

extolled as a remedy in this affection; yet like too many others, it has failed. The only influence I can attribute to it in that regard, is due to its known effects in quieting excitation of the nerves.

6th.—What was the mode of death?

"Death is best defined to consist in a cessation of excitability." This may be brought about, (1st) by anything that annihilates the sensibility of the nervous system; (2d) by anything that exhausts this excitability. If only a portion of the nervous system be affected, death may ensue, simply from the arrestation of some function indispensable to life. Exhaustion is the consequence of persisting super-excitation, which is necessarily caused by an excitant. This excitant may be of such a nature, as to cause normal manifestations; and it may also be of such a nature as to cause abnormal-spasmodic-manifestations. If the former, the patient dies simply exhausted; if the latter, death may be caused, but is not necessarily, by arrest of some vital function. The first may be relieved by procuring rest; the second by coördinating the manifestation and then rest. Consequent on, or coincident with exhaustion, is congestion of one or all the nervous centres, and thereby pressure and consequent cessation of function. (Apoplexy from excess.)

Again, if the excitant be of such a nature as not to deteriorate the blood, life may be saved even after congestion has taken place; but if it has already deteriorated the blood, life can be saved only by the elimination of the poison; since a congestion of normal blood is obviously less dangerous than a congestion of abnormal. For, remove the congestion of the former and a normal stimulus is still there; while in the latter case an irritant would remain. Certain poisons are of this class of excitants. Those that directly attack the nervous centres, produce an immediate effect and death is speedy; while those that affect the nervous system, only after deteriorating the blood produce a slower death. To this latter class belong the poisons called septic. The former may fail to cause death through want of intensity; the latter, from being neutralized, or by being speedily eliminated from the system. I believe that the poison of rabies belongs to the latter class, and hence that the tendency is to a death by exhaustion, manifested by signs of congestion of the nervous centres. (Paralysis, Apoplexy.) But this termination is not always reached, since, du-

ring the stage of super-excitation, spasmodic actions may take place, and apnœa ensue. English and French writers, insist on asphyxia as the mode of death; while the German assign also palsy and apoplexy as the termination.

My patient being a German, doubtless preferred dying after the mode in vogue in her fatherland. That she did thus die, I am convinced, by the sensation of pain and fulness in the head; by the upturned eye-balls; and by the easy sinking into sleep—none of which symptoms were noticeable until half an hour before death.

7th.—What is the nature of the poison?

On this subject, I am forced to confess, that the above case throws no new light.

From experiments, made with common saliva, it would appear that the injection of this fluid into the veins or arteries will produce symptoms almost identical with rabies. Nay, it has been asserted that the saliva of a healthy dog, received into a wound, can cause in certain unknown conditions of the system, all the manifestations of hydrophobia. Hence, it would appear that there is a principle in healthy saliva, which, if taken in sufficient quantities into the circulation, will act as a poison.

Those curious on this subject may consult vol. 8 of Ranking's Abstract; South's Notes to Chelius, &c.

8th.—How does it enter the system?

Dr. Marshall Hall (Diseases of the Nervous System, Lond., 1851, p. 334,) says: "The poison is probably inserted into the substance of the fibrillæ of excitor nerves." That the nervous branches are wounded is highly probable, but that the rabies is due to such wounding, I deem it very improbable; for, if his supposition were true, there would be a speedy manifestation of the symptoms, or, at least as in tetanus, when cicatrization took place; and, moreover, the wound would exhibit some anomaly in healing; neither of which takes place—hardly as an exception to the contrary as a rule. The wound heals as kindly as any other bite, and pathognomonic symptoms do not manifest themselves often not till the lapse of months—nay, even years. Moreover, the experiments above alluded to, rather countenance the opposite view.

Hence, it is more probable to suppose that the poison enters the

system by means of the absorbents, remains either dormant, or most likely undergoes a zymotic change. That it does not pervade the whole system, at first, is probable, from the fact that on its breaking out, the neighborhood of the wound is the part first affected. Moreover, the prophylactic treatment is wholly based on this idea, and thus, also, therapeutics strengthens the view in opposition to the supposition of Marshall Hall.

The above case merely corroborates the fact long ago observed.

9th.—Is rabies a curable malady?

If the view of its pathology, which I have but partially developed in the sixth section, be correct, we may confidently pronounce it curable. But, as from our ignorance of the nature of the poison, we are unable to select an antidote, the successful cases will be few. Many cases, like the one above described, present a stage of quiescence, as if the disease had exhausted itself, before destroying the patient. In this stage, suitable medication, it seems to me, would not fail to support the patient and hasten the elimination of the morbific matter. Patients have been saved in the collapse stage of cholera, why not in this stage, which so nearly resembles it?

10th.—What treatment promises to be the best?

Knowing no antidote, we should endeavor to prolong existence, in order that the patient may have as alternative the kind of death most remote, viz: exhaustion with congestion. To attain this, it is obvious that all means should be used to still the inordinate actions, as perfect quiet, absence of light, sedatives, baths, &c.; and, if the remission takes place, stimulants.

I would recommend the following course, modified to suit the case. The patient to be kept, as above mentioned, under circumstances to ensure quiet. A complete evacuation of the bowels, either by pill or by an enema introduced by means of a flexible tube into the colon; a bath, warm, cold or vapor, using that kind which is practicable in the particular case; ice ad libitum as long as it can be swallowed; chloroform, inhaled if posible, or introduced into the bowel, repeating it as often as judged necessary; lastly, stimulants externally and internally. I prefer quinine and ammonia given in a beef tea enema, frequently repeated. If spasm of the glottis threaten to be fatal, tracheotomy should be resorted to. I would

give nothing by the mouth except pieces of ice to ally thirst, for obvious reasons. By such means, with close attention, I believe that some cases can be saved. From cases reported, (Ranking's Ab.) chloroform promises more than any medicine of its class.

It is usually given by inhalation, but this is often impossible from the spasms that are excited by it. I tried that method in the above case. It was my intention to give it by enema, but before I could procure my apparatus the patient expired.

Much more might be said on this subject; but as I did not contemplate writing an article on hydrophobia, and only wished to present several points in the above case, I shall leave the subject for future study.

ART. VIII.—ON THE REPUTED CAUSES OF YELLOW FEVER, AND THE SO CALLED SANITARY MEASURES OF THE DAY.

BY M. MORTON DOWLER, M. D.,

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I propose, in the present communication, to offer a few reflections on the notions, ætiological and preventive, that to a great extent prevail here touching the subject of yellow fever. In proceeding to do so, I shall offer a few remarks appreciative of the filth theory, as deducible from the condition of things in and about Gormley's Basin and Canal, during the epidemic of 1853. I may say, once for all, at the outset, that I am no advocate of filth, nor for any government or individual who is. Filth can never be salutary, but the reverse, when it has any effect at all. It is not only clearly noxious to the *physique* of our species, but it offends our moral sense. It sustains the same relation to the body that we are assured sin does to the soul; and, as body and soul mutually re-act on each other, so do sin and filth intermingle, and, as it were, become confounded with each other. The wages of filth, as well as the wages of sin, should be by every possible means avoided by "a Reform Council." But, filth though bad, is neither a basilisk nor a Gorgon's head; nor is it the cause of yellow fever. Sanitarian philosophers in our midst appear to regard it as all of these. The physician who looks beyond the hold of a ship, or who raises his thoughts above the contents of the gutter, is regarded by many as in-

capable of discerning the *punctum saliens* of all epidemics. The ship-hold and gutter-philosophers desire to claim sway; and in their crusade against filth, on the yellow fever basis, go for expending millions of the public money, and devouring the commerce of New Orleans. But *revenons à nos moutons.* All are familiar with that remarkable caldron, the Gormley Basin and Canal. The basin occupies the large square bounded by Felicity Road, Dryades, St. Andrew, and Hercules streets; and the canal, which is about eighty feet wide, leads from the basin towards the swamp, for a quarter of a mile, and there, at Ellen street, suddenly terminates in a ditch two feet wide. The canal and basin were originally about ten feet deep, but during the last eighteen years they have constantly been receiving the discharges from St. Andrew street and from Felicity Road, and from all the streets that enter into the latter at an acute angle. None of the deposits ever having been removed from the basin and canal, they have gradually filled up, till within the last three or four years the dregs of the whole area have begun to show themselves above the surface of the water. After a heavy rain the basin has about four inches of water in the centre, with just sufficient depth to cover the two deltas formed at the *embouchures* of the gutter of St. Andrew street and Felicity Road. This enormous area, from which no filth ever yet made its escape, has no other outlet than the narrow ditch in which the canal terminates, and at every point within the area a pole can be thrust down seven or eight feet, piercing, at the same time, thrice the number of kindred ingredients to those enumerated in the "Wierd Sisters'" catalogue. For all we know, indeed, it may contain some of the "grease that sweaten from the murderer's gibbet," for it was formerly the site of the gallows. The appearance of the hot sun, after a rain, speedily covers the caldron with a deep green mantle, which a few hours of solar action converts into an elevated black foam. As the evaporation goes on, nearly the whole space becomes uncovered, the basin yields up its dead, and the whole necropolis of departed animal and vegetable life lies naked to the rays of the sun. To crown all, the whole district is occupied by a series of soap-factories and tanneries, which no precaution can prevent from exhaling an offensive odor, as stale animal matter is here the material operated upon.

If there be a word of truth in the "filth theory," would not the

yellow demon have naturally erected his throne on the vapors of the Gormley canal, and after having smitten all in the immediate vicinity, have carried the war into other districts? Such, however, was not the case. The wayward disease did, it is true, early make its appearance at "the caldron," rapidly carrying off one Kehm and his wife, the former dying with the black vomit. There appears to be a discrepancy with regard to the dates of these cases. My own recollection fixes them early in May. The sanitary commission, who have "sent for persons and papers," have fixed the cases about the first of June. The discrepancy is, however, quite immaterial as affecting the conclusions which I have drawn from these cases, namely: *First.*—That they were amongst the earliest, if not the very first cases, that occurred. *Secondly.*—That they not only clearly occurred without any communication with the shipping, but that they made their appearance, and terminated fatally many days before it has been pretended that there was any case on board of any ship that came into the port of New Orleans. *Thirdly.*—That Kehm and wife could not have contracted the disease by personal contagion, as they had not been in contact with any person laboring under yellow fever. *Fourthly.*—That Kehm and wife did not communicate the disease to any person, though immediately surrounded by unacclimated persons; for no person in the neighborhood was attacked for six weeks after the time of Kehm's wife's death, following the date given by the sanitary commission; and after the expiration of that time no less than fifteen persons were attacked in the same square almost at the same time.

Now, we would ask the filth theorists, how came it to pass that the disease abandoned the shores of the Gormley Canal and Basin for six weeks, in which the hot month of July was included, and betook itself to the front of the Fourth District, attacking squares which were but sparsely populated, free from stagnant water, and comparatively free from filth? From the beginning to the end of the epidemic I was in the midst of it, and I can positively affirm that the disease showed no special predilection for localities having a concurrent excess of heat, moisture, and animal and vegetable putrifaction; the driest, most elevated squares, the cleanest streets, houses and yards, being alike, and simultaneously, invaded with places in which the reverse prevailed. The early appearance of the two yellow fever cases at the

canal and basin, and the disappearance of the disease from these places for six weeks, while it ravages the front of the Fourth District, shows conclusively that the "ingredients of our caldron" did not in the least affect the health of the neighborhood, but that the disease actually ran away from the most awful repository of filth in the corporate limits of New Orleans, and that it is probably a matter of supreme indifference, so far as yellow fever is concerned, whether the rear of the Fourth District be girt with the piney woods of Biloxi, the bluff of Natchez, the sand hills of Vera Cruz, the rocks of Havana, or the seething canals and basins that "boil and bubble" in the rear of the Crescent City. In fact, the filth theory is wholly untenable, not only from the entire history of yellow fever in this community, but from every analogy known to science. Every product, whether solid, fluid, or aëriform, or imponderable, whether resulting from the decomposition of animal, vegetable, or mineral matter, produces effects which are remarkably uniform. They do not discriminate between the acclimated and the unacclimated, nor are their noxious or innocuous properties at all modified by such circumstances. For instance, the deposits at Gormley's Basin give off amongst other substances, sulphuretted, carburetted, and phosphuretted hydrogen, and ammoniacal gases, substances which with ordinary ventilation merely give evidence of their existence by offending the nostrils, in bad ventilation become oppressive, and when highly concentrated quickly destroy life. The effects are uniform in all climates, and to assume that the mere inhaling of a mixed atmosphere, which one person breathes with perfect impunity, should give another the yellow fever, another the cholera, and a third the typhus fever, is simply a *reductio ad absurdum.* Our knowledge of these gases demonstrates that the reputed cause is insufficient to produce the effect, and there is not a shadow of proof to show that it does produce it. The exhalation from departed animal and vegetable life mingle with our every breath, and it has never been shown, nor ever can be shown that they act otherwise than uniformly. Touching the filth question, let us not be misunderstood. Let every alderman "search the scriptures." There is scarcely a chapter in that sacred book which does not admonish all to "wash and be clean." Common decency also speaks aloud. False and improbable theories, however, can do nothing else than lead to useless

and extravagant and corrupt legislation, and to lull the public into a false security.

The distinguished Professor Riddell, of the University of Louisiana, has adopted the atmospheric theory of yellow fever which he deduces mainly from the filth platform. His views are liable to all the objections which we have urged, besides others peculiar to themselves. He holds the following language:

"That the disease has been marked by characters of infection, and infectious communicability, the poisonous matter (doubtless some species of living organism) maturing its germs or spores on the surface of solids devoid of life, surrounded by impure air; which germs become diffused in the impure atmosphere.

"Three peculiar conditions seem to favor the development of the infection. 1st.—The absence of ozone, the great chemical promoter of oxidation, which absence permits the undue development of obscure cryptogamic life. 2d —Abundant emanations from decomposing and disintegrating organized matters, complex products, gaseous, liquid and solid, the pabulum or blastema of cryptogamic growths. 3d.—The presence of the specific organism, whose perfected spores constitute the material cause of yellow fever."

The cryptogamic origin of yellow fever is untenable for two reasons: In the first place, it is denied that such cryptogamia are known to exist, although Professor Riddell appears to have no doubt on the subject; and in the second place, whatever forms "of obscure cryptogamic life" may be discovered to exist, all analogy shows that they cannot be admitted as the cause of yellow fever no more than the gases of which we have spoken; for all the poisonous fungi or cryptogamia must be admitted to act pretty uniformly and to injure all persons alike who are subjected to them. Acclimation could no more render a poisonous fungus innocuous than it could render a poisonous mineral so. Professor Riddell's poisonous cryptogamia contravenes all toxicology which teaches us that all animal, vegetable, or mineral poisons not only affect those who never have before been affected by them, but that they will act equally a second, a third, or a twentieth time. We need not follow the learned professor in relation to his views of ozone. The condition of the atmosphere in regard to that shadowy agent, whether plus, or minus, does not appear to have any

effect in the production of the ordinary cryptogamia of the botanists, and why on "the obscure forms of cryptogamic life?" Did ozone exist in such quantity in the atmosphere as to prevent the development of *any* form of either vegetable or animal life, including "the obscure forms" of Professor Riddell, can it be otherwise than probable, nay certain, that the whole population would be thereby asphyxiated?* Professor Schönbein who first described ozone, says that its effects on the lungs are similar to those of chlorine and bromine; that a mouse was killed by it in five minutes, and that he himself, was seriously affected by breathing an atmosphere charged with it. Neither Berzelius nor Schönbein were able to say positively what ozone is.

According to Professor Riddell's hypothesis there was an absence of ozone in the atmosphere of New Orleans, and of course at Gormley's Canal and Basin, and, also according to the same, of course in the latter locality an abundance of the "pabulum or blastema of cryptogamic growths," as there was no lack of "emanations from decomposing and disintegrating organized matters, complex products, gaseous, liquid, and solid, ;" and yet, the disease showed no preference for the locality, first appearing and then leaving it for many weeks. Waiving all opposition to Professor Riddell's gratuitous and imaginary premises, and supposing he had actually discovered the non-ozonic state of the atmosphere, and that it was charged with myriads of *fungi venenati*, he would not have thereby made the first step towards elucidating the ætiology of yellow fever.

As was to be expected, the disease reappeared at the Gormley Canal, but it was not till about the first of August. And here it will naturally be asked whether or not all the non-ozonic, cryptogamic and filth agencies—the latter needing neither retort nor microscope to prove their existence—tended to increase the number of cases or render them more malignant when they occurred: or whether the patients would have fared any better in the hill-side towns and piney-woods where the disease prevailed? The number attacked by yellow fever in 1853, in New Orleans, could not have been less than 27,000, and the number of deaths were probably from the same cause not less than 9,000, making a mortality of thirty-three and one-third per cent., of those who suffered. Here is a picture truly frightful to contemplate, but it bears no comparison to the ratio of mortality that occurred in most of the country towns, villages and plantations, many of them

dry, elevated, and free from filth agencies. In many of these localities nearly the whole population was attacked, and death occurred at a ratio that would have carried off our whole unacclimated population in less than forty days! Comparatively speaking, New Orleans was but lightly scourged, but she lost one in three of those attacked as a general result. As might naturally be expected some portions of the city exhibited a higher and some a lower ratio of mortality, and according to my observation, which was very extensive, though the disease attacked at the Gormley Canal, as it did throughout the city nearly all of the unacclimated, it was far less malignant there than in any other portion of that suffering region, the Fourth District of New Orleans, excepting around the Saraparu Market. On examining my records, I find that I attended forty-seven patients immediately on the basin and canal, the most of them residing in soap factories, nine of them being children under 10 years of age, the other thirty-eight were of various ages; twenty-seven males and eleven females. The women and children all recovered; four of the males died, one a young man, aged about 24 years, who died in Ellen street, was apparently doing well till he was taken one night to the upper story of the soap factory, and I believe his death resulted from the heated air that arose from the furnaces, rather than the intensity of the disease; another was a young man of about the same age who was removed from Ellen street to Magazine street, where he died of black vomit; a third was aged 60, and was affected with tuberculosis of the lungs; and a fourth aged 35, relapsed after going out to work, and died with black vomit. The forty-three who recovered were mostly mild and manageable cases. In the square occupied by Mr. Ebinger's soap factory and residence, which is bounded on one side by the Gormley Canal, and on another side by the Gormley Basin, and which receives the whole venom of the caldron, there were fifteen cases; occuring, however, after the 3d day of August. I attended on twelve of these cases. All fifteen recovered. There was not a death on the square around which exists more filth—more of the "pabulum" of which Professor Riddell speaks, than can be found in any square in New Orleans, and where according to the filth theory, the maximum of mortality ought to have been found.

As for the treatment pursued, whatever may be its merits, one of

the most important elements of the success in these cases was the mild type of the fever; for, in pursuing the same general course in certain other quarters of the city, no such result could be obtained. I attended thirty-five cases in the squares around the Saraparu market without losing a case—one of these too being a case of black vomit—whilst in the neighborhood of Washington street, in one house, three were attacked and two died; in another, five were attacked and three died; in another, three were attacked and two died. To show that the disease was malignant without any discernible local cause, in the house in which the three out of five died, no less than six persons were attacked in the adjoining house, and every one recovered. I found but little difficulty with children. Some were attacked with convulsions, but rarely. In them the disease was so mild that in many cases I found the *vis medicatrix naturæ*, altogether trustworthy, yet all toxicology shows that children are fearfully sensitive to all poisons, including the cryptogamic and gaseous. "Doubtless," says Professor Riddell, "the poisonous matter" is "some form of living organism." I should say that doubtless the professor is mistaken, for if the disease has such orgin it would not be so merciful to children. Immersed in a common atmosphere, in a common room, the stalwart father falls mortally poisoned, while the tender infant is affected with a mild fever, which in a few hours spantaneously goes off in a perspiration. Strange fungi! strange gases! strange poisons! A strange foundation have the advocates of these theories! A strange pretense for all the extravagant systems of disinfection, quarantines, and Quixotic schemes of drainage! Professor Riddell gravely proposes to restore the atmosphere to its wonted quantum of ozone!

Touching the great epidemic of 1853, Professor Riddell comes deliberately to the conclusions "that the towns and plantations of the Southwest have *this year* derived their yellow fevers from New Orleans;" and, further, "that although black vomit fever, or types of yellow fever, *may perhaps originate in this region*, yet, the *germs* of our epidemic of 1853, have probably been derived from countries further South." The "germs" alluded to are, according to the professor, the "specific organism, whose perfected spores constitute the material cause of yellow fever." If no further evil could result from the above than the mere enunciation of purely speculative ideas, on the

part of the able professor, we should have no demurrer to enter in the premises. But, when all the evils of expensive, ruinous, and oppressive legislation are sought to be saddled on the people, under no better pretence than the groundless dogmas here put forth, the case is different. The professor is a member of a learned and salaried commission, who are sitting in judgment on the causes and prevention of yellow fever; in the employment of the city government. The opinions and theories put forth should be worth paying for, and worthy of being a guide to legislation. The public will hold him emphatically to the proof of what he has set forth in his official report, published by authority of the State. For example, at what time, place, and under what circumstances, and before what witnesses were cryptogamic bodies discovered, "whose perfected spores" Professor Riddell categorically deduces to be the "material cause of yellow fever?" Were they (if they exist,) more numerous last summer, than the summer of 1852? What were the circumstances of time, place, witnesses, and re-agents, that led to the non-ozonic revelation?

If these views of the professor be correct, then why is not he consistent with himself, and instead of saying "that is proper and feasible for New Orleans to have *some kind of quarantine;*" why did he not recommend, in the name of common humanity, the most rigid and uncompromising system of restriction? "Some kind of quarantine," indeed! Alas! if it be true, as he suggests, that the great epidemic was freighted into New Orleans, what can excuse the luke-warmness here exhibited in relation to quarantine? Would any commercial or pecuniary sacrifice be too great to save 27,000 persons from the bed of suffering and 9,000 from the bed of death? And, if it be true, that the pestilence was freighted out of our city to ravage six States of the Union, what sacrifices ought we not be willing to make to prevent our city, if possible, from being the scourge of the South! According to Dr. Riddell's ætiology, we ought to stop at nothing, even to the absolute interdiction of the Mississippi, when the yellow fever makes its appearance in New Orleans. No ordinary quarantine could be trusted. Of what avail is it that yonder ship has passed the ordinary quarantine, and no cases of yellow fever have occurred, and she is pronounced clean? Who can say that she may not contain, at the same time, "perfected spores" of millions of the Riddellian cryptogamia?

Now, we deny that importation played any part in creating the epidemic of 1853; and we are prepared to prove that the causes of yellow fever, whatever they may be, had produced the terrible black vomit case of Kehm, and also the case of his wife, independently of anything "further South." It is easy, throughout the country, to designate scores of instances in which the disease appeared without any possibility of its having had its origin either directly or indirectly in New Orleans. We further maintain that clear and unequivocal proof of isolation and independence of origin, in a single case of yellow fever, is evidence of more value in determining the question of importation and exportation, than all the coincidences, successions of events, and consequences that Professor Riddell has elicited from his extensive correspondence throughout the country, ten times told.

In order to have carried the disease from New Orleans and to have rendered it epidemic throughout the great Southwest, and at the same time to reconcile the whole with his ætiology, an impossible prodigy must have been performed, and Professor Riddell's "three peculiar" [atmospherical] "conditions which seem to favor the development of the infection," must also have been shipped on board of the steamers and carried along with the fugitive patients! The non-ozonic, the miasmatic, and the cryptogamic conditions of the New Orleans atmosphere must have been freighted on the same boats with the flying people. No one can abandon this ludicrous position, and adhere to the professor's ætiology, without abandoning the whole allegation, unjustly charged against New Orleans, that she created an epidemic in six States of the Union. If the New Orleans atmosphere was not shipped into the country, the three conditions must have existed in the country at the time of, or before the shipping of the yellow fever patients, and consequently the allegation falls to the ground; and, if as Professor Riddell admits, and as all ought to know, "the disease has not been personally contagious," to what other conclusion can even he arrive, than that the great epidemic could have extended itself in no manner inconsistent with entire independence of its doings in New Orleans.

The disease might have been carrried into New Orleans and carried out of it; but it could not have become epidemic from anything that could be carried in or out, even on Professor R's. ætiological conclusions. The disease of 1853 was in no respect different from the dis-

ease which has occurred here every summer, either in the epidemic or sporadic form, for the last twenty-five years; and during the epidemics of '37, '39, '41, '43, '45, and '47, had it been possible for New Orleans to have originated a Southwestern epidemic, it would have made its appearance. The unknown conditions, however, which produce the disease were present in New Orleans and absent from the country, and, hence, all the power of steam in wafting away goods, patients, bed, and bedding, could not confer an epidemic on any plantation, town or city, any more than could absolute non-intercourse have prevented the general epidemic of 1853. The mere matter of fact is very true, that the disease was, during the great epidemic, carried into the country in the bodies of individuals; but the epidemic was not. The whole was merely "carrying coals to Newcastle;" and New Orleans, instead of being chargeable with contaminating and poisoning the mighty ocean of the atmosphere throughout six States of the Union, has been placed before the world by her chivalric sons simply in the light of an angel of mercy to the stricken Southwest. New Orleans was indeed the great position of safety against a common enemy, whose favorite field, and multitudinous birth-places were, in 1853, in a special manner in the wide spread country itself.

There can be no worse sample of bad faith exhibited, than to take any hand in deceiving the public in relation to our city on the health question. The truth should be frankly asserted, and published without either croaking or palaver. I am far from considering New Orleans an unhealthy city, so far as her resident and native population is concerned. The articles that have, from time to time, appeared in this journal, by my preceptor and brother, its present editor, and Dr. S. A. Cartwright, have held up in their true light the fallacies that have been promulgated by those who have attempted to write down the reputation of the city with regard to salubrity. But what will be thought by every physician, possessing the commonest capacity for observation, of this specimen of see-saw disbelief displayed by Dr. Riddell, when he says, "black vomit fevers, or types of yellow fever *may perhaps* originate in this region." Not "yellow fever" quoth he, but "black vomit fevers or types of yellow fever," and only "perhaps originate in this region," at that. Professor Riddell surely is more at home with his retorts, in the Laboratory of the University, than in pronouncing on the nativity

of yellow fever. This question can be put at rest by too many reliable witnesses who do not dogmatise, but who know. We ourselves are fortified with eighteen years of observation and experience here, and as an off-set to what our able professor of chemistry has said, we assert what we know, and have seen. Not a summer has passed, during the whole period, in which we have not seen *bona fide* cases of adynamic and hæmogastric yellow fever, with black vomit. The local origin of yellew fever is fully shown from the fact that it has appeared here annually, either epidemically or sporadically, for the last twenty-five years. The unmitigated form of yellow fever is not to be mistaken. Cholera itself does not imprint its ghastly character on the body with more fearful truth. A doubting professor of chemistry must throw off his Pyrrhonism. The disease has appeared in the Charity Hospital every summer for the last eighteen years, and the chances are, that for every single sporadic case in the Hospital there were twenty out of it, throughout the city. The disease has not only become native and local here, appearing either epidemically or sporadically every year, but so pervading an influence has the disease, that it imprints its character on numerous other diseases throughout the year. Hence, as we have occasionally witnessed, measles, scarlet fever, and dysentery, sometimes terminate in black vomit; also intermittent, remittent, and continued fevers are found occasionally to terminate in black vomit, jaundice, and hæmorrhages. Space will not permit us to enter further into proofs that the *bona fide* yellow fever makes its appearance here every year. On this point we stand fortified.

Such then being the facts, notwithstanding the doubts of a professor of chemistry, with what blighting and damning force do the doctrines of the quarantinists and importationists strike at the prosperity and prospects of our beloved city of New Orleans! If the disease can be imported, it can be exported. According to the above doctrines a sporadic case carried into the country, or into another city, might originate an epidemic. If the disease then appears here either epidemically or sporadically every year, what can the doctrines of quarantine do save to invite the whole world to lay our outward bound commerce under the most ruinous restrictions? All the expensive mummeries of fumigation and disinfection of goods must be endured, all the robberies, extortions, and vampyreism of officials. If a quarantine be use-

ful it is not useful to New Orleans. Its advantage could only accrue to those who bring it to bear against our commerce and our interests, and treat our city as a pest-house; and if a *scintilla* of proof cannot be adduced to show that the yellow fever ever did spread from importation, and if it can be shown (as it can be) that the disease has been repeatedly imported into this city and other cities without spreading—if it can be shown that it is imported without spreading, and spreads without importation, then are the quarantinists struck dumb as the sheep before the shearer. Their doctrines are not only untrue, but injurious to this community.

The leading argument, indeed, which has been made use of in New Orleans in favor of quarantine is, that it will create *confidence!* There is not, in the whole dictionary, a word suggestive of more fearful associations than this word *confidence.* Let the histories of rotten banks, broken hearts, dishonored virgins—nay, the history of the world bear witness. Nothing is, nor has been, more common than confidence unworthily bestowed. And, are we justifiable in invoking the confidence of the people and lulling them into security, on the strength of a measure which has been a universal failure? What, save the most absolute proof of utility, should challenge confidence in the premises? Is the *confidence* argument justifiable on any principle of good faith or fair dealing?

The history of sporadic yellow fever in this community, besides being of the greatest interest in showing, *first*—that it may appear, as such, for years without becoming epidemic; *secondly*—the local and indigenous origin of yellow fever; *thirdly*—the obsolute inutility of quarantine; and, *fourthly*—the impossibility of spreading the disease into the country by exportation, presents also some other curious matters for consideration. "Commerce is king," and it is no more permitted to any physician to report cases of yellow fever with impunity, in the absence of an epidemic, than to foretell and "encompass the king's death." This is well appreciated in New Orleans. The laws of the counting-house are inexorable; and the unfortunate wight who takes hold of a sporadic case of yellow fever, and reports it, gets hold of the "Bottle Imp." He is soon given to understand that there is no such case in either the day-book or ledger; and, he

is appealed from to "our most eminent physicians, who have each ten times his practice, and have not yet seen a case." If possible the disease must be ignored; but it has been found quite impossible. In some seasons, since 1837, the sporadic cases have become so numerous that they have been reported under different names; and amongst others was the very general appellation of *"febris flava!"* on the same principle that gave the *craziness* of George the Third, the name of *"His Most Gracious Majesty's indisposition!"*

Well might our mortuary records of sporadic yellow fever be marked with a *cætera desunt.* For instance, the cases of Kehm and wife, already alluded to, occurred long before the epidemic was declared—early in the season. He died ejecting the black vomit on his sheets. Afterwards his wife died also with the disease. A reverend father of the church administered the extreme unction, and a dozen persons followed them to their graves, now visible; yet, neither certificates, records, nor any written proofs can be found in the archives of the cemetery to show that any such persons ever either lived or died! Yet, we are often told that we cannot go behind the record, which is a finality! When such *lapses* as these are allowed to occur, what becomes of our implicit reliance on the records?

What man of common information would place implicit reliance on statistics based on mortuary certificates rendered, to a great extent, on the diagnostics of the laity-certificates, numerously rendered by citizens, scavengers, deputy-coroners, barbers, and all the charlatans and quacks, that batten on the credulity of the public?

There has been no end to the assigned causes of yellow fever in this community. There is no end to proposed legislation; but no guide to sound legislation. It is in vain, in the present state of our science, that we call on our law-makers to regulate their action by our electrometers, our hygrometers, our thermometers, our barometers, our rain gauges, and our river-gauges, in expending the money of a tax-ridden people to legislate the yellew fever out of New Orleans. Legislation had better come to a stand, when the blind offer to lead the blind. The theories deduced from this meteorological battery are all liable to the same objections that may be urged against the filth theory. These instruments *ad hoc,* are dangerous; for their uses are not understood in making out the result. Let the use of things be learned before

used. Let us not share the fate of the nervous youth, who was so fearful of lightning that he procured himself a pocket lightning-rod to hold in his hands in a thunder-gust! We hope to see a flood of light thrown on the causes and prevention of yellow fever by the sanitary commission. They will do much if they batter down prevailing errors.

The truth is, the causes by which by far the greater portion of the mortality of a city or community is produced are either conjectural or wholly unknown; though it is in the investigation of these that speculative medicine has exhibited its boldest flights and its greatest self-sufficiency. As a matter of course, in all climates a constant train of mortality lies in wait on the path of all that lives. Death though certain at last, has for its causes in every community those which are avoidable, and those which are unavoidable. To draw as far as possible the distinction between these two classes of causes, and as far as possible to apply the remedy to the former is to do all that lies within the competency of sanitary legislation. No earthly consideration can be of greater importance to any community than such distinction and such action. It overtops almost all other considerations known to medicine—not only securing the maximum amount of salubrity, but solving at once the question of the real salubrity or non-salubrity of any given district, and, consequently serving as a guide to the embarking of capital, labor, and life in the same. The magnitude of these objects has attracted the attention and occupied the pens of the most eminent inquirers in Christendom with results which though useful and interesting, are little satisfactory to the public mind. The controversial has largely predominated over the positive. The ground here proposed is, it must be confessed, one beset with extraordinary difficulties, and what is most remarkable in too many of those who have occupied it is, that the mania to do and to discover that which is impossible has thrown into the shade the great measures of known possibility, and practicability. The spirit that now characterizes sanitary movements in our midst fully illustrates the truth of this position and promises a grainless harvest. Nothing is attempted but the impossible. Nothing at present appears to secure sanitarian attention save only the causation and prevention of epidemics—subjects probably wholly beyond the ken and control of man. The avoidable causes of daily death exist in almost every square—are seen unheeded and unremedied.

We have no right from anything that is known, or from anything that is likely to be discovered, to rank yellow fever as one of the *avoidable* causes of mortality. Its epidemic appearance must be contemplated, in the present resources of human knowledge, as merely an ultimate fact—an accumulative manifestation of the general scheme by which all that is vital shall throw off vitality, and as little capable of explanation as are first principles or self-evident truths. Why matter *constantly* gravitates towards the centre, and why the yellow fever *inconstantly* makes its appearance in certain cities, are, for all that has been determined, equally inexplicable. Epidemics have existed in all ages, and no human agency has ever stayed their march, limited their boundaries, or shortened their duration. None of their laws or habitudes have ever been changed or annulled by man. Epizootics have also ever existed, and the animals useful to man have been swept away; and the sea itself has been invaded, and submarine pestilence has thrown to the surface millions of the finny inhabitants, that a few days before gambolled in health beneath the waves. *Epibotanics* (if such a word may be coined,) lay waste the domain of the agriculturalist. Beneath the surface of the soil the fatal law is executed, and the food of the Irish nation (the potato) becomes gangrenous. Who has decended into the sea, explored the soil, or scanned the mighty ocean of the atmosphere, and can pretend that, in all this, we have to do with an avoidable cause of mortality? Who can ward off the execution of this inexorable law of accumulative mortality? The people of New Orleans may exhaust their private fortunes for protection, the government may lavish its millions, sanitary laws may grow into codes, and sanitary officers into standing armies, but New Orleans, and all other cities, must be visited by epidemics. "The pestilence that walketh in darkness and that wasteth at noon-day," will walk and waste. At the present writing an ordinance is proposed to be passed by the Common Council, which revives and reëmbodies all that has hitherto been found expensive, useless and impracticable. The whole twenty-two sections must soon prove a dead letter and fall into disuse. The undertakings set forth in this ordinance are almost too stupendous for belief, and involve an interference with private rights and an extent of jurisdiction that can expect no quarter and

will receive none. The fourth section, amongst other things, gives the proposed Health Department power over the *elements* themselves! It says: "It shall be the duty of this department to have *surveillance and control over everything* that may affect the salubrity of New Orleans, or have a tendency to impair the same." Sublime surveillance and control! which of course includes the agent that produces yellow fever!

How is the provision in article four to be carried out, regulating the number of cubic feet of space for lodgers—one thousand cubic feet for each adult—unless it is proposed to partially rebuild the city, and provide houses for the laboring class, who, to economise, crowd into small houses. If the poor cannot pay for the requisite number of cubic feet, will the Health Department undertake to furnish it. Or, will not the people, outraged by this inquisition on the internal economy of their household affairs, eject these agents from their premises as intruders on their constitutional rights.

For the present, we take leave of our subject. I propose, in a future number of this journal, should time permit, to enter into the consideration of some of the *avoidable* cause of mortality in New Orleans. They are multifarious; their name is legion.

ART. IX.—CHLOROFORM IN STRANGULATED HERNIA.

BY B. F. TAYLOR, M. D.,

OF LOUISIANA.

Of all the maladies to which the human body is subject, that of irreducible hernia is probably the most formidable and important, calling forth the greatest amount of surgical and anatomical knowledge, requiring the most prompt and decisive action, as well as the utmost skill and dexterity in the performance of an operation, when that is rendered necessary by the inefficiency of the means usually employed to accomplish its reduction. In most surgical diseases, the patient can have the advantage of consultations with the most talented and experienced of the faculty, but not unfrequently an hour's delay will prove prejudicial to the success of the surgeon and fatal to the prospects of the patient.

It is known to practical surgeons, that the best directed efforts of the taxis, the use of cathartics, opium, bleeding and tobacco injections, not unfrequently fail in relaxing the strictured parts. In view of these difficulties, it has often occurred to me—since the introduction of chloroform as an anæsthetic agent in surgical practice—that the full influence of this article would fulfil the indications above spoken of, and probably dispense with—in the majority of cases—this formidable operation.

A case in point occurred recently, in my practice, where I had an opportunity of testing the correctness of these views. The patient, a middle aged man, had been afflicted with a hernia on the left side for a considerable length of time. Upon one occasion, when employed in wood-chopping, the hernia became strangulated; when I was sent for, and reduced it readily by the taxis. More recently, this man being engaged in the field with his hoe, strangulation again occurred. Upon visiting him I found his suffering to be very intense; pain in the abdomen and stomach, vomiting, &c. The tumor was very large and tender to the touch. The stricture, as is most generally the case in inguinal hernias of long standing, was situated at the external abdominal ring. The taxis was resorted to, and persevered in, but without beneficial results. He was then put under the full effect of chloroform. Relaxation of the external oblique muscle, and the stricture at the ring, was evident to the touch. The taxis was again resorted to, when the gut readily returned with a distinct gurgling sound.

This case is reported for the sole purpose of calling the attention of surgeons to the practical value and use of this most potent of remedies. It is an isolated fact, it is true; but in all cases where relaxation of the voluntary muscles is the object to be obtained—such as luxations, hour-glass contractions of the uterus, &c.—we are strongly convinced that the practitioner's most sanguine hopes will be more than fully realized.

Part Second.

EXCERPTA.

Art. I.—*The Perchloride of Iron.*

The French Medical Press has been and still is occupied with the therapeutic virtues of the perchloride of iron. Some of its advocates give it as good a character as Shakspeare gave to certain medicaments: "I have seen a medicine," quoth he, "that's able to breathe life into a stone, quicken a rock,"—curing—"unpleasing blots and sightless stains, foul moles and eye-offending marks—rotten diseases of the South, imposthumes, lime kilns i' the palm, and the rivelled fee simple of the letter take and take again such preposterous discoveries:

> The sovreign'st thing on earth
> Was parmaceti for an inward bruise."

M. Pravaz' remedy falls little short of Will Shakspeare's in its claims, being adapted to aneurisms, varicose veins, erectile tumors, nævi materni, fistulæ, ulcers, wounds, cancers, and so on.

Early in January, 1853, M. Lallemand read a paper in the Academy of Sciences upon the new method of curing aneurisms by the injection of a few drops of the concentrated preparation of this substance,—a method discovered by the late M. Pravaz, of Lyons, and tested by MM. Lallemand, Giraldès, Debout, and others.

The fundamental principle of curing aneurism by M. Pravaz' method, namely, by the coagulating of the blood at the seat of the lesion or aneurismal sac, had been enunciated prior to its demonstration by M. Pravaz: "M. Leroy d'Etoilles published several Memoirs in 1832—5, and 1844, the purport of which was, that aneurism

might be successfully treated by the injection of coagulants." But he who proves, discovers.

It appears, however, that M. Leroy d'Etoilles indignantly disclaims all pretensions to this discovery which he calls "disastrous," in view of its failures, and consecutive effects as inflammations, mortifications and deaths.

M. Pravaz and his friends found upon dividing the large arteries as the carotids of sheep, horses, and other animals, that the injection of from three to ten drops of this solution within the ends of the divided vessels, produced a firm, immovable clot, completely arresting all hæmorrhage in four or five minutes. When the quantity was too large, the clot was dissolved! An excessive quantity caused excessive inflammation with gangrene and sometimes death. The animals used for experiments were killed at various periods reaching to two or three months. The arteries were found to be obliterated above and below the site of the operation.

Much importance is attached to the quantity and still more to the quality and strength of the solution. Three or four drops were found sufficient for an aneurism as large as a pigeon's egg; a larger one, having a larger quantity of blood to be coagulated, requires a greater number of drops, say ten or twelve to the ounce of blood. M. Pravaz used the solution in its most concentrated form, which he injected into the vessel with a small trocar, which is made to enter obliquely, while at the same time the flow of blood in the vessel must be arrested by pressure for four or five minutes until a firm plug is formed in the tube.

M. Leroy d'Etoilles, "thinks that the injections should not be attempted in the aneurismal sac, but in the tube of the artery above. The canula does not always penetrate the concentric clots so as to reach the fluid blood; consequently, the remedy does not act upon the circulating current. The inflammation in so large a part is often excessive; the amount of coagulating fluid must be more than would be required in the normal tube. He thinks that the clots formed may be driven from the sac by the force of the heart's action, and that they may plug up some of the smaller arteries, and hence cause gangrene of the limb; and he thinks this explanation applicable to the cases related by MM. Malgaigne, Alquié, and Jobert."

During the year 1853 quite a number of aneurismal cases were reported in the French journals as having been treated by this method. On the 9th of November, M. Malgaigne, (Rev. Thér. Méd. Chir., No. 23,) one of its strongest opponents, read a paper to the Academy of Medicine on this subject, enumerating eleven cases of aneurism treated with the perchloride. Four died. Two recovered. The residue, most of whom suffered much from the injection, remained uncured. Even the successful cases he regarded as deplorable, because they would lead to further, and, as he viewed the matter, disastrous results.

The veteran Roux—since dead—took a part in these discussions. He looked upon the new method with distrust, and as being full of hazard; he preferred the ligature, which he had applied eighty-four times—having seen gangrene following in only three cases, and secondary hæmorrhage in only five.

More recently, however, the perchloride of iron has gradually gained ground. It has been used more freely, and in a more concentrated form, than had been advised originally by Pravaz. In a case of false-aneurism of the brachial artery, thus treated successfully, the patient subsequently died of another disease; post mortem examination showed an obliteration of the brachial artery, with a dense clot occupying the aneurismal sac.

The formation of a clot in the sac, merely, how firm soever that may be, is probably not the essential point after all. The editor of this journal might, from his own experience in the ligation of arteries for aneurism, show that in case of traumatic aneurism, where the original wound had been completely healed for weeks, the clot proved firm, resembling, under the microscope and to the naked eye, dense, semi-organized membranes, mingled with firm parenchymatous and fibrous masses, not dissolving in water, but susceptible of being unravelled or separated.

M. Roux, though an advocate for the Hunterian method, and opposed to the perchloride injection, thought favorably of the local application of cold, wherewith to produce a clot and a cure. M. Valpeau observed that he had, by a freezing mixture of ice and salt, produced clots in the aneurismal sac, but these dissolved apace and the disease re-appeared. Pravaz' method probably produces not

only a clot in the sac, but one also in the proximal end of the artery; or failing in this, the cure fails. This is doubtless the fundamental, but overlooked point.

M. Giraldès, in his own name and in that of M. Goubaux, read a paper (April 18th, 1854,) in the Imperial Academy of Medicine, giving an account of some experiments performed at Alfort, with the perchloride of iron, from which he concludes that the solution having the strength of 45° to 50° (Baumé's areometer) ought not to be employed in treating aneurism and erectile tumors, on account of the casualities [accidents] that may ensue. He, therefore, recommends 30°, and still better 20°, as the preparation adapted for these cases; 30° to 45° may be employed in aneurismal sacs, *(Kystes hématiques)* and as a modifier of suppurating wounds.

Prof. Judkins, in a communication in the Ohio Medical and Surgical Journal, dated at Paris, Nov. 5th, 1853, says:

The treatment of aneurism, varix, &c., by injections of the perchloride of iron, is receiving a great deal of attention. I have seen its efficacy tested in an aneurism of the bronchial artery, under the care of M. Valpeau, at La Charité: it failed, and he was compelled to ligate the artery: it has failed also, in some of the other hospitals. Before the American Medical Society of Paris, last week, Professor Brainard, of Chicago, stated, that the failure in these cases, was owing to the fact that the canula for conveying the solution into the sac, had never been carried far enough, so that the injection was thrown between the layers of the old coagula, and could have no other effect than that of causing irritation and inflammation. He mentioned the steps of M. Valpeau's operations, and called attention to the fact, that true arterial blood did not follow the introduction of the canula. Prof. B. stated that he had used a solution of the lactate of iron, as injection in vessels, which caused their obliteration—this he reported in 1850. The quantity of the article used at each injection, varied from one to three grains. Recently, in London, he produced a complete cure of a large aneurismal tumor of the orbit by the same remedy. This case is reported in the London Lancet of October last.

In another letter Prof. Judkins says:

The new method of treating aneurisms by injecting into the sac a solution of the perchloride of iron, has, for several months past, engaged the attention of the whole profession in France, and I doubt not, but that a corresponding interest has been felt in other parts of the medical world.

Prof. J. A. Murphy, of Cincinnati, in a letter dated in December,

1853, at Paris, thus writes concerning the proceedings in the Academy of Medicine. M. Le Blanc:

All discoveries have had their dark days, and I do not believe it just to give an unfavorable opinion on this one, after the small number of facts which exist in science. He then gave the history of the question. He brought up the experiments of M. Pravaz upon animals, and the results which followed them; the manner in which this question was brought before the Institute by a communication made by M, Rayer, in the name of M. Lallemand. Then after a just tribute to the paper of M. Malgaigne, he passed to the relation of the experiments which he had undertaken, in connection with M. Debout, on animals, and some of which had been seen by M. Professor Denon Villiers. These experiments had for their principal aim the establishment of the innocuity of perchloride of iron. Now, this innocuity has appeared almost complete, and, excepting a little fever and anorexia, no accident has been noted on the horses experimented on. The dose of the perchloride was injected sufficiently large without producing any unhappy results. M. Le Blanc insisted on the necessity of maintaining for a sufficient length of time, the ligature or the compression above and below the point where the injection is made, if we wish to obtain a firm and adherent clot. Without this precaution, the clot detaches itself and is carried into the circulation. He explains in this manner the rapid disappearance of the clot noted in some experiments, and which have been falsely attributed, he believed, to absorption. M. Le Blanc desired with all his heart, that it was possible to realize the desire expressed by his honorable colleagues, MM. Gerdy and Moreau, to see the perchloride of iron tried, upon animals, in cases of aneurisms. Unhappily that is not possible, since domestic animals are not subject to this disease. Besides, it is difficult to cause artificial aneurisms on beasts. However, he proposed to make some experiments on this point of trying to produce, on animals, aneurisms in the regions that this disease occupies by preference in man. He will try also the action of perchloride of iron in cases of varices, a disease which we meet with sometimes in domestic animals. As to the irritant and caustic properties attributed to the perchloride, we are, in this respect, in a complete error. A solution of the salt of iron may very well become irritant and caustic, but it is when it is old, for then there is formed a certain quantity of perchloric acid. For the rest, it is easy to neutralize this action by adding to the solution, before using it, a little hydrate of peroxide of iron, which destroys the acid.

M. Velpeau's support of this new method of Pravaz has been too equivocal and wavering to please either its friends or its enemies.—Editor.

M. Velpeau: we were not ready to judge definitely this question of perchloride of iron. I do not believe that the accidents are inherent to

the method even. The gangrene of the fore-arm which Mr. Malgaigne reported does not seem to me to be the consequence of the injection of the perchloride; there is an unknown cause. There was some inflammatory accidents, in truth, we could fear them a great deal. Pravaz had announced the perchloride as something very irritating. I was forced to employ it in one case. I introduced into the bottom of a vast wound, some boulettes of charpie saturated with perchloride of iron, without any inflammatory accident.

Behold a remedy which coagulates the blood in the vessels; in experimenting, this liquid does not act in an irritating manner, then there is nothing to fear from inflammatory accidents. This liquid coagulates, that is certain. If in coagulating the blood in the vessels, we can arrest the course of the blood, there is reason to experiment with the perchloride of iron on animals, but not on men.

There is a point on which I wish to give an opinion to our young colleagues. It is that the clot has a tendency to adhere. I have seen this in the large veins of horses, in which M. Le Blanc had made some injections of perchloride of iron; there is reason to believe that this clot may become organized in remaining adherent to the vein or can be reabsorbed, if it adheres, a canal more or less large, can be formed below it. But here is a difficulty, whether we inject the perchloride or some other liquid. It is not the same thing to inject an artery, or a healthy vein, or more an aneurismal pouch. Here you have a pouch which is not more clearly organized, or you have some concrete layers not organized. Another difficulty is that the orifice of the artery at the bottom of the aneurism is not always open.—*West. Lancet.*

For the arrestation of secondary hæmorrhages, following amputations, a preparation is recommended having the strength of 45° to 49°.—*Rev. Thér. Méd. Chir., May*, 1854.

It may be proper to state that the friends and enemies of this hæmostatic, in France, have indulged in a spirit of wrangling as to the properties of this article, some contending that the failures of the article to cure are owing to its bad preparation, *(mauvais; très-mal préparé; détestable;)* differing essentially from that of Pravaz; and that M. Dubuisson alone prepares it properly!

M. Malgaigne, however, maintained before the Academy that M. Dubuisson is wholly ignorant of the method of preparing the perchloride of iron: he adds—"M. Dubuisson has said, that the experiments made in Paris were not with his perchloride. It is certain that when in Paris, he placed in the hands of several surgeons the perchloride, which he had of M. Rousseau. Concerning the above, I entertained some doubts, and I enquired of our honorable colleague M. Loubeiran, who informed me that M. Dubuisson understood nothing of the

preparation of the perchloride of iron. M. Gobley, who has been for several years, under the direction of M. Loubeiran, occupied in its preparation, has also given me the same statement. M. Dubuisson has asserted that his concentrated perchloride of iron contains hydrochloric acid, I believe it truly, for it is very badly prepared. It is easy to make a concentrated solution of it, that will contain none of the hydrochloric acid. When well made there is no deposit in it; M. Dubuisson has sent some to the Surgical Society, which forms a deposit, therefore, it is not a good preparation, and yet it was with this bad specimen of the article, that they have operated in Lyons."

Should the remedy come into general use in New Orleans, it is hoped that the apothecaries will see to its purity, density, &c.*

The preparation of the perchloride of iron:

The preparation of the solution has been made public by M. Burin Dubuisson, pharmacien of Lyons, and is described as follows: Take of sulphate of iron of commerce (emerald color,) two pounds; water, six pounds; pure iron-filings, the fifth part of a pound; sulphuric acid, half an ounce: put the whole into an enamelled cast-iron vessel and leave the latter upon the sand-bath until gas is no longer given off; filter, add to the fluid half a pound of liquid hydro-sulphuric acid, and allow to rest for twelve hours. Put, after that time, the solution upon the fire, boil for half an hour, and filter. To the filtered liquid add six ounces and a half of pure concentrated sulphuric acid; and place the mixture into an enamelled cast-iron vessel, which should be but half filled. Boil, and add in small quantities, pure nitric acid, until it causes no longer the escape of red fumes. Remove the vessel from the fire, add to the fluid twenty-five or thirty times its weight of cold water, and the whole of the iron will be precipitated in the state of peroxide by the addition of a slight excess of ammonia. Wash the precipitate by decantation with pure water a great many times, and dry it in the air by spreading it in thin layers upon linen.

This dry and pulverized oxide is then calcined to redness in a large and shallow iron vessel, so that the temperature may not rise too high. This is the martial saffron of the shops, which is in fact the peroxide of iron, when prepared as above.

The perchloride of iron is then obtained in the following manner: Take of the peroxide of iron, resulting from the process just described, six ounces and a half; pure and white hydro-chloric acid, two pounds; mix and allow the action to go on without fire for five or six

*Dr. Turpin, of New Orleans, recently mentioned to the editor of this journal, that he had been, and still was, using this preparation externally for a congenital erectile tumor, and with some apparent benefit in arresting its ratio of growth.

hours; then put the vessel on a water-bath, and boil until the almost complete solution of the oxide is obtained; this should be done in a porcelain capsule, weighed beforehand. The liquid is decanted to separate the undissolved oxide, and the former is carefully evaporated upon the water-bath, constantly stirring, to the consistency of thick syrup, which is then weighed. Half of this weight of distilled water is then added, the heat is kept up for a few moments, and the whole is thrown on the filter. The capsule and the filter should now be washed with a quantity of water, equal to that used in the last place, and to the first fluid obtained, as much of the second is added to get a mixture of the density of from 43.5 to 44°.

By proceeding in this manner a very limpid fluid is obtained, with a slight acid reaction, but perfectly pure, having reached the maximum of saturation, and always identical. It may be kept without any of the salt being thrown down, provided the bottle be well stopped; the color is dark-brown when the liquid is looked at in full, and of a greenish-gold color when held to the light, or seen in a thin stratum. Five or six drops of this fluid, mixed with the white of an egg diluted with six drachms of water, are sufficient to coagulate the whole into a mass in the space of fifteen seconds. This mass firmly adheres to the bottom of the glass when the latter is turned up, and takes a pretty long time before it slowly drops, when the watery parts begin to run off, as the serum separates from coagulated blood.

At the meeting of the Surgical Society of Paris, held May 4, 1853, M. Debout presented the two carotids of a horse, into which perchloride of iron had been injected. In one of these vessels, for the distance of two inches, the artery being held by the fingers above and below, only six drops of the perchloride had been thrown. The clot formed had, however, been re-dissolved by the current of blood, and carried into the torrent of the circulation. The lining membrane of this vessel was healthy, except a small spot where an abscess was about to form. In the other carotid fifteen drops of the perchloride had been injected for two inches and a half of its length, the upper and lower part of this length of artery being also compressed by the fingers. This vessel remained plugged all the time the animal lived. On a post-mortem examination, the whole length of the clot was found adherent to the parietes of the artery, the lining membrane being the seat of suppurative inflammation. The vessel was quite obliterated both above and below in consequence of adhesive inflammation.

In a subsequent meeting, M. Debout gave a minute description both of the experiment and of the post-mortem appearance, and concluded that M. Pravaz, the discoverer of this property of the perchloride, was right in advising injections of very small doses. M. Debout very properly added, that compression of the artery above the sac (as proposed and carried out by Dr. Bellingham, of Dublin,) should be resorted to as an adjuvant of the injection, as there is a tendency in the blood remaining free in the artery to break down the clot formed by the perchloride.—*Ranking's Abs. Jan.* 1854.

Messrs. Abel and Bloxam, in their excellent Hand-book of Chemistry, (1854,) give the following formula:

Sesquichloride, or Perchloride of Iron, Fe_2Cl_3.

To obtain this salt in the anhydrous state, coils of iron wire may be introduced into a tube of hard glass, and moderately heated in a pretty abundant supply of chlorine. It then sublimes in brown, crystalline scales, which are deliquescent, and very soluble in water, alcohol, and ether, yielding red-brown solutions.

Hydrated sesquichloride of iron is prepared by dissolving iron in a mixture of hydrochloric and nitric acids, or by treating the sesquioxide with hydrochloric acid. On evaporating the solution, yellow scaly crystals may be obtained, of the formula Fe_2Cl_3–|–5Aq. When these are heated, they evolve hydrochloric acid, and a compound of sesquichloride with sesquioxide of iron remains. A similar compound is deposited as a brown precipitate, when a solution of sesquichloride of iron suffers spontaneous decomposition, or when it is boiled for some time.

The alcoholic and ethereal solutions of this salt are decolorized by exposure to light, (proto-) chloride being formed, together with products of oxidation of alcohol or ether.

Sesquichloride of iron is decomposed by vapor of water at a red-heat, hydrochloric acid and crystallized sesquioxide of iron being produced:—

$$Fe_2Cl_3\text{–|–}3HO=HCl\text{–|–}Fe_2O_3.$$

M. Moissenet (Hôpital de la Salpètrière) uses the perchloride of iron as a hæmostatic in cancerous affections of the uterus and breast, attended with hæmorrhages. He uses a wash (thirty grammes of the perchloride to two hundred and fifty of water,) for repressing fungous growths, and as a wash or injection in cancerous affections.

Aneurismal cases treated by injections of the perchlorine of iron.

1.—*M. Niepce's Case.*—This case, which was one of popliteal aneurism, was brought before the Académie des Sciences on the 18th of April, ultimo. Five minutes after the injection the pulsation had ceased, and the tumor solidified, and on withdrawing the canula no blood followed. The next day, and the days following, acute inflammation set up in the sac and the surrounding tissue; and on the 11th day there was distinct fluctuation on its inner side. A small puncture in this place gave exit to about 10 grammes of purulent serosity, with immediate relief to the inflammatory symptoms. Twenty days after the operation, all that remained of the aneurism, was a solid tumor about the size of a nut, and the patient was virtually well.

2.—*M. Lallemand's Case.*—This was a case of varicose aneurism, at the bend of the elbow. It was brought before the Académie des Sciences on the 9th of May. In this case the beatings ceased, and

the tumor solidified immediately after the injection. Acute local inflammation followed, ending in the effusion of a small quantity of sero-purulent fluid, which had to be evacuated by puncture. Eventually a considerable portion of the tumor sloughed away, but there was no hæmorrhage; and the parts healed with rapidity, and the patient got thoroughly well.

3.—*M. Dufour's Case.*—The patient in this case was a tolerably strong and healthy man, with a large aneurism of the right carotid, extending from the chin to the clavicle, and causing great pain and difficulty of breathing. The skin over the tumor was greatly distended and congested, and upon the point of bursting in several places. The solution of perchloride was first injected on the 2d of June, 1853, the sac having been first punctured by a needle. Blood followed the puncture, but this stayed immediately on making the injection. The operation gave rise to great local pains, but the throbbing and fluidity continued a week later; the inflammation in the sac continued, in spite of the cold applications used to repress it, and a dark, sanious fluid oozed from the opening through which the injection had been made. Under these circumstances two grammes of the solution were injected; violent pain and heat followed, as before, but on this occasion there was some evident coagulation in the sac. The inflammatory symptoms, however, progressed in spite of everything that could be done to repress them, and next day a gangrenous patch had formed on the superior and upper part of the tumor. This continued to extend, and three days afterwards the aneurism burst by a large rent, and the patient died instantly, a coagulum of the size of a hen's egg, and apparently the result of the second injection, being extruded at the time. M. Dufour is disposed to think that the operation might have been successful, notwithstanding the unfavorable state of the patient, if the solution had been a little more concentrated, and if it had been injected more liberally.

4.—*Mr. Wm. Adams' Case.*—In this case, which was that of an infant, aged three months, the posterior tibial artery was wounded in the operation of dividing the posterior tibial tendon for club-foot, and a pulsating tumor discovered ten days afterwards. Direct pressure by a graduated compress and bandage was applied, and the pulsation diminished, but a very small slough formed from the pressure not having been relieved for four days, and a copious arterial hæmorrhage took place. Pressure, a little above the aneurism, appeared to command the bleeding, and this was therefore tried; but it was discontinued on the second day, in consequence of the extension of inflammatory swelling to this part of the leg. A second arterial jet followed the removal of pressure.

On the 12th of May, 1852, a small quantity, (from five to ten drops) of the concentrated solution of perchloride of iron (prepared by Mr. Taylor, of Vere street) was injected into the aneurismal sac, which was about an inch in diameter. At the time of the injection,

loosely clotted blood plugged the small cutaneous ulcer, and through the clot the extremity of a long and finely-pointed glass syringe, containing the styptic, was introduced, and carried to the bottom of the sac, as nearly as possible to the situation of the wounded vessel. The femoral artery was compressed before the injection, and for five minutes afterwards, so as to insure the blood acted upon being as nearly as possible in a stagnant condition; a most essential point. The first effect observed was, that the exposed surface of the clot became firmer, and that straw-colored serum oozed from its substance. About twenty minims of serum oozed out in this way, affording conclusive physiological evidence of the firm coagulation of the blood, which was also indicated by a general feeling of hardness over the sac. A piece of lint and bandage were applied.

May 13th.—Ulcer concentrated and filled with firm clots. The surrounding skin, which presented a tense, shining, swollen, and slightly reddened appearance previous to the operation, was now pale, and less tumefied. It seemed that a process of shrinking and contraction had commenced, and that no inflammatory results need be apprehended. Progressive improvement took place; the ulcer healed in a week; shrinking and contraction advance.

May 25th.—A deep puckered cicatrix, and a little deep-seated induration, alone indicated the seat of the aneurism. The treatment of the deformity was proceeded with; the tendo achillis was divided on the the 1st of June, and the restoration of the foot was in a few weeks as complete as in other cases.—*Ranking's Abs. Dec.* 1853.

In the New York Journal of Pharmacy, for May, 1854, it is stated that M. J. Ruspini, "a distinguished Italian chemist, has substituted with success the acetate of sesquioxide of iron in place of the perchloride of this metal, in the treatment of hæmorrhages. The experiments of Dr. Pravaz upon the employment of chloride of iron for the cure of aneurism are well known. It appears after several trials of a similar kind by Ruspini, that the acetate of the sesquioxide is far superior to the perchloride as a hæmostatic, and the former salt has the additional advantage of containing an organic acid whose action on the animal economy never produces any unpleasant consequences."

Art. II.—*Chloride of Zinc.—Aneurism Treated by the External Application of Chloride of Zinc:* By M. Bonnet, of Lyons.

M. Bonnet announces the complete cure of a patient suffering from subclavian aneurism, seen by M. Lallemand upon the fourteenth day of the treatment of the disease, by successive applications of the paste

of chloride of zinc. The chloride of zinc, penetrating by imbibition into the deeper parts of the tumor, produced complete coagulation of the blood, which was detached from the walls of the aneurismal cavity without any hæmorrhage. The patient quitted the hospital three months and a half from the commencement of the treatment. He could at that time take a walk for half an hour at a time, and presented, as the only trace of his disease, a superficial ulceration, which was nearly healed. The clot, which came away with the eschar, was forwarded to M. Lallemand by M. Bonnet.

The patient, æt. 25, had received in the left supra-clavicular region a blow with a knife, which, dividing the whole brachial plexus, and touching either the subclavian artery or one of its branches, was followed by profuse hæmorrhage, and subsequently by the formation of an enormous aneurism. The rapid enlargement of the tumor, and the frequent discharge of blood from the wound, threatened the life of the patient, and rendered immediate assistance necessary. The application of a ligature between the inner border of the scaleni muscles and the aorta was rejected, both by M. Bonnet and his colleagues, as too dangerous and too difficult. The galvano-puncture was also negatived, because it was impossible to suspend even for a moment, the pulsations and the *bruit de souffle* in the aneurismal pouch. The injections of the perchloride of iron were not then known (January 2); and, possibly, the enormous size of the aneurism, its proximity to the heart, and the impossibility of compressing the artery which conveyed the blood to it, would have counter-indicated its employment. In this difficult position, M. Bonnet determined upon cauterization by the paste of chloride of zinc.

The first eschar was produced in the centre of the tumor. Every two or three days, Bonnet removed with a bistoury, the superficial layers of the slough, and for five weeks he continued a series of applications of the paste, which penetrated daily both in depth and extent. Upon the fourteenth day, the pulsations and the *bruit de souffle* ceased; at the end of the second month, the eschar began to detach itself without any hæmorrhage. The clot came away with the eschar. Bonnet affirms, that modern authors are wrong in attributing this method of treatment to Ambrose Paré, Guttani, or Marcus Aurelius Severinus; in none of the original works is it mentioned. The ancients knew nothing of the chloride of zinc, which was introduced into practice by M. Canquoin.

Bonnet first thought of employing this escharotic in the treatment of aneurism, by observing, for the last ten years, its power in coagulating blood and arresting hæmorrhage. He found, 1837, that it got rid of varicose veins without the occurrence of the bleeding which frequently ensued after the application of potassa fusa. Both he and his colleagues—MM. Berrier, Bouchacourt, Desgranges and Vallet—have often taken a firm coagulum from vessels exposed to its influence. He has seen it entirely remove hæmorrhoids, varicoceles, and erectile tumors, by penetrating into the vessels by imbibition, coagulating the

blood, and rendering the whole tumor firm. That it arrests arterial as well as venous hæmorrhage was shown by its action upon goitre, as stated by M. Philipeaux, in a memoir founded upon some of Bonnet's clinical lectures.

The eschar separates about eight days after the application of the paste; it can, therefore, be easily retained by successive applications for a month or even longer.

MM. Bonnet and Gensoul have lately proved that the chloride of zinc may cure the disease known as aneurism by anastomosis. A tumor occupying the summit of the head, an inch in thickness and of rounded surface, about half a foot diameter (15 centimetres), consisted of an immense number of anastomosing arteries, pulsating with a loud *bruit de souffle*, and nourished by seven large trunks coming from the frontal, the temporal and the occipital regions, each about the size of the brachial artery. Injection was insufficient and impossible; pressure continued for a year, failed, and the surgeons were talking of tying the carotid arteries. The cauterization by the paste of the chloride of zinc effected the separation of the tumor, without any hæmorrhage whatever from each of the nutritious arteries. The result of the case not yet complete, is to be communicated.—*Ranking's Abs., Dec.*, 1853.

The following formulæ for preparing the paste of the chloride of zinc are given from Prof. Dunglison's New Remedies, 601, Fifth Edition.

Chloride of zinc may be applied as a caustic, by means of a moistened hair pencil, either alone or mixed with an equal portion of oxide of zinc, or sulphate of lime, or according to the following forms:

Pasta zinci chloridi.

Paste of chloride of zinc.

Pâte de Canquoin.

Canquoin's caustic paste.

		A.	B.	C.
R.	Zinci chlorid.	p. i.	i.	i.
	Farinæ tritici	p. iv.	iij.	ij.
	Aquæ fontan.*	q. s. ut fiat pasta.		

R. Zinci chlorid. p. j.
Farinæ triciti p. iss.
Antimonii chloridi. p. ss.
Aq. font. q. s. ut fiat pasta.

* To each ounce of the chloride of zinc twenty-four to thirty drops of water may be added.

Art. III.—*Acetate of Iron, for Aneurism.—Radical Cure of an Aneurism of the External Maxillary Artery, by the Injection of the Acetate of Iron:* By Dr. Filippo Lussana, of Milan.

Antecedent to the experiment of Pravaz, a celebrated Italian surgeon, Monteggia, had suggested in the treatment of aneurisms "the injection of astringents into the sac, after entering it with a trocar, in order to obtain a prompt and durable coagulation of the blood, the artery being compressed above the tumor." (See *Istituzioni Chirurgiche*, vol. ii, 2d ed., Milan.) The idea of this operator was never carried out, however, and it remained for the skilful and persevering Pravaz to introduce this great improvement into the practice of surgery. Soon after the researches of Pravaz were made known, and the use of the perchloride of iron in aneurisms had been followed by some disasters, Dr. Ruspini commenced a series of experiments with a view of substituting some less deleterious agent for the perchloride of iron. He finally employed the acetate of the sesquioxide of iron, a salt which contains no principle injurious to the economy, and which possesses great hæmostatic power. This agent had only been employed in cases of slight hæmorrhage and in experiments on animals, when Dr. Lussana determined to employ it in an operation on an aneurism. His experiment resulted most successfully.

M. Gelmi, a girl of twenty-two, presented an aneurismal tumor of the size of a large walnut, between the corner of the mouth and angle of the lower jaw. This tumor was punctured with a small lancet; arterial blood immediately issued in jets. A fine syringe was introduced, and ten drops of the ferruginous solution was injected. The hæmorrhage and pulsations were immediately arrested. The operation caused but little pain and no inflammation succeeded it. In ten days the tumor had sensibly contracted. An exploratory puncture was made in it without causing any hæmorrhage.—*Va. Med. and Surg. Jour.—From Gaz. Méd. de Paris, of Feb. 25th*, 1854.

Art. IV.—*General Bloodletting in Insanity.*

The American Journal of Insanity, edited by T. Romeyn Beck, M. D., for April, 1854, contains an elaborate article of 118 pages upon "Bloodletting in Mental Disorders," by Pliny Earle, M. D., of New York city, in which the latter analyzes with much ability, one hundred and thirty-six authorities, illustrative of this proposition, namely, "to what extent, in regard to both frequency and quantity, is the abstrac-

tion of blood required in the treatment of insanity;"—from all of which, he draws the conclusions which follow:

A reply to the proposition at the commencement may now be attempted. It is evident, however, from the very nature of the case, that no positive, definite answer, couched in terms as fixed as the figures representing numbers, can be given. It must be merely approximative. I shall endeavor to convey it in a series of facts, truths or inferences, which I hope are fairly deduced from the substance of the foregoing pages.

1.—Insanity, in any form, is not, of itself, an indication for bloodletting.

2.—On the contrary, its existence is, of itself, a contra-indication. Hence, the person who is insane should, other things being equal, be bled less than one who is not insane.

3.—The *usual* condition of the brain, in mania, is not that of active inflammation, but of a species of excitement, irritability, or irritation, perhaps more frequently resulting from or accompanied by anæmia, debility, or abnormal preponderance of the nervous over the circulatory functions, than in connexion with plethora and enduring vital power.

4.—The excitement, both mental and physical, produced by this irritation, can, in most cases, be permanently subdued, and its radical source removed by other means, more readily than by bleeding.

5.—Yet insanity may be co-existent with conditions,—such as positive plethora, a tendency to apoplexy or paralysis, and sometimes sthenic congestion or inflammation, which call for the abstraction of blood. Therefore,

6.—Venesection in mental disorders should not be absolutely abandoned, although the cases requiring it are very rare.

7.—As a general rule, *topical* is preferable to *general* bleeding.

8.—In many cases where the indication for direct depletion is not urgent, but where bloodletting, particularly if local, might be practiced without injury, it is safer and better to treat by other means, equalizing the circulation and promoting the secretions and excretions.

9.—The physical conditions requiring bloodletting more frequently exist in mania than in any other of the ordinary forms of mental alienation.

10.—Insanity following parturition, other things being equal, is to be treated by bleeding less frequently than that which has its origin in other causes.

11.—If the mental disorder be the direct result of injury to the head, the treatment must be directed to the wound, or its physical effects, not specially to the psychic condition.

12.—In many cases where insanity is accompanied by typhous symptoms, and in some where the aspect is that of acute phrenitis, active stimulants alone can save the patient, and direct depletion from the circulation is almost certainly fatal.

The following extract from the last number of Ranking's Abstract, exhibits the state of opinion on the continent of Europe with respect to venesection as a remedy for insanity, and corroborates the conclusions of Dr. Earle:

Prejudicial Effects of General Bleeding.

Dr. Webster in his report on French Asylums, says:

Although it was not originally intended in the present remarks to discuss the medical treatment usually pursued in French Asylums, one point seems, however, of so much importance that it deserves some notice in these pages; particularly, as great unanimity of opinion prevails amongst the physicians of departmental institutions, with whom I had an opportunity of conversing upon the question. I now refer to the employment of bloodletting as a remedy in cases of insanity. Without an exception, every practitioner was decidedly opposed to the *general* abstraction of blood in maniacal patients, as they consider it not only unnecessary, but often injurious. In many cases venesection produced so much depression, that attacks of mania, which otherwise might have been of short duration, under a different but more judicious mode of treatment, were thereby prolonged, and even ended in fatuity.

Exceptions Requiring Bloodletting.

Of course particular cases of insanity presented themselves where inflammatory symptoms appeared so decided, or in which apoplectic congestion existed to such an extent, that local or general abstraction of blood was then absolutely necessary; nevertheless, these examples were exceptional, and only confirmed still the observations made by the most experienced officers, medical officers of French asylums, respecting the baneful consequences of bloodletting in most cases of mental disease, which came under their cognizance. Indeed, one gentleman remarked "the delirium of insane patients was never modified by frequent and copious bleedings, but often the reverse."

Being supported in these practical conclusions by the opinions of many English physicians, it cannot be too strongly impressed upon the minds of young practitioners, or of those who may not have had much experience in treating cases of insanity, to be always exceedingly chary of using the lancet.

Art. V.—*Intermittent Pneumonia:* By M. Constant, M. D.

Dr. Constant, practising in one of the marshy districts of the department of the Lot, draws attention to the signs which distinguish what he terms intermittent pneumonia, as when they are overlooked the disease proves rapidly fatal.

1. The initial shivering is usually more intense and prolonged than in ordinary pneumonia. 2. The pleuritic pain is felt early and always in front of the chest, although the pulmonary congestion is almost always localized posteriorly. It is much more amenable to blisters than to leeches. 3. Violent cephalalgia is one of the earliest symptoms, being either frontal or sincipital, and it is often accompanied by severe lumbar pain, which observes the same stages of increase and decrease as itself. 4. The shivering is followed by intense heat. which after several hours gives place to abundant sweating. 5. The pulse, during the paroxysm, in place of being full, strong, and vibrating, as in ordinary pneumonia, is rapid, soft, undulant, and compressible. 6. There is never any purulent expectoration, these pneumonias never proceeding beyond the second stage—*i. e.* red hepatization, the pulmonary engorgement being rather a sanguineous congestion than inflammation. 7. Auscultation and percussion are of the highest value, often revealing the disease when unsuspected. A distinctive feature is the rapid passage from the first to the second stage of the disease, so that eight or twelve hours after auscultation had revealed only a slight circumscribed *râle*, a whole side will be found hepatized. Under the influence of large doses of quinia this rapidly disappears, giving way to returning subcrepitant *râle* during the remission of the fever, but returning again during the paroxysm, if this have not been cut short.* 8. The crepitant *râle* of the first stage is almost always moist, the parchment-cracking *râle* only having been heard for a short period, two or three times in more than sixty cases. It invades large surfaces rapidly, being heard posteriorly, sometimes laterally, but never in front. 9. This form of pneumonia especially affects the posterior part of the lower lobes. 10. It especially appears in summer and autumn, while ordinary pneumonia prevails in spring and winter. 11. It attacks all ages indiscriminately, except early infancy. 12. The blood which flows from a vein is often below the normal temperature, very black, and deficient in plasticity. After rest, its surface acquires a bluish color, especially if the patient is taking quinia. The clot is slow in formation, and soft. The buffy coat is absent, or very thin, and inclines to a bluish color. This condition of the blood, conjoined with the soft pulse and rapid hepatization, constitutes the chief distinctive sign of the affection.

In this district, during winter, purely inflammatory pneumonia is met with; but in proportion to the high temperature and the production of malarial emanations, this inflammatory element is replaced by the paludal one. There are, indeed, three forms met with. 1. Simple pneumonia; 2. Spring inflammatory pneumonia, complicated with the in-

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* The editor of the N. O. Medical and Surgical Journal recollects a case of congestive intermittent from the Coast, (the written history of which might somewhere be found in his XXII vols. MSS.,) in which the dullness of hepatization was observed before death; but the post mortem examination about two days after percussion, revealed no lesion of the lungs—owing it is supposed to one of two causes: either the re-actionary fever before death, or the persistence of capillary and calorific action after death, had removed the pulmonary congestion.

The judicious reader will appreciate the significant facts re-produced in this article from Drs. C. and A.

termittent paroxysm. 3. Summer and autumn intermittent pneumonia. The first requires bleeding and antimony; the second antiphlogistic treatment with quinia, given either simultaneously or subsequently; and the third, quinia in combination with external revulsives. These forms may still undergo further admixture, according as the inflammatory or paludal element prevails, requiring appropriate modification in the treatment.—*Ranking's Abs., Dec.* 1853.

Periodicity of Pneumonia.

L. H. Anderson, M. D., in his report to the last annual meeting of the Medical Association of Alabama, upon the diseases of Sumterville and its vicinity, arrives at the following conclusions concerning Pneumonia:

1.—The pneumonia of South Alabama is probably less severe than the same disease in more northern latitudes; and the intermittent or remittent fever accompanying it is more dangerous than the pulmonary inflammation.

2.—The treatment should be directed chiefly against the malarious element of the disease. Quinine should be freely given for its antiperiodic effect, and mercury for its influence on the secretions, particularly that of the liver.

3.—An exception to this pathology and treatment is to be made when the attending fever is of a typhoid character. Here the dothinenterite is the more important disease, and the pneumonia should be treated with strict reference to it.

* * * I became convinced, from the decided, though often obscure periodicity that takes place in the disease, as it appears with us, (and perhaps in all miasmatic districts,) *that the remittent fever which accompanies it is the true source of danger, and that the fever being relieved, the pneumonia will spontaneously amend.* This conviction gradually growing upon me, I diminished, in each successive case, the quantity of tartar emetic and mercury given, and increased that of the quinine, till at last I came to make it the chief remedy, and used the others merely as adjuvants.

* * * There is usually no chill after the first one, or if any it is so slight as not to be noticed, and the fever is commonly not of a very high grade. The patient getting no better, however, a physician is sent for about the fourth or fifth day. He then finds the disease fully formed, the pulse frequent, small and tense, fixed pain in the thorax a troublesome cough, with a viscid expectoration colored with blood, tongue foul, and red at the edges, perhaps dry, the stomach nauseated, and the bowels either constipated, or, if purgatives have been given, discharging watery and brownish evacuations. The percussion will be dull or flat on one side of the chest, or a part of it, and the respiration crepitant, absent, or mucous, according to the stage of the inflammation. We have here to contend with a remittent fever, with gastro-intestinal disturbance, complicated with inflammation of the lung, more or less

extensive. We cannot cure such a case by a *coup de main*, but finding a number of organs implicated, must treat the affection of each, with reference to all others. Quinine alone is inadequate to the cure, but it should be liberally given during any period of remission that may be observed. At the same time, moderate mercurials, combined with laxatives or opiates, as the condition of the bowels may demand, so as to rectify their secretions, should be given. Cups, fomentations, or blisters should be applied to the chest, and syrup of squills, alkaline drinks and tartar emetic in dilute solution, prescribed as the judgment of the practitioner may indicate.

Dr. Anderson deviates from "the anti-malarious treatment" of pneumonia whenever the accompanying fever is of a typhoid character.

Dr. A. gives the following case as an illustration of his treatment of Southern pneumonia:

A servant of my own, aged 50, of good constitution, and having ge nerally the best health, was taken 15th April, 1852, about 8 A. M., with a chill of an hour's duration, succeeded by smart fever, pain in the head and side, cough, and tenacious rust-colored expectoration. He had had an ordinary cold and cough for several days, which did not prevent him from attending to his usual duties, and I had seen him early in the morning of the day be took the chill, and given him some directions as to what I wanted done, I then rode away from home, and returning about midday, found him confined to bed, and complaining as above mentioned. The pulse being tense, and the pain in the head and side considerable, I bled him about eight ounces, and cupped him on the affected side, with some feeling of general relief. I then gave him 15 grs. quinine, with a laxative. Four hours after I gave him eight grains quinine additional, and at night, finding the cough abated, the expectoration less tinged with blood, and the pulse slower I repeated the dose. He passed a tolerable night, and in the morning there was scarcely a vestige of the pneumonia, save the cough, remaining. He got eight grains more of quinine, had no further chill or fever, and in a few days was up again. The case seemed to me at first a very threatening one, but seeing it so soon after the seizure, I determined to give the quinine very liberally, and to rely upon it almost wholly for the cure.

Art. VI.—*Small Pox—Vaccination—Re-vaccination.*

In a pamphlet by Joseph C. Hutchison, M. D., physician to the Brooklyn City Dipensary of New York, reprinted from the New York Journal of Medicine, upon "vaccination, and the causes of the prevalence of small pox in New York, 1853—4," it is shown that in

this city during five years 2,396 have died of the above named malady—681 of whom perished in 1853: Dr. H. thus sums up the results of his researches:

From the materials presented in the preceding pages, and from other sources, the following conclusions may be legitimately deduced:

1.—That the recent extensive prevalence of small-pox in New-York may be, to a great extent, attributed to the *neglect of vaccination.*

2.—*To imperfect vaccination,* which may occur: (1.) From not observing the regular progress of the vaccine disease. (2.) From a want of proper regard to the state of the recipient. (3.) From injuring the vesicle. (4.) Performing the operation at too early an age. (5.) Using lymph taken at an improper stage of the disease. (6.) Peculiarity of the temperament of the recipient. (7.) Vaccinating with lymph deteriorated by age, or any of the above causes.

3.—To epidemic influence.

4.—To a neglect of re-vaccination.

On Small-pox and Vaccination; an Analytical Examination of all the Cases admitted, during Sixteen Years, at the Small-pox and Vaccine Hospital, London, with a view to illustrate the Protective Influence of Vaccination: By F. Marson, Resident Surgeon at the Small-pox Hospital.

The author concludes as follows:

1.—That natural small-pox destroyed about one-third of all whom it attacked.

2.—That small-pox after small-pox was of comparatively rare occurrence; that a second attack of natural small-pox was rare, but not often fatal, and that protection seemed to be the law. That after inoculated small-pox an attack of small-pox had more frequently led to fatal results; but there is reason to presume that the virus used for inoculation, like a great deal of the lymph used at the present day for vaccination, was often taken at too advanced a period of the disease, and thus did not afford the full measure of protection it was capable of affording if taken at a proper time.

3.—That vaccination performed in infancy afforded almost complete protection against the fatality of small-pox to the period of puberty; that a variety of circumstances conspired to make it almost impossible to ascertain exactly in what proportion to the vaccinated cases of small-pox subsequently occurred, or might occur, if all persons lived to an advanced age.

4.—That, as a matter of safety, it would be well for all persons who were vaccinated in infancy to be re-vaccinated at puberty; this measure being more especially requisite for those who were either indifferently or doubtfully vaccinated in infancy, and still more necessary for those who, though vaccinated, had no cicatrix remaining. Finally, as a matter of precaution, it would be desirable that all persons

should be re-vaccinated on small-pox existing in the house where they were residing—a precaution, however, that will cease to be necessary to advise when all persons have the benefit of proper and efficient vaccination.—*Ranking's Abst.*

The Results of Re-vaccination, as observed in two hundred and fifty-seven Cases: By W. B. Herapath, M. D., Surgeon to St. Peter's Hospital, Bristol.

Dr. Herapath arrived at the following results upon the occasion of an extensive re-vaccination amongst the children of two large public schools in Bristol—the Red Maid's School and Queen Elizabeth's Hospital. All the children had had small-pox or cow-pox before admission.

1.—That three cases re-vaccinated within seven years were not again susceptible to vaccine.

2.—That vaccine, after the interval of from eight to seventeen years, does not prevent the reception of vaccine again, except in 22-174 per cent.

3.—That the distinctness or imperfection of the vaccine cicatrix does not materially alter these results.

4.—That variola does not prevent the formation of the vaccine vesicle, except in about 23-53 per cent.

5.—That the occurrence of small-pox subsequently to vaccination does not destroy the susceptibility of the human system to again receive the vaccine poison except in about ten per cent.

6.—That in all the previous cases, whenever the secondary vaccine vesicle assumed its perfect form, its subsequent history was the same as if the system had not previously labored under vaccine variola or varioloid.

7.—It is probable that the protective influence of vaccination has diminished in consequence of repeated transmission of the vaccine matter through the human body.

8.—It is desirable that re-vaccination should be extensively followed, as one means of giving additional protection to the masses.

9.—That when possible, the stock of vaccine should be renewed by going back to the original source.—*Ranking's Abst.*

Art. VII.—*Quinine in Cholera.*

The Editor of this Journal, has, for years used quinine in connection with morphia, laudanum, brandy, &c., by both the mouth and rectum with satisfactory results in the treatment of cholera. It may be given as an injection and retained in many cases, if but a small quantity of liquid be used.

From Ranking's Abstract, (Jan. 1854,) the following notice of the treatment of cholera by quinine, &c., is taken:

The Editor of the Medical Times and Gazette observes, that the lesson we may learn from the experience of the past, is not to persevere in modes of treatment which have proved useless. He remarks: Of all the remedies used during the previous epidemics, with the exception perhaps of opium as a palliative in certain stages, quinine probably is most deserving of a further trial. Modern experience in India is leading to increasing confidence in its powers. Towards the close of the epidemic of 1849, it was used in London. Mr. Spencer Wells injected a solution into the veins in four cases. Dr. Parkes did so in two others. He also injected the solution of salicine without any marked effect, besides alcohol, both in simple warm water and in saline solutions. The experiments were only made in desperate cases, and no recoveries followed, but the power and duration of the re-action which took place were very remarkable, and, as phlebitis only followed in one case, encouragement is afforded for a repetition of the trials in cases not so absolutely hopeless as those in which they were formerly made.—*Med. Times and Gaz., Oct.* 8, 1853.

Art. VIII.—M. Velpeau's *Opinions of the Value of the Microscope in Cancerous Tumours of the Breast.*

In the work lately published "On the Diseases of the Breast," M. Velpeau expresses himself touching the value of the microscope for the diagnosis of cancerous tumours of the breast as follows:

If by means of the microscope it were possible to arrive at a knowledge of the intimate nature of pathological products, the instrument would be of great practical value. The diagnosis of cancer would acquire much certainty, and lose its vague and unsatisfactory nature, as nothing is more easy than to obtain small fragments of cancerous tumours by means of the grooved needle, &c. Some microscopists of the present time do not hesitate to believe in these wondrous results, but my remarks on the cancerous cell, in another part of the work, will prevent my readers from crediting such things. Microscopical examinations may determine the nature of cancerous tumours, when the latter are removed from the body; but, in a clinical point of view, these examinations lead, in the present state of knowledge, to dangerous errors, if any importance were attached to them. When, by the bed-side of the patient, the microscopist declares that the cell submitted to his examination is of a cancerous kind, can the surgeon take such a declaration as a rule of conduct? Will any one form a decided opinion upon so uncertain a testimony? But this is not all: suppose even the cancerous cell were the fundamental element, the *sine qua non* of a cancerous tumour,

who would venture to say that that cell is not to be found in a tumour just examined, merely because it was not discovered in the fragment placed under the field of the microscope? Is it not possible that the grooved needle, though thrust with much care into the suspected texture, may only bring to light non-malignant particles, though the tumours may in reality contain many cancerous cells; a cancerous tumour is, after all, composed of different elements—viz., cellular tissue, fat, vessels, and sometimes hardly altered mammary texture. The most skillful microscopists agree that the whole of a tumour should be examined and that its different layers and lobules should be carefully studied, before a positive opinion can be given. Thus, to be quite sure that a tumour does not contain cancerous cells, must not the former be broken up *ad infinitum*, and every particle be placed under the field of the microscope? I may then say of the microscope what I said of pain, "it throws no light upon pathology, and gives rise to much doubt and uncertainty, especially where a solution is most needed—viz., in the first stage of cancerous tumours. In fact, I do not think that it is possible to diagnose tumours of the breast better with the microscope than by the ordinary symptons and clinical observations."—*London Lancet, May*, 1854.

Art. IX.—*Yellow Fever of Mobile, in* 1853*; its Causes, Cure, &c.*

Wm. H. Anderson, M. D., presented to the Medical Association of Alabama, which held its seventh annual session (Jan. 10th, 1854,) at Montgomery, a report on the yellow fever of Mobile, of 1853; in which he asserts that this disease, in that city, "was as contagious as the small pox." This postulate he attempts to sustain in the following statement, which is given in extenso: "Within our own experience we could enumerate more than a dozen families who were isolated from the disease, who escaped till late in the season, and who took it within a few days after some transient person came into the family, either sick with the fever or on the eve of being down with it. If such cases occurred only in one or two families, we might pause and doubt, but they happened so often, and under such circumstances, that we cannot attribute them to anything else but absolute contagion. We have heard the argument of others, whose capacity for observation we have the highest respect for, but they have failed to convince us that the fever did not spread in many families by contagion. The history of the disease at Spring Hill, at Fulton, at Citronville, and up

in the pine woods, eight, twelve and twenty miles from the city, is to us conclusive evidence. A family living eight miles from Mobile, in a healthy pine country, has no communication with the city, and remains in perfect health until the middle of October. About this time a stranger arrives, just from the city, he is seized with the fever, and in a week from that time four or five of the family are taken down, and half the number dead with black vomit. Would it not require a logic more subtle than any yet practised, to convince us that that fever was not communicated by contagion. The limits of this report will not permit us to dwell longer on this subject. We have, throughout, studiously avoided theorizing, and attempted to state facts as briefly as possible. Were we writing an essay on yellow fever, we would go over much ground that we have not touched upon, but the object of this paper is to be as short and practical as possible."

Nevertheless, Dr. Anderson holds the following language with regard to the cause of the epidemic at Mobile:

With regard to its *cause*—we must candidly state that we are at an utter loss to know to what cause to attribute the past epidemic among us. It made its advent when the air was clear and serene, when the breezes were soft and pleasant, when the temperature was agreeable—and, indeed, when everything gave promise of a delightful summer and autumn. It raged with equal violence in the sunshine and the shower; and left, only, when there was no longer any food for it to ravage on.

We have purposely avoided entering into any dissertation with regard to the particular system on which the poison spent its violence. Volumes have been written upon this subject, and yet we are completely in the dark as to whether the nervous or sanguiferous system becomes the receptacle and bears the brunt of the poisonous influence. In malignant cases, *both* are profoundly altered, but which is the *first* in the category, we know with no more certainty than we do whether cryptogamia, or animalculæ, or electric influences, are the destructive agents which cause the disease. We cheerfully consign all such dissertations to those who are fond of mystery and speculation.

During the months of April, May, June, and July, Mobile was quite healthy, much more so than it generally is at that season. The month of August, at its commencement, offered nothing unusual in the way of disease, of temperature or of atmospheric phenomena of any kind. There were a few cases of bilious remittent fever, mild in their type, and yielding readily to the usual treatment. So far as we could ascertain, there was no appearance of malignant symptoms in any case, and it was a matter of general remark that the fevers yielded very promptly to remedial measures.

Early in this month, however, the pestilence which had ravaged New-Orleans and almost decimated its population, came upon us. Before its advent, we had expected it, and knowing the constant intercourse between the two cities, had been much surprised that we so long escaped. The daily reports from our sister city had raised the public mind to the highest pitch of excitement, and the actual appearance of a dozen cases among us caused the panic-stricken inhabitants to fly in every direction. The first eight or ten cases that occurred in Mobile were among persons who came from New-Orleans, and so far as your reporter could ascertain, the majority of them proved fatal. In about ten days after these cases occurred, others appeared simultaneously in different parts of the city, and some of them wore from the commencement a very malignant aspect. In this particular, as in many others, this epidemic differed from those of previous years. As a general rule, the yellow fever in Mobile, commenced in one portion of the city and extended itself slowly towards other parts, so that there was ample time for the inhabitants of distant districts to fly unharmed from the plague. Within three days, however, from the date of the first case *originating* here this season, there were perhaps fifteen or twenty cases of genuine yellow fever under treatment, and many of these were remote from each other, as if the poison had located itself in some eight or ten different centres, from which to radiate until every neighborhood was completely infected. The tables of mortality will show that this radiation went on with fearful rapidity. How many persons fled it is impossible to ascertain, but it is supposed that in three weeks, not more than ten or twelve thousand were left in the city, and of this number it is probable that four or five thousand were attacked in less than a month after the disease appeared.

The large majority, perhaps nine-tenths of the first cases, were among the poor and destitute—those who lived in crowded and badly-ventilated apartments, and who were necessarily exposed to the sun and the night-air. But in the course of a fortnight or three weeks all classes were seized with the infection, and many of the most malignant cases occurred among the better classes of the population, who, unable to leave, had taken all precautionary measures that prudence could dictate.

Having never been professionally engaged in an epidemic of yellow fever before the past summer, your reporter can draw no parallel from experience between the late and previous epidemics, in many important phases of the disease. With regard to the frequency of hæmorrhage for instance, it would be interesting to know what relation the late epidemic bore to previous ones. Some of our most experienced practitioners are of opinion, that the hæmorrhages during the past summer were less frequent than in former years. Be this as it may, it is quite certain that a large number of cases suffered from hæmorrhage, either from the nose, the gums, the bladder, or the bowels. Most generally the oozing took place from the gums, and although this symptom was always indicative of a malignant attack, yet still it

was not regarded as highly unfavorable; on the contrary, when the oozing from the gums supervened on the fourth or fifth day, it seemed often to take the place of the black vomit, and to inspire some hope of the recovery of the patient, provided it was not very profuse. Hæmorrhages from the bowels were more unfavorable than those from the gums—at least they were so regarded by many practitioners. Little has been said about the appearance of the tongue in this report, for it was so variable, and of so little importance as a diagnostic sign, that it is scarcely worth dwelling upon. In many cases this organ was clean, or nearly so throughout the attack; in others it was furred, white, brown, dry or moist, without any of these states being particularly indicative of the state of the stomach and alimentary canal.

Dr. Anderson, while "giving his testimony in favor of the quinine treatment," seems to ignore the other very powerful agents always given along with the quinine. Thus, when twenty or twenty-five grains of quinine were given, from fifteen to twenty of calomel bore it company.

With regard to the treatment of yellow fever, it was in Mobile very variable, and it is difficult to say which plan was the most successful. In the most malignant cases, it is our belief that no treatment was available, and if such cases recovered at all, it is doubtful whether medicine had anything to do with hastening the happy event. A case, for instance, which, if let alone, would have proved fatal in forty-eight or sixty hours, was beyond the reach of medical aid. The patient seemed doomed from the commencement of the attack. If, however, the case would have lasted four or five days, there was more time for medicine to act, and such cases were often brought under the control of medicine on the third day, and cured on the fifth or sixth.

Every practitioner, of course, lauded his own peculiar mode of treatment, and thought it most successful. With due deference to the opinion of several highly respectable physicians, who opposed what is called the quinine treatment, we must give our testimony as decidedly in its favor. This treatment was much used in this city. Your reporter and his associate in practice being much prejudiced in its favor, from the experience of former years, used it from the very commencement of the epidemic to its close. They treated upwards of eleven hundred cases, and without claiming any superiority in success, they feel sure that their tables of mortality will compare favorably with those of any other physicians in the city. They used quinine in almost every case, regardless of age, sex, idiosyncracy, or any other circumstance. They have every reason to be pleased with their manner of treatment, and with their present experience, would not exchange it for any other that they have heard of. The marked and almost magic effect of a large dose of quinine at the outset was so apparent, that they would have considered it little short of trifling with human life to have adopted any other treatment. They will not deny that

there were cases in which it did no good; in fact, in those cases where there was at the commencement decided congestion of the brain, it may sometimes have done harm, but such cases were very few, and could hardly have been aggravated by any medicine that could be given.

What was the *modus operandi* of the quinine is not so satisfactory. It may have been a powerful sedative, undoubtedly it was in many instances. It may also have had the effect of annihilating the poisonous influence that was so fast hurrying on the blood of the victim to a state of decomposition, and depressing the vital power. It may, like mercury in syphilis, have substituted one poison for another. We cannot tell *how* it acted, but we are sure, from experience, that its action was favorable in the highest degree. Many patients can be cited who could not use quinine in ordinary intermittents without great trouble in the nervous system, but who, in yellow fever, could take with impunity five-and-twenty grains without experiencing any other effect except a temporary deafness of a few hours.

It must not be inferred that quinine was the only powerful remedy given. We never trusted to it alone, but always gave it combined with calomel or blue mass. The usual prescription was as follows:—*R.*: Quinæ sulph. grs. xx. or xxv.; sub. muriat. hydr. grs. xv. or xx.: M. ft. pulv. and give at one dose, in a little syrup.

If this was rejected, repeat it immediately. In the course of twelve hours this dose was repeated, minus five grs. of the quinine and five or ten of the calomel. If the second dose produced decidedly bilious evacuations, the following prescription was given on the second day: —*R.*: Quinæ sulph. grs. xxv.; sub. muriat. hyd. grs. x.; mass. hyd. grs. xv. *M.*: make into pills No. x.: give three every three hours.

Rhubarb and magnesia either singly or combined, were administered, if the calomel did not act of itself. A large blister, six by eight inches, was applied at the onset to the epigastrium. This was considered as highly important, both to allay the nausea and vomiting, and to excite the liver to action. If this organ could not be brought into action by the third day, the plan pursued was to salivate the patient as speedily as possible by small doses of calomel every hour or two. To meet the symptoms of nausea, many prescriptions were resorted to, and among them the phosphate of lime (one drachm dissolved in six ounces of water,) a few drops of lemon juice in a tablespoonful of pure sweet oil, and sometimes a little morphine, deserve particular notice. We must confess, however, that the morphine was not given if it was possible to do without it. While on this subject, it may be remarked, that the quinine itself had a powerful influence in allaying nausea and vomiting. Very often, on visiting a patient the first time, he would be found with distressing sick stomach, which was immediately relieved by a large dose of quinine and calomel. In some inveterate cases, however, the symptom could not be allayed, and it was quieted only by death itself. The tendency to hæmorrhage from the gums was often checked by solutions of tannin and creosote, alum,

and other astringents used as a mouth wash ; and sometimes tannin and creosote were administered internally, but we think with doubtful effect.

In a large proportion of yellow fever cases, there was a period in the disease when stimulants were indicated, and very highly serviceable. This period came on generally about the close of the third day, and it was accompanied with a slow gaseous pulse, a feeling of anxiety, a disposition to wakefulness, a tendency to sigh and to take deep inspirations, and a feeling of general sinking of the system. All kinds of stimulants were resorted to, but those of a gaseous nature seemed to fullfil the indication best, and to be most grateful to the patient. Ale and porter were freely used, and champagne often became necessary when the former were too heavy, and disagreed with the stomach. Iced champagne was highly grateful to the patient, &c.

Art. X.—*Yellow Fever of Selma, Alabama, in* 1853.

J. C. Marks, M. D., in his report on the diseases of Selma, during the year 1853, gives an account of the prevalence of the yellow fever in that town.—*From Trans. Ala. Med. Association.*

July and August were proverbial for a freedom from almost all diseases. The weather continued hot and dry until near the end of August, when a change took place, and we had abundant rains for three or four weeks. In July there were a few intermittents, and one or two cases of bilious fever. The last month in the third quarter, September, was noted for the appearance of yellow fever in our city; and we have now to turn from the pleasant contemplation of almost uninterrupted health, afforded us by the first two months, and record the destructive career of that fell disease—note its debut upon our stage, its tragic performance of death and desolation, which for weeks hung around us the habiliments of grief and mourning.

Selma having been heretofore exempt from an epidemic of yellow fever, although the disease had been twice introduced here on former occasions, its approach in the present instance was little expected and little feared. Its ravages, though uncompromising in other places around us, brought no misgivings to the minds of our citizens, who, lulled to sleep in fancied security, dreamed on of the continued reign of hygeia and exemption from the dread destroyer.

The first case of unmistakable yellow fever occurred about the 17th of that month. The subject of it resided near the river, but had not been to Mobile, or any place where the disease prevailed, and at that time no case had been brought here by the boats. Its course was rapid and fatal. No other well marked case was witnessed until the 7th of October.

On the 8th, other cases of yellow fever occurred, and on the 9th still others. On the 10th, the first and only case brought here by the boats was landed, and died in a few hours. On the 14th there were two deaths from the disease, and a number of new cases, presenting the characteristic symptoms.

It had now evidently assumed the form of an epidemic, and many of our citizens becoming alarmed, quickly fled the place, leaving us, in two or three days, with only some thousand or twelve hundred persons, out of a population of three or four thousand. The disease continued to rage with fearful violence for about two weeks, attacking nearly all classes of our inhabitants, irrespective of condition, age or sex: the negro, however, seemed most exempt, not more than two or three cases occurring among that variety of our population.

On the 26th of the month we were favored with cool and frosty weather, and the frightful career of the epidemic was immediately arrested. It is true we had several cases afterwards, among those who had been exposed previously to the morbific influence, yet there was a marked decline in the number of cases from the above date, and on the 13th of November the disease entirely disappeared.

So extensive was its prevalence here, that few families indeed, who remained in town, escaped wholly a seizure among their members, and in a few instances two or three in a family were attacked; still, when we regard the number of victims, we discover the mortality to be less than we had supposed, and, if we have been correctly informed, will not compare with that witnessed in many other places this year. It has been estimated with considerable precision, that about one hundred and twenty cases occurred in the city during the epidemic, of which some thirty-two terminated in death, which makes the per centum of mortality only about one in four.

During the whole course of the disease there were manifested two forms, or types—one violent, and to some extent unmanageable, the other mild; the former predominated in the beginning, the latter in the decline of the epidemic.

There was nothing in the history of the epidemic that could, by the most favored construction, be made to support the opinion of the contagionists, but, on the contrary, everything was presented that could be desired to confute and overthrow it.

Treatment.—The remedies employed by us in the management of this fearful malady were such as seemed best calculated to overcome the state of congestion upon which it was conceived to depend. We had, indeed, but one general indication presented to us; but, inasmuch as there were different types of the fever, that is, different degrees of this congestion, it became necessary that there should be a corresponding difference of degree in the activity of the treatment resorted to; and we may here observe that we believe the great discrepancy of opinion which has long existed amongst medical men, with respect to the most judicious means of combatting the disease, has been owing to the want of settled notions of its pathology, and especially the

failure to recognize the presence and importance of this invariable condition of congestion. The name of the disease has, as a consequence, been mainly treated, and as the type of the fever varied, that is, degrees of the pathological state, congestion were presented at different places, and at the same place at different seasons or years, and even in the same epidemic, there must, of necessity, from the fluctuations attending the success of every proposed measure, have been the most conflicting and variously empirical treatment employed; while, in reality, these alternately praised and condemned plans of management are equally certain and reliable—equally to be depended upon, when the condition of the disease is recognized, and they are addressed to the degrees or prevailing type of that condition to which each is suited.

We have said that there were two forms of the fever presented to our notice—one violent, the other mild. In the first we sought the removal of the extensive and dangerous congestion by the following measures:

Cathartics.—These were employed usually in the beginning of the attack, and were carried only so far as served for the free and complete actions of the bowels, the evacuation of their acrid matters, and a judicious depletion of the congested vessels. We commonly give a full dose of calomel and rhubarb, or colocynth, to accomplish this purpose.

The Warm Bath.—This in the outset of the disease, provided the febrile action was not already so high as to be above the secreting point, when of course it would but act deleteriously, we regarded of the first moment. It was prescribed both locally and generally, with the object of inducing free diaphoresis, and a salutary determination to the surface, in which we frequently succeeded. Its use, however, was not persisted in after the first thirty-six hours, because its relaxing and debilitating effects then rendered it hazardous.

Bloodletting.—Venesection exerted the happiest effects in every case, whether employed by myself or others, in which its use was restricted to the form of the disease under consideration, and the robust constitution. Indeed, we know of no remedy whose immediate and necessary influence over the essential pathological state better fits it for such cases, especially in the early stages; and we confess ourselves at a loss to conceive how such a weapon of successful conflict with so terrible a disease could have fallen into disrepute, save only from an indiscriminate resort to it, in mild or low forms, and weak and enfeebled constitutions, or the failure to follow up and perpetuate its good effects over the congestion, by the prompt and judicious exhibition of the *great remedy,* whose action in turn it so much favors, and of which we shall now speak.

Mercury.—In some form mercurials, of all the means used, were deemed the most reliable, and they were prescribed in every case of violence.

We generally commenced the mercurial treatment, for its specific

effects soon after the operation of our purgative, or the employment of other preparatory measures, and by the exhibition of calomel in small and divided doses. The quantity prescribed was usually about two grains, combined with from two to three grains of quinia and one-eight grain of morphia, which was repeated every two or three hours, as circumstances seemed to require.

Art. XI.—*Typhoid Fever.—Typhoid Fever of Alabama.*—(From Dr. L. H. ANDERSON'S Report, Trans. Ala. Med. Ass.)

The average age of the subjects was about 20, and the average duration of the attacks about thirty-five days; the shortest about twenty, the longest about fifty days. About two-thirds of the cases were blacks, and three-fourths females. The whole number of cases in the vicinity was about sixty-five, of whom twelve died. Two of these cases were not seen by your reporter at all—three, in consultation: of the other seven, three only were under his entire management. One of these died, after apparent convalescence had set in, of intussusception; autopsy revealing but two small ulcers near the caput coli, nearly healed. Another was worn out by suppurative discharge from the uterus, twenty days after the typhoid symptoms had disappeared; the third would probably have recovered, if she could have been kept free from the excitement of company.

Post Mortem Appearances.—I have made but three autopsies, the notes of which I beg leave to present.

Case 1.—* * * aged 12, black, slave of Mr. Asa Amason, died 21st June, 1853, of typhoid fever, as was supposed. I had not seen the girl during the illness, but being called upon by her owner to take charge of a number of others who were ill with the same fever, I insisted on making an autopsy as a preliminary. She had been ill about four weeks, had taken several doses of calomel, and a good deal of quinine; bowels had been alternately constipated and relaxed, and purgatives and opium had been prescribed accordingly; no hæmorrhage except free bleeding from the nose ten days before death.

Examined twelve hours after death. Emaciation—abdomen distended. A diagonal incision from the left hypochondrium to the right ileum, sufficiently exposed the parts I wished to examine.

Stomach healthy; lower part of duodenum and jejunum slightly injected.

Ileum intensely injected on its outer surface, and exhibiting many dark and purple patches, some almost black. Numerous ulcerations of Peyer's patches and the solitary glands, some almost perforating the intestines; more abundant near the valve.

Colon intensely red without and within, especially near the caput,

several ulcerations in it. It becomes almost healthy near the rectum. Contains very bilious looking fæces.

Peritoneum highly inflamed, about three pints of yellowish serum in its cavity; the inflammation seemed greatest under the parts covered by the blister, which had been applied twenty-four hours before death. Pelvis half full of pus, apparently from softening of the mensenteric glands.

Spleen four times the natural size, not appreciably softened.

Liver healthy, except peritoneal coat: juicy with bile; gall bladder full of clear, healthy looking bile: bile in duodenum.

Note.—In this case there is little doubt that the ulcerated patches were much irritated by the acrid bile so abundantly drawn from the liver by the calomel. They were no doubt aggravated by the blister also.

Case 2.—Hannah, black, slave of same. Aged 20, stout, and of fine constitution. Had been ill thirty-six days. Fever grave at the first, but bowels not deranged. Improved so much in fifteen or eighteen days as to be able to go about the house, though still perhaps feverish. It was supposed she got to an apple tree in the yard, and ate some of the unripe fruit. Was taken down again with fever; bowels distended and painful; some headache. Cups and fomentations were applied to the abdomen, followed by slight pustulation with tartar emetic; under which treatment the pain and tumefaction subsided. The pulse was generally seventy-six. In a few days the bowels became loose, and the discharges yellow and watery. They were restrained by acetate of lead injections, and sulphate zinc given internally, after which the bowels became costive, and were opened daily by enema, the fæces having nearly a healthy appearance. She got the saline solution four times a day, with twenty-five drops oil turpentine morning, noon and night, with buttermilk and water for diet. The fever appearing to be periodically remittent about the twenty-fourth day, quinine and salicine were given freely to break up the periodicity. No permanent benefit resulted from it. About the thirtieth day, she seemed better, tongue moist and clean, some appetite, and pulse less frequent. In a day or two, however, she began to suffer with great distress in the abdomen; retention of urine, requiring the catheter; and she died on the thirty-sixth day.

Autopsy three hours after death. Great emaciation; abdomen flat and shrunken; large ulcer on sacrum. *Head* not examined. *Larynx, trachea, bronchia* and *œsophagus* pale and bloodless.

Lungs not examined. No doubt healthy, as there were no pectoral symptoms before death.

Stomach full of amber colored fluid, and the broth, wine, &c., she had taken for several days before death. Mucous membrane pale and healthy; duodenum pale; jejunum also pale: *it has five intussusceptions;* one, eight inches long; one five; one, three; two, one and a half

inches in length. No inflammatory appearances about the invaginations.

Ileum, injected throughout; two small ulcerations of the solitary glands near the caput coli. *Colon*, ascending and transverse portions injected; becomes very pale near the rectum: moderately filled with healthy looking fæces.

Liver healthy, gall bladder moderately full of deep amber colored bile; very white externally. Spleen healthy, not enlarged. Peritoneum very dry; no traces of irritation.

Note.—This woman would probably have recovered but for the invaginations. The ulcers in the ileum were very small and seemed to be in a healing condition.

Case 3.—Nelly, aged 22, black, slave of Mr. W. T. Simms, taken about 1st October, 1853, with chill, followed by continued fever; got blue mass and quinine—the latter freely for several days without abatement in the fever. I saw her a week after she was taken. She was sitting up, and could hardly be persuaded to go to bed: some appetite; tongue red and clean, bowels open freely: pulse one hundred and twenty-eight; pain in the neck and back of the head, which was relieved by applying chloroform and aqua ammoniæ. Made her lie down, and restricted her from eating. To take the saline solution three times a day. She improved so much in a few days, that she was thought to be getting well; but her bowels became deranged, and after they were checked, she suffered with retention of urine, requiring the catheter, and died on the nineteenth day.

Autopsy eight hours after death. Emaciation not great, abdomen not too full, (nor was it much painful or distended during her illness.) Opened the abdomen from the umbilicus to the iliac fossa, so as to expose the caput coli and the lower part of the ileum. This part of the bowels was of the deepest purple, and the lower part of the ileum and the upper colon, are one mass of ulceration, nearly black, and have a gangrenous odor; the bowels being three-fourths of an inch thick. Higher up the ileum, and lower in the colon, the ulcerations were not so aggravated. The transverse and descending colon was comparatively healthy.

Note.—No ulceration could scarcely be conceived more exaggerated than this; and it is remarkable that such frightful disease could exist with so little tumefaction and pain, and so little disturbance of the action of the bowels during life.

Treatment.—In all cases that have been managed by your reporter during the last year, a general expectant plan has been pursued, and every day's experience adds to the conviction of its being, in the majority of instances, the safest course. I have seen many cases which had been actively treated by venesection, purging, quinine and other anti-febrile remedies, but have always been satisfied that they were materially injured by such measures. The weight of medical

testimony on the subject, seems decidedly in favor of allowing the disease to run its course, so long as no urgent local symptoms, requiring interference, are observed.

One of the principal difficulties I have found in the treatment of this fever, has been that of keeping the patient quiet. It has been impossible to prevent them from being too much nursed, and too much visited by their friends; and though, as the community generally are becoming more familiar with the affection, and the importance of repose both of mind and body, the evil diminishes, yet it still exists to some extent. In my experience with the disease, negroes have got through with it more safely than whites, for the reason that there was not so much anxiety to be constantly doing something for them, and that they were not troubled with company. They have the advantage, too, of being less susceptible of excitement than the white race.

The following, in brief, is the course of management I have commonly pursued. If I saw the patient in the first few days, and found him constipated, and with a foul tongue, bad taste in the mouth, &c. I gave a laxative generally, of blue mass and comp. ext. colocynth, to unload the alimentary canal freely. After this, the bowels usually remain sufficiently open, and further purging is unnecessary, unless constipation should again occur; when simple injections, or a few grains of comp. ext. colocynth, given at night, are sufficient to keep them in a soluble condition. I then put the patient upon the saline draught or solution mentioned in a previous part of this paper, composed of carb. (or bi carb.) potassa, nitrate potassa, bi carb. soda and muriate of soda, each one drachm, water a pint. Give a tablespoonful four, six, or eight times a day. This solution allays thirst, and is generally agreeable to the patient. Some become fond of it, and desire to take it oftener than directed. I adopted this combination as a substitute for the chlorate of potassa, so highly recommended by Dr. Stevens, as being a less expensive, equally as efficient and more accessible every where. A good many cases of some severity have been treated during the year successfully without my seeing them, with no other medicine than this solution, and every day's experience gives me more reason to be satisfied with the prescription. I have never known it to affect the bowels, and rarely to irritate the stomach, and it seems to me to have no other action than a refrigerant one. The diarrhœa of typhoid fever is for the most part easily held in check by opiates and astringents; and the prescription I have found most successful is solution of acetate of lead 2 grs. to the ounce, given every two or three hours, with or without a small quantity of acetate morphine. Tannin, or decoction of oak bark, with burnt brandy, is also a good prescription, when the lead offends the stomach.

To excite diaphoresis, the effervescing draught, or spiritus mindereri, are good preparations, though the former often irritates the bowels. In the fever of the past year, however, I have not found diaphoresis to be as important as it formely seemed.

The heat of the surface should be allayed by frequent sponging

with vinegar and water, and when the head is hot and painful, pouring cold water upon it, occasionally, is highly beneficial.

For pains and tumefaction of the bowels, I have used dry cupping and fomentations, and very rarely blisters: sometimes croton oil or an antimonial counter-irritant. After the first week or two, especially if the tongue be dry, the oil of turpentine in doses of ten or twenty drops, three or six times a-day, has seemed to me to act very favorably. It no doubt prevents or exerts a curative influence upon the intestinal ulcerations.

Rigid abstinence I consider indispensable. It is often very difficult, however, to secure proper forbearance on this point in the friends of the patient. Toast or barley water, rice water, thin gruel, slippery-elm mucilage, or, what is perhaps best of all, ice water (though we cannot always get it in the country,) is all the patient should be allowed, as long as the force of the fever is kept up. In the latter stages, when the system needs a more nourishing diet, broths, beef tea, Borden's meat biscuit, and articles of that sort, are necessary to support the patient, and build up his strength: the return to solid food should be by the most cautions gradations, always remembering that there is no disease in which relapse is more frequent or more dangerous than this.

The cases of congestive fever and of jaundice which I saw, were mostly out of the immediate neighborhood of Sumterville; and only one, in which both diseases were combined, need be noticed. This happened in a young gentleman of good constitution and very regular habits, who had spent the summer in Texas, and had had frequent attacks of intermittent fever, so slight, as, with the assistance of a little blue mass and quinine, not to prevent him from attending to his ordinary business. He was taken with a chill one morning, while hunting, and in a few hours began to pass urine mixed with about one-third of its quantity of blood. During the night, as the fever went off, the urine became pale and natural, and though he took quinine freely, he was not sufficiently careful of exposure to the air, and had another chill about the same hour as on the previous day, so slight as to be scarcely recognized, but followed by light fever, in which bloody urine was again copiously passed. Quinine was repeated in still larger doses, and the patient kept covered up till some hours after the time for the chill to return, and an immediate cure took place.

The progress of this case, and the means by which it was cured, left no doubt on my mind of its being a pernicious intermittent, in which the congestion attacked the kidneys, instead of the brain, the liver, or the stomach and bowels, the parts usually invaded, and the kidneys being organs of small size, and unable to support the load forced upon them, allowed the blood to strain through their texture along with the urine. I had met with several cases of this character in previous years, but they were all in an advance stage, and the renal hæmorrhage had become a permanent symptom. They were all consequent upon neglected chill and fever, and I have no doubt that

the hæmorrhage was intermittent at first in this instance, and migh have been cured by the timely use of quinine. The strictly conges tive nature of the disease, however, never occurred to me until I me with the case just detailed, and I have no doubt that it would have ended like the others, but for its intermittent character being ascertained so early in the attack, and the antiperiodic treatment at once adopted. I will not dilate further upon it, but respectfully submit it with the rest of this hastily written report, to the consideration o the Association.

Typhoid fever of Alabama.—(Report of M. G. Merriwether, M. D. Montgomery county.)

During the summer months there were cases of typhoid fever, and on one plantation it was very severe; and, what was remarkable, of nine infants under one year of age, every one had the disease or some disease so similar that neither my father nor myself could detect the difference. Until I saw those cases, I had believed that children under two or three years of age were not subject to the disease. Of the nine cases, five died; each having an attack of pneumonia supervening on the typhoid fever, which is a fatal symptom so far as my experience goes, in adults as well as children. The rose-colored spots, laid down in the books as distinctive of the disease, are not observable in half the cases; in fact, vibices are more frequently seen. I saw one case during the season, of vibices in the negro of unmixed African descent. Treatment of typhoid fever: the expectant or French plan of treatment, except the frequent administration of the oil of turpentine in combination with laudanum, in thick mucilage of gum-arabic and water. The oil of turpentine appears to have a special curative effect on diseases in which the blood is changed from its normal constituents, and should be used in all diseases which have a hæmorrhagic tendency. The abortive, or Dundas plan of treatment, in typhoid fever I have never seen tried exactly as it is recommended—that is, in the administration of quinine in doses of from 20 to 40 grains—but I have seen a case in which the patient took 500 grains during his illness, which was about three weeks—the case terminated fatally. Since that time, I have been afraid to give the great febrifuge in the doses recommended by Drs. Dundas, of Scotland, and Fenner, of New Orleans.

Typhoid fever of Alabama.—(Report of J. W. Crawford, M. D., of Centreville.)

On two farms, both belonging to the same individual, there were thirty-five cases of this fever, all well marked; out of this number, there were ten deaths, which, it must be admitted, was a frightful mortality. It is due to myself, however, as I attended all these cases, to say that only two cases proved fatal, to which I was called at the commencement or during the *first week of the attack*. In two

of the fatal cases, the attack had lasted two weeks; in two others, three weeks; and in four, eight days before I was called; and a number of those who recovered, had no regular medical attendance for a week or more after taking their bed. Quinine was given to two of the fatal cases in the commencement, by their master; and to others by myself, in large and repeated doses, but without the fever being in the least checked. Post mortem examinations were made in two cases, by my friend Dr. Moren and myself; an account of which, as requested by the Association, I attach to this report. One other case, the son of the owner, aged about fifteen years, originating on this farm, and treated by Dr. Moren and myself, commenced as an intermittent, but after the second paroxysm, assumed a congestive character, and terminated fatally at the end of the second week, with typhoid symptoms. The day after the first chill, he was going about as usual, and was persuaded to go into the river to bathe, where he remained some time. He was treated with calomel and morphia, in small doses; *the cold dash*, *wet sheets*, mustard and dry cups to the spine and the whole abdomen, to relieve the congestion; and quinine, in ten grain doses. The quinine, it was thought, did not act favorably, but increased the depression. At the end of the first week, vesicles containing at first transparent serum, afterwards becoming opaque and degenerating, when not rubbed into a scab, were discovered coming out on the back, and spreading to other parts of the body and limbs, until the larger portion of the cuticular covering was destroyed. After the typhoid symptoms set in, the treatment was expectorant, with stimulants and astringents, to check the bowels.

Case 1.—Mary, aged 38, the mother of two children, one 17 years old, the other 1 year: has had for the last four years an abdominal tumor, which was supposed to be ovarian, from which she suffered very much during her last pregnancy. Has been sick eight days. Has had from her master an emetic of ipecac., followed by blue mass and quinine. Found her with a furred tongue, pointed, fiery red at tip and edges; tremulous when protruded; no pain or thirst; skin hot and dry; abdomen somewhat tender, and distended; no diarrhœa, but stools liquid, yellowish and offensive; wakefulness, but no delirium; pulse one hundred and twenty to the minute. She was given, blue mass one grain, ipecac. half a grain, and opium one-fourth of a grain, every two hours; verat. viride eight drops every four hours, to be increased one drop at each dose until ten drops are taken,

unless nausea is produced; mustard to abdomen, to be followed by mush poultices. On next day, only one operation from bowels for past twenty-four hours; skin not so hot; pulse reduced to one hundred; in other respects about as before. To continue medicine. Next morning Mary was found in collapse, sweating, cold extremities, &c., and continued to sink in spite of stimulants, until ten o'clock when she died. She had not taken the medicine during the night.

Post-mortem four hours after death by Dr. Moren. As she had had no important head or chest symptoms, these cavities were not examined. Liver and gall bladder healthy; the latter moderately distended, with green bile, spleen enlarged considerably and *very much* softened. Kidneys and urinary bladder healthy, and the latter containing some straw-colored urine. Stomach and intestinal tube without any marks of disease until the lower portion of the ileum was reached, when we found one of the patches of Peyer's glands, more than an inch in diameter, thickened, indurated, and raised two lines or more above the mucous membrane: still nearer the end was an irregular, ragged ulceration, an inch wide, with thickened edges; and nearer still, in fact partly on the ileo-cecal valve, was another ulceration, smaller, but similar to the last described. Several of the mesenteric glands were very much enlarged. Ovaries healthy. The uterus was found to be a nodulated mass, larger than a child's head: there was situated on it, and partially buried in its walls, five tumors, from the size of a small orange down to one inch in diameter. They were all of the same character; not fibrous, but slightly vascular, and had much the appearance of cerebrel matter, only they were more yellowish. In laying open the cavity a small one was found presenting on the inside, which did not show on the outside. How could such an organ carry to its full term a child and expel it? yet, that this one did, I know, as I was present at its birth. The labor was regular and easy.

Case 2.—Alex, a large muscular boy, a brother to Mary: has been sick over two weeks, but not considered much so; on Saturday rode on horseback, getting up and down, and catching the horse himself, three hundred yards to another cabin, saying he was tired staying at his own. He was considered convalescent by his owner. I was called to see him on Sunday night, and found him in a profuse cold sweat; extremities cold; intellect clear; no pain; abdomen *very tender*, but not distended; the discharges from his bowels offensive, and being passed without his control; pulse one hundred and thirty, small, thready and scarcely perceptible. Ordered mustard to abdomen; friction, with dry mustard to extremities, and warmth; and internally brandy, opium in large doses, camphor and acetate of lead. He had rallied some on the next day, but afterwards sunk gradually, and died on Thursday night, having had but one discharge from his bowels, which was just before his death: it was said to be principally blood. His mind was clear all the while.

Post-mortem by Dr. Moren, six hours after death. On opening

the cavity of the abdomen a pint or rather more of serum, mixed with pus and flakes of lymph, was found; and on further examination two perforations were found in the ileum, one eight inches from its termination, and the other three or four more; one was perhaps two lines in diameter, and the other rather larger; around each, the peritoneal coat was much thickened by lymph, but not attached to any other portion. On opening the intestine, two other ulcerations were found, one involving the ileo-cecal valve, and the other higher up the ileum than the perforations. Colon filled with dissolved blood. No other disease in small intestines. Mucous membrane of the stomach, in two places of the great curvature, thickened, and so soft as to be removed with the slightest touch, and, throughout its whole extent, very much injected. Spleen very considerably enlarged, and so much softened that the finger can be passed into it at any point with scarcely an effort. All the other abdominal organs, with those of the chest, healthy. Head not examined.

Art. XII.—*Goitre in Alabama.*

W. Taylor, M. D., of Talladega, concludes his report to the Ala. Med. Association, (1854,) with the following summary:

1.—That goitre exists in some sections of the State, to the extent of about one per cent. of the population, and will in time, unless proper measures are adopted to prevent it, assume an endemic form.

2.—The disease being confined principally to the older geological formations of the State, it will, in all probability, never prevail to any considerable extent in the more recent systems.

3.—That the cause of goitre is pretty certainly traced to the character of the water used in the infected districts.

4.—There is strong reason to believe, from the observations of M. Chatin, and the scarcity of iodine in goitrous districts, that the disease is not produced by any positive agent, but has its origin from the want of a due amount of iodine in the waters of said districts.

5.—That goitre is not found in regions where the waters are impregnated with an appreciable quantity of iodine; hence it will never exist, to any considerable degree, in the tertiary and cretaceous systems, nor in the coal meadows of our State; since iodine is abundantly diffused throughout these formations.

6.—Iodine is the remedy, in the treatment of goitre, and this fact, under the circumstances, is strong corroborative evidence that the disease originates from the want of that agent; and if the absence of iodine is the cause of bronchocele, it is but reasonable to expect that great good may be obtained by impregnating the water used as drink, as a prophylactic measure in goitrous districts. May not the remedy

in time become as certain a preventive of goitre, as vaccination of small-pox?

7.—Iodine does not seem to form any chemical combination with the blood, nor with any of its elements, but seems to impart something to the system—the lymphatic system especially—necessary to the maintenance of its healthy functions.

8.—Surgery has done but little for the relief of the goitrous patient. Extirpation has been attempted, with unfavorable results; it is now totally abandoned by the profession. The seton has been resorted to with limited success. The formation of an abscess in the tumor has, in some instances, resulted in a spontaneous cure; tying the thyroidal arteries has also been attended with partial success.

9.—Certain gum resins are applied externally in South America, and also in some parts of the republic of Central America, with reputed efficacy.

Art. XIII.—*Functions of the Spinal Cord:* By T. Lockhart Clark, Esq.

The principal results of Mr. Clark's investigations, made on the ox, calf, cat, rat, mouse, and frog, are drawn as follows:—1. That the posterior roots of the spinal nerves consist of three kinds; two of them entering the posterior gray substance at right angles; the third kind with different degrees of obliquity, tending upwards, a small proportion only of the latter taking a longitudinal course, and becoming lost in the posterior white columns. 2. That in no instance were any fibres of the anterior roots seen to ascend with the anterior white colums, before they had entered the gray substance. 3. That besides the transverse bundles forming the anterior roots, a continuous system of exeedingly fine transverse fibres issue from the anterior gray substance, and become lost as they proceed towards the surface of the cord. 4. That from the preceding facts, it may be inferred that nearly all if not the whole of the fibres composing the roots of the spinal nerves, proceed at once to the gray substance of the cord; and that if any of them ascend directly to the brain, it must be *those only* of the *posterior* roots which run longitudinally in the posterior white columns. 5. That the communication betwen the sensorium and the spinal nerves is not established by the posterior white columns, but by the antero-lateral columns, especially the lateral. 6. That many of the fibres belonging respectively to the anterior and posterior roots in different regions of the cord, terminate there by forming with each other a series of loops of various sizes and lengths; and that it is not improbable that some of them may reach even as far as the brain. It is not perfectly denied by the author that a portion of the roots may be connected with the vesicles of the cords, but he considers the evidence of any such connexion as very

unsatisfactory. 7. The fine longitudinal fibres described by Stilling have not been found by the author. He is inclined to believe that the gray substance of the cord does not transmit impressions to and from the brain. 8. That there is great correspondence in the fibrous arrangement between the gray substance of the cord and the chiasma of the optic nerves. The author further remarks that the circumstance of the nerve-roots diverging upwards in the cord and intricately intermingling with each other, may explain why impressions made at one particular spot are communicated to distant parts of the cord, so as to excite simultaneous and sympathetic actions in classes of muscles which otherwise would appear unconnected.—*Br. and For. Med. Chir. Rev. April*, 1854.

Art. XIV.—*The Effects of the Recumbent Position Physiologically Considered:* By Mr. Richardson.—*(London Lancet, June*, 1854.*)*

The author commenced by stating that though the fact that the horizontal posture often affords marked relief in syncope is generally admitted, no very distinct attempt had hitherto been made to explain the principles on which it acted. One view, however, had fixed itself in the professional mind, and required to be carefully refuted. This view is—that the horizontal posture relieves syncope by allowing the blood to reach more freely the brain and medulla; so that these centres, gaining energy by this process, re-act on the heart and supply it with new vigor. This theory has been supported by Dr. Pulteney Alison, Dr. Ash, Sir G. Lefevre, and many other authors. Sir G. Lefevre has related a case in which syncope ocurred on the patient assuming the erect position. The swooning was found to be connected with the presence of varicose veins in the leg, and was prevented by the application of bandages. But Mr. Richardson showed that the diminution of blood in the brain in this case was secondary to the fact that the propelling power of the heart was to a great extent lost through the mechanical impediment in the circulation,—an impediment which the bandages relieved. It was obvious that the blood detained in the lower extremities could not reach the brain without first passing through the heart. Any renewed force which the heart might receive from the nervous centres would be useless, unless it contained blood in which to expend its force. When we transfuse blood, we do so to fill the heart with its natural stimulus—not for the immediate purpose of exciting the nervous centres. The recovery of consciousness, on laying a person in the supine position, is no proof of the correctness of the pypothesis above mentioned, for when consciousness ceases through syncope, it ceases as a consequence of the failure of the circulation, and returns in proportion as the circulation is re-established. Mr. Richardson had observed that the first symptom of recovery from syncope

was invariably the return of the heart's heat; and that volition, consciousness, and animal heat followed. In some instances the action of the heart fails, while the functions of the nervous system generally remain perfect; and, on the other hand, the manifestations of the nervous system may be suspended by narcotic poisons, while the heart continues to act with power. There may also be extensive disease in the cerebro-spinal axis and yet the heart's action remain unaffected. Again, in the animal kingdom, the size of the heart and the activity of the circulation bear no relation to the development of the nervous system, while in the formation of the vertebrate-embyro, the heart pulsates before it is in any way connected with the nervous centres. Mr. Richardson next proceeded to offer his own theory of manner in which the horizontal position produces its good effects. The explanation appealed to mechanical laws alone, and was very simple. It must be remembered that the arterial blood, sent from the heart, first ascends; and that the venous blood descends from the upper and ascends from the lower parts. When blood is withdrawn from the upper part of the erect body, the heart loses in the end its power to drive the blood over the aortic arch; hence the blood, losing its *vis a tergo*, stagnates in the veins of the lower half of the body. At the same time, the heart, not having sufficient power to force its blood to the brain and other parts, consciousness necessarily ceases, together with muscular motion and the production of animal heat. Death would now soon occur from the heart ceasing to pulsate, and from the blood coagulating in the veins. But at this moment the body falls, or is laid down, and the blood contained in the lower half of the body is poured by simple gravitation into the heart, and again excites it to contraction. Thus the whole circulation is restored, and the brain and every part of the body receiving a fresh supply of blood, resume their proper functions; but to no one of these parts is due the least credit for having restored the heart. When blood is withdrawn from the lower part of the erect body, the chances of recovery are much lessened, for what was in the former case a reservoir now becomes a running cistern. The recumbent position is, however, equally valuable, since it leads to a free distribution of blood through the vessels above the heart. It might be advantageous in these cases to put the head slightly lower than the trunk, until the cause of the hæmorrhage was removed. But as a general rule, the simple horizontal position is all that is required. In cases of syncope, dependant on an overburthened state of the heart, or on debility of the cardiac walls, the horizontal posture relieves by allowing the blood to flow more easily through the pulmonary artery and aorta, and by rendering the venous current more equable.

Several experiments on narcotized animals were here related by the author, in which, after having laid bare the heart, he had placed the body in various positions, and abstracted blood. The experiments all confirmed the mechanical view that had been described. The paper was concluded with the remarks on the physiological and practical bearings of the views adduced.

Part Third.

REVIEWS AND BIBLIOGRAPHICAL NOTICES.

Rev. I.—*Pneumonia: Its Supposed Connexion, Pathological and Ætiological with Autumnal Fevers; including an Inquiry into the Existence and Morbid Agency of Malaria*: By R. La Roche, Member of the American Philosophical Society, of the American Medical Association; Fellow of the College of Physicians of Philadelphia; Corresponding Member of the Imperial Academy of Medicine of Paris, &c. &c. Philadelphia: Blanchard & Lea, 1854.—8vo. pp. 502.

"A great book" is said to be "a great evil." This, we have been painfully taught ever since the medical press of the country has been teeming with *foreign tours* either "with or without notes," which have overlaid our native literature and compelled us to look abroad for what we can manufacture *much better at home;* better, because much more adapted to our condition and more applicable to the real wants of the country. To counteract all these foreign, and to a certain extent, injurious influences, the purity as well as the usefulness of our literature, demands from the constituted authorities, the protection of a copy-right law: with the prospect then of fair remuneration, and of being put upon an equal footing, we should no longer hear the exclamation of "who reads an American book—what does the world yet owe to American physicians and surgeons?" If in spite of all this competition, where the copy-right costs nothing, two such works—as that at the head of this article, and Drake's great work on the Mississippi Valley—can be brought forward, surely an international copy-right is all that is wanting to furnish the comparatively slumbering talent of the profession the opportunity of development. They are, each, a worthy tribute to the genius of the country—alike remarkable for extensive research and profound erudition.

We are, we confess, proud of *this* volume. There are evidences throughout its five hundred pages, that the author has been careful not to give advice, which he has not taken himself; and that is, "in thoroughly making himself acquainted with the literature of the subject on which he writes." And so far from any accusation of ostentation being brought against him, from the necessarily large multiplication of references, there is little difficulty in detecting the difference between a vain and meretricious display of authorities, and when it is used only for corroboration or correction. In every page we observe the untiring scholar, the skillful analyst, the accomplished medical philosopher. We rejoice to welcome back to the ætiological department of the profession, one of the most distinguished of that corps of writers that, more than twenty years ago, elucidated, with their brilliant pens, the medical annals of our country.

Probably, in no department of the profession is this so much wanting, or in which so much good could be expected to be derived from it by the community at large, as in the important, and we are disposed to believe, little studied and less understood subject of ætiology. Confining themselves to pathological specialities, the sweeping causes that overwhelm *communities* in their desolating track, are, by the mass of our writers, left consigned to "occult causes," and the term "constitution of the atmosphere," is a complete barrier to all further investigation. This should not be so; and, we trust, the time is not far distant, when these "epidemic meteorations" will be so fully analyzed that all the physical qualities of the atmosphere will be thoroughly understood; it is alone, thus, that we can think of and apply a remedy.

The great object of the work of Dr. La Roche is to show that there are not sufficient grounds for the "idea of a close connection, as regards both causation and nature, between thoracic inflammation and malarial fevers of various grades and types," not "only on the other side of the Atlantic but in various parts of our own country, and especially in the Southwestern States."

In our own humble opinion, the sentiment thus expressed, as pervading the professional mind, is believed to be derived by them, more from *authority* than *observation;* and an importance has been given to it, to which it is not justly entitled. Be that as it may, it is now set-

tled beyond controversy; no man will be, hereafter, bold enough to defend an hypothesis so demonstrably shown to be untenable. In the extended, welcome and approbatory extracts which have been made from this work by the principal journals of the country, numerous or lengthy extracts will not be expected of us: we shall endeavor to condense his principal propositions and proofs in as narrow a space as possible.

Our author has proved, to our entire satisfaction at least, "that pneumonia is of common occurrence where fevers are seldom or never seen; that pneumonia is not necessarily prevalent where fevers prevail; that they occur at different seasons; that they appear under the influence of opposite winds; that while pneumonia is a common endemic of a locality, the other is not; that while the one is more liable to occur in high, airy and exposed situations, the other occurs in valleys, low grounds, and on the banks of streams; that fevers are influenced by the nature of the soil, which is not the case with pneumonia; that these are arrested by the very thermometric conditions, which give rise to pneumonia." To which the Dr. might have added with great propriety, that the "drying power" which is so fatal to periodic fevers, is exactly that condition which gives rise to pneumonia, more than any other whatever.

The liability of the different races to these two diseases is shown to be in a primary inverse ratio: "that the negro is very slightly subject to periodic fevers, and remarkably so to pneumonia: that the exemptive power of acclimatizing does not apply to pneumonia, although it does to malarial fevers."

The extent to which the protective power of acclimatization furnishes defence against the recurrence of fever, it seems to us, is much overrated everywhere—further than an observant experience will warrant. Nobody pretends to doubt, that during the time a new country is passing through its "transition state," from forest growth to entire cultivation, that no human being exposed to it, enjoys the alledged immunity; *nor does the actual occurrence of the fever itself entirely exempt one.* It is a matter of common observation, that an equable and still life—the ordinary routine life of one's home, the habituation to things around one, is much less apt to be followed by disease—whether epidemic, endemic, or even an occasional visitor (as cholera,

&c.)—than the stranger, the traveller, the new comer, the immigrant; those who have, by this very fact, broken into their own routine of habits of living, eating, sleeping, exercises &c., and have made themselves much more liable to the numerous new influences around them. Now beyond these and the meteorological condition, whose action and influence can be easily explained physiologically, we very much doubt whether there is any farther acclimatization, with *the single exception of yellow fever* and that is against itself alone, while the individual continues in a hot climate. That there is none to filth, and all the nameless exhalations from the soil, is conclusively shown from the large mortality the natives of Egypt are subject to from the plague, the indemic fever of that country; and the constantly injurious and often fatal consequences known to result from over-crowding human creatures together in the filthy localities of all large cities.

That pneumonia and malarial fevers are not *convertible diseases*, is also a proof mentioned, by our author, why they are not the same; that they may both exist in the same individual, at the same time, may be considered another; nor, is it any answer to this to say that typhoid pneumonia prevailed some years ago as a wide spread epidemic; that there should be causes giving rise to pneumonia in an epidemic degree, and that it should be accompanied with a fever of a typhoid character, detracts not at all from the truth of the observation.

Different periods of life are also adverted to, as furnishing different susceptibilities. The *two extremes of life* are very subject to pneumonia, and *middle life* to autumnal fevers, while in the opposite condition they are comparatively exempt.

That each should have periodical tendencies is no proof of identity. The law of periodicity is impressed upon the whole system of our living, and applies to nearly, if not every, pathological as well as to every physiological condition.

We wish we could give as hearty a concurrence to what our learned author has said of malaria as to that which has preceded. We can certainly say this much, that no defender or expounder of its many occult principles and attributes, has exercised a power of analysis—a critical acumen or force of argument, comparable with the present, and that if he has not carried conviction wherever his luminous pen

has touched the subject, it must arise from the fact that there *can be no such specific agent.* So far as the term expresses a morbific state or influence of the atmosphere of certain localities—indeed, in whatever impairs the purity of the air—no matter what may be its precise cause or nature, we have no objection to, satisfied as we have long been that something else than mere physical qualities of the atmosphere and the physiological susceptibilities of the individual, is required to produce, at least, a large amount of our maladies. Nevertheless, it can hardly be denied that a very considerable proportion of the diseases to which our race is subject, can be most satisfactorily accounted for by extremes and transitions of temperature and moisture, and dependant conditions alone, and pneumonia is eminently one of them. Imperfect observations are the result of defective experiments. Our senses alone are not sufficiently acute and well-trained to ascertain these changes, nor do we take the trouble to record the most obvious, much less those which alone can be discovered by instrumental observations: they soon pass off from memory into the sea of oblivion, and are often, after a while, denied.

Of the various atmospherical agents, inimical to our health, an accurate knowledge of the hygrometrical condition is the most recent, if not the most important point gained, to aid the practical physician in his investigations in ætiology. The late applications testify to its great importance. When it shall have been more extensively used, it will aid in furnishing a key to many parts of practical medicine of inestimable value. The discovery of any valuable truth in medicine is of great importance—beyond the abstract truth itself. It fixes the fact, it corrects the exuberance of the imagination, curtails the "false facts," so long the bane of the science and the stumbling blocks of progress, and substitutes the nakedness of truth for ingenious explanations. So let it be with hygrometry. Let us study it with care and apply it with exactitude. Of itself, it is but one of the several conditions to produce that complicated result (termed "fever") which is the great avenue to death of unnumbered millions of our race, it being calculated that one-sixth of the entire annual mortality fall victims to it.

Our author has been eminently successful in showing that pneumonic and malarious fevers arise from different conditions externally; they do so likewise internally; the structures, respectively affected,

are different also. While the first affects the parenchymatous, the second attacks the mucous structures. And, here we cannot avoid the remark that our *treatment* often produces a symptom to which undue importance is attached, and which is the cause of much obscurity here. *Bilious* pneumonia is often an artificial disease, (if not a misnomer) produced by medication, and is the result of a revulsive action on the liver, whose active depletion is invited to equalize the lost balance in the system—it is a perturbation and often hazardous practice. In delicate states it is the game of the gambler—"all or none"—and its chief danger consists in the *uncertainty* of our ever being able to *force secretion*, and the *certian injury* we commit if we fail.

The French pathologists, by their different and more soothing treatment, have not so many "*bilious* diseases" to treat; they act less upon the theory of antagonizing forces; they seldom revulse upon one organ to relieve the suffering condition of another. Their main means of relief are either those of "expectancy," or depletion direct and especially local, and with at least equal success, the constitution sustains less injury, and the patient runs less risk.

With these few hasty remarks, we take leave of our talented countryman; we have read his work with as much pride as pleasure; it is a fountain of instruction, where all may imbibe valuable knowledge. It will long remain a standing monument to his fame—may it ever stand, to teach the aspiring student that the true road to usefulness and reputation, is by industry and indefatigable research.

E. H. B.

Rev. II.—*Types of Mankind, or Ethnological Researches, based upon the Ancient Monuments, Paintings, Sculptures, and Crania of Races, and upon their Natural, Geographical, Philolographical, and Biblical History;* illustrated by selections from the inedited papers of Samuel George Morton, M. D., (late President of the Academy of Natural Sciences, at Philadelphia,) and by additional contributions from Prof. L. Agassiz, LL. D.,; W. Usher, M. D.; and Prof. H. S. Patterson, M. D.: By J. C. Nott, M. D., Mobile, and Geo. R. Gliddon, formerly U. S. Consul at Cairo. Royal 8vo. pp. 738. Philadelphia: Lippincott, Grambo, & Co. 1854.

Article First.—To the pages of Professor Agassiz, in this learned work, the present notice will be restricted, leaving the residue of this large book, so far as it pertains to the Medical sciences, for future examination.

The rôle of Professor Agassiz is quite limited, though his subject

is of vast expansion and of high import, requiring the most profound research, abundant illustration, together with a wide survey of the analogical evidences wherewith to fill up many existing hiatuses in direct experiment and positive demonstration. All who have heard or read Professor Agassiz' lectures, and who are acquainted with his geological, zoölogical, physiological, palæozoic, and embryological researches, will admit his ability to give a far more satisfactory "tableau of the natural provinces of the animal world, and their relations to the different types of man," than his eighteen meagre pages of this work afford.

Prof. Agassiz' prefatory note, indicates his purpose in this paper, namely: to give "some general remarks upon the natural relations of the human family and the organic world surrounding it; in the hope that it may call the attention of naturalists *to the close connection there is between the geographical distribution of animals and the natural boundaries of the different races of men*—a fact that must be explained by any theory of the origin of life which claims to cover the whole of this difficult problem. I do not pretend to present such a theory now, but would simply illustrate the facts as they are, to lay the foundation of a more extensive work, to be published at some future time."

It is evident that a foundation of this kind should not proceed upon abstract propositions in advance of the proof. Whatever evidence can be adduced in favor of Professor Agassiz' weighty postulates—weighty if proved—ought to form the foundation. All the known facts of the palæozoic series constituting the fossiliferous strata, from the oldest to the most recent; the identity, difference, succession, and gradation of organic forms, including their ætiology and zoölogical relations and changes, should have formed the "high argument" of his abstract theory. The succession and variations in the organic world —not its original creation—may be illustrated at every step by the books of stone which the earth's crust presents. And Prof. Agassiz is the philosopher preëminently qualified to open the book of stone and read this concrete Word of God correctly, if any one can: what is, is—its *origin* is beyond the ken of philosophy—let him read what is. The palæozoic ages furnish many editions of the concrete word, the physical λόγος, as well as many volumes: thus, the lower and upper Silurian formations consist of nearly twenty volumes, in which the

fishes predominate. The second grand palæozoic series of the great stone book—the carboniferous—in three volumes; the saliferous oölitic and the chalk formations, treating of reptilians; while the tertiary, the eocene, the miocene and the pleiocene, describe the mammalian series, the more immediate precursors of the book of humanity, the first lines of which open thus: "In the beginning God created the heaven and the earth. So God created man."

Professor Agassiz advances the following hypothesis touching the origin and unity of the human race:

"1st.—Either mankind originated from a common stock, and all the different races with their peculiarities, in their present distribution, are to be ascribed to subsequent changes—an assumption for which there is no evidence whatever, and which leads at once to the admission that the diversity among animals is not an original one, nor their distribution determined by a general plan, established in the beginning of the creation;—or,

2d.—We must acknowledge that the diversity among animals is a fact determined by the will of the Creator, and their geographical distribution part of the general plan which unites all organized beings into one great organic conception: whence it follows that what are called human races, down to their specialization as nations, are distinct primordial forms of the type of man.

The laws which regulate the diversity of animals, and their distribution upon the earth, apply equally to man, *within the same limits and in the same degree;* and that all our liberty and moral responsibility, however spontaneous, are yet instinctively directed by the All-wise and Omnipotent, to fullfil the great harmonies in Nature."—xxv. xxvi.

In the following extract the reader will find a summary of Prof. Agassiz' notions of species, varieties, unity, diversity, &c.

In order not to enter upon debateable ground in answering the first of these questions, let us begin by considering it with reference to the animal kingdom; and, without alluding to any controverted point, limit ourselves to animals well known among us. We would thus remember that, with universal consent, the horse and ass are considered as two distinct species of the same genus, to which belong several other distinct species known to naturalists under the names of zebra, quagga, dauw, &c. The buffalo and the bull are also distinct species of another genus, embracing several other foreign species. The black bear, the white bear, the grizzly bear, give another example of three different species of the same genus, &c., &c. We might select many other examples from among our common quadrupeds, or among birds, reptiles, fishes, &c., but these will be sufficient for our purpose. In the genus horse we have two domesticated species, the common horse

and the donkey; in the genus bull, one domesticated species and the wild buffalo; the three species of bear mentioned are only found in the wild state. The ground upon which these animals are considered as distinct species is simply the fact, that since they have been known to man, they have always preserved the same characteristics. To make specific difference or identity depend upon genetic succession, is begging the principle and taking for granted what in reality is under discussion. It is true that animals of the same species are fertile among themselves, and that their fecundity is an easy test of this natural relation; but this character is not exclusive, since we know that the horse and the ass, the buffalo and our cattle, like many other animals, may be crossed; we are, therefore, not justified, in doubtful cases, in considering the fertility of two animals as decisive of their specific identity. Moreover, generation is not the only way in which certain animals may multiply as there are entire classes in which the larger number of individuals do not originate from eggs. Any definition of species, in which the question of generation is introduced is, therefore, objectionable. The assumption, that the fertility of cross-breeds is necessarily limited to one or two generations, does not alter the case; since, in many instances, it is not proved beyond dispute. It is, however, *beyond all question*, that individuals of *distinct* species may, in certain cases, be productive with one another, as well as with their own kind. It is equally certain that their offspring is a half-breed; that is to say, a being partaking of the peculiarities of the two parents and not identical with either. The only definition of species meeting all these difficulties is that of Dr. Morton, who characterizes them as *primordial organic forms*. Species are thus distinct forms of organic life, the origin of which is lost in the primitive establishment of the state of things now existing, and varieties are such modifications of the species as may return to the typical form under temporary influences. Accepting this definition with the qualifications just mentioned respecting hybridity, I am prepared to show that the difference existing between the races of men are of the same kind as the differences observed between the different families, genera, and species of monkeys or other animals; and that these different species of animals differ in the same degree, one from the other as the races of men—nay, the difference between distinct races are often greater than those distinguishing species of animals one from the other. The chimpanzee and gorilla do not differ more one from the other than the Mandingo and the Guinea negro: they together do not differ more from the orang than the Malay or white man differs from the negro. In proof of this assertion, I need only refer the reader to the description of the anthropoid monkeys, published by Prof. Owen and by Dr. J. Wyman, and to such descriptions of the races of men as notice more important peculiarities than the mere differences in the color of the skin. It is, however, but fair to exonerate these authors from the responsibility of any deduction I would draw from a renewed examination of the same facts, differing from theirs; for I maintain distinctly that the differences observed

among the races of men are of the same kind or even greater than those upon which the anthropoid monkeys are considered as distinct species.

Again, nobody can deny that the offspring of different races is always a half-breed, as between animals of different species, and not a child like either its mother or its father. These conclusions in no way conflict with the idea of the unity of mankind, which is as close as that of the members of any well-marked type of animals; and whosoever will consult history must remain satisfied that the moral question of brotherhood among men is not any more affected by these views than the direct obligations between immediate blood relations. Unity is determinal by a typical structure, and by the similarity of natural abilities and propensities; and, unless we deny the typical relations of the cat tribe, for instance, we must admit that unity is not only compatible with diversity of origin, but that it is the universal law of nature.

This coincidence, between the circumscription of the races of man and the natural limits of different zoölogical provinces characterized by peculiar distinct species of animals, is one of the most important and unexpected features in the natural history of mankind, which the study of the geographical distribution of all the organized beings now existing upon the earth, has disclosed to us. It is a fact which cannot fail to throw light, at some future time, upon the very origin of the differences existing among men, since it shows that man's physical nature is modified by the same laws as that of animals, and that any general results obtained from the animal kingdom regarding the organic differences of its various types must also apply to man.

In the above passage Prof. Agassiz asserts "that there are entire classes of animals in which the larger number of individuals do not originate from eggs," and yet, in his Principles of Zoölogy published in 1851, he holds the following language:

"§ 275.—That all animals are produced from eggs *(omne vivum ex ovo)* is an old adage in Zoölogy, which modern researches have fully confirmed. In tracing back the phases of animal life, we invariably arrive at an epoch when the incipient animal is inclosed within an egg." 132.

In the same work, however, one is surprised to read thus:

"§ 328.—We have shown in the preceding chapter, that ovulation, and the development of embryos from eggs, is common to all classes of animals and must be considered as the great process for the re-production of the species. Two other modes of propagation, applying, however, to only a limited number of animals, remain to be mentioned, namely, *gemmiparous* re-production, or multiplication by means of buds, and *fissiparous* re-production, or propagation by division; and also some still more extraordinary modifications yet involved in much obscurity." 156.

These discrepancies are not alluded to with a view of underrating the great and well merited reputation of Prof. Agassiz as a profound naturalist and physiologist, but for the purpose of keeping questions not fully settled, still open for further research, that Nature may be interrogated rather than the authority of men how learned so ever they may be.

In vain may philosophy call to its aid, physiology, comparative anatomy, geology, palæontology, ethnology, archæology, monuments and manuscripts in order to account for man's origin. He who will not accept the Mosaic history, abandons the last plank, and must sink in hopeless uncertainty. If the miraculous origin of man be admitted, so from the same source, it is reasonable to suppose came his anatomical diversities as to color, hair, crania, and the like; or, if these changes resulted from former climatic laws which ceased after the effectuation of their finalities, more miracles must be admitted, which certain philosophers reject altogether, while others seek as far as possible to explain the origin and primary types and variations of man by laws now in operation, without rejecting miraculous interposition where philosophy fails. "In the beginning God created the heaven and the earth."—"Here will I hold."

The visionary speculations of Lamarck, from whom the author of "The Vestiges of Creation" has borrowed much inconclusive ratiocination and downright rant, have not, in a single instance, explained the origin of an animal, much less man. The massive intellect of Prof. Agassiz is not biased by these fictions concerning the assumed omnipotence of the abstract law of progressive development stimulated by inexorable want, or *besoin*, as Lamarck calls it, which is inaugurated as a positive entity in place of the *Infinite Ens*, and which instead of explaining the creative acts, throws an impenetrable veil of darkness over the past. Will the remote past ever reveal to science the origin of the universe? The vibrating pendulum of eternity seems to answer—*Ever?* Never! Ever? Never! [Editor.

Rev. III.—*Handbook of Chemistry: Theoretical, Practical and Technical:* By F. A. Abel, Professor of Chemistry at the Royal Military Academy, Woolwich; and Assistant-teacher of Chemistry at St. Bartholomew's Hospital; and C. L. Bloxam, formerly First Assistant to the Royal College of Chemistry. With a Preface by Dr. Hofmann: and numerous Illustrations on Wood. Philadelphia: Blanchard & Lea, 1854—8vo. pp. 681.

Chemists, apothecaries and doctors, both actual and intending, might be deceived by the title of this work, called "A Handbook;" that is, they might suppose that it is like many other handbooks—a mere compilation, pirated from the living and the dead, and worked up in the closet, without skill, without analyses, without method, and without experimental research. Such is not the case with this able work. Its scope—theoretical, practical, technical and analytical, without being exhaustive, is nevertheless very comprehensive and complete—running through the whole gamut of the science.

The wisdom of the ancients, in whatever pertains to scientific chemistry, compares most unfavorably with that of the present generation; affording, indeed, a subject in which contrast rather than parallelism predominates.

Chemistry—organic and inorganic, theoretical and practical—is virtually a modern science; yet, notwithstanding the secure basis on which its general principles rest, the progress of discovery, the details of new analyses, both qualitative and quantitative, all conspire to establish the presumption that, other things being equal, the newest chemical books are the best; at least, the work above mentioned appears to belong to this category, since it contains in a high degree the postulates and the practice, the science and the art of chemistry, methodically arranged and extensively illustrated by wood cuts, intercalated in the text, so as to occupy a small space.

The following extract is given,* not as a specimen of the researches of Messrs. Abel and Bloxam, but as the latest summary of information concerning an agent little known, namely, *Ozone:*

Ozone.—This remarkable body was first discovered by Schönbein. He detected it in the atmosphere (by means of tests to be presently described,) and found it to be formed in almost every instance of electric discharge into the air; also, when water is electrolyzed, and when phosphorus is allowed to act upon moist air at ordinary temperatures.

Preparation.—Ozone is best obtained by placing a piece of recently

* See the Article on the Perchloride of Iron, in the present number of this Journal.

scraped phosphorus, about half an inch in length, into a clean bottle (of about two quarts capacity), in the bottom of which is as much water as will half cover the phosphorus; the mouth should then be closed slightly (to prevent any mischief ensuing if inflammation of the phosphorus should take place), and the bottle set aside. Ozone is almost immediately produced, its formation being indicated by the ascent of a column of vapor from the piece of phosphorus, and the luminosity of the latter in the dark. Ozone may be detected in the bottle within a minute after the introduction of the phosphorus; if allowed to stand for six or eight hours, the air in the bottle will be abundantly charged with it. The phosphorus should then be removed, and the air freed from phosphorus acid by agitating some water in the bottle.

Properties.—The ozone thus obtained (in admixture with air) has the following properties: it is a colorless gas, possessing a very peculiar odor, which, when concentrated, much resembles that of chlorine, but when diluted is precisely the odor observed when an electric machine is in action. When air has been powerfully charged with ozone, it can be inspired with difficulty; it acts powerfully on the mucous membrane, producing very disagreeable sensations; small animals immersed in it soon cease to exist. Pure ozone must therefore be highly poisonous.

Ozone is insoluble in water; it possesses powerful bleaching properties, and also acts as an energetic oxidizing agent, transforming phosphorus into phosphoric acid, and powerfully oxidizing many metals, converting them and their lower oxides into the highest oxides they are capable of forming. Thus, lead and silver are converted into oxides, antimony and arsenic into arsenic acid and antimonic acid; the salts of the protoxides of manganese, cobalt, nickel, are decomposed by it, the acids being evolved and the binoxides formed. It also decomposes many hydrogen acids (*e.g.* hydrosulphuric acid), and oxidizes organic compounds. It combines with chlorine, bromine and iodine, and is in many respects analogous in its action to the binoxide of hydrogen.

Two views are entertained respecting the constitution of this body; the one, that it is oxygen in an allotropic condition—the other, that it is a compound of oxygen similar to binoxide of hydrogen. The former is the view which possesses the greater number of supporters, particularly since it has been proved that, on passing dry ozonized air through a redhot tube, the destruction of ozone by the heat (it being only capable of forming at ordinary temperatures) is unaccompanied by the production of any water.* Many organic compounds, such as ether and turpentine, when exposed to the action of air and light un-

* By very recent researches, Baumert believes that he has shown the ozone obtained in the electrolysis of water to consist of a teroxide of hydrogen. He passed the perfectly dry ozone, first through a tube containing anhydrous phosphoric acid, which was unaffected by it, then through a tube heated to redness, and lastly, through a second tube, containing phosphoric acid, which indicated the presence of moisture produced in the decomposition of the ozone. The proportion of oxygen was determined by passing the ozone into a standard solution of iodide of potassium

dergo peculiar changes, and acquire very powerful bleaching and oxidizing properties, apparently by association with ozone.

Tests for Ozone.—The most delicate test for the presence of ozone is prepared in the following manner: one part of pure iodide of potassium and ten parts of starch are boiled together, for a few moments, with two hundred parts of water, and white filtering paper is saturated with the liquor thus obtained. Such paper is immediately turned blue when introduced moist into ozonized air. If introduced dry it will remain colorless, but becomes blue immediately upon being moistened.

Paper prepared with a solution of sulphate of manganese is also a good test for ozone, becoming rapidly brown from formation of binoxide when introduced into ozonized air. [Editor.

Rev. IV.—*The Science and Art of Surgery: being a Treatise on Surgical Injuries Diseases, and Operations:* By John Erichsen, Professor of Surgery in the University College, and Surgeon to University College Hospital. Edited by John H. Brinton, M. D. Illustrated by 311 engravings on wood: Philadelphia: Blanchard & Lea, 1854.—pp. 908, 8vo.

The material processes and obvious eventualities of surgery address themselves directly to the senses, and may, to a very great degree, be appreciated by all classes of persons, as in the reduction of a dislocation, the amputation of a limb, the tying of an artery, the removal of a tumor, &c. On the other hand the physician may save a hundred lives were the surgeon saves one, and yet in the former case, neither the patients cured, nor the observers, can determine in a satisfactory manner what the therapeutic value of his services may have been. Hence, a physician may not be an operating surgeon, but a surgeon must be a physician—aye, and a physiologist too—otherwise, he must sink to the character of a mere mechanical operator, who is seldom to be trusted out of the sight of the physician.

Hence arises the questionable expediency of studying and practicing specialities in medicine. The oculist, the aurist, and even the mere operative surgeon, generally succeeds well or ill, just in proportion to their comprehension and appreciation of anatomy, physiology, pathology, and therapeutics. The enlightened and conscientious surgeon holds, as a fundamental article of faith, that of all the means of cure, surgical operation must be kept in abeyance, or be avoided altogether, as a reproach on the healing art and as a proof of its imperfection, unless it shall afford the patient a reasonable certainty, or at least a probability of a more favorable result than that founded on the

best devised plan of medical treatment. The dangers resulting from an operation are often great, and require the utmost skill of the physician to obviate them and complete the cure. The grandest generalizations of the surgeon are those of the physician, as in the doctrines of inflammation—"first, last, midst, and without end"—ranging from concussion to hyperæmia—from effusion to gangrene—from snake-bite to frost-bite—from contusion to ulceration—from carbuncle to syphilis—from scrofula to cancer—from gonorrhæa to phlebitis—from rickets to necrosis—constituting a duality in unity, and a unity in duality.

But, it is time to pass from these platitudes to the work named above; a slight examination of which warrants the conclusion that, notwithstanding the avalanches of surgical works recently hurled forth upon the medical world, all possessing much merit, Prof. Erichsen's work, for its size, has not been surpassed; his 908 pages, profusely illustrated, are rich in physiological, pathological and operative suggestions, doctrines, details, and processes, and will prove a reliable resource for information, both to physician and surgeon, in the hour of peril.—EDITOR.

Rev. V.—*On Rheumatism, Rheumatic Gout and Sciatica: their Pathology, Symptoms and Treatment;* By HENRY WILLIAM FULLER, M. D., Cantab., Fellow of the Royal College of Physicians, London; Assistant Physician to St. George's Hospital, &c. &c. New York: Samuel S. & William Wood, 1854—pp. 322, 8vo.

This is a valuable, well timed, and very complete monograph, upon the causes, symptoms, various forms, nature, seats, and pathological anatomy of rheumatism; including the general and special treatment adapted to its stages and diversities—comprehending the various complications of this malady, particularly that most formidable one, heart-disease, as carditis endo-carditis, peri-carditis, valvular alterations, fibrinous vegetations, serous effusions and infiltrations; also, articular, pulmonary, pleural, cerebral, sciatic and neuralgic affections. The author investigates the rheumatic diathesis—its parallelism with gout; alludes to mal-assimilation, the morbidity of the blood, the predominance of lactic acid, and other phenomenal manifestations of this malady.

Dr. Fuller has examined into the therapeutic value of the remedial

agents usually employed in the treatment of rheumatism—purgatives, baths, mercury, antimony, cinchona, colchicum, guaiacum, nitrate of potash, lemon juice, alkalies, and their salts, and, above all, opium. Of the latter, he says:

I can testify most strongly to the value of the sedative in full and repeated doses: in doses far exceeding in amount the quantity usually adminstered. In the early and most painful stage of the disease, occurring in adults, it may often be given with the greatest advantage, to the extent of six or eight grains in the course of twenty-four hours; and to children, without the slightest fear, in half grain doses every three or four hours. * * * I have never seen it check secretions, or produce the slightest cerebral disturbance. I am satisfied that in many instances it has materially hastened the period of convalescence, and has lessened the frequency of inflammation of the heart. Therefore, whilst I join issue with those who would treat acute rheumatism by opium alone, I admit most fully the advantage of its employment in conjunction with other remedies, and in quantity sufficient to allay or subdue pain.—p. 83.

The editor of this Journal, can from experience recommend large doses of opium, morphia, and Dover's powders, in the earliest stages of the most acute form of rheumatism; articular infiltration, tumefaction, and pain and fever, usually subside gradually, sometimes suddenly, as soon as the system is brought under the influence of opium. In this manner he treated, not long since, the President of the Board of Assistant Aldermen of New Orleans, Dr. Dalton, himself an accomplished physician, who suffered a sudden and violent attack. As soon as the opium subdued the pain, though without narcotization, the fever, infiltration, and articular engorgement and tumefaction, began steadily to decline; the ferrocyanate of quinine and apperients completed the cure in a week. Opium is not only a subduer of irritation, pain and restlessness, but of inflammation itself.—Editor.

Rev. VI.—*Women: Her Diseases and Remedies: A Series of Letters to his Class:* By Charles D. Meigs, M. D., Professor of Midwifery, and the Diseases of Women and Children, in Jefferson Medical College at Philadelphia; Member of the American Medical Association; of the American Philosophical Society, and of the Council; Vice-President of the College of Physicians of Philadelphia; late, one of the Physicans of the Lying-in Department of the Pennsylvania Hospital, &c. Third edition, revised and enlarged. Philadelphia: Blanchard & Lea, 1854.—8vo. pp. 672.

Napoleon, the prisoner at St. Helena, was told by one of his exiled companions that which would never have been uttered to Napoleon,

the Emperor, at the Tuileries, namely—that he had no style! Yet his bulletins affected the style of Ossian. Dr. Meigs has been much—if not wisely—criticised on account of his remarkable style, even by those who have accorded to him the highest qualifications as a thinker, teacher, practitioner and instructive writer.

It was but recently that periodicals, pamphlets and octavos, put the question—"Have we a Bourbon amongst us?" In the same vein it may be asked—Have we a style amongst us? Has Dr. Meigs a style? Ask not—Is his style faultless; but, is it original, characteristic of any school, or proper to himself, something by which he may, as a writer, be identified. It is not intended on this occasion to give an affirmative or negative answer to this question, further than to say that if Dr. Meigs has a style of his own, he must be, independent his preëminent scientific knowledge, an extraordinary genius. But, without going into the natural history and the composite character, and neological tendencies of Dr. Meigs' composition, a few remarks may be allowed illustrative of the question—Have we a style among us?

Let it not be supposed that the writer of this article claims for his own style any excellence whatever. A just conception, by no means, implies the ability to execute. Some one stated that he who undertook to criticise poetry must himself be a poet; to which, Johnson replied that, by this rule, "he who kills fat oxen must himself be fat." Johnson was himself a great critic in poetry—not a great poet.

The writers of the preceding generation dedicated their works to some powerful patron or to posterity, and barely escaped starvation. The authors of the present day dedicate their books to the multitude, caring little for posterity, and grow rich. Milton sold out at £15; a modern novelist gets as many thousands, and lives to see a score of editions. Yet the works of the former class shine with an increasing lustre; while those of the present generation, with few exceptions, will pass away, meteor-like. The reigning passions, the temporary interests, and utilitarian views of the moment are reflected upon the multitude; which, being delighted with a panorama of itself, leaves antiquity and posterity to take care of themselves, as no better than old fogies and humbugs—to use the cant phrases of the day.

Among an host of living writers, some are correct and elegant, not a

few are eloquent, pathetic and natural, enlightening the understanding, swaying the passions and directing the destinies of humanity, showing that "the pen is mightier than the sword;" yet, it will be difficult to designate among the whole, one that is recognizable by originality—one who has a well defined style, proper to himself, belonging to a distinctive school, except that which may be called the composite, in which there is but little individuality or unity of type. It is from the title page, and not from the style that the reader, with rare exceptions, learns who the author is, and how to distinguish him from all others.

Individuality of style by no means implies personal individuality. In that "Mighty orb of song—the divine Milton"—his personality, his great thoughts and his sightless eye-balls, seem to go together. The same is true of Young, Burns, Byron and many others. But when the reader launches on the vast stream of Shakspeare's mind, humanity, not Shakspeare is individualized. Each person of the drama thinks, feels, wills, acts and speaks, without losing the unity of a common type. The colorless diamond reflects the greatest variety of lights.

The exuberance of medical observations, experiments, and ratiocinations and inductions, now extant would be far more attractive, more read, and consequently, far more useful, were they embodied in a suitable style.

It will be found, as a general rule, that a feeble, redundant and inaccurate style in medical composition, is characterized by corresponding defects in mental ability, both as regards accuracy in observing and reasoning.

The student of language must fall back upon the last century and the earlier part of the present for models of pure English composition; as nearly every great master of that era had a characteristic style proper to himself, equally good with that of his neighbors', though different in kind. Among these various masters and different styles one may be preferred to the other, without incurring the charge of bad taste—whether Bunyan, Blackstone or Junius, Swift or Addison, Gibbon or Goldsmith, Hume, Robertson or Johnson—be chosen. These writers are not known solely by an exuberance of exclamations, dashes, colloquial phrases, antitheses, neologies; nor by

exaggerated bad spellings and solecisms of language carefully copied from among the vulgar, mixed up with studied periods, both long and short, ornate diction and pathetic climaxes—throughout thin serials and thick octavos.

Among the popular authors of the day it would be difficult to find one whose style is so original and withal accurate, as to admit of a striking and easily recognized parody or comic imitation. Without reliance on this test, which is among the weakest, it is possible to travesty Johnson, Milton, and burlesque Homer; while a burlesque or imitation of many of the present race of writers could only be found out by means of a title page and preface, just as in the case of a certain innkeeper's sign, whereon a horse and a bear had been painted, under each of which, it was found necessary to put the name, that the public might distinguish the one from the other.

Critics lay down certain postulates, often no better than platitudes, for the formation of style—the style of science they think should be simple. True. But this simplicity, properly understood and applied, is of all acquirements the most difficult and the last attained. He who is master of this, must have mastered all the other qualities of style. A dry, didactic, by no means implies a simple style, much less an interesting one. Medical facts reduced to numerical tables, are, for example, less read than the most indifferent composition in which the same truths may be detailed. The Principia of Newton is didactic, technical, mathematical, and in Latin; but, if its great truths can for the most part be taught without mathematical formulæ, let no one grieve thereat. If the cause of science can be advanced by ornaments, tropes and figures—by Carlyslisms, Meigsisms, and neologisms—grieve not; although Jonathan Swift hated foreign words and idioms, as much as Johnson loved Latinisms and magnificent periods.

In writing on medical subjects, perspicuity is the fundamental quality; but if the writer can, by any other properties of composition not incompatible with the first, throw attractions around his topics so as to interest his reader the more thereby, he is justified in doing so. Here, at least, the end justifies the means.

Obscurity in writing and in thinking, dislocated sentences and arguments, a lack of syntax and a lack of sense, are, with rare exceptions, connected as warp and woof. In the office of this Journal are

rejected essays, in which there is not a single stop; in which the syllables are disconnected; in which capitals form the middle of words—some being sprinkled about like sand, as if by accident; papers in which the spelling is right only by chance, clearly showing that the medical colleges have granted the degree of M. D., to men who cannot spell the word p-h-y-s-i-c-i-a-n except as follows—phisisian; patenst; elusteration; resent [recent]; phelm [phlegm]; greate interprize; haveing; takeeng; asure; stile; maladys; facalty; tecnhical [technical]; modren; nesesary; delereum; once [ounce]; wether [weather]; brused; penus, &c. Now, these papers are, as such usually are, as defective in matter as in manner; as incoherent in thought as in grammar.

Suppose, what rarely if ever happens, that a writer whose style is inaccurate, his words ill-chosen, his sentences badly arranged and obscure in import, possessed, nevertheless, profound knowledge; suppose that he should write the history, including the pathology and therapeutics of yellow fever for the last three centuries, as deducible from thousands of volumes, pamphlets and reports, now passing rapidly to oblivion; and suppose, furthermore, that the same subject should be selected by another writer possessing the same degree of scientific knowledge, and the additional qualification of an accurate and elegant style—like that of Sir William Blackstone, Dean Swift, Addison, Johnson, or, still better, good John Bunyan,* Hume, Gibbon or Robertson—which of these histories would be preferred? which would be the most useful? which would stand the scrutiny of talents and time, and serve as a light-house to the future voyager upon the dangerous, dark and uncertain coast to which humanity shall be often borne, when the perilous storm of pestilence shall rage?

It is not fine writing, as it is improperly called, that should be aimed at by medical authors; but that more difficult style, the elements of which are simplicity, clearness, and strength; in which nothing is redundant, nothing deficient—consisting of proper words in proper places; which convey the greatest amount of knowledge in the smallest space; which suggest more than they express; which give

* Although Bunyan's personages are allegorical, his narratives rival those of ancient and modern times; being admired alike by the learned and the unlearned—and the more, as time rolls onward.

to the longest essay, brevity; and, which make an indelible impression on the memory.

Look into the narrative passages in the historians already mentioned, and compare them with the reports of medical cases—which latter, also, are essentially narrative. Now, which of these classes will contain the greatest number of facts and ideas? which will be arranged best? which will be expressed in the most systematic manner? Which will be the easiest to understand, will make the most enduring impression?

Dr. Meigs censures medical writers for their disgusting tediousness, dullness and jargon. He says of his own style:

"I will write in the same language I should address to any one of you, whom I might be instructing in my library here, at home. Whether such a mode of writing might prove agreeable to the brethren, so as to meet their approbation, remained to be seen. If I should fail in this attempt, I may still hope that some one else will invent a new and happier method than mine, to get rid of our medical dullness and time-honored clergyableness."—viii.

It may be allowable to give, in this place, a few of Dr. Meigs' "pet" words and phrases, which are for the most part so often repeated, that, had Falstaff been one of the young gentlemen to whom the Letters are addressed, he might have sworn, as on a former occasion: "O, thou hast damnable iteration!"

"Pet vaginal; scrobicle; maladive; delimitation; delimitary membrane; ovi-posit; intestinal tractus; life-status; status of texture; penultimate of life; wild hetorologue; saccadée; ballooning of the belly; atmospheric mixt; privities; granular retinacula; cellular tela; panoptical; perpend; neurosity; vector of the ovulum; fecundative conflict; the uterus dispensing an aura; is it depôt? depôt unchanged; understandable; uncognoscible; clergyable pride; clerkly; clergyableness; macilent; intempestive; hypogaster; outsider classes; *quo* modo disparitions; auscult; ausculted; gestasio-ventosa; strange vitality; powers of the stroma; endangium; endangial malady; endangitis; endangial membrane; hila-porule; arterioles; venueles; prava ingesta; somatic innervation; hetorologue organ; a good index expurgatorius.

The blood is confined within and bounded by the endangium, which is the delimitary membrane and its manufacturing apparatus. The endangium, as solid, contains the cause of the hæmatosis, in the fœtus, and *a fortiori*, in man.

The arterial system, by injecting oxygen, develops the brain and the whole nervous system, the flash of life: where its injecting force is greater, the life is greater; where it is less, the life is feebler.

Dr. M. thus speaks of a tympanitic lady:

You are not pregnant; you are resonant upon the whole abdomen, down to the very hypogastrium, and there is no click to the fœtal heart. The motions you have felt and the distention you have suffered were motions of borborygmi and the inflation by gasses, &c.

The stroma of ovaries is the true sexual tissue of females; in fact, that stroma is sex for them; after a long tractus its strange vitality and economic connection.

It is clearly a case of physical sin, and absolute rebellion against the specific authority and laws of the economy.

The brain will, generate and radiate its nerve force, its biotic force to the organisms: the aphrodisiac power to a status of texture.

The whole spinal cord vibrates under the tension of the vital forces.

The motor cords excite the muscular tissues, the sensitive cords feel the pressure. This is an after pain. *An after pain*, therefore, is a good thing and a natural.

The uterus *grows* smaller and smaller.

The surgical cervix [of the womb] projects half an inch.

Burdach is a German Kanteio-Schelling-ian-Okenian physiologist; cure your hæmatosic membrane; your hæmatosic tissue is not in health; your endangium is out of order, weak and pale; the endangium perfect; no, siree; tilly-vally; exquisite citadine; artistical imagination with perfectionings; no neurosity developed; immiscence; literary martinets.

Suppose that Dr. Meigs, in asking Miss Helen Blanque to make him a cup of tea, were to use the following language: "add to *quant. suff.* of *thea* chinensis, the oxyd of hydrogen saturated with caloric."

The witty Helen would quote Shakspeare, thus:

"An honest tale speeds best, being plainly told."

Tautology.—I have read the balance, of the rest, of the residue, of the remainder of Dr. Meigs' first letter.

"Damnable iteration," says Falstaff.

"In front, the vagina is attached to the bladder and urethra; behind, it touches the rectum, and is soldered to that gut." There can be no need in the world to tell young gentlemen that a soldered thing touches—the greater implying the less. "The womb contracted by the tonic action of its muscular fibres; (equal to water turned into ice by the abstraction of free caloric,) "hyperæmic status;" (the status is implied or rather constitutes the hyperæmia,) "functional forces;" is in the same category: so are the phrases "injuriously deranged organs; chronical, long continued maladies; uterine globe; modifications of altered organs; very enormous; recumbent rest; I am opposed to

ovariotomy on grounds of objection; an epidemic cause is uncognosible, recondite, and beyond the scope of the human understanding; direful effects of life-force; the life-force excited with preternatural energy; the tension, and pressure, and intrusion, gave her much pain; quasi organization, and losing the power of increase from atrophy of the most distal and attenuated branches; vile quackery and charlatanism; a great moiety of cases; stringent necessity," &c.

Dr. Meigs, in one part of his book, utters the most violent maledictions against medical statistics, while nearly every letter is full of statistical tables, reasonings and conclusions, concerning the duration of pregnancy, therapeutics, post mortem appearances, convulsions, change of life, &c.

Dr. Meigs urges with great pertinacity the advantages that must result from the popularization of medicine, than which few errors can be more mischievous. With these views, it is remarkable that Dr. Meigs should use a style which is a polyglot of Greek, Latin, French, German, Italian, and other tongues unknown to the people. Dr. Meigs recommends the making of anatomical drawings and diagrams at the bedside, illustrative of the organs that may be affected by disease, as the vagina, bladder, rectum, and represents that these have convinced the understanding and won the confidence of his patients. Hunter dissuaded medical students from all these pictorial illustrations of anatomy, before having seen the dissected subject, as the impressions made by the former method must be erroneous, and must be effaced or unlearned, before the true anatomy could be fixed in the mind.

That Dr. Meigs may have met with female patients who were polite enough to compensate him for his pains, by assuring him that they comprehended his anatomical and physiological illustrations, is not wonderful, since this would be mutually complimentary to the teacher and his pupils. It is, however, probable that out of the female medical college of Philadelphia, there is not a lady in that city who understands the anatomy and physiology of her womb, excepting perhaps one midwife in five hundred. All such labors at the bedside, and in the school-rooms, must fail in conveying that exact information required to form a sound judgment. Indeed, books on domestic medicine are, for this reason injurious, because they cannot be compre-

hended by the pupil, while they shed an uncertain light, which confuses rather than guides in the hour of peril.

Dr. Meigs' book on Woman is rich in suggestions, in physiology, pathology and therapeutics. In the physiology of the nervous system he marches with the multitude of excito-motor mystics. He appeals to "neurosity" in all his difficulties; says much of the sensory and motory cords, as if they really existed as anatomical facts; he tells you how to treat the motor, and how the sensiferous cords, as if there could be no sin committed in dynamical neurology, nor in anatomical neither, though he admits of "*physical* sin."

Nevertheless, Dr. Meigs' books sell—they ought to sell—being among the very few which make readers think, reason, feel, and act in the right direction.

The moral teachings of Dr. Meigs are pure, elevated and apostolic. It is here that his earnestness gets the advantage over his hard words and exuberant neologies; while passing from "grave to gay, from lively to severe," he touches the chords of the human heart, which make "the whole world akin." Here, too, the English idioms abound. The waywardness, the weakness, and sickness of woman are depicted with whatever dignity these subjects allow. In relating the occurrences in the sick room, which are sometimes comic as well as tragic, his playfulness and quaint humor never sink into vulgar witticism, or sensual hilarity. The tear and the smile are twins without difference.

In his bedside pictures, where danger is imminent, his directions are like his style on these occasions, clear, graphic, and simple. Dr. Meigs' personality often appears in his book, but without intrusion, and without incurring the charge of egotism. The reader wishes to know the inmost thoughts, the private opinions, and the individual experiences of such a gifted man; and, if like Bossuet, he were to apostrophize his own gray hairs, no one would complain.—[Editor.

Part Fourth.

MEDICAL INTELLIGENCE.

Art. I.—*Professor Meigs' Protocol* to Critics.*

PHILADELPHIA, *May* 6, 1854.

Dear Sir:—As it is common to all men to prefer commendation rather than reproof, I could not well avoid a feeling of regret not unmixed with surprise on receiving your note of the 1st inst., which was marked "confidential." I regret in the first place, that your Journal should be the medium by which I am to be assailed, and I was surprised to find that you should use it against a work of mine with which you are apparently not dissatisfied. At least I gather from the tenor of your letter that you do not disapprove of the tract in question.

I am much obliged to you, sir, for the favorable expressions and kind wishes contained in the closing paragraph of your letter, and beg to assure you you are quite correct in supposing that I "make a better use of 'my' time than those who read reviews of themselves after having written the best books extant." I believe there are not a few reviews of my publications that I have not read, and while it is true that I should thankfully receive and strive to improve every truly obvious suggestions in the way of emendation, I confess I have but very little concern in the opinions of angry and unreasonable or incompetent writers of criticism, some, of which, I have found to be beneath contempt for knowledge or temper exhibited by them.

I hope that I have not by any one been charged with the indecency of praising my own writings which have often been the subjects of very sharp comment. I know and admit that my writings have many faults, but I claim that even were I a good writer, I have been too

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* The Editor is responsible for the caption of Dr. Meigs' letter. The Editor's note to Dr. M. was unnecessarily marked confidential. It communicated the isolated fact that the review was asked, not because of the reviewer's dissent, but because of the reviewer's ability. Those who turn to page 837 of the May number of the Journal will see the propriety of the note mentioned. Dr. Meigs had not seen the review, nor did he know who the reviewer was, nor could he have been apprized of any special ground of dissent, at the time he penned his letter, as will appear in the sequel.

busy a man to write with care or with very special regard to the manner of expressing my thoughts. If I should have waited for time to write, I should never have made public a line on medical topics, and yet, as you know, I have written a good deal: perhaps I should have been a wiser man if I had never published a paragraph on medicine! and were I governed only by the opinions of these young gentlemen of our brotherhood who *do* most of the American medical reviews, I should long ago have resolved never thereafter to open my mouth in their presence, but holding my peace, leave them alone in their self-sufficiency.

It is a difficult thing for a man to judge on a question of this kind. Here now are young people in New York and Virginia, and elsewhere, who review not my books only, but me, even when my books are not in the caption; and who inform the public that I cannot write English, and that what I do say is wholly unintelligible, and worse and worse, that what I have written is "unworthy of his (my) eminent position."

I have not claimed to be in an eminent position, saving and excepting only, that I shall ever deem it a fortunate and creditable circumstance that I am sustained by my colleagues of the College, conjointly with whom I have labored as a public instructor of students of medicine, in perfect harmony and concord for a great many years. This I presume, I may without vanity, be allowed to regard as an enviable position, seeing that our medical brethren in the States do send to us a great number of their pupils, which is a certain mark of their confidence and respect.

I know not then what these young gentlemen mean by "his eminent position," unless they be pleased to refer to my writings, which nevertheless they do reprobate, and I might say, truculently condemn and destroy—if they be indeed, destroyed by these public spirited and most learned guardians of our sacred fane!

What would you have me to do Dr. Dowler? Shall a man lay his hand on his mouth and his mouth in the dust, because a * * * writer of squibs shall deem him unworthy of his "eminent station."

I do think that Heaven knows I never wrote for my own sake, but for the sake of my brethren, to whom, I owe an unpayable debt of thanks and grateful respect for their goodness, by me scarcely deserved. I say that I am deeply in debt to my medical countrymen for the some thousands of their students whom they have permitted to hear my public lectures, and for their approbation of my writings, most clearly expressed in the fact that they have taken 15 or 20,000 volumes of them from my booksellers, and are now asking me for others that I am preparing to send them. In fact I have just finished for the binders a new and enlarged edition of my Letters on Woman, which I hope may be found emended as well as augmented, for it was much abused, with the rest.

I repeat then, what ought I to do? Am I to believe the young gentlemen, the sophomore *scollards* [?] who assail me, or may I not

venture rather to rely on the seniors, my brethren, who buy 20,000 volumes of my medical tracts, and ask me for others that are forthcoming. I have too good an opinion of American doctors to think they would purchase so large a library that has in it neither English nor common sense.

As to the particular tract which you tell me is to be reviewed in your forthcoming number, I will be so weak as to confess, I should be sorry to find it a failure, not on account of the personal mortification merely, but because I have good reasons to believe it contains much sound and wholesome instruction, well fitted to aid the young and inexperienced brethren in a difficult department of clinics; wherein many, nay the majority of us, commit the most scandalous blunders and do the most blameable mal-practice.

I hope I have not the least desire to rescue the volume, however much I confide in the principles and methods which I have inculcated, from a condign condemnation. Yet I confess it is hard for me to understand how it should be, that, while supposing myself to be very intimately acquainted with the history and bibliography of that particular subject, I should make the grave mistake of regarding the book as not only a useful but an original and novel exposition of these matters, if it should in the end prove to be not worth a rush. Assuredly considering the place I have long occupied as a practitioner and teacher, the duty I owed to my brethren of being a man of studious habits as well as a careful observer of diseases and results of treatment, I ought by this time to have learned something worthy of being told to others. Still, your reviewer may be a person far more variously and accurately informed than I, and so, prove himself quite able to show that I have learned nothing in cases that have attracted much of my attention for many years. Let him in that case, cut my book into shreds if he will; I shall endeavor to think no evil of him on account of his evil intent towards me, or my book rather. If he rails at us, much happiness and self-gratulation may he find in his railing. I shall endeavor to find contentment, nevertheless, and to that end perhaps I might do well to read in the Bible:—In the 2d Chapter of II Book of Kings, I shall find a story concerning the prophet Elisha; he was old and well stricken in years, and so am I; he had a bald head, and so have I; he went on his way in the world, and so do I; he met angry and naughty boys, so have I; they scorned his gray hairs and hooted at his bald crown; probably they thought him unfit for his "eminent station," and they cried out upon him, "go up thou bald head, go up thou bald head." The prophet turned and "cursed them," so do not I; and the Lord sent two she bears out of the mountain and "they tare forty-and-two of those children that day." I am very sorry for the poor dear little Jew boys that were torn, and I hope my reviewers may keep clear of all such, and other vermin. And I even go so far in humanity as to trust, humbly, they will not feel themselves hurt by the reflection

that their brethren and mine have bought some 20,000 volumes of medical works from a writer whom they so greatly disapprove.

I heartily reciprocate your kind wishes for my welfare, and while I regret you should use your Journal to do me hurt and damage, I am not the less an admirer of your talents and industry, and I rest with respectful consideration,

Your serv't, CH. D. MEIGS.

Dr. B. Dowler, New Orleans.

P. S.—Were it not that you have marked your note to me "confidential," I would invite you to use your pleasure as to the insertion of this into a number of your Journal—not that I am desirous to defend my book against criticism, but only in the view of saying what I believe to be quite true—that I have reason to look upon my writings with less doubt as to their usefulness, on account of the undeniable fact that they have met with considerable favor at the hands of the medical public in our country. Yet after all, perhaps your reviewer may have been pleased to say nothing that I should not be willing to agree to. In that case I should have no answer to make.

C. D. M.

Art. II.—*Experiments showing the Contagious Properties of the Splenic Blood of Sheep affected with Charbon.* Translated for this Journal, from the Revue de Thérapeutique Médico-Chirurgicale, of May 15th, 1854. By C. R. NUTT, M. D., of New Orleans.

The following experiments, reported in the Academy of Medicine and in the Institute, have produced a great sensation, not only in the agricultural world but in the highest circles of the medical *corps*. M. Godeau says: About one year since I made known to the Society the labors of certain physicians and veterinary surgeons in the Department of Chartres, in relation to the disease known as charbon, among our domestic animals. A commission was appointed, of which I was chairman, for the purpose of laying before the public the result of their joint labors upon the investigation of the causes of "charbon," and to ascertain if it belonged to the class of septic poisons.

The cause of the scourge which decimated the flocks of Beauce, Brie, Picardie, and a large portion of the wool-growing districts, was considered by Prof. Delajond "to be a sanguine plethora, produced by too nutritious grasses, resulting in the too great a proportion of globules, fibrin and albumen of the blood." According to this view of the disease, it was not considered contagious or communicable to

man or animals. From its rapid progress, frightful ravages, simultaneous existence in the same localities in which the malignant pustule of man was found to occur with individuals tending the flocks, or who had handled those diseased, or inoculated from the blood or pus taken from the spleen, the physicians and veterinary surgeons of Beauce were at once led to suspect its contagious nature.

The experiments instituted for this purpose have shown that this disease of sheep, the charbon of the horse, and of the cattle, is caused by a septic poison, and is communicable to both man and animals. With a view of ascertaining the identity of the malady of Chartres with that of Beauce, a commission, of which M. Godeau was chairman, was ordered to report, and also upon the improved hygienic treatment.

Experiment 1.—On the 29th of July, at 11 o'clock, A. M., a sheep and two rabbits were inoculated with the blood taken from the spleen of a sheep, which, with seven others, had died the day before on the estate of Mme. Pocheron. Seventeen hours after death the inoculation was made on the inside of the thighs, by two or more punctures. On the 1st of August, the sheep died, at 4 o'clock, A. M., sixty-five hours after inoculation; and the rabbits on the 31st of July, at 9, P. M., fifty-eight hours after inoculation. Autopsy of the sheep:—Petechiæ on the surface of conjunctivæ; spleen exhibited diffused softening throughout its structure, yielding like pap on pressure; mesentric glands softened; throat highly injected; serous fluid of a reddish cast found in the abdomen, &c. The autopsy of the rabbits showed no appreciable lesion except in the blood, which was thick and turbid.

Experiment 2.—On the 1st of August, immediately after the autopsy of the sheep and rabbits, at 10 o'clock, A. M., a sheep and two other rabbits were inoculated as before, with the blood of those which had died from inoculation. The sheep died on the 3d of August, at 5, A. M., forty-three hours after inoculation. The rabbits died the same day, two hours later than the sheep. Autopsy showed the same lesions as in the former cases. A number of similar experiments were tried upon sheep and rabbits with slightly varying results. On the 3d of August, the blood from the spleen of a sheep was used to inoculate a young and vigorous mare. The punctures were made on the inner side of the thigh. The mare died on the 7th of August.

Autopsy of the mare :—Ædematous infiltration diffused throughout the sub-cutaneous cellular tissue, with effusion in the abdomen of a small quantity of sero-sanguineous fluid ; softening of the mesentric glands and spleen.

Experiment 6.—A she ass was inoculated with pus taken from the spleen of a sheep which had died at Mme. Mapé's. The punctures were made on the inner side of the thigh, and on the left side of the chest, near the medium line. The ass was inoculated on the 18th, and died on the 22d of September. It presented an enormous abcess on the left side, having all the characteristics of charbon. The autopsy was the same in appearance as that of the mare. From an incision made in the tumor, which presented a yellow infiltration, an infectious odor escaped, produced by the disengagement of confined gas.

Experiment 7.—A sheep and two rabbits were inoculated with the blood of the ass, on the 23d of September, at 6 o'clock, P. M. The sheep died on the 25th of September, at 11 o'clock, P. M., fifty-three hour after inoculation; and the two rabbits on the 26th of September, at 5 o'clock, A. M., six hours later.

Experiment 10—On the 7th of August, a cow died on the estate of Bijou, whither, some days previous the flocks of Mme. Pocheron, decimated, as we here stated, were brought. One sheep and two rabbits were inoculated with the blood of this cow. On the 8th of August at 7 P. M., the inoculation was performed. On the 10th, the rabbits died at 5 o'clock, A. M., thirty-three hours after inoculation. The sheep died the same day, at 3 o'clock, P. M., forty-four hours after inoculation. It is a fact worthy of remark—the simultaneous death of the rabbits in each experiment, two minutes scarely intervening.

A summary of the experiments shows that thirty-five inoculations with the blood of the spleen, in the first and second degree, were practiced, of which nine were upon sheep, twenty upon rabbits, one upon a mare, another upon ass, and four upon chickens. All these animals perished with the exception of two chickens inoculated from the rabbits. The experiments are not as numerous as we desired, in consequence of the small number of flocks, compared with years past, attacked this summer, though the disease was unusually fatal. In the prosecution of these experiments all means were used to procure animals isolated from the disease, and those brought from abroad were

the subjects of inoculation. The following facts, concerning the disease, as it prevailed at Mme. Pocheron's, show very clearly its communicability to man. On the 28th of February, at the request of Mme. Pocheron, I visited her farm, where, upon examination, I found that eight of her sheep had died of charbon. On the same day, a woman on the farm had introduced her finger into the rectum of many of the sheep, for the purpose, as she stated, of disengaging the confined air, which was killing them. I advised Mme. Pocheron to have her flocks moved to Bijou, to low marshy grounds to pasture, which she accordingly did the following morning. The Juncaceæ and Equeselaceæ (rushes and scouring rushes) abounded in these grounds, and other aqueous plants, very different from the valley of Subdray, where the soil was dry and calcareous, affording a rich aromatic and highly nutritious pasturage. This was done in accordance with the views expressed by M. Delajond of the cause of the disease. No benefit, however, was experienced by the change, and as the mortality continued to decimate the flock, Mme. Pocheron, frightened by its ravages, called another veterinary surgeon to my aid, who treated the whole flock without mitigating, in the slightest degree, the violence of the disease. The per diem mortality was fifteen to twenty, when the flock, on the 2d of August, was driven back to Subdray. In despite of medical treatment, and the removal of the flock, which consisted of four hundred, the disease continued till the entire flock perished. On the 2d of August, the woman who had applied her finger up the rectum of several sheep, as before stated, for the purpose of relieving them, fell sick and complained of great prostration, (said she was foundered,) continued to grow worse in spite of medical treatment—the charbon fever manifested itself, and only terminated with her death, on the night of the 3d and 4th of August.

An infant, aged three years, which had amused itself with steeping its hands in the blood of the sheep, which had been skinned, was attacked on the same day (2d August) with malignant pustule on its upper lip. Grave symptoms supervened, but it was relieved by repeated applications of the actual cautery. The following Saturday, August 6th, a shepherdess who skinned many of the sheep was soon after attacked with a milignant pustule on the back of the left hand, accompanied with grave and alarming symptoms, when she too was

relieved by the use of the cautery and the most active treatment, by my colleague, M. Gerin.

On the 11th of August, another shepherdess was attacked with malignant pustule on the chin, produced by the spattering of blood, two days previous, whilst aiding me in opening the sheep. This was likewise relieved by repeated application of the cautery, by M. Gerin. Whilst at the Subdray the contagious nature of charbon, from animals to man, was thus strikingly illustrated, at Bijou, whither the flocks had been removed by my advice, the disease appeared in a horse and a cow, both of which died, the one on the 11th, and the other on the 7th of August. The blood of this cow afforded experiment No. 10, as above related. In the history of this malady, it appears that for many years past, its ravages here have not been confined to sheep, but have extended to cattle and horses, with terrible mortality.

In 1846, M. Adolphe Mignon lost one-fourth out of eight hundred sheep; seven horses out of twenty. In 1850, M. du Troncay, at Prieul, lost three hundred and twenty out of three hundred and fifty, five horses out of seven, and one bullock.

These facts, resting upon the most satisfactory testimony, go to prove in the most conclusive manner, that the physicians and veterinary surgeons of Beauce have shown that the malady among the sheep is identical with the charbon of the horse, malignant pustule of man, and the malady of cattle. That each and all these are the result of a septic poison, capable of being communicated from animals to man, and from man to animals. Every variety of treatment was exhausted in combatting this disease, without results sufficiently satisfactory to offer any mode of cure to the public. We trust, however, that further investigations into the nature of this disease will enable us to mitigate, if not to cure this terrible malady. GODEAU.

Art. III.—*Obituary Notice—Prof. H. S. Patterson:* By H. J. RICHARDS, M. D.

With feelings of deep regret we announce to the Southern medical fraternity the death of Dr. H. S. Patterson, of Philadelphia; late Emeritus, Professor of Materia Medica and Therapeutics, in the

medical department of Pennsylvania College; Fellow of the College of Physicians; and Recording Secretary of the Medical Society of the State of Pennsylvania, &c.

Dr. Patterson's health had been in a declining state for the past three years. A voyage to Europe in the summer of 1852, so greatly restored his powers as to permit himself and his friends to hope, for a brief period, that the progress of his malady had been stayed; but the resumption of his arduous professional and literary labors overtasked his physical strength, and he was compelled to abandon the active exercise of his profession. In July, of last year, he resigned the chair of Materia Medica in the institution with which he had been connected, in the conviction that he was taking a final leave of the scenes and pursuits of his daily life. Nevertheless, in his resignation of its hopes and prospects; amid the distraction of pain, and his anxiety for a young and cherished family soon to be deprived of their natural protector, his ceaselessly active and disciplined mind was ever projecting and executing. The biographical memoir of the late Dr. S. G. Morton, prefixed to the late work of Gliddon and Nott, was undertaken in the scarcely formed hope of his ability to complete his "labor of love." The revison of a large portion of the proof-sheets from the stereotyper, as stated in Mr. Gliddon's preface to that work, also occupied and beguiled a portion of the weary and monotonous hours of his sick chamber. In a letter to the writer, dated in February, of this year, he stated that he was occupying himself in the intervals of his paroxysms of acute suffering in collecting and arranging materials for a work on the Cabala. The fresh page that every day of life presented was never left blank; until upon the last leaf of his life's record, is inscribed the melancholy "FINIS"—it is ended.

Dr. Patterson's position as a practitioner and teacher of medicine was prominent in that city which, probably, presents the finest array of medical talent, and may be regarded as the medical metropolis of America. As a practitioner he evinced those practical qualities which are to be attributed to long and careful clinical observation, combined with a strictly scientific course of study. As a teacher he was the reliable exponent of current medical literature and opinions: and the steady opponent of empiricism in all its phases. His lectures

were frequently revised, and enriched from the French and German schools of pharmacy, whose literature he regularly perused.

Scarcely a month has elapsed since the subject of our brief notice made his last living sign to the world. From a bed of sickness and suffering he uttered his testimony—since hallowed by his death—to the worth of a departed contemporary. The friend and biographer of MORTON, is now entitled to the same friendly tribute, which he so cheerfully and eloquently recorded to the memory of his fellow and co-laborer.

We cannot doubt, that from among the many friends to whom he was in life endeared—by his goodness and amiability of disposition, no less than by the worth and more shining qualities to which we have briefly adverted—a willing and competent hand will undertake the dicharge of this grateful duty.

To the above graceful tribute to departed worth, the editor subjoins the following memoranda, abridged from the June number of the New Jersey Medical Reporter:

Dr. Patterson was born in Philadelphia, Aug. 15th, 1815; received the degree of M. D. in the University of Pennsylvania, in 1839, and the honorary degree of A. M. from Newark College, in 1843; in the same year he became professor of Materia Medica, and acting professor of chemistry in the Pennsylvania College of Medicine; in 1846, physician-in-chief of the Blockley Alms-house. Owing to indisposition he resigned his professorship, traveled through Europe in 1852, in pursuit of health and scientific information. The next winter he spent in Florida, with little improvement in his health; whence he returned to the bosom of his family, and sunk into his bed where "he wrote the inimitable biography of the late Samuel George Morton, and two critical notices of the 'Types of Mankind.'"

One of these was published in the last number of this Journal. All his previous bibliographical notices for the "Reporter," were written in his chamber. We will view him at this point for a moment, where we now are—in his chamber. We saw him, wasted by disease, and yet firm in mental power. He spoke to us of his sufferings, as a victor speaks of conquered enemies: they may have been strong, but the mind had soared above them. His words were few but they *spoke*. Some words sound only; *his*, uttered meaning. As he entered the stream that separates the noise of time from the stillness of eternity, and felt its waters to chill his blood, and its cold waves to approach

the centre of life, the spirit still seemed to hold its gaze upon an unending existence, and with a firm and steady, yet quiet advance, he passed away, and left these words of the psalmist to answer the question—How did he die? "Thy rod and Thy staff, they comfort me." Thy rod for discipline, Thy staff for support; they both comfort me.

A word about his acquirements. In the profession of his choice, Dr. P. was thoroughly versed. Its history was familiar to him; and in discussing the doctrines of those who have given to our profession its *caste*, in times, ancient and modern, in all countries, he seemed quite at ease, both in social intercourse and in the lecture-room. His knowledge of the Latin and Greek languages enabled him to cull from the past, much that was embellished by his own rich style, in the various literary and scientific productions which have emanated from his study. The Hebrew he knew so familiarly as to be able to read with a critical eye, its translations; and to dwell upon the imagery of this virgin tongue with delight and profit. In the modern languages he was quite a proficient: the French, German and Italian, he read and spoke with ease, and had so acquainted himself with the classical poetry of the latter, as to be always ready to render quotations off-hand, but without ostentatious effort at display. In Egyptian philology he found much to interest his taste for studying oriental symbolisms, and had his life been prolonged, he would probably have published his notes on the subject. In the practice of his profession he was gentle, kind and skillful.

His Disease.—When a prominent member of our profession passes from earth, it is expected that his disease should be known to his professional friends, and that the post-mortem revelations made by the scalpel, should be the common property of the profession. With a few exceptions, we believe this is usually the case, and we admire the devotion to science, manifested by domestic and personal associates who cheerfully allow the profession to use these means for advancing the common interests of our race. In presenting the following statement, we do not write from any examination into the diseases of Dr. Patterson, made by ourself, but from information obtained from his physician, friend and colleague, Prof. Darrach, of this city. In April of last year, (1853,) Dr. Darrach made a critical examination of his case, and gave to his family and friends the following diagnosis, which was repeated before the college class, in an introductory lecture, delivered in October last, as a reason for Dr. P.'s resignation from the faculty. He declared his retirement to be owing to "pleuritic and cardial sequelæ of rheumatismal influenza, which, however much it embarasses the mechanism of the arterial circulation, and emaciates and weakens, has not lessened the pulmonary function, nor in the least abated his emotions, passions and intellect." Dr. P., was exposed during a stormy night in 1844, and "contracted an influenza which established a liability to attacks of neuralgic rheumatism." There followed upon this an "insidious, painless, chronic pleurisy, which plastered down the right lung; and, subsequently, a similar persistent

transmutation was made upon the valves of the heart, to embarrass their action." These sequelæ, according to Dr. D., "occasioned an atrophy of the right, and a vicarious enlargement of the left lung, which displaced the heart to the middle sternal region of the right side of the chest.

The Autopsy.—Thirty hours after death, the emaciated frame was exposed for dissection, and the following appearances are reported. (The cranium was not examined): "*Thorax*—heart, moderately enlarged, displaced towards the right side, extending one and a half inches beyond the articulation of the right ribs, with the cartilages—pericardium healthy, about two ounces of fluid in the cavity—right cavities; auricle, filled with a very dark colored heart clot; endocardium, normal; walls thinner than normal—ventricle, walls thinner than normal—tricuspid valves not affected—clot clinging to chordæ tendineæ—semilunar valves of pulmonary artery red, thicker than normal, end fleshy to feel—long clot in the pulmonary artery—left cavities; auricle empty—endocardium normal, except mitral valves, which were thickened with deposits; the deposits being in lumps or beads along the edges, and causing the valves to curl upon themselves. Ventricles, nothing peculiar. Valves of aorta, red, thickened and fleshy to feel; the redness of these valves, together with those of the pulmonary artery, could not be washed off by water, and they presented a strong contrast with the color of the rest of the lining membrane of their respective arteries.* This condition was more morbid in the valves of the aorta than in those of the pulmonary artery.

Lungs—right firmly bound down by adhesions; being almost one-fourth the natural size. The lateral diameter greatly diminished: the vertical slightly. At the apex, the air vesicles were very much dilated, and few in number; the great portion of lining tissue being absorbed. No evidence or sign of tubercle. Lung did not crepitate, the lower portion being tough and carnified. It contained air sufficiently to float it in water. At the postero-inferior portion, was a patch of lardaceous membrane, of about the sixth of an inch in thickness. Left lung adherent, but by no means as firmly as was the right; hypertrophied and crepitant throughout—much congested and œdematous. No evidence of tubercle in any part of the organ. The adhesions of both lungs were so firm, that much of the costal pleura was torn out in the attempt to liberate them. Bronchial tubes—fine injection—fibrous appearance internally, with hypertrophy of follicles.

April 28th, 1854.

* How true soever it may be, in this particular case, that the color mentioned was altogether morbid, it should be constantly borne in mind, that very soon after death, as well as at later periods, the membranes of the heart and its appendages, and, indeed, almost all the tissues and organs, may form causes purely physical—as endosmosis, exosmosis, and gravitation, according to position—acquire an intense color, and become literally dyed like a piece of cloth. Washing will not remove this color, in many instances. The bowels become black or red throughout their extent from effusions of black vomit or blood—in the same way the bile often discolors or dyes surrounding tissues. The same causes which tend to efface morbid redness, especially where the post-mortem examination has been delayed, as in the above case. Another cause, which often tends to obliterate vascularity, turgidity and redness, is that of vital capillary action, which persists after ordinary death.—[*Editor N. O. Med. Jour.*

Art. IV.—Prof. SANFORD.—*Lithotomy.—The First Case of Lithotomy performed on the Male, in the State of Iowa.*

Joseph Harding, aged thirteen, who had been treated five or six years for stricture of the urethra, having been sounded, was found to have a calculus, which Professor J. F. Sandford, M. D., of Keokuk, an accomplished surgeon and scholar, removed, by the lateral method, while the patient was under the influence of chloroform. The stone weighed half an ounce. The patient soon recovered. The case is reported in the March number of the Western Medico-Chirurgical Journal.

This case has an interest and is worthy of record in two respects: first, there were some symptoms which are almost invariably present when vesical calculi exist, which were absent here; and, second, it is he *first operation for stone upon the male,* ever performed in the State of Iowa. This latter consideration will give it an interest in the surgical annals of our commonwealth in the minds of some who may not see, in the recital of the symptoms and surgery of the case, any thing new or out of the range of former observation.

Art. V.—*The Mortality of the City of Memphis, Tenn.*

Prof. Quintard's able report on the health of Memphis for 1853, published by order of the City Council, affords the following notice of

Yellow Fever.—It will be perceived that three cases of this disease, are recorded on the books. The first two cases had been exposed to the causes of the disease in New Orleans, and the third case, that which occurred in September, was returned by the sexton as bilious fever—as we were informed by the secretary of the board of health—but it was a case so like yellow fever that the secretary in his weekly publication of the deaths, reported it as such. There was a good deal of discussion at the time, as to the exact character of the disease. The occupation of the individual was such that he was frequently exposed to contact with persons from the affected districts. No quarantine regulations were adopted by our municipal authorities, though boats were landing the sick and dying at our wharf nearly every day, from New Orleans and the towns below us. Sixty-two cases were carried from the landing to the hospital, many of them in a dying condition. The first case admitted by Dr. Smith, hospital physician, was on the 24th of July. The last case recorded on the hospital books, was on the 28th November. Of the sixty-two cases

at the hospital, from boats, thirty-six proved fatal; and of this number, twenty died within twenty-four hours after admission.

Prof. Quintard in dissenting from one reputed cause of yellow fever at Natchez—namely, the grading operations, long pursued in that city—says:

Natchez has time and again been visited by epidemic yellow fever, and notwithstanding a rigid system of quarantine, it, with nearly all of the towns below us, was scourged by the pestilence during the last summmer. There have been and are causes for this prevalence of the disease in Natchez, which do not exist in Memphis. Nor can this be traced to the grading. In 1851, the city of Memphis paid, for street-grading, $27,806 99; in 1852, $40,519 25; in 1853, 40,931 92. In 1851, the mortality of the city was 717; in 1852, 705; in 1853, 412. It is apparent, therefore, that notwithstanding the work of grading was vigorously carried on during 1853, the mortality greatly diminished.

Prof. Q. estimates the mortality of the city of Memphis in 1853, at less than two per cent., in a population of more than 15,000: the deaths having been 412.—*Report &c.*, 1854.

Art. VI.—*The American Medical Association.*

The American Medical Association, which met on the 2d of May 1854, at St. Louis, adjourned after a session of three days. From the Iowa Medical Journal, the following items have been gleaned concerning the proceedings of that body:

Dr. Pope, chairman of the Committee on Prize Essays and Volunteer Communications, now made the following report:

Mr. President:—The Committee on Prize Essays and Voluntee Communications, respectfully report that the essays submitted to thei consideration were nine in number, of which one was presented as volunteer communication. The committee have carefully examine the whole of these essays, and bestowed upon them the attention whic a sense of the importance of the duty assigned them imposed. The feel free to say that some of these essays possess undoubted meri both in matter and style, and they admit in them evidence of hig scientific attainment as well as a familarity with the graces of comp sition. But whilst cheerfully according these claims to their author the committee have preferred to be governed in the choice, by co siderations of originality and practical import, rather than of me theoretic speculation, however finely portrayed. The committee ha consequently concluded to award but a single prize. The essay selec

ed is entitled "An Essay on a New Method of Treating Ununited Fractures and certain Deformities of the Osseous System." It bears a motto in French.

If it pleases the Association, I will now break the seal of the packet superscribed by the same motto, and declare the name of the successful competitor.

Dr. Pope then broke the seal, and announced the name of Professor Brainard, of Chicago.

Dr. McPheeters moved that Prof. Daniel Brainard take the stand, and give the Association an abstract of his new mode of treating fractures, &c.; which motion was carried, and Prof. Brainard accordingly came forward, and in an able manner gave the requisite information.

Dr. Mussey, of Cincinnati, made a motion to suspend the order of regular business, to allow Dr. Linton of St, Louis, to express his views with regard to the pathology of the yellow fever. A suspension of business was made for this purpose.

He expressed his views very clearly and at some length on this subject. He advocated the idea that vegetable decomposition was not necessary to the production of the autumnal diseases of this country. He considered yellow fever nothing more than an aggravated type of bilious fever, caused by the retention of hydro-carbonaceous substances in the blood. In other words, the agencies producing yellow fever were Northern blood subject to the heat of Southern latitudes.

A motion was made and carried, that Dr. Linton be requested to draw up the substance of his remarks, to be presented to the committee on publication.

The following officers were elected for the ensuing year:

For President—Charles A. Pope, of Missouri. *Vice-Presidents*—E. D. Fenner, of Louisiana; N. S. Davis, of Illinois; William Wragg, of South Carolina; John Green, of Massachusetts. *Secretaries*—Edwin S. Lemoine, of Missouri; Francis West, of Pennsylvania. *Treasurer*—D. Francis Condie, of Pennsylvania.

Art. VII.—*Health of Philadelphia.*

At a meeting of the College of Physicians of Philadelphia, March 1, 1854, Dr. Ruschenberger presented the Annual Report on Meteorology and Epidemics, for the year 1853.

During the year 1853, the thermometer fluctuated between 9° and 94° of Fahrenheit's scale: the range being 85°. For the year 1851, the range was 87°; and, for the year 1852, it was 96°.

The mean temperature of the year was 54.56° F., which is 2.52° F. above the average mean of sixty years.

The quantity of rain and snow which fell during the year was 40.657 inches, which is 3.103 inches less than the yearly average.

The mean temperature of the year 1853 was 54.56° F., which is 2.52° F., above the standard mean.

The total number of deaths in the city and liberties of Philadelphia, according to the reports of the Board of Health, for the year 1853, is 9,711, which is 444 less than the aggregate mortality for the preceding year. Deducting the number of stillborn (571), the total mortality is 9,140; or, 515 less than for the year 1852.

Including the stillborn, 2,722 died within the first year, 968 between one and two years, and 1,018 between two and five years. Excluding the stillborn, 4,237, or, 46.33 per cent. of the mortality was of children under five years of age.

Between the 19th of July and 7th of October, there were 170 cases of malignant or yellow fever; of which, according to the valuable reports of Dr. Jewell, 128, or, 75 per cent. died.

The total number of deaths from every species of fever named in the reports of the Board of Health for the year 1853, was 1,005, or, at the rate of 10.99 per cent. of the total mortality.

The number of deaths from scarlet fever was 384, or, at the rate of 4.20 per cent. The number of deaths from this disease during the past four years has been at an average rate of 4.37 per cent. of the whole mortality.

Art. VIII.—*Human Petrifactions.*

Dr. Porcher, one of the editors of the Charleston Medical Journal, late a traveller on the continent of Europe, in a communication to that Journal, dated at Florence, Italy, says:

The celebrated petrifactions of human flesh, by Segato, are deposited here. The art died with him. I saw a table made with the different portions of the human body in mosaic—muscle, heart, liver, kidney, &c., quite easily distinguished. The corners are garnished with hardened brain, and a bit of kidney or liver decorates the centre. Another preparation was the entire head and bust of a young girl, with the skin rather whiter than natural, but its structure perfectly preserved.

Art. IX.—*Excito-Motory System of* Dr. M. Hall.

The following conclusions of Dr. Davey, "On the Excito-Motory System of Marshall Hall," are copied from the June number of the Virginia Medical and Surgical Journal:

That the excito-motory phenomena, as explained by Dr. Marshall

Hall, rests on a basis which even *he*, up to this time, has failed to recognize; that the reflex actions, properly so called, are nothing more than an amplification of the ordinary spinal functions. He denies the correctness of Dr. Hall's experiments; that the more complicated anatomy of the spinal cord, suggested by Dr. Hall, and sought for by Mr. Grainger, has not yet been realized: says that there is no necessity for any distinct division of the nervous (spinal) system, for the production of the excito-motory phenomena: that the exercise of the functions of the spinal cord is due to the peculiar influence of the ganglionic nervous system, exerted on its organism, &c., &c.

Art. X.—*Cholera.*

As the last sheet of this Journal was in the hands of the printer—June 28th—letters and journals both foreign and domestic, received at the office of the editor, seem to warrant the conclusion that there exist cholera zones to the North and Northeast, and to the South, and Southwest of New Orleans. The cases of cholera in the civil hospitals of Paris for twenty days, ending on the 3d of May last, were, for each successive day, as follows: 4, 7, 9, 11, 11, 17, 16, 21, 19, 24, 41, 76, 64, 45, 48, 26, 21, 23, 28, 18, 25; the number treated in the civil hospitals from Nov. 1, 1853, to May 3, 1854, amounted to 1,782: discharged, 661—died, 897—remaining, under treatment, 224. The 10th and the 11th arrondissements, which have suffered most from cholera, according to the latest accounts, are reckoned among the richest and most salubrious places, especially in the neighborhood of the government offices, (le quartier des ministères,) where several distinguished personages (hauts personnages) have died, and where the disease has been most prevalent. The oscillations of the disease in the military hospitals were analogous with those already enumerated in the statistics of the civil hospitals.

In several of the principal towns in Scotland, cholera has recently appeared. It seems, that in numerous places, as Boston, New York, Nashville, and the cities of Mexico and Kingston, this malady is assuming an epidemic character. From dispatches received in New Orleans, it appears that for forty-eight hours, ending on June 23d, eighteen deaths from cholera had occurred in Nashville, Tennessee. Prof. Quintard, of the Memphis Medical College, in a letter dated June 3d, at the Poplar Springs in Randolph county, Tennessee, says: "I have had about thirty cases of cholera on the plantation during the last three weeks. It is prevailing all through this section of country."

Although the sanitary condition of New Orleans has been favorable

since the close of the epidemic of 1853, and the more so as the summer advances, yet, in the winter, isolated cases of cholera occurred; they still occasionally happen, while the general mortality is rather declining than augmenting.

From the published reports of interments in the city of New Orleans for the four weeks ending with the 25th of June, the mortality has declined as follows: the week ending June 4th, 194; 11th, 184; 18th, 189; 25th, 170.

Forewarned, forearmed! Observe cleanliness! Unhealthy articles of diet, unripe fruit, exposure to the weather, and immoderate labor should be avoided at all times, but more particularly when cholera prevails. Take no nostrums—take a doctor. Medicines, miscalled cholera *preventives!* are cholera *incentives!*

Art. XI.—*A Voice from the Office of the New Orleans Medical Journal.*

Contributors will oblige the Editor by sending their communications to this office two months in advance of the time of publication, in order that they may appear in the first part of the Journal,—the printing of which begins immediately after the last issue. The Editor contrary to his wishes has been compelled to furnish the first articles in the present number, owing to the late period at which the original communications were received.

The labors of the Editor are sufficiently onerous without writing articles for the Journal, which is intended to be a medium for diffusing the researches of other and abler laborers in the field of science—a field comparatively new, varied, peculiar, and of vast expansions. In all the medical regions of the civilized world, from the Seine to the Ganges—from the more Northern States of the Union to the Scandinavian Peninsula, the medical mind has been, and still is, active—thousands of the richest contributions have been made to the common treasury of science. Is it so in this great region washed by the Gulf of Mexico? Are the medical men of this region willing to trail eternally in the rear, skulking from their duty? Aye, their duty! They have reaped the benefits of the labors of others. Are they not bound to make some return? If Southern physicians will neither write books, nor essays for Medical Journals, what proof will there be that the Southern medical mind is superior to or even equal with that of the North? Will a slavish dependance on the great masters of science in other latitudes win their respect? Never! The physicians of the South must be their *equals*, not their *apes*. Let each do his utmost to advance medical science, and our brethren of other States and climes will not withhold their respect and gratitude.—EDITOR.

THE NEW ORLEANS MEDICAL AND SURGICAL JOURNAL.

VOL. XI. NOVEMBER, 1854. No. 3.

EDITED BY

BENNET DOWLER, M. D.,

Corresponding Member of the Academy of Natural Sciences of Philadelphia; Fellow and Honorary Vice President of the Medico-Chirurgical College of the same city; Fellow of the Medical Society of Virginia; Corresponding Member of the Society of Statistical Medicine of New York; Fellow and a Founder of the Royal Society of Northern Antiquaries of Copenhagen, &c. &c.

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BOOKS AND PAMPHLETS RECEIVED.

I.—*Auscultation and Percussion:* By Dr. Joseph Skoda. Translated from the Fourth Edition, by W. O. Markham, M. D., Assistant Physician to St. Mary's Hospital. Philadelphia: Lindsay & Blakiston, 1854, pp. 380. From Mr. T. L. White, Bookseller, 105 Canal street, and Mr. J. B. Steel, Bookseller, 60 Camp street.

II.—*Healthy Skin:* A Popular Treatise on the Skin and Hair; their Preservation and Management: By Erasmus Wilson, F. R. S., Second American, from the Fourth and Revised London Edition: with illustrations, pp. 291. Philadelphia: Blanchard & Lea, 1854. From Mr. J. C. Morgan, Bookseller, Exchange Place.

III.—*Principal Forms of the Skeleton, and Structure of the Teeth:* By Professor R. Owen, F. R. S., pp. 329. Philadelphia: Blanchard & Lea, 1854. From Mr. T. L. White, Bookseller, 105 Canal street.

IV.—*Principles of Animal and Vegetable Physiology:* A Popular Treatise on the Phenomena and Functions of Organic Life. To which is prefixed an Essay on the Great Departments of Human Knowledge, &c. By J. Stevenson Bushman, M. D., 102 illustrations; pp. 234. Philadelphia: Blanchard & Lea, 1854. From Mr. T. L. White, Bookseller, 105 Canal street.

V.—*Principles of Comparative Physiology:* Intended as an Introduction to the Study of Human Physiology, and as a Guide to the Philosophical Pursuit of Natural History: By William B. Carpenter, M. D., F. R. S.; Author of "Principles of Human Physiology," &c. New and much improved Edition; 309 illustrations, pp. 752. Philadelphia: Blanchard & Lea, 1854. From Mr. J. B. Steel, Bookseller, 60 Camp street.

VI.—*Report of the Sanitary Committee of the Board of Health, upon* the subject of Slaughter-Houses and Soap-Boiling Establishments in

Cities—their Effects upon Public Health; together with the Location of Cow-Stables in Cities, and the Effects of Feeding of Still-Slops upon the Milk, &c. Made to the Board, August 22, 1854. Pp. 15. Philadelphia.

VII.—*Poisoned Wounds:* Their Distinctive Features, Classification, with Remarks upon the Classes, and a Special Treatise upon the Nature and Treatment of the Wounds resulting from the Bites of Venomous Reptiles. Experiments, &c. Being a Report of a Committee to the Medical Association of Missouri: By A. F. Jeter, M. D., of Palmyra, Mo. Pp. 32. Quincy, 1854.

VIII.—*Observations on some of the Remedial Properties of Simaba* Cedron, and on its Employment in Intermittent Fever: By S. S. Purple, M. D. Pp. 16. New York, 1854.

IX.—*Charge to the Graduates of Jefferson Medical College of Philadelphia*: Delivered March 11, 1854, by Professor Robley Dunglison. Pp. 15.

X.—*Illustrated Catalogue of Blanchard & Lea's Medical, Surgical and* Scientific Publications. Pp. 64.

XI.—*Elkoplasty, or Anaplasty applied to the Treatment of Old Ulcers.* Also, A New Mode of Treatment for Delayed or Non-Union of a Fractured Humerus: By Frank H. Hamilton, A. M., M. D., Professor of Surgery in the Medical Department of the University of Buffalo, Surgeon to the Buffalo Hospital of Sisters of Charity. Pp. 19. New York, 1854.

XII.—*A Monograph on the Fœtal Circulation:* With three illustrations on wood: By E. R. Peaslee, A. M., M. D., Professor of Anatomy in Dartmouth College and in the New York Medical College, and of Surgery in the Medical School of Maine. From the American Medical Monthly, May, 1854. Pp. 26. New York.

XIII.—*Report of the Special Committee of the Board of Regents of the* Smithsonian Institution. Pp. 21. 1854.

XIV.—*Quarterly Summary of the Transactions of the College of Physi*cians of Philadelphia. From May 3, 1854, to July 19, 1854, inclusive.

XV.—*Medicinal Properties of Bailey's Spring, Lauderdale County, Ala.*, and Cooper's Well, Hinds County, Miss. By S. C. Farrar, M. D., of Jackson, Miss. Pp. 24.

XVI.—*Observations on the Asiatic Cholera, as it appeared in Cincinnati*, in 1849–'50. By THOMAS CARROLL, M. D., reprinted from the Western Lancet for June, 1854. Pp. 75.

XVII.—*Difficult Labors and their Treatment*: By M. B. WRIGHT, M. D., of Cincinnati. For which a Gold Medal was awarded by the Ohio State Medical Society. Pp. 32. Cincinnati, 1854.

XVIII.—*Proceedings of the New Orleans Academy of Sciences*, 1854. Pp. 71.

XIX.—*Constitution and By-Laws of the same.* Pp. 22.

XX.—*Proceedings of the Boston Society of Natural History.* January, 1854. Pp. 16.

XXI.—*Proceedings of the Academy of Natural Sciences of Philadelphia.* January to August, 1854. Pp. 44.

COMMUNICATIONS RECEIVED.

Communications have been received from DRS. WOOTEN, BOZEMAN, FENNER, POWELL, PALMER and J. R. DOWLER.

REMOVAL.

EDITOR'S RESIDENCE.—Delord street, between Camp and Magazine sts.

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THE NEW ORLEANS

MEDICAL AND SURGICAL JOURNAL,

FOR NOVEMBER, 1854.

Part First.

ORIGINAL COMMUNICATIONS.

ART. 1.—ON THE TREATMENT OF YELLOW FEVER, AS IT OCCURRED IN MOBILE, IN THE FALL OF 1853.

BY N. WALKLY, M. D.

Mr. Editor:—In the last number of the transactions of the Alabama State Medical Association, I notice several reports on the yellow fever of last year, in Mobile and the State of Alabama; each of them detailing the treatment usually pursued; and no one of them willing to adopt any other treatment than that which they had found *so* successful. If these gentlemen had given any statistics, whereby we might ascertain the relative mortality of those treated by them, we might make a comparative estimate of the value of the modes of treatment pursued. The reporter from this city, Dr. Wm. H. Anderson, (an extract from his report is published in the July number of your Journal,) states that himself and his associate in business treated, by the quinine treatment, as it is called, eleven hundred cases; but gave no account of the fatality attending that mode of treatment.

In the early part of the epidemic, I pursued the identical course of treatment detailed by him, and was much pleased with its effect in the early stage of the disease; but after the fever left, there was a tendency of the patients to sink, and their stomachs were so irritable that no

mode of stimulation that I could adopt, would keep them up; and I lost forty-two per cent. of all thus treated. I soon concluded that the treatment was not the best that could be adopted, and fell back to the expectant plan of treatment, which I had used in previous epidemics, and continued it through the balance of the season. I will now give the details of that treatment, as pursued by me, and the result.

Every one who saw the late epidemic, must have been convinced that the disease was not identical, nor had any affinity with our ordinary endemics, (supposed to be occasioned by malaria,) but that the disease was "*sui generis*," and was not caused, or much modified, by atmospheric or other changes, except that of temperature, (it is usually extinguished by a temperature beyond 40° Fahrenheit,) and that it would run its course in spite of all violent treatment, except that which caused death. It was therefore evident, that until we could find an antidote for the specific poison occasioning the disease, the indications were to adopt such a plan of treatment as would not obstruct, but rather assist nature in her efforts to throw off the disease.

In most of the cases attacked, it was found that the action of the stomach was suspended for several hours before the manifestation of the disease, as an emetic would usually unload the stomach of the food taken at the one or two previous meals, undigested. Consequently, in those cases where a loaded state of the stomach existed, an emetic was indicated, followed by a mild purgative; these, if given at all, were administered during the first hours of the fever, before the gastric congestion was set up; and if not administered at first, were dispensed with altogether. The reasons for dispensing with the evacuant, if not administered in the first four or five hours, are these: The stomach soon become so much congested that nothing would lie on it, or very little, except calomel, and that would frequently be thrown up; and if it was not ejected, the stomach was so paralized by the disease, that the medicine was not acted on, and remained in the stomach to increase its irritability, until after the fever had left; and then it prostrated the patient by salivation or hyper-purgation. If a loaded state of the bowels existed, an enema was sometimes administered if the first dose of medicine did not operate in five or six hours. Its object was merely to relieve the bowels, so as to render it unnecessary for the patient to get up until he was strong enough to do so without fainting; and no operation was deemed of advantage

until after the expiration of the fourth twenty-four hours from the onset of the disease.

The excessive pain in the head and back, was temporarily relieved by a hot mustard foot bath.. A deep tub or bucket containing it, was placed on a chair beside the bed, and the patient turned round so as to let his feet hang in it, and remain in the bath for twenty or thirty minutes, or until the head and back were relieved. This usually afforded relief for three or four hours, and then was repeated, during the febrile stage of the disease.

In addition to the diaphoretic effect of the bath, it acted as a derivative, by withdrawing from the head to the lower extremities the excess of circulation, thus temporarily relieving its congestion, and affording great comfort to the patient. As I have before remarked, the stomach would retain but little if anything without becoming irritated; but it usually bore cold water or ice better than anything else; yet, if much of that was given it was soon ejected. Some of the physicians who pursued the general treatment pursued by me were partial to warm teas, orange leaf and sage teas, given in small quantities at a time, and found them of advantage as diaphoretics. But it was of great disadvantage to load the stomach with much of anything, as, if that organ was distended by anything, vomiting was induced, which was hard to check. As the action of the salivary glands, together with all the other glands of the system was suspended during the febrile stage of the disease, the mouth and larynx became dry and parched, and a sensation of great thirst would be complained of, which would seem to call for the special action of mercury, to increase their secretions; but experience taught that no advantage was derived by its administration, for reasons expressed before, viz: it remained inert in the stomach, and was not absorbed until there was no use for it, that is, until after the fever had run its course. The thirst was best appeased by wetting the mouth frequently with iced water, swallowing as little as possible.

No nourishment whatever was administered in the first or febrile stage, and nothing else was administered during the 30 or 40 hours that the fever lasted, except as above specified.

After the fever had left, the patient was watched, and if it was craved, light nourishment was administered in small quantities. If any irritability of the stomach existed, it was quieted by the internal administration of

carminatives, an infusion of serp. Virginiana, water slightly acidulated with aromat. sulphuric acid, &c., and the external application of spice plasters, mustard, and other rubefacients over the epigastrium.

On the third night, seldom sooner, between eleven o'clock at night and four in the morning, the patients, if they did not commence convalescing before that time, showed a tendency to sink, commenced sighing, and their pulse showed a tendency to diminish in frequency and volume. This stage needed to be watched for, and met with appropriate stimulants. Usually brandy and water set best on the stomach; some liked mint juleps; some could not bear either, and could take champagne, or cider, porter, or ale, and freqeuntly we would have to run through the whole catalogue of stimulants to get one that would stick; and then failing, would have to resort to sponging with tr. cinchona and tr. capsicum, equal parts, or some other external stimulant, and failing with them, resort to blisters. I made it a rule to see every patient between midnight and morning of the third and fourth nights, and I have no doubt but many were saved thereby, as at that hour nurses are apt to be sleepy and careless, and most of them are disposed to think their patients are out of danger when the fever leaves.

In administering stimulants care should be taken in not administering too much, as that would induce gastritis, which is not desirable. Usually convalescence was established by the close of the third twenty-four hours, and they were, in a majority of cases, discharged on the fifth or sixth day.

Considerable care was necessary in convalescence, as a little imprudence either from over eating, or exposure, would cause a relapse. If in convalescence, the liver failed in regaining its functions, a little blue mass was exhibited, or if needed, the kidneys were assisted by a diuretic.

I give no mercurials, myself, in these cases, except the small amount of calomel contained in an ordinary dose of the "compound cathartic pills" of the Pharmacopœia, which was the purgative administered in nearly all the cases.

I used no depletion by bleeding; did not apply a leech, or a cup during the season; nor did I see but three patients bled by others, and they all died.

In the commencement of the season it was necessary to give neutral mixture, in teaspoonful doses, as a placebo, to keep the

patients' friends quiet, until the patients themselves had time to get well. By the first of September, however, this could be dispensed with, as most were willing to trust and obey their physicians' orders without question.

In making the comparison below, I would not intimate that my want of success militates against the so called quinine treatment, when properly administered in yellow fever, but that I did not know the secret of administering it properly, or that my cases were those in which it was not applicable.

The disease, in the first stage, evidently is not inflammatory, and does not call for depletion, and will not become inflammatory except from complications, or overloading the stomach. In those cases where black vomit occurred, I found very dilute aromat. sulf. acid, or sour butter milk, in teaspoonful doses every fewminutes, of more value than kreasote, matico, or anything else that I could use; but cases treated as above were very rare.

By this simple plan of treatment, between the 12th of August, and the 1st of November, 1853, I treated three hundred and thirty-six patients consecutively, and out of that number ten died, or a little less than three per cent.

The average duration of the febrile stage, in adults, in the above, three hundred and thirty-six cases was thirty hours; the longest forty hours, and the shortest twenty-six; of children under twelve years, the average was twenty-six hours; the longest twenty-eight hours, and the shortest twelve; the younger the children were, the shorter the fever ordinarily.

In the twelve quinine cases, nine were adults, and three were under twelve years of age. The duration of fever in the adults was an average of thirty-nine hours; the longest seventy-two hours, and the shortest twenty-four.

The average of children was twenty-four hours; longest twenty-eight, and shortest twenty hours.

I am well convinced that excessive medication by inducing gastritis, extends the febrile stage much longer than it would naturally run, if let alone, and by prostrating the patient, diminishes greatly the chance of recovery.

ART. II.—REMARKS ON THE ADMINISTRATION OF CHLOROFORM.

BY C. S. FENNER, M. D.,

OF MEMPHIS, TENN.

It is now seven years since Dr. Simpson, of Edinburgh, published the results of his experiments, showing the anæsthetic powers of chloroform. These experiments were repeated by others, with results so satisfactory, that in the brief period of a few months, the use of chloroform became common throughout the entire civilized world, and met with such universal favor, that it was generally resorted to preparatory not only to the severer, but also to the most trifling surgical operations, such as the opening of an abscess, or the extracting of a tooth. Very soon, numerous instances were reported, in which death resulted from its administration; a majority of these cases occurred in the hands of dental surgeons, and those who were not regular members of the medical profession; but the same unfortunate result occasionally happened to men of the highest professional skill and learning, who had had extensive experience with its administration, and whose standing was a sufficient guaranty that every precaution had been used to prevent unpleasant results. These cases became so numerous that many eminent physicians entirely abandoned its use; while others administered it with great caution, and only to relieve the pain attending the more severe surgical operations, or to quiet convulsive muscular contractions.

At the present time, there exist a great contrariety of opinions in regard to the safety of chloroform as an anæsthetic agent; all, however, agree that there are some peculiarities of constitution to which it is decidedly inimical. To detect such idiosyncrasy, in all instances, before trial, is a desideratum, the present state of our knowledge of the subject has failed to accomplish. M. Baudens published in the London Lancet, of October, 1853, some very judicious rules for the administration of chloroform; they are the best I have seen, and are well worth a careful perusal by every one in the habit of using this agent; and if strictly adhered to, will, I have no doubt, considerably diminish the dangers attending its use.

M. Bauden's rules are republished from the Lancet, in the January number of the American Journal of the Medical Sciences, page 208.

I have used chloroform, principally as a remedy for the pain attending urgical operations, since 1848, and generally with the happiest effect; wo or three instances, however, have occurred that have considerably haken my confidence in the safety of the remedy, and have induced me o administer it very slowly, and largely diluted with atmospheric air; t least at first, and closely to watch its effects, always keeping the finger f one hand on the radial or temporal artery while the inhalation is going on.

In August, 1851, I visited a negro woman about forty years of age, whose perinæum had been ruptured during parturition, throwing the ectum and vagina into one opening, for some three or four inches; she had been in this condition for several years, constantly confined to her bed—had suffered much, and at the time I saw her was in feeble health. She, as well as her master, was anxious that an effort should be made to relieve her, which could be done only by denuding the ruptured surface, bringing the parts together, and confining them, so as to facilitate union by the first intention. She inhaled chloroform from a towel; anæthesia was speedily induced, without material change in the pulse or respiration. When the operation was about half completed, she returned to perfect consciousness, conversed freely and intelligibly, and asked to be put to sleep again, which I declined doing, and proceeded with the operation. In some ten or twelve minutes, her pulse began to fail, and soon became extinct; immediately after which, respiration ceased. I dashed several slippers of cold water in her face, without reviving her, and then resorted to artificial respiration, by placing my mouth to her lips, inflating her lungs, and with the hands on her chest forcing out the air. This was continued regularly, by myself and others, for half an hour; but to no purpose. I, at the time, attributed her death to the shock of the operation on her system, debilitated and worn out by long suffering; and was sustained in this opinion by several highly intelligent medical gentlemen, who honored me with their presence and assistance. Still, she had taken chloroform, and it may have exerted a deleterious influence.

In October, 1852, an active healthy negro girl, 12 years of age, was brought to me from a distance, with a long and deep cicatrix on the side of her neck, disfiguring her person, and greatly impeding the natural motions of the head. I intended to dissect out the cicatrix, and if possible, to draw the edges of the sound skin together. She was placed on

a table, and a folded towel on which were thirty or forty drops of chloroform, held within three or four inches of her nose, one edge of the towel resting on her chin; after four or five inspirations, her pulse suddenly sank so as to be scarcely perceptible; her respiration became convulsive; her extremities assumed an icy coldness, and her face presented an exceedingly pallid appearance; the towel was immediately removed and cold water dashed in her face; she gradually revived, but respiration continued embarrassed for several minutes. I have no doubt but death would have ensued, had she inhaled the chloroform a few moments longer.

The next case was still more marked, the patient recovering after suspended animation had lasted several minutes.—July 5, 1854, a gentleman of dark complexion, and decidedly bilious temperament, about 28 years of age, called at my office, requesting me to perform an operation on him for phymosis, with adhesion of prepuce to the entire surface of the glans, and to the lips of the urethra. As the operation would be attended with considerable suffering, owing to the extreme sensibility of the organ, I suggested the propriety of his inhaling chloroform sufficient to deaden the pain, but not to produce its full anæsthetic effect. He was pleased with the idea, and procured an ordinary half ounce prescription phial, nearly full of chloroform. His health was good, and he had never suffered from cerebral, cardiac, or pulmonary disease. He was placed on his back, and a few drops of chloroform, largely diluted with atmospheric air, inhaled from a folded towel. As soon as I discovered the first symptoms of anæsthesia, I began the operation, the patient at the same time conscious of suffering considerable pain. As I was dissecting the prepuce from the glans, my attention was arrested by the extreme slowness of his respiration; I placed my fingers on his wrist and found him pulseless, and breathing not more than four or five times in a minute; I dashed cold water in his face which produced two or three rapid, convulsive inspirations, when he ceased to breathe, having every appearance of being dead; I instantly resorted to artificial respiration, and kept it up for several minutes; directing an attendant to procure a bottle of ammonia, but before he returned, the patient revived, and although he had been apparently lifeless, was conscious of every thing that had been said and done; after he revived I completed the operation. The quantity of chloroform inhaled could scarcely have exceeded one fluidrachm, for the phial still re-

nained two-thirds full; a very large part of that used must have evapora-ed without having passed to his lungs.

The quantity of chloroform required to produce anæsthesia, varies greatly in different individuals; some become insensible in two or three minutes from the inhalation of thirty or forty minims, while others require three or four fluidrachms, or even more, and have to breathe from ifteen to twenty minutes to produce a similar effect; indeed I have had ome few cases (generally persons of sanguine temperament, of light complexion, and light or red hair,) that I could not carry beyond the second eriod, or stage of excitement.

April, 1853, I attempted to induce anæsthesia in a gentleman of strongly narked nervo-sanguine temperament, preparatory to an operation for fistula in ano, and utterly failed, although I continued the effort for half an our, and used more than a fluidounce of chloroform; it produced great rterial excitement, with determination of blood to the brain; the patient omplaining of a peculiar sensation of pressure at the top of the head, but vas perfectly rational all the time, and acutely sensitive to the touch of he knife. It is sometimes the case, that when it is difficult, or deemed nsafe to go beyond the period of excitement, an operation may be performed, and although the patient may complain bitterly of pain, yet when ie comes to himself, will have no recollection of having suffered at all.

September, 1853, I gave chloroform to a plethoric, obese, old gentleman, but could not induce full anæsthesia, and removed a large steatomatous tumor from his back; he struggled violently, requiring three or four ssistants to keep him on the table during the operation; but when the ffects of the chloroform passed away, he had no recollection of having uffered any pain, and was not even aware that the tumor had been removed.

There seems at the present time a growing disposition to substitute ome other article for chloroform, and sulphuric æther seems to be more enerally preferred, as its administration thus far, has, as far as I am ware, been unattended with fatal results, which is certainly a powerful ecommendation in its favor. But if no absolutely safe substitute for hloroform could be found, I still should regard it as a great boon, and hould feel justified in resorting to its use, in cases where it is evidently ot contra-indicated, preparatory to severe and painful surgical operations, uch as are likely to produce a powerful shock to the system.

I regard the risk as partially counterbalanced by the lessening of the

danger attending the severe shock to the system, and the quieting of mental excitement usual in such cases. Persons have died from the extraction of a tooth, and not unfrequently, from bleeding; yet the risk attending these operations is too small to be calculated. No one would withhold the use of the forceps or the lancet for one moment, in cases in which they are indicated, from any fear of fatal hæmorrhage or tetanus in the former, or phlebitis in the latter. So with mercury; the cases are numerous in which it has been productive of the worst results, if not proving fatal; still, from its local action on the jaws and face, causing caries of the facial bones, immobility of the lower jaw, sloughing of the lips, cheeks, &c., often leaving the patient in such condition that death would be preferable; yet, notwithstanding these unfortunate effects, the benefit derived from mercury is so evident that I trust very few educated physicians would wish to banish it from the Materia Medica, because it cannot be prescribed with absolute safety. The same may be said of many other medical agents, and chloroform forms no exception. When administered by an experienced hand, and proper judgment exercised in selecting suitable subjects, the danger attending its administration will be so small as scarcely to be calculated, when we take into consideration the benefit derived from its use, in relieving an immense amount of human suffering.

ART. III.—CASES OF CONGESTION OF THE BRAIN.

BY B. BROOKS, M. D.

OF WHEELOCK, TEXAS.

Case 1.—On the morning of July 9, 1854, was requested to see a stout, robust negro man, about thirty years of age, who had been taken ill on the preceding afternoon. Had had a slight chill, after which he soon become comatose, and remained in that condition until I saw him, the following morning.

His extremities were cold; pulse feeble, beating 100; profuse perspiration; bowels constipated; involuntary discharges of urine; head hot; pupils a little dilated; carotid arteries throbbing violently:—Treatment—bled him twenty-four ounces from the temporal artery; applied mustard to forehead, back of the neck, along the spine, and to the wrists and ankles; gave brandy and carbonate of ammonia freely;

had his legs and arms rubbed with a strong decoction of red pepper, though all without any good effect, for the patient sunk and died in about four hours after I first saw him.

Case 1.—On the 23d of August, was called to see T. B., 15 years of age, a stout, healthy young man, who had been attacked about three hours previously. He had gone in to the watermelon patch, for the purpose of getting a watermelon, and when found, an hour afterwards, was speechless, and entirely insensible. Found him exceedingly restless, requiring force to hold him on the bed; extremities cold; pulse feeble, and beating 90; skin dry; pupils natural; jaws almost immovable; bowels constipated; involuntary discharge of urine Applied mustard to the forehead, back of the neck, spine, wrists and ankles, keeping it on until blisters were drawn. The jaws were so firmly set that it was with considerable difficulty they could be forced open sufficiently with a spoonhandle, to introduce medicine into the mouth. Gave five grains carbonate ammonia every hour, until the circulation was equalized. To quiet restlessness, dissolved extract hyosciamus in water, and gave a dose every hour. In about ten or twelve hours after I first saw the patient, there appeared to be a slight remission in the disease, at which time commenced giving quinine in five grain doses every two hours, continuing it until four doses had been taken. Upon visiting my patient next morning, found him still speechless and insensible, though his pulse was reduced to 80, and had more volume than on the preceding day. I should have given calomel on the day he was attacked, had it not have been so difficult to get him to swallow anything, even in a fluid state. This morning, (24th,) his bowels had not been moved; I, therefore, ordered twenty grains of calomel, with two drops of croton oil, to be taken at once. Probably not more than half the dose was swallowed, though sufficient to produce, in the course of two or three hours, four or five large bilious evacuations, which were discharged involuntarily. Continued the quinine in five grain doses, lengthening the interval to three, and finally four hours. In the afternoon, after a short, but refreshing nap of sleep, the patient asked for water, being the first time he had spoken since his attack. Upon handing him the water, I perceived that his jaws had become movable, and that he could drink without difficulty. I now commenced giving calomel, in two grain doses, every four hours, which, together

with the quinine, was continued a couple of days. At the end of four days, the patient was able to sit up in bed, and from this time on, convalesced without an unfavorable symptom.

The first remedy that suggested itself to me, upon seeing this patient, was the abstraction of blood from the temporal artery, or one of its branches, but having seen this remedy tried in several cases, with doubtful effect, I determined upon the course above indicated, and put it in requisition at once. If congestive fever or congestion of the brain be a disease of the nervous system, as suggested by some writers, it occurs to me that the loss of blood has a tendency to increase the prostration that exists, thereby rendering the disease more dangerous. I combined the croton oil with the calomel in order to facilitate its action, thereby hoping to produce a revulsive effect upon the alimentary canal, and I was not disappointed in my expectation, for it certainly exercised a very salutary influence over the disease.

ART. IV—A REPLY TO DR. BOLING'S EXPERIMENTS

With Phosphorus, and his Remarks upon its Dose and Action when given in the form of Alcoholic Solution or Tincture.

BY S. AMES, M. D.

An account of these experiments was published in the May number of this Journal for this year (1854.) They were instituted, it seems, while Dr. Boling was in search of a cardiac sedative equally prompt, powerful and reliable as the veratrum viride, but without the objections which, in his opinion, pertain to the properties of that remedy. After trying by experiments on healthy persons whether the yellow jessamine would not furnish the kind of sedative required, and being disappointed, he was induced by my paper on the treatment of pneumonia, published in the last January number of this Journal, to make similar experiments, also on well persons, in order to test the sedative properties of phosphorus in an alcoholic solution, the remedial operation of which is there ascribed to a sedative action. The experiments thus prompted led Dr. Boling to three principal conclusions, viz:

1. That phosphorus is not a sedative;
2. That it is not a stimulant; and

3. That it is not poisonous when given in an alcoholic solution or tincture.

These conclusions, it seems, were derived exclusively from these experiments. Dr. Boling expressly disclaims having any experience of the properties of phosphorus in acute diseases in the concluding paragraph of his paper, which I here copy:

"In conclusion," he says: "I would here remark that though prior to these experiments I had taken and given a good deal of the alcoholic solution of phosphorus, I have never in any instance given them (it) in a case of acute or dangerous disease."

The first and third of these conclusions, broadly as they are expressed in Dr. Boling's paper, including the action of this elementary substance both in health and disease, are so directly opposed to the results of my own experience of its action in inflammatory affections of the lungs, but especially in pneumonia, that there is a seeming obligation on my part, in the absence of any attempt of the kind on the part of Dr. Boling, to account for the discrepancies, apparent or real, between the results of his experiments on healthy subjects and of my experience at the bedside.

Dr. Boling has, indeed, invited such an investigation in so many words; but I am influenced in undertaking it less by any desire to detect and point out, to adopt his language, any fault or deficiency in the mode of conducting the experiments, though this easy task becomes necessary for the purpose of explaining the difference between his facts and mine, than to establish or confirm in the minds of others a just confidence in the remedial powers of phosphorus, which, in some acute affections of the lungs, are, in my opinion, equaled by but one other remedy. In carrying out this object, I expect to show that the experiments neither justify the conclusions based on them, I allude especially to the first and third, nor in any material respect affect the correctness of my own observations, made in a different field of inquiry, and in altogether different circumstances.

It is important to keep in view these several conclusions, in the order here set down, an arrangement not adopted in the paper under review, as for the sake of perspicuity it is required to examine the experiments and the remarks on them in reference to each conclusion separately.

The first question before us then is as to the proof afforded by these experiments that *phosphorus is not a sedative.*

Dr. Boling selected two subjects for his trials with this medicine: one a

mulatto child, seven years old; the other an adult black, under treatment by means of the bandage and a recumbent posture, for an ulcer of the leg, but otherwise in good health. Besides these, Dr. Boling submitted himself to some experiments, but, as they are noticed by him only in a general way, and more in reference to its poisonous than sedative action, their consideration is deferred for the present.

Before proceeding to the immediate examination of the experiments, I wish to bring into notice a matter of some interest incidentally connected with the subject now before us. From the mode in which the experiments were conducted, as well as from a portion of the comments thereon, I am induced to think that Dr. Boling had misconceived my opinion in regard to the power of phosphorus over the action of the heart, or as a "cardiac sedative." The experiments seem to have been begun and carried on in the expectation, or the hope, of finding in this article a sedative prompt and powerful in health, as well as in disease, like veratrum viride, and, consequently capable of overcoming any of the ordinary physiological influences over the pulse. Now if this be true, and the opinion was derived from any thing I have said, it is certainly a misconception of my views of its properties which requires to be corrected. By referring to my paper on the treatment of pneumonia it will be found that I have nowhere spoken of phosphorus as an active sedative agent over the action of the heart. In all that is there said of the *dose* of phosphorus no mention is made, even of its sedative powers; and this it may be seen is in striking contrast with what is said of the saturated tincture of aconite in speaking of the dose of that article. As regards the latter a great deal of pains was taken to point out that its remedial and poisonous action is the same, namely, *sedative*, and that on this account great caution was required in giving it; while in regard to the former, no notice is taken of its sedative action in connexion with any possible injurious effects that might arise from it on this account. The only reference to its sedative properties occurs in attempting to account for the mode of its curative operation in diseases of the lungs, and here its sedative action is spoken of only in connexion with its medicinal action, or its action in medicinal doses, the latter being contrasted with its poisonous action which is *not* sedative. My opinion of the extent and kind of sedative power it exerts in disease, is expressed in the following extract, from page 431, (vol. 10th of this Journal.) "Its action on the lungs seems from its effects to be

especially directed to the minute bronchial tubes and the air cells; and in inflammation to the capillary vessels rather than to the heart;" that is, though showing some direct sedative influence on the action of the heart, its chief action is indirect, through the capillary vessels which are the seat of the inflammation. At two or three other places I have spoken of its sedative influence over the general circulation, in medicinal doses in contrast with its poisonous action in larger doses, which is said by all authors to be stimulant, but always of course in reference to this opinion as to the mode of its sedative action, but have never represented it as existing to any poisonous extent. All the warnings that I gave, and they might with the greatest propriety be repeated here in view of the experiments of Dr. Boling, in regard to the danger of too large doses, founded more on what I had read than of what I had seen its effects, refer exclusively to its poisonous action which is of another and different kind.

A misconception of this kind might possibly arise, not from any direct remark of mine attributing to phosphorus great powers as a direct sedative in disease, and much less in health, about which I expressed no opinion at all, but as an inference from the following passage: "There is a point at which it (phosphorus) ceases to be medicinal, or sedative, and becomes poisonous, or stimulant. Thus it is not possible to produce with it the extreme depression which follows large doses of aconite; for when the dose is enlarged for this purpose beyond a certain point a new and opposite action is set up in which the (sedative) power is lost or merged in the local inflammation it excites and its concomitant influence on the nervous system and the general circulation." This is strictly true in fact, but the inference which the words may bear, for the opinion does not seem carefully worded, that its poisonous is the only limit to its sedative action, is certainly erroneous. But then, such an inference is contradicted, it seems to me, by everything I said on the subject elsewhere.

Having set this matter right, so far as any misconception of the degree and kind of sedative power of this article, for I am satisfied that its direct power of controling the action of the heart is very limited, may have arisen from any inadvertence of mine, I take up now the examination into any sources of fallacy to be found in Dr. Boling's experiments, so far as they relate to the sedative operation of phosphorous on persons in health and disease.

In all experimental trials of this kind, it is to be remembered that the state of the pulse is the thing to be observed, and is alone to be looked to for any indication of the effect of the medicine given; and consequently, that all collateral inducements to changes in the action of the heart should be carefully avoided. Such being the case, it is not without some surprise that one observes in the record of these experiments an absence of any especial care to avoid those normal causes influencing the state of the pulse, which, being common to all persons, of whatever age, are almost constantly in operation during our waking hours. Indeed, their possible influence seems to have been overlooked, except in the simple instance of the recumbent posture, which is mentioned apparently in view of its effect on the pulse; though the difference between that state and an erect position or active exercise in connection with such effects is not alluded to. The presence of these causes is mentioned, but not in this connection. Thus, it is found in several instances, that nearly an entire day is consumed in an experiment; the pulse being counted almost every hour, without any mention of the time of taking food, though it is said generally, that the subjects took their meals as usual; so, too, we are told that the experiment was begun sometimes immediately before, at others immediately after eating; and again in the middle of the intervals between the meals; and on one occasion it is especially stated that the medicine was given soon after a hearty breakfast; so, too, in regard to posture and muscular exertion; one of the subjects was generally called in from play, when the pulse was to be counted or the dose given; or was riding, in the course of the experiment, in his master's buggy, and attending to his horse at the different stopping places; and in one experiment he was asleep a part of the time and awake at another. In all these instances the facts are spoken of without allusion to the certain influence over the pulse, of alternations of fasting and repletion, of active muscular exercise and rest, and of sleeping and waking. Yet these are well known to be active sources of perturbation in the action of the heart, which if not voidable, ought, it would seem, to have been carefully noted, and, as far as possible, taken into the estimate in counting upon the results.

It is plain that under such circumstances, so certain usually to produce considerable changes in the pulse, moderate powers in the medicine, whether sedative or stimulant, might not be manifested at all; or at least not so manifested as to afford reliable proof one way or the other. It is

well known, in fact, that the influence of digestion and muscular exertion over the action of the heart is frequently sufficient to overcome, or render insensible, the power of some pretty active medicinal agents.

But the influence of these common physiological causes, though requiring to be noticed as one of the elements of fallacy, is of much less moment in this discussion than they would otherwise be, on account of the obvious presence of another cause, operating more powerfully to disturb the pulse, and, consequently, affecting more decidedly the results of the experiments. The evidence of the presence of such a cause, and of its controlling influence in both subjects, not only over the pulse, but over the action of the ordinary physiological causes, is found in the record of the experiments; and to the details necessary to point this out, I wish for a brief space to call especial attention.

The two first experiments on the little boy, "Sam," are very significant of the singular fact just referred to:

"On the 25th of February, having kept him (Sam) in a recumbent posture some time, his pulse being at 102, at one o'clock, P. M., I gave him seven drops of the saturated tincture (of phosphorus); at two o'clock, P. M., pulse variable, from 90 to 108 (eighteen beats while counting it); three, P. M., (subject asleep) pulse 94; the dose of seven drops repeated; four, P. M., (subject asleep) pulse 104.

"February 26th, ten o'clock, P. M., pulse 100; one and a half o'clock, pulse 114; two o'clock, pulse 100"—(a rise and fall of fourteen beats at intervals of half an hour.) "No phosphorus given to-day. The experiments already given were conducted with the subject in the recumbent posture. In the following, he was generally called in from play, when a dose was given or the pulse to be counted."

In these two experiments the subject appears to have been, more than in any of the subsequent ones, freed from those ordinary incidents which operate to derange the action of the heart in health. For example, he was lying down all the time, so that no influence could have been exerted by changes of posture, or of more active muscular exertion. Again in the first experiment he was asleep at two consecutive countings; the pulse still exhibiting the same tendency to change as when awake; it being found at the last counting, ten beats quicker than when counted while asleep one hour before. Thus while the tendency of sleep is to reduce the frequency of the pulse and give it uniformity, it is found to be not

only faster in this subject, when asleep at one time than another, but faster by eight beats when asleep at four o'clock, than when awake at one o'clock. The cause of these changes was therefore sufficient to overcome the usual sedative influence of sleep—a very powerful one, too. We are not told whether "Sam" took any food shortly before or after one o'clock, on either day, nor is it material here to know; for if so, its effect could have been only to give a uniform acceleration to the pulse while the process of digestion was going on; not, certainly, to produce this kind of vacillation. There is still another appreciable physiological cause, whose operation would be more likely than any other to bring about this kind of changes, and that is mental agitation; but we have no direct evidence of its presence. If present, it would probably have shown its presence by other signs, which could hardly have failed to attract the attention of Dr. Boling, who does not, however, allude to it. Nor could the medicine given have had any influence to derange the pulse in this way; for seeing that the changes in these and other experiments, occurred equally when the phosphorus was or was not given, and equally when given in comparatively small or large doses. Dr. Boling, himself, very properly excludes it from any agency in producing them.

Here, then, we find in two experiments, one of them occupying three hours, and the other one hour only, an extreme variation in the pulse of eighteen beats in the minute in the one, and of fourteen in the other; while in both the pulse rose and fell so suddenly as not to be really the same at any two consecutive countings; and this when all the ordinary incidents affecting the rythm of the heart's action, were either absent or obviously inoperative to produce such effects. Excluding, therefore, all these ordinary incidents, what other conclusion is left, the subject being in good health, than that there was in him some inappreciable peculiarity or idiosyncrasy of considerable force, operating to produce these striking anomalies in the heart's action.

In the subsequent experiments on "Sam," the circumstances were less favorable for the manifestation of this peculiarity; one of the ordinary physiological influences, namely, muscular exertion; and one extraordinary one, not physiological, namely, large doses of alcohol, were present in these and absent in the others. The influence of the alcohol may be estimated with some assurance of accuracy, for, being the vehicle for the doses of phosphorus, the time of giving it, and the quantity, are noted; but

we have no means of estimating the probable effect on the pulse, of the additional physiological cause for its changeableness. We are not told, for instance, in which of the experiments he was called in from play to take the experimental dose, or to have his pulse counted; nor whether, when called in from play or at other times; the pulse was counted while standing, or in some other position. All the information we get on these important points, so sure ordinarily to derange the pulse, is that he was *generally* called in from play. So too in the experiments which were partly conducted while the subject was riding in the buggy with his master, no especial reference is made to the circumstances in which the state of the pulse was noted. Nor have we any information in this, any more than in the other experiments, at what time "Sam" took his meals, nor in which he did or did not take food, though we are assured that both subjects took their meals as usual, and with their usual appetite and relish, and of course, in the usual quantity and variety; but, as we do not know the hours of breakfast, dinner, or supper, we cannot apply this general assurance to the particular experiments. These incidents, I hardly need say, are of a kind to have had a decided influence over the pulse, but whether they did so in this instance; or when, and to what extent; are things that we are not permitted to bring into the estimate, for want of these essential details. Nevertheless, an analysis of the experiments as we find them recorded, brings into view, in despite of all counteracting causes, the manifestation of the same controlling powers that is so plainly exhibited in the former experiments.

The first evidence of this that I shall notice is, that the pulse took a *lower* range, and was not more variable, in these subsequent experiments, when the pulse should have been accelerated by an erect posture, or the more active muscular exertion of boyish play, than in the two first, when the subject was lying down all the time, and asleep a part of the time. Thus the average of the two first experiments is respectively, leaving out the fractions, ninety-nine and one hundred and four beats to the minute; while the highest average in the subsequent ones, excluding the effect of overpowering doses of alcohol, was one hundred, and the lowest ninety; while the general average in the former, the subject being at rest, is one hundred and two. The general average in the latter, when he was engaged in active exercise, actually falls to ninety-six. Why did changes of posture, and the alternations of rest and muscular exercise, fail to

have their usual influence in quickening the pulse? The pulse ought to have been very much accelerated, but we find it slower. How could this happen unless there had been some other cause at work more powerful than they, in its influence over the action of the heart.

Secondly: the suddenness and extent of the changes in the pulse. Thus: March 10th, the pulse at 12, M., was one hundred; at 1, P. M., it varied from one hundred, to one hundred and six. This means, probably, that the variation occurred while the pulse was being counted at that time. At 2, P. M., pulse one hundred and four; 3, P. M., pulse one hundred and eight; 4½, P. M., pulse ninety-two; 6, P. M., pulse eighty-eight,—a fall of twenty beats in the minute, in three hours; and *five minutes* later, ninety-six.

Thirdly: similar changes occurred while the subject was under the influence of large doses of a powerful stimulant. Thus: March 12th, at 5, and at 5½ o'clock, P. M., "Sam" took, at each time, two hundred drops of anhydrous alcohol,—the vehicle for the doses of phosphorus then given,—that is about four drachms of anhydrous alcohol in the course of half an hour. The result as regards the pulse is thus noted: 5, P. M., pulse one hundred—two hundred drops given; 5½, P. M., pulse one hundred—two hundred drops given; 6¼, P. M., pulse one hundred and seven; 7½, P. M., pulse ninety-four.

That is to say, the pulse rose seven and *fell* thirteen beats in the minute, within two hours after taking the last dose of alcohol; and of course, while the subject was still under its influence. Now, it would seem hardly possible that the pulse could have fallen so much, for it is to be noticed that two hours and a quarter after the last dose, it was six beats slower in the minute than just before the doses of alcohol were taken; or at all, while acted upon by a quantity of this prompt and powerful stimulant, which is equivalent to about an ounce of good brandy, given to a child seven years old; unless there had been some unusual cause of perturbation in the action of the heart.

Another significant fact, leading to the same conclusion, is exhibited in the succeeding experiments. Thus: March 13th, 8, A. M., pulse ninety-two—five hundred drops (more than half an ounce of anhydrous alcohol, the menstruum for the dose of one-tenth of a grain of phosphorus) was given. One hour afterwards, namely, at 9½, A. M., the pulse was still at ninety-two; ten beats *below* the average in the other

experiments, in which the subject took neither an artificial stimulant, nor muscular exercise.

These experiments, if they give us no clue to the nature of the cause of these singular interruptions to the normal rythm of the heart's action, afford some decisive evidence of its power. We have seen that the vacillations in the pulse occurred under all circumstances, with two exceptions, in both of which a large dose of an active stimulant was operating to control it. The pulse rose and fell almost equally while under the influence of about half an ounce of alcohol, as when none was given; while a larger dose, more than half an ounce, served only to make the pulse regular, and apparently to *reduce* its frequency, for it is found below 92 in but three countings in all the other experiments on this subject. It required a larger dose still to overcome this physiological tendency, to change so far as to exhibit the usual effect of a stimulant on the pulse. Thus: March 17th, 4 P. M., pulse 104, nine hundred and ten (910) drops, "being," says Dr. Boling, "exactly one ounce (of an hydrous alcohol being the menstruum for the dose of phosphorus) given at a dose. And here is the result: half after 4, P. M., pulse 116; 5, P. M., pulse 120; 7, P. M., pulse 104, and the subject feels very well." The dose of alcohol, it is seen, is a very large one, if we take into consideration the tender age of this subject. Good brandy contains less than fifty-three per cent. of alcohol; the dose, therefore, was nearly the equivalent of two ounces of brandy. The age of the subject being seven years, the dose for him should be about one-third of that for a man of middle age; so that the quantity of alcohol taken by Sam, at one dose, was equivalent to nearly six ounces of good brandy for a man of middle age. Yet the pulse was quickened only for the space of something over two hours; at the third hour it was found to be the same as at the time the dose was given. The pulse fell *sixteen* beats in the minutes between the second and third hour. Was this owing to any narcotic effect from the alcohol? It seems not. "The two hours immediately succeeding the two last doses," Dr. Boling tells us, "Sam spent riding in my buggy, and attending to my horse, at the different stopping places, and at the end of the experiment, the subject feels very well."

What estimate, then, shall we make of the power of that cause

perturbing the heart's action, in this subject, when half an ounce (four hundred drops,) of anhydrous alcohol failed to affect it at all, and five drachms (five hundred drops,) served only to steady it, and, apparently, rather to reduce than to augment its frequency, and a full ounce, equivalent to nearly two ounces of brandy for the child, or six ounces for an adult, quickened it but for a short time, something over two hours? May we not at least conclude that a cause, so resisting to such active stimulating influences, may have been sufficient to prevent any manifestation of a sedative power not absolutely poisonous?

Abundant evidence of the same peculiarity is exhibited in the adult subject, in whom this vacillating tendency in the action of the heart, was not less striking than in the other, if due allowance be made for the difference in age. For example:

"March 4th, eleven o'clock, A. M., pulse 80; half-past twelve, P. M., 72; three, P. M., pulse 68. No phosphorus given to-day."

March 7th, twelve, M., pulse 78; half-past twelve, pulse 68; 80 drops (of the saturated tincture of phosphorus) given; two and a half, P. M., pulse 76.

March 10th, half-past seven, A. M., pulse 76; half-past eight, A. M., pulse 70; ten, A. M., pulse 80; eleven, A. M., pulse 80; twelve, M., pulse 82; half-past one, P. M., pulse 76; two, P. M., pulse 68; three, P. M., pulse 74; half-past four, P. M., pulse 80; six, P. M., pulse 82. No phosphorus administered to-day.

March 11th.—No phosphorus was given, but the pulse being counted at intervals, was found to vary about as it did yesterday.

The extreme range of the pulse is here seen to reach ten, twelve and fourteen beats to the minute, suddenly, and without any apparent cause. The same thing is true generally, of the other experiments on this subject; the pulse in this, as in the other subject, being very seldom the same at any two consecutive countings, no matter how short the time between them. Thus the changes occurred equally when the phosphorus was or was not given; the man was all the time recumbent, so that neither posture nor exercise could affect his pulse; there seems to be no reason to believe that mental agitation operated in this, any more than in the other subject; and finally the pulse did not, in a single instance, exhibit the regular diurnal ebb of the heart's

action from morning to night. The taking of food, by this subject, is particularly spoken of, in connexion with one experiment only, and in this it is worthy of notice, that the pulse was more steady, on this than on any other day, though we find the usual order of the diurnal change reversed, the pulse increasing in frequency, instead of diminishing as the day advanced. Thus:

March 15th, nine o'clock, A. M., pulse 72; 200 drops of the saturated tincture (of phosphorus, holding in solution about one-fourth of a grain,) given; ten, A. M., pulse 72; eleven, A. M., pulse 74; one, P. M., pulse 76. A short time before he took the 200 dops, he ate a hearty breakfast, and at two, P. M., with decided relish, a substantial dinner of bacon, cabbage, potatoes and corn bread; four, P. M., pulse 80.

The unusual regularity of the pulse in this instance may have been accidental; nevertheless, it is singular as a coincidence, that this is the only day in which it exhibited a similar regularity in its variableness; the effects of digestion seeming like the stimulus of the alcohol in the other subject, rather to steady the pulse than to increase its frequency, which is known to be its usual tendency.

Excluding, therefore, in both subjects all known and appreciable causes from any agency in producing these remarkable and peculiar physiological changes in the action of the heart, or making all proper allowance for their effects, we find remaining a degree and kind of eccentric action, whose cause, it is evident, must have had a predominate influence over the result of these experiments. In such a condition of the heart's action, so perseveringly and actively variable, arising from a cause capable of resisting not only the usual physiological stimuli when known to be present, but also the power of a very active medicinal agent, administered in large doses, may we not suppose that even a pretty active sedative influence would be as powerless over the pulse of these subjects as the stimulants proved to be? In truth, this source of fallacy seems to be of that precise kind which would necessarily preclude any other than the negative results actually obtained by Dr. Boling in these experiments.

I have spoken of the variableness of the pulse in these subjects as remarkable and peculiar. Dr. Boling, however seems to view the matter in a different light. "The pulse," he remarks, "as most per-

sons know, and as every one may satisfy himself of, will be found to vary in many persons, if not in every one, somewhat in frequency in the twenty-four hours, and often even when felt at short intervals, will be found so to vary from trivial or inappreciable causes. Now, this is no doubt true within certain limits, and as a general remark, but not true, I think, in its application here. There is a regular diurnal variation in the pulse of all healthy persons, and this is liable to interruptions from several sources, all of which, however, whether trivial or not, are, I believe, well known, and consequently appreciable if the cause be inquired into; and their influence may be estimated ordinarily, in all questions in which the issue may depend on the state of the pulse with sufficient accuracy for all practical purposes, when they cannot be altogether avoided. The error here, therefore, is not so much in the general truth affirmed as in its application to the cases before us. If it was meant that such changes in the pulse as occurred in these subjects are common to persons in health in like circumstances, or indeed, in any circumstances, then the remark, I believe, is not justified by the experience of physicians generally—certainly not by that of professed physiologists who are universally silent on the subject of any changes in the pulse of persons in health occurring independently of those physiological influences, already so frequently referred to, which constitute the recognized sources of interruption to the normal regularity of the heart's action. So far as my individual experience goes it coincides with all the authorities I have consulted on this point. Indeed I do not remember to have seen even in disease such a kind of perturbation in the pulse, so continuous, abrupt, and wide in its range, except in some acute affections of the brain, and, though I am not sure, in some forms of hysteria.

We turn now from the fallacies in the experiments to a more important fallacy in the conclusion from them. The latter is not dependent on the former, but inheres in the conclusion itself, and would be the same if there had been no fallacies in the experiments. The conclusion, which is a general one as to the sedative action of phosphorus, is so stated as to cover two points not necessarily connected with each other, viz : *First*, that phosphorus does not act as a sedative on persons in health ; and, seccondly, that it does not

act as a sedative in disease. The latter is a mere inference from the former, inasmuch as Dr. Boling has not tested the effects of phosphorus in acute diseases in a single instance. It is, therefore, strictly an *a priori* inference, the truth of which has not been distinctly tested by any experiments. The fallacy in the conclusion lies in this inference, from the negation of physiological effects to the negation of therapeutic ones.

The inference is fallacious, because there is no necessary connection between it and the facts on which it is based. It might be true that phosphorus is *not* a sedative in health, and yet it might be true that it *is* a sedative in disease. Medical experience has furnished innumerable examples of the general truth, that negative facts concerning the physiological effects of a medicinal agent afford no reliable proof as to its therapeutic action—and this because the facts *are* negative. This rule is reversed as regards positive facts obtained in the same way. If, for instance, phosphorus had proved to be sedative in these experiments, then the conclusion would have been legitimate and proper that it is also sedative in disease, and this without any other proof. And why? Not certainly in virtue of an *a priori* inference like that of Dr. Boling, from the facts in one set of circumstances to the truth of the same facts in other circumstances, or of any evidence abstractly to be derived from the former facts, but solely in virtue, that *experience* has long ago established the general truth of such influences from positive facts; the law being subject, however, to many exceptions, that the manifest effects of a medicinal agent are equally shown in disease as in health. But no such law applies to negative facts, and hence it is that while in the one case, the experience of medical men has furnished beforehand the necessary experimental evidence to establish the truth in many instances of such an influence from positive facts; such an inference from negative facts, lacking this stored-up proof, requires to be supported by an especial and direct experimental proof in every instance. In the one case the facts indicate a certain truth; in the other but a probable one at best. To establish the latter it is required that the indication from the facts obtained in experiments on well persons, shall be verified by experiments on sick ones. But this Dr. Boling has not, it seems, in this instance thought it necessary to do.

Instances illustrative of this argument, and of the fallacy of *a priori* inferences of the kind we are examining,—in which it is shown that decided and even powerful curative effects are brought about by the aid of medicines which have either no poisonous action at all on well persons, or are given in doses too small to affect them sensibly, or in which the curative action is apparently the very opposite of their sensible physiological action, are equally numerous and familiar. I shall select for illustration here only two such instances, which, I think will suffice on account of their especial bearing on the example before us.

Lemon juice, it has recently been discovered, exercises a speedy and very efficient curative agency in acute rheumatism,—an inflammatory affection in which contra-stimulants or sedatives are necessarily the only effective remedies. Now is there any thing in the physiological effects of lemon juice which would indicate this therapeutic action? Is it a cardiac sedative administered to persons in health? I believe not; no mention is made of any sensible physiological effects from it by either of the few authorities I have consulted, though among them is the elaborate work of Dr. Periera, the last edition, where one might expect to find them, if any where. Certainly it has no considerable power in this respect, if any at all; and yet it is said to reduce the force and frequency of the pulse in this disease with a degree of power not equaled by the most active sedatives known to the Materia Medica. Let us suppose, then, that when this discovery was first announced,—a discovery of such value that it has been said on high authority in England, to mark an era in the history of practical medicine,—physicians, instead of testing its value in the circumstances of disease in which its sedative power was affirmed, had set about to determine whether this was true or not, by giving it to well persons, to try in fact, whether it would cure rheumatism by giving it to persons who had no rheumatism to cure,—what the *a priori* inference from such experiments would be, as well as its value when made, is obvious enough. Would they not, in fact, have necessarily come to the same general conclusion in regard to the sedative power of lemon juice that Dr. Boling was led to by his experiment in regard to phosphorus?

Again: It is well known to medical men practicing in the Southern and Southwestern States, particularly in the newly settled parts there-

of, that quinine acts more efficiently as a sedative in a certain class of diseases than any other medicine. The pulse may be—every one must have seen it—reduced by a single dose, but always by a few doses, if the circumstances be suitable, twenty, forty, or even fifty beats in the minute in a very short time. And yet if the same medicine be given to a person in health in the same doses, or even larger ones, it may be that the pulse will not be effected at all, and is just as likely to be made faster as slower. An example in point, remarkable on account of the unusual size of the doses, has been recently related to me by Dr. Reeves, of this city, who gave to a man with chronic hypertrophy of the spleen, but without fever, the enormous quantity of an ounce of quinine in the course of four days, in doses of forty grains, given three times daily. Dr. Reeves did not observe any material change in the man's pulse during the whole experiment, for such it was. What change there was, indicated an increase rather than a diminution of its frequency. Numberless other instances of its inaction on the pulse of well persons, as also in some forms of febrile disease* will occur to the memory of every physician who has made much use of it. My own experience with it as regards its action when I have taken it, or given it to others, is to the effect that quinine, like lemon juice, does not so effect the pulse in health as to give any intimation of its extraordinary sedative power in some forms of disease. Such, no doubt, is also the result of the experience of physicians generally, if not universally. But if so, how false a guide to the truth would be experiments made with quinine like those made by Dr. Boling with phosphorus?

But not to fatigue the reader with further illustrations of an obvious truth, we here close the review of these experiments, so far as they relate to the sedative action of phosphorus, by summing up the conclusions from all that has been said, which, I think, are embraced in the following propositions:

First: That the fallacies in the experiments were of such a kind as to preclude any other than the negative results actually obtained from them,

*This fact, which I had frequently observed before, was very forcibly brought to my notice last fall in the first case of yellow fever that I treated. In this case it was apparent that quinine aggravated all the febrile symptoms proper—singularly enough too, the case presented the features of a quotidian remittent, exhibiting, the three first days, decided morning and evening remissions and exacerbations. I had never seen quinine act in that way, that is as a stimulant, in a remittent fever before—nor indeed in any fever, nor in the absence of fever—but I had never before treated yellow (remittent) fever. Supposing that this stimulating effect was only apparent, I gave it in several other cases; but they only served to confirm the opinion I had formed of its effects in the first case.

and consequently, that they afford no reliable proof as to the sedative power of phosphorus in health; and,

Secondly: That if the experiments had been so conducted as to insure accurate results, they would furnish no reliable evidence as to its effects in disease.

The sedative action of phosphorus in health is one question; its sedative action in certain forms of disease is another question. Whether it is sedative in its action on persons in health I do not certainly know, and as I have expressed no opinion about it heretofore, so neither do I intend to express one now. There are, however, some positive facts relating to this point, which I wish to put on record here, to be used, if desirable, for reference, if more of the same kind should be brought to light hereafter. At present, they are too few in number, and the circumstances attending them have been too little varied to justify anything like a general conclusion. A part of them, which are referred to by Dr. Boling, is contained in a note in my paper on the treatment of pneumonia, which I copy:

"Two young gentlemen, my personal and professional friends, have recently been engaged in some experiments on persons, in order to test the effects of phosphorus on persons in health, they themselves being the subjects of the experiments. These gentlemen found (in several experiments) that the force and frequency of the pulse was invariably reduced by a single dose of two drops of the saturated alcoholic solution; the changes in frequency ranging in the number of pulsations to the minute between eight and twelve beats."

To these may be added three experiments recently made on myself, while convalescent from an attack of sickness, but still confined to bed, without appetite, and taking food, only in a very small quantity, of the lightest kind, at a time, and this at long intervals, circumstances highly favorable for such a trial. The dose, (for I took but one in each experiment, in two of them,) was three drops of a saturated alcoholic tincture, and in the other thirty drops of the diluted tincture used by me in practice, the last being equivalent to either of the others. I ought to mention, as a fact of some importance in experiments with either of these preparations, that I dropped the doses into water cooled by ice to about 40° of Fahrenheit, from which almost no vapor is given off, though it makes the water a little turbid; whereas, if dropped into a tumbler or

other open-mouthed vessel like it, into water at 70° and upwards, it gives off a volume of phosphorus vapor so large as to render the quantity remaining very uncertain; there can be no doubt that the dose is in this way frequently very much reduced. The results of my experiments corresponded with those of Dr. Pollard and Dr. Oliver. The greatest depression in the pulse was twelve beats in the minute. This effect was evanescent; the pulse always returned to the same state as before the dose was taken within an hour and a half after the greatest depression was reached. The longest time required to produce this effect was one hour and twenty minutes; the shortest one hour; the change in the pulse was perceptible in twenty minutes in two experiments, and in thirty minutes in the other.

A question is raised by Dr. Boling relative to the proper size of the medicinal dose of phosphorus, which being incidentally connected with its sedative action as a therapeutic agent, may more properly be discussed here than elsewhere. This question, implied rather than directly expressed, is as to the possible efficacy of this medicine in the doses recommended by me in the treatment of pneumonia. The implication is, that given in doses so minute as they were estimated to be by Dr. Boling, there must have been some mistake in attributing any efficacy to them; especially when it was shown that it could be given in a dose many hundred times greater to a person in health without any appreciable effect of any kind. This last fact, which is dwelt on with some emphasis, is exhibited in the experiment on Sam, 10th of March, in which he took at one time nine hundred and ten drops of a diluted tincture, which held in solution about one-tenth of a grain of phosphorus. "Here," says Dr. Boling, "a child, seven years old, took at a single dose, one thousand eight hundred and twenty (1820) of Dr. Ames' doses for an adult."

The phraseology here is somewhat peculiar; it is not said that the child took one-tenth of a grain at one dose, but eighteen hundred and twenty of Dr. Ames' doses; so that the objection would seem to be founded more on the relative quantity that could be given without effect than to the absolute size of the dose. This supposition is confirmed by a subsequent remark: "Yet not only did my subjects take it in doses so immeasurably greater than the doses with which such effects are said by Dr. Ames to have been produced by it," &c. Now, if such

is the meaning, this instance is not the best that could have been selected for the contrast. A still stronger one could have been found in the record of cases, hereafter to be noticed, in which three, six, and even twelve grains of phosphorus has been given at a dose, with no more sensible effect than Dr. Boling witnessed from his dose of a tenth of a grain. But phosphorus does not stand alone in this particular. The books furnish some stronger contrasts in regard to other poisons whose medicinal doses are better known. I have space but for one example. I suppose an eighth of a grain of calomel, repeated like the doses of phosphorus every three or four hours, must be an efficient medicinal dose for an adult in certain circumstances—in fact, an adult may sometimes be speedily poisoned with it. A case is related by "Taylor, on Poisons," in which a child eight years old was badly poisoned by three doses of two-thirds of a grain each, given at intervals of twenty-four hours. In contrast with this I give the following case, which came within my own knowledge while a student of medicine: A child between two and three years old got hold of a vial of calomel by accident, which she broke while playing with it; and liking its sweetish taste, ate up a full ounce of it before she was discovered. But the child showed no sensible effect from this enormous dose, except that the next day a single free evacuation of the bowels took place. Thus, a child less than three years old, took, almost without sensible effect, thirty-eight hundred and forty efficient doses of calomel for an adult person, or six hundred and forty such doses as proved sufficient to poison a child three times older. This is an extraordinary case as regards calomel; but it is well known that this remedy has often been given to adults in doses of two, three, and four drachms—that is, from nine hundred and sixty to nineteen hundred and twenty efficient medicinal doses, without any sensible effect. The books on poisons are full of similar contrasts. It may be concluded therefore, that the quantity of a poison which can be taken with impunity, relatively to the proper medicinal dose, is no measure of the efficiency of the latter. Hence, the medicinal dose of phosphorus might be relatively as small as that supposed by Dr. Boling, and yet be an efficient one—a kind of question, however, which may always be easily enough decided by experience, but never by *a priori* conjectures.

But the more important question which I suppose was also intended to be made, is an objection to the absolute quantity employed by me and others medicinally. And here I wish to remind the reader, or rather to request him to continue to bear in mind, that this objection is in the nature of a conjectural inference, merely, from the supposed size of the doses given by me, and from what Dr. Boling saw of its action, or rather want of action, in a much larger dose given to a healthy subject. Dr. Boling had not tested its medicinal efficacy in any dose in any acute disease.

How large the dose really is, I had not made a subject of inquiry before the appearance of Dr. Boling's paper; nor, perhaps, should I ever have done so but for the estimate there made of it. The object of all my trials with phosphorus in varied doses, which had been confined to diseased subjects, but especially to those affected with pneumonia, was to ascertain the smallest quantity of an alcoholic solution capable of producing decided medicinal effects, and yet be without danger of producing, in any condition of my patients, those disastrous consequences which are said by authors to have resulted from doses that in any other substance, except, perhaps, aconitine, would be too small to produce any sensible effect. Having secured this object, the actual quantity of phosphorus contained in a dose, became a matter of perfect indifference.

Dr. Boling's estimate of the dose is as follows: "In a vial containing an ounce of anhydrous alcohol, I placed four grains of phosphorus, in another two grains, and in another one grain. At the end of fourteen days—the time usually necessary for the preparation of tinctures by maceration, the time recommended by a majority of the pharmacopœias—of the four grains, about one-fourth or less was dissolved; of the two grains, about one-half or less; and of the single grain there remained a portion undissolved. It is fair, I think then, to say that the saturated alcoholic tincture, instead of containing four grains to the ounce, contains in reality but about one grain to the ounce. It may possibly be a little more; it would seem as likely to be a little less." Proceeding on this asumption, the dose is estimated to be about the one-sixteen thousandth part of a grain—a quantity, as he remarks, almost inconceivably small. Let us see how nearly correct this estimate is.

Seeing that there was one obvious source of fallacy in Dr. Boling's experiments to ascertain the quantity of phosphorus that alcohol will dissolve, I requested and obtained the favor of Mr. Williams, principal assistant in the drug store of Messrs. B. S. Thiess & Co., a gentleman remarkable for the care and accuracy of his pharmaceutical manipulations, to institute some experiments, of which the following is an account:

1.—Twenty grains of phosphorus were put into an ounce by measure of alcohol not anhydrous, sp. grav. 812 (at 70 deg. Fahrenheit) and allowed to digest nearly twelve days. The phosphorus was then removed from the vial and weighed. The loss was found to be three grains. A small part was wasted in transferring the powder* to the scales, which Mr. Williams estimated at about one-eighth of a grain.

2.—Two grains were put into a measured ounce of anhydrous alcohol (sp. grav. 794 at 60 deg. Fahrenheit) on the 9th of June. This was all taken up in eleven days.

3.—Four grains were put into an ounce of alcohol, (sp. grav. as in the preceding experiment,) on the 9th of June. Twenty-one days afterwards there remained undissolved, according to the estimate of Mr. Williams and others, not more than a quarter of a grain.

4.—Thirty grains were put into a measured ounce of alcohol (sp. grav. as above) on the 13th of June. Twenty days after, namely, on the 2d of July, the remainder was carefully removed from the vial, and weighed. The loss was found to be five grains. There was no perceptible waste in this experiment.

5.—Twenty grains were put into a measured ounce of alcohol (sp. grav. as above) on the 13th of June. On the 2d of July, the remainder having been carefully transferred from the vial to the scales, weighed thirteen and seven-eighths grains. The loss, consequently, was six and one-eighth grains. There was no perceptible waste in this experiment†.

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*In this, as in the succeeding experiments, the phosphorus was first reduced to powder, after the manner of M. Leroi, in order to save time by facilitating the solution.

†An excellent mode of preparing solution of phosphorus for use has been adopted by Mr. Hudson, of the house of E. Fowler & Co, druggists of this city. It is to put ten or more grains of good phosphorus into an ounce, by weight, of absolute alcohol, contained in a strong vial, of the capacity of three or four ounces, which is to be carefully stopped and sealed. The alcohol is then heated in a water bath to 150 degrees, Fahrenheit, when the vial is taken from the bath and violently agitated; when cooled to 80 degrees or less, it is again heated as before; and this process is repeated a third time; after which it is rapidly cooled to 60 degrees, and the clear liquid decanted. The whole process requires hardly an hour, and the result is a perfectly transparent solution, ascertained by several trials to contain one grain of phosphorus to one hundred grains of alcohol. The exact strength of the solution, as determined by its specific gravity is 4.84 grains of phosphorus, in 480 grains of alcohol. This preparation has these advantages, that its strength is definite, and when dropped into water it gives off very little vapor.

The difference in the quantity taken up in the two last experiments, is to be attributed, no doubt, to the circumstance that the phosphorus used in the last, was more minutely divided. It is probable that neither solution was a saturated one, the time employed being too short to complete it. The pharmacopœias recommend the digestion in either to be continued four weeks; but I am satisfied that a solution in alcohol, to be saturated, requires a much longer time, but less, in proportion, as the particles presented to the alcohol, are smaller.

Care was taken, in the four last experiments, to see that the alcohol was really absolute, and the phosphorus pure; that it had the characteristics said to indicate a pure article, being translucent of a light amber color, and melting at 108 degrees of Fahrenheit. These precautions are indispensable in such experiments, for the reason that phosphorus kept in a bottle, in water, and exposed to the light, undergoes important chemical changes, among which is known to be the oxydation of a part of it. The same change occurs when it is put into alcohol containing water over that chemically combined in it. The oxyde being insoluble in alcohol, and the phosphorus insoluble in water, it is seen how much these circumstances might affect the strength of the solution.

The solution or tincture that I first employed medicinally, on the visible effects of which my opinion of its medicinal action was founded, was made with all necessary care, to secure a saturated solution of pure phosphorus in absolute alcohol. Both the phosphorus and alcohol were tested for this purpose. It was allowed to digest more than four weeks before any part of it was diluted for use; but only a small part being used at a time for this purpose, the digestion of the remainder was continued much longer. Now, if we suppose the solution made in this way was equivalent in strength to that obtained by Mr. Williams, after only three weeks digestion, about which I think there cannot be a reasonable doubt, it must have contained at least six grains to the ounce, and the diluted tincture six-tenths of a grain, instead of one-tenth, as estimated by Dr. Boling; and, consequently, that the dose recommended by me, was at least six times larger than his estimate of it.

Still the dose remains a very minute one; so much so that physicians who are in the habit of measuring the curative power of a

remedy, chiefly by its quantity, might find a difficulty in according any efficacy to one so small as this. The objection to it on this account occasions no surprise when made by those who have neither employed the drug medicinally, nor become acquainted with the peculiar activity of its physiological manifestations, in a much more minute quantity than is contained in the dose recommended.

If a solution containing one grain to the ounce, which is the strength of that used by Dr. Boling, be diluted with nine additional parts of alcohol, so that the dilution shall contain one-tenth of a grain to the ounce, and this be dropped into water, in the dark, it gives up a luminous flash with every drop as it touches the water. Supposing eight hundred drops to be contained in an ounce, the one-eight thousandth part of a grain thus becomes palpable to the sight Again: If sixteen drops of this very weak solution be put into four ounces of water, a teaspoonful of this mixture, containing half a drop of the tincture, or the one-sixteen thousandth part of a grain becomes palpable to two other senses, having a decided smell of phosphorus, and an aliaceous taste. Thus, it is seen that the dose estimated by Dr. Boling, though almost inconceivably small, as an arithmetical quantity, is really more appreciable than many large and very active doses of other medicines. But more than this.

If eight drops of this diluted tincture be mixed with one drachm, or one hundred drops of water, a single drop of this mixture, which contains but the one-hundred thousandth part of a grain, has a taste and smell of phosphorus strong enough to be easily recognised by those who are familiar with its sensible qualities.

But if four drops of a diluted tincture containing six-tenths of a grain to the ounce, which I suppose is the strength of that recommended by me, be put into an ounce of water, a teaspoonful of this mixture, which contains about the twenty-six hundreth of a grain, has a taste and smell of phosphorus strong enough to be disagreeable to most persons; and half a drop of this tincture, that is a drop of tincture of half the strength, will give off a luminous flash if dropped into water in a dark place.

The inference from these facts is obvious, but still like the opposing objection, is merely hypothetical. Let us for the better understanding of the matter reduce the argument of both hypotheses to a brief form

of expression, thus: on the one hand the implication is, that it is not reasonable to suppose, *for all former experience contradicts it*, that a dose of any medicine so small as the one-sixteen thousandth of a grain can have any medicinal power; on the other it is urged, that it *is* reasonable to suppose, *for, all former experience confirms it*, that a dose of an active poison large enough to be tasted and smelled, may have some power as a remedy. If phosphorus is an exception to all other known medicines in exhibiting its sensible qualities in a quantity so minute, so it may be, and is likely to be, an exception, in having a medicinal effect in a quantity in which such an effect could not be expected *a priori* of any other known substance. But if this is a reasonable supposition applied to the dose of a sixteen thousandth of a grain, to which only the implication in Dr. Boling's paper applies, it is much more likely to be true of a dose which is appreciable to the sight, as well as to the taste and smell, and whose measure is the quantity estimated by Dr. Boling many times multiplied.

Still the question remains unsettled. If the inquiry stop here one is placed in the dilemma of having to decide between two opposing hypotheses, neither of which establishes a truth. One may do very well as an offset to the other, but this is all; neither proves any thing, unless it be that the other is not true. Something more therefore is required on the one side or the other; and this is the test of direct observation.

After all, then, that may or can be said in arguments of this kind relating to subjects of physical inquiry, the matter has always to be brought for settlement to this arbitrament at last. And consequently the only efficient, perhaps the only proper reply to the hypothetical objection under review, as applied to the doses recommended and employed by me and others is that the point in question had already been tested over and over again by the actual experience of competent observers; not for a brief period; by a single individual; on a few subjects; or in the same place; but for a series of years, and by a number of physicians, and in a considerable number of subjects, some of them living in places widely separated from the others; all tending to give an assurance as strong as is usually obtained in regard to new remedies or to old ones introduced under new auspices, that phosphorus in the doses recommended by me, varied no doubt by others and

by an occasional want of proper care in the preparation of the tincture; but whether in doses larger or smaller, has exhibited medicinal qualities of the highest value in inflammatory affections of the lungs, but particularly in pneumonia, as they appear in this climate. Before publishing the results of my own experience with this remedy, I consulted several physicians as to whether their experience of its efficacy generally accorded with mine, but especially as to its sedative action in pneumonia, and found there was no discrepancy of opinion as regards either. Some were consulted verbally, others by letter. Among the answers to the letters there is one which so well expresses the uniform opinion of those who have had much experience with it, that I will take the liberty to extract a portion of it, though I have not asked permission to do so. The letter was written by physicians associated in practice, which will account for the occurrence of the plural pronoun, and was dated May, 1853. After saying that "no death from pneumonia has occurred in our practice since we adopted this treatment," they add: "as regards the effects of phosphorus in such doses as recommended in your treatment, so far as our experience goes, it fully concurs with your own. In almost every case in which we have administered it there has been a uniform and steady diminution of the frequency of the pulse with a marked quietude of the whole system. At first we were disposed to attribute this sedative influence to the aconite and quinine, but on suspending these and continuing the phosphorus, we found the same condition of things continued; and in no single instance have we found the pulse increased in frequency, or nervous excitement produced by the proper administration of this remedy." Among the other physicians of my acquaintance, some thirteen or fourteen in all, who have treated this disease by means, partly or chiefly, of this remedy in similar doses, I believe there is none, whose experience of its efficacy might not be properly expressed in the words of the latter paragraph of this letter; nor do I think there is one who entertains any more doubt about its curative power in the doses spoken of, than he does of the curative power of any other remedy that he may have ever employed in this disease. I ought not to omit, in this connection, a reference to the recorded statistics of the results of the treatment of pneumonia by this as one of the chief remedies, in which the mortality is found in sixty-eight consecutive cases,

occurring in the course of a little over four years, reduced to less than three per centum, including one case in which the patient evidently died of a brain affection, which complicated the attack from the beginning. The result of the treatment has been the same or even more favorable, as I have been informed, in the practice of other physicians.*

2.—The second conclusion of Dr. Boling from his experiments, in the order I have adopted, namely, that phosphorus is not a stimulant, I pass over, with the single remark, that my observation of effects in disease had led me to the same conclusion, as regards its medicinal action.

3.—We come now to the third and last conclusion, from these experiments, viz: that phosphorus is not poisonous given in an alcoholic solution.

This conclusion is derived from several experiments, in which doses of this medicine, dissolved in alcohol, of various sizes, were given to the two principal subjects, and were taken by himself. To *Sam*, the largest dose given was nine hundred and ten drops of tincture, containing less than a tenth of a grain to the ounce; the quantity of the tincture given being about an ounce, the dose was about the tenth of a grain. The largest dose given to the other subjects, was two hundred drops of a tinctnre, containing less than a grain to the ounce, or about the fourth of a grain. Dr. Boling took, himself, five drops of this tincture, three times daily; allowing eight hundred drops to the ounce, each of these doses contained about the one-hundred-and-sixth of a grain. As it is not material whether these doses are estimated with entire accuracy, we may leave out of consideration the probable waste in dropping them out, by the conversion of a part of the phosphorus into hypo-phosphoric acid, which probably escaped in the form of vapor, or may have been taken in the place of phosphorus proper.

In reference to the dose of two hundred drops, or one-fourth of a grain, given to the adult subject, we find the following comment:

*I do not wish to be understood here as intending to give an undue importance to this remedy, or to attribute to it alone the favorable results of the treatment of pneumonia with this and other remedies; I believe that aconite is equally valuable, and that quinine is frequently indispensable, while opium and blisters are valuable adjuncts, and tend very much to secure a favorable result. What I mean to say, is that in the treatment of pneumonia in this climate, it deserves to rank as a leading remedy, and one more valuable than any other single remedy I have used except aconite. Nor do I by any means intend in what I have said in defence of the dose I have been in the habit of using, to intimate that there is any specific dose which is peculiarly efficacious: What I mean to say is that, in my opinion, formed after much experience with larger doses, this is large enough for ordinary purposes in the treatment of pneumonia, while it is not too large to be perfectly safe. I have to day, however, conversed with a physician, who doubts the propriety of giving it even in this dose in cases where there is a gastric complication.

"How much further the dose might be augmented with safety, and without appreciable effect, I am at present unprepared to say; but reasons, I think will appear as we proceed, that will render it not improbable, that the quantity of alcohol, rather than any suppositious quantity of phosphorus, the preparations, as prescribed and given, may contain, should form the only necessary limit to the dose."

It is evident from this, as well as another instance in which Dr. Boling has expressed a similar confidence in its harmlessness dissolved in alcohol, that he had not studied either the medical or toxicological history of phosphorus very thoroughly. The confidence derived from his own very limited experience with it, in this form of administration, would not, I presume, have been entertained, if the larger experience of others in other modes of giving it, had been sufficiently consulted. The medical history of this article shows that it has been given with a like impunity in ether, or almond oil, or in substance made into pills, with conserves, in larger doses, and for a longer time than Dr. Boling gave it to either of his subjects, or took it himself; yet its poisonous properties in either of these preparations, does not seem on this account to have been questioned.

It is an established fact that phosphorus, in any mode of using it, exerts its poisonous power very unequally at different times, even in the same person, and in circumstances apparently alike in other respects; and this eccentricity is more striking, when its effects in disease are compared with its effects on persons in health. Thus, while it has been given occasionally or frequently in very large doses without doing any harm, it has been known to prove fatal in comparatively very small doses. M. Magendie quotes a number of authorities, showing that at one time the common dose given by physicians was two or three grains, and upward, even to twelve grains. Dr. Mentz, it is said, gave to one of his patients a dose of two grains made into a bolus, and repeated the medicine, three grains at a time, the next night and morning with the most happy effect; "quietude was immediately produced, with sleep and gentle perspiration." * In another case he gave six grains, also in two doses, with benefit. Wolff reports twelve cases,

* I had thought that the opinion of its exclusive stimulant operation, was universal among the older, as well as the more recent authors who treat of phosphorus, but I find, besides the example in the text, that its sedative action is affirmed in this way by several others. Thus M. Leroi, speaking of the luminous pills, first prepared by Kunkel, each an eighth of a grain, says: "They are endowed with a sporific and calming property."

extracted from the case book of his father, in which phosphorus was given in doses of two or three grains, from which "the results were so extraordinary that the author was induced to call phosphorus a divine remedy." M. Magendie also refers to Alphonse Leroi, who, having seen the German physicians give it in the doses of six, eight, and even twelve grains per diem, took three grains himself; but not with the same impunity he had seen others give it. In speaking of its effects, M. Leroi says it is a wonder it had not killed him.* Indeed, this kind of dosing could not be long continued; and we find accordingly, that the false security engendered by it was sadly dispelled by the homicides, reported or otherwise, committed in its use; or by its dangerous effects in a less degree. "In the midst of all this success," says M. Magendie, "Wickard, in the second part of his writings, related cases and experiments which ought to put people on their guard against the rash employment of this remedy." So, too, a society of physicians in London, in a report on the properties of phosphorus, in the London Medical Review, while commending its medicinal powers, add that, "this powerful and active remedy should be employed with the greatest caution." And this caution was so often repeated, as the disastrous effects of its employment in such doses became more and more frequent, when, at last, the eyes of the medical men were opened to see the danger, that it was soon banished from practice. After a time it was introduced into medical use again, in small doses, but was soon given up again. It was introduced a third time in still smaller doses, and a third time banished; and it is curious to observe, in tracing out the medical history of this potent drug, how the doses grew successively smaller and smaller as the experience of its pernicious effects, in doses still diminishing, was developed in the two last periods. In the first the dose was two grains, and upwards, as high as twelve; in the second, it was from a grain to the tenth of a grain; while in the third it fell from a fourth to a twentieth, fortieth, and even to the one-hundredth of a grain.† At the same time we find the warnings of its unruly and dangerous nature as a poison, quite as urgent and impressive in the writings of those who employed it in smaller doses, as in

* Eds. Dic. Med.

† From two to five or ten drops of a solution in ether, or almond oil, containing four grains to the ounce. The ethereal solution is the officinal preparation of the French codex; the dose is from five to ten drops, or from the fortieth to the twentieth of a grain.

that of those who gave larger ones. In all this time the various sized doses were not only given with frequent impunity, but with the highest commendations of their success as a remedy; and yet we find it no more able to maintain its place in medical practice in the smaller than in the larger doses. The doses had been reduced from six to twelve hundred times, and were still so much too large that physicians were compelled to stop prescribing it. The smallest quantity reported to have produced death in a single dose is about the tenth of a grain; another case is reported in which one-third of a grain was fatal: in another case, a grain taken in the course of four days, produced death. Dr. Chapman poisoned the only patients, three in number, he ever gave it to, though fortunately he did not kill them, with doses of one eighth of a grain. These are *reported* cases. How much harm may have resulted from giving it in doses from the twentieth to the one hundredth of a grain, frequently repeated, we have no especial report of; but that there must have been a good deal is evident, from the urgent cautions which are mingled with or follow the commendations of its curative effects.

The causes which thus render phosphorus harmless at one time, in a given dose, and at another a deadly poison, in the same or a much smaller quantity, have not elicited much inquiry. Its frequent disastrous effect in any dose that had been used, was supposed, by many, to unfit it for medical uses altogether, and there inquiry seems to have stopped. My limited experience has led me to believe that this eccentricity is connected more with the condition of the stomach when the medicine is taken, than with any difference in the modes of administering it; it is more apt, for instance, to show its deleterious qualities in case of abstinence, as in the experiments on myself, and as is ordinarily the case in febrile diseases, but more certainly, when to abstinence is added an irritable stomach from disease. A careful study of the cases I have been able to find reported, tends to confirm this opinion. However this may be, the fact that phosphorus is thus irregular or eccentric in the exhibition of its poisonous properties must, necessarily, derive the experiments of Dr. Boling of the weight they otherwise might be entitled to as proof that it is not poisonous in alcohol. A greater number of experiments, it appears, have been performed with other preparations of it, known to be poisonous, in which it proved equally harmless, in doses from four to

forty-eight times larger, than the largest given to either of his subjects; its administration has also been continued a much longer time than in his experiments with a like result. Alibert, quoted by Dr. Eberle, gave to some epileptic patients a grain a day for two months without any bad effect; on the other hand, a dose not larger than the largest, given to one of his subjects, has been known to prove fatal, while its bad effects, in doses from two to ten times smaller, but frequently repeated, served to banish the medicine not merely from medical practice, but to a great extent from the list of officinal medicines. Hence the impunity with which it may be given at times in certain doses, so far from being held as a proof that it may always be given with impunity in the same, or smaller doses, is rather looked to as a warning against trusting a poison so subtile and treacherous at all; or, at least, in any doses that had been employed, even to the one hundredth part of a grain. What is thus known to be true of phosphorus in any other mode of giving it may be, and I have no doubt is, true of its solution in alcohol; nor does it seem to me that the very few experiments of Dr. Boling tend at all to invalidate such a conclusion; for it is evident enough, from the preceding abstract of its history, that if they had been made with phosphorus in any of the modes of prescribing it heretofore in use, the results might have been just the same as they were when made with an alcoholic solution.

There remains to be said a few words about Dr. Boling's experiments on himself. I copy the greater part of the paragraph, containing his comment on them, and the conclusion they led to, as the best mode of presenting both fully and fairly before the reader. He says:

"In connection with this experimental practice on myself, I will again call attention to the views of Dr. Ames, in regard to the effects of phosphorus in their (his) doses. Thus, he says, that it cannot be continued in the smallest quantity mentioned, half a drop, for any great length of time, without inducing considerable disturbance of the stomach, shown by nausea, or vomiting burning heat, and a feeling of oppression at the epigastrium. Though he admits, that in the quantity of two drops, a single dose, or perhaps a few doses, may be given with impunity, yet he would evidently regard a lengthened use of it, in such a dose, as a very serious matter, and tells us of one instance in which dangerous effects resulted from the administration of three doses, of two drops each, at intervals of twenty-four hours. It is most desirable that we should yet

be able to discover and explain the cause of these discrepancies. While Dr. Ames tells us that doses of half a drop cannot be continued for any great length of time, without the most serious results, I have myself taken it in doses of five drops—just ten times the quantity—a long time, and for eight days, without omission of a single dose, without effect. While under his observation, for a cumulative action, dangerous effects resulted from three doses, of two drops each, administered at intervals of twenty-four hours, being in all six drops taken in the course of three days, yet I have taken, for eight successive days, three doses, of five drops each, or fifteen drops per day without effect. Indeed, unless I should discover something in its action which has never been manifested in any of my experiments, from my own experience with the article, and from all the lights at present before me, I should not hesitate, were it not merely for the trouble of the thing, to continue it in the same manner for years." These emphatic antitheses are continued in the succeeding paragraph, which I also copy.

"In the healthy subject, at least, any effect of the article resulting in nausea and vomiting, could be easily appreciated, and not readily mistaken; yet, not only did the subjects of my experiments take it in doses, as mentioned, so immeasurably greater than the doses with which such effects are said by Dr. Ames to have been produced by it, but they took it under circumstances that were well calculated to favor the production of such an operation. Thus, while they sometimes took it in the intervals between the meals, they also took it at times immediately before eating, and at others immediately after eating. On several occasions, I myself, having forgotten my dose, which I usually took just before eating, until I had partly finished my meal, have called for my phial, taken the dose and proceeded with my meal without disrelish, or any subsequent manifest effect."

We shall find, I believe, no greater difficulty in discovering and pointing out the cause of these discrepancies, which Dr. Boling thinks is most desirable, than was found in discovering the discrepancies themselves. The first step in this process is to point out some errors in these extracts which in themselves go a good way towards effecting this desideratum.

The first error that I shall mention is, that what I said of two drop doses is applied, inadvertently of course, to the half drop doses. The latter is spoken of by me only as liable to produce considerable disturb-

ance of the stomach when long continued; while the former are said, in effect, to be unsafe, *in the treatment of pneumonia*, if continued for any great length of time. The "most serious consequences" therefore, should properly refer to the effects of the larger doses only.

Another error arises from a wrong construction of the following passage. "Its effects are cumulative; that is to say a dose which singly is not large enough to produce any sensible effect may become very troublesome or dangerous after several repetitions at intervals *of three or four hours*. This *quality* was developed in one instance by repeating it in a dose of two drops of the strong alcoholic solution three times at intervals of twenty-four hours." Dr. Boling construes this to mean that dangerous effects resulted from the three doses given at intervals of twenty-four hours; but the reader will see that the troublesome or dangerous effects refer only to the dose repeated every three or four hours, and that the cumulative quality alone is referred to in speaking of its repetition once a day. Such I believe is the proper grammatical construction; it was at least what was meant. In point of fact, no dangerous effects were produced in this case. The subject took the two first doses without any sensible effect, but a short time after taking the third, symptoms of a pretty severe stranguary came on, which lasted several days. It is altogether probable, however, that if the dose had been repeated several times more, dangerous effects would have resulted from it. A third error, the source of which is in part explained in the two preceding paragraphs, is in the comparative estimate of the quantity of phosphorus taken by Dr. Boling and that given to my patients, to which the danger of serious consequences was ascribed, if continued any great length of time. Dr. Boling took five drops of a tincture containing less than one grain to the ounce, three times a day, supposing it to be a full grain—each dose was about the one hundred and sixtieth part of a grain—my patients took two drops of a tincture which, as we have seen, there are the best reasons to believe, contained at least six grains to the ounce; each dose, therefore, allowing as in the other case, eight hundred drops to an ounce, was about the sixty-seventh part of a grain, given every three or four hours, more than double the quantity of the other, and repeated twice as often. The doses taken by Dr. Boling, therefore, instead of being ten times greater, were less than half the size, or more than twenty times less than the estimate. The aggregate taken by him in twenty-four hours, was about the fifty-third part of a grain; the daily ag-

gregate given to my patients was from the eighth to the eleventh of a grain; that is, nearly from five to seven times greater. The half-drop doses, to which danger of exciting considerable disturbance of the stomach was ascribed by me, if continued long, being of the same tincture, contained, of course, one-fourth the quantity of phosphorus in the two drop doses, or the two hundred and sixty-eighth part of a grain; each of these doses, therefore, is somewhat more than half (as one to one and two-thirds) the size of each of the doses taken by Dr. Boling, and their daily aggregate in twenty-four hours, compared with that of Dr. Boling's doses, is larger by from a fourth to a third. The largest quantity given to either of his subjects in one day was two hundred and seventy-two drops of a tincture having less than a grain to the ounce, or about the third of a grain of phosphorus. The daily aggregete of the half-drop doses given to my patients, if repeated every four hours, is about the forty-fifth part of a grain; so that the difference, instead of being "so immeasurably" great as supposed by Dr. Boling, is only as one-third of a grain is to the forty-fifth of a grain; and the measure of the difference is almost exactly as fifteen to one.

But these are minor matters. By far the most important source of these discrepancies is in the different circumstances in which the medicine was taken by Dr. Boling, and given by me. Dr. Boling took it in health; I gave it in disease; an essential difference, which in the inception and progress of these experiments seems to have been entirely overlooked. It must be remembered that in all I had said of the effects of phosphorus, in the part referred to by him, I spoke of its effects in the treatment of pneumonia only. In this disease, in this climate, the stomach and bowels are usually in a condition to be most unfavorably affected by irritant ingestæ of any kind. This condition I took some pains to point out in my paper on the treatment of pneumonia, in rendering a reason for the more frequent poisoning by calomel and tartar emetic in its treatment with these remedies, in this than in higher latitudes. There is, in fact, with us a peculiar and decided tendency in this disease to take on gastric or gastro-enteric inflammation from even mild irritants. In this condition, phosphorus, an irritant poison, whose action as such is always directed especially to these organs, is much more likely to show its poisonous effects than in a healthy condition of these organs. Here, then, was an empty and irritable stomach, prone to take on inflammation from slight causes, to invite a development of the poisonous action of phosphorus.

On the other hand, Dr. Boling took the medicine in health and at his meals. The importance of this last circumstance as regards its probable influence over the action of the medicine is shown by the fact that some authors recommend it to be taken only on a full stomach, for the purpose of avoiding its poisonous effects. It is equally true of other irritant poisons, that a full stomach renders them less active as poisons, cæteris paribus. Dr. Boling, it is true did not take his doses, literally, on a full stomach, but taking a dose *just* before eating is, practically, or so far as concerns the irritant action of a poison whose action is slow in developing itself, the same thing as taking it during or immediately after a meal. Now this was not only the time at which Dr. Boling took doses of the one hundred and sixtieth part of a grain, but the time also when he gave to one of his subjects the largest dose that either of them took, namely, a quarter of a grain. The influence of health, exercise and the regular meals, in modifying its effects I have frequently witnessed. In the experiments made on myself in whom nearly all such influences were absent, each dose of three drops of a saturated solution gave rise to disagreeable and even painful effects on the stomach, as I have seen it do in smaller doses in the treatment of pneumonia; the effects on myself were severe enough to prevent me from carrying the investigation, as I intended to do, any further than the three experiments mentioned. Nevertheless, I have often prescribed in doses of one and two drops in chronic affections of the bladder or urethra, and in seminal weakness, twice or three times a day when the patient took his usual meals, and attended to his ordinary business without any such effects, though continued many days. If therefore Dr. Boling took the medicine, as seems to have been the case, in a condition of the stomach unfavorable to the development of its local action on this organ, and my patients took it in circumstances the most favorable for the development of such an action, we may find in this difference alone a sufficient explanation of the difference in its effects, even if the doses had been equal.

But leaving these minute, and probably, to the reader, tedious details, let us sum up, in a general view, the principle sources of the discrepancies between Dr. Boling's experience and mine; separating the apparent from the real. If I have written with sufficient perspicuity, it has been seen in the progress of this paper, that all the discrepancies are included in, *first:* the different circumstances in which the facts have been observed

on either side; *secondly:* in the difference between facts observed on one side, and inferred on the other; in other words, between facts of experience, and facts obtained by *a priori* reasoning.

In the first, the discrepancy is apparent, not real, even if we admit that the experiments gave accurate results. It may be true under this admission, as I have said before, and I think, shown, that phosphorus does not act as a sedative on sick persons; but does so act in certain forms of disease. The difference in the circumstances, health on the one hand, and disease on the other, in which the experience was obtained, might alone make all the difference apparent in its effects. So, too, as regards the poisonous action of this remedy in certain doses, its eccentricity, and other circumstances connected with the condition of the subject to whom it was given, may very well account for the difference in its effects. So far, then, there is no real discrepancy; a difference in the facts there is, because both were not looking at the same thing exactly; but there is no clashing or incongruity, as might be supposed on a superficial view of the matter. Either set of facts might be true, without in any respect impeaching the integrity of the other.

Not so, however, as regards the second. Here the difference is not between facts observed on either side; but between experimental facts on one side, and *a priori* inferences on the other. When Dr. Boling concluded, from his experiments, that phosphorus dissolved in alcohol neither acts as a sedative nor as a poison, in certain doses in disease, he overstepped the bounds of legitimate induction, and the real discrepancies began to appear. The utmost limits to which the experiments permitted him to go, is, that phosphorus is neither sedative nor poisonous, in the doses in which he gave it to persons *in health*; there was no legitimate warrant for the supposition that it does not so act in certain forms of disease. Dr. Boling saw correctly, in the circumstances in which he placed himself; but reasoned incorrectly from what he saw. His reasoning brought him to conclusions directly opposed to the experience of others; and, curiously enough, he supposed that the former are more reliable in proof of the matters in question than the latter; and affirms, in effect, that the naked inferences, shorn of an attempt, even, at verification, are the true facts; and the direct observations of others, made in the very circumstances about which he reasoned, but did not observe, are the false ones. Here, then, is a real and irreconcilable discrepancy, not,

however, difficult of explanation. The truth is, as we have just seen, Dr. Boling explored one field of inquiry, and I another; and seeing nothing where he was, concluded there was nothing to be seen where I was. He did not come over into my field, in order to see with his own eyes, on the spot; but chose rather to look from afar off, and through an *a priori* telescope, which hardly even permitted any thing to be seen as it really is. Looking thus from his position, he thought he saw better what was going on where I was, than I did on the spot; but in the nature of things this was impossible; and with the chances so largely against his seeing right, through this medium, it is no impeachment of the natural accuracy of his vision to suppose that he saw wrong.

The facts obtained in these different modes are incompatible; on one or the other side they must be wrong. The reader, who has now seen both sides of the argument, must determine the matter for himself. But if still in doubt as to which are the true, and which the false, and the subject is of sufficient interest to induce him to take the trouble, let him bring the questions at issue to the arbitrament of his own direct observations in the circumstances, in which, only, the sedative and curative, or the poisonous action of phosphorus, in certain doses, has been affirmed by me.

ART. V.—RESEARCHES UPON THE ANATOMY, PHYSIOLOGY, NATURAL *History and Cure of the Tape Worm or Tænia Solium.*

BY BENNET DOWLER, M. D.

General Remarks.—Anatomy and physiology of tæniæ.

An erroneous opinion has prevailed in regard to the study of parasitic worms, namely: that the physician's business lies solely in the treatment, and not in the natural history of these animals. But a knowledge of their origin, causes, laws of development, habits and natural history, promises more than anything else to lead to the best means for their prevention or removal. Here the physician, more than the mere zoologist, has a motive which should impel him to study. Although, on the part of the physician, the mere hope of enlarging the boundary of his knowledge of nature is laudable, yet, he has a higher purpose in view, namely: that of relieving suffering humanity.

Ignorance, empiricism and accident are less likely to lead to useful results than systematic and well directed investigations, and the more so, as the subject is profound and difficult. As a proof of the value of anatomical knowledge in therapeutics, it is sufficient to allude to the cephalic extremity of the tapeworm; for, whether there be a head or not, there is a minute, very long, threadlike portion, supposed to be the anterior end of the worm, whence the numerous large joints are, doubtlessly, derived or developed. Thus the discharge of a hundred yards of fully developed tapeworm from the caudal end promises little if any permanent relief, while the discharge of the cephalic and almost miscroscopic end guarantees a cure. This subject will be resumed hereafter.

In C. Bonnet's works, (7 vol., 4to.,) published more than a century since, containing an elaborate description, with plates, illustrating the tapeworm, microscopic views of this animal's head have been given. These have been, as I venture to think, servilely copied by his successors, though some have deviated so far as to give a sharp, spear-pointed figure to the head, as in Gregory's Practice, (1831) and in other works.

In his pathological anatomy, Prof. Vogel, (tapeworm, cestoidea) describes the head of this parasite as being "very minute, only becoming evident by the application of magnifying glasses. It then exhibits four lateral mammillary suckers, and between these, in the middle of the head, an arched eminence, in whose centre a minute, scarcely perceptible opening exists, upon this are sometimes seated hooks in double rows." 423. The internal evidence derived from this and many similar statements, as well as from physiological anatomy, shows that the functions ascribed to the microscopic head and cephalic microscopic hooks of this animal, are imaginary or rather impossible. No one pretends that he has seen the animal feeding by its supposed microscopic mouth, or microscopic suckers, or holding fast to the bowels by its microscopic hooks, as will more fully appear in another part of this paper.

Prof. Vogel continues: "the growth of the worm takes place in this manner: from the neck, new joints are continually evolved, which push those behind still further back, and develop themselves in proportion as they regress. The hindmost joints are, therefore, the oldest, and at the same time the most perfect; yet it appears that new joints are developed not only from the neck, but may be introduced between

the perfectly developed articulations, even at the posterior end of the animal"—423.

Sir A. Carlisle, says that the head of the tapeworm, when accidentally lost, is regenerated—an assertion difficult to disprove, but still more difficult to prove or believe.

The compilers of the Cyclopædia of Practical Medicine say, "authors who were ignorant of the animal possessing a head, or who thought that it was made up of mutually adhering cucurbitine worms, asserted that new joints were formed at each end to replace those passed by stool. From the time that the head was detected, Andry and others began to believe that it was only to the hinder extremity of the body that such addition was made. At length Pallas and Muller showed that the increase did not take place at either extremity, but that the elongation occurred chiefly in the filiform portion of the animal next the head. The joints of the young tapeworm are so very close and small as to give it the appearance of being merely wrinkled rather than articulated; but on inspection through the microscope it is found to consist of innumerable articulations. The received opinion of the present day is, that the animal get no new joints, but only develops those which existed from the first in a rudimentary state." Prof. Vogel admits, as above mentioned, that new joints are developed at both extremities of the worm, as well as "between the perfectly developed articulations." Developmental analogy in comparative physiology rather favors the doctine that the new joints are evolved from rudimentary forms which are multitudinous.

Several of the most eminent writers persevere in the statement, that the *hindmost* articulations are the *oldest*, the others being developed in *front*. To the same effect is the account given by Gluge in his excellent work on Pathological Histology, (Prof. Leidy's translation, 1853.) Thus in Plate X, Entozoa, fig. 9, *a*, the small end, which is the cephalic extremity, is called "*the youngest and anterior part of the body;*" and that part represented by fig. 9, *b*, *c*, is termed "*the posterior and older part of the same.*" Is not this account wholly erroneous? Have not micographers and naturalists shown, and does not analogy teach, that the anterior or cephalic extremity consists, not of newer, but more crowded rings, the rudimentary elements of all the subsequent articula-

tions—which latter on being fully developed and matured, ultimately drop away? Minuteness does not prove seniority.

Prof. Vogel says: "when the last joints have attained to their perfect development, and are filled with mature ova, they detach themselves, and either uninjured, or in a state of decomposition, are evacuated by stool. Since the joints which are thus thrown off, are continually being replaced by new ones, it is absolutely necessary, in order that the annoyance caused by the worm shall cease, that the whole animal, including even the extremity of its head, should be evacuated."—424.

Bremser, a high authority, in helminthology, contends that no one has ever seen an entire tapeworm, because the caudal, or posterior articulations, impregnated with ova, detach themselves and pass off, while those next the head develop themselves unceasingly.

There has been much blundering about this worm both in and out of the profession. If the animal is developed from the head, backward, it is an important point in the cure, to ascertain whether the head has been discharged, for the frequent discharge of the posterior joints may be natural, not an indicant of the death or decay, but of the growth and activity of the worm.

Bremser mentions, as an important practical fact, "that of the many hundred persons cured by him of the tapeworm, not a single individual has seen the head come away." Now this is not at all surprising, because the general opinion represents the head as a large and terrific one, whereas, it is a microscopic object, and, for this reason, likely to escape the most careful observation.

As a proof that the anatomy of the head of the tapeworm has not been satisfactorily demonstrated, it is only necessary to read the different and conflicting statements by microscopists of the greatest eminence.

The hooks which surround the reputed mouth of the tapeworm, are variously described, both as to shape, number, and function. Invisible to the naked eye, they are, nevertheless, said to be used by the animal for the purpose of laying hold of the mucous membrane of the intestines. That an animal, often many yards in length, should anchor itself firmly in the bowels by these microscopic hooks, is against all analogy. A Man-of-war might as well use for its anchor a feather.

In the Museum of Animated Nature, (vol. II, 423, fig. 3906,) these hooks are represented as amounting to thirteen, while many authors

mention only four, or an indefinite number. In the Cyclopædia of Practical Medicine it is said that "the interior of the animal is made up of amorphous cellular tissue, without any abdominal cavity, and that it is often extremely difficult to detect the head of the tapeworm. The head, which is a minute tubercle pisiform body, terminating the thread-like neck, has on its most prominent and anterior part a circular aperture, or depression, surrounded by sharp, curved processes or hooks, and supposed by some to be the rudiment of an alimentary canal. These processes are, however, not constantly found; being supposed by Bremser to disappear by age. On the somewhat angular projection on the sides of the head, are situated four equi-distant apertures, or suckers, which seem to be two mouths. The depression in front, by some called the mouth, is so minute as occasionally not to be visible without the aid of a microscope. The head is a third or a fourth part of a line, or even less in width."

This account is inconsistent with itself; for if "the animal is made up of amorphous cellular tissue without any abdominal cavity," (which is the true exposition) the assumed suckers and mouth could not perform the suctorial operations like the leech. It will be shown in this paper, according to the latest and best authorities, that these organs and functions are imaginary.

The Cyclopædia already quoted, continues,—"it is by the suckers on their head that they (tæniæ) stick to the walls of the intestines so firmly, that if force be applied, they break rather than lose their hold,"—a hold by means of the microscopic suckers of a microscopic head! while "prone—long and large, full many a rood," this intestinal "leviathan with fixed anchor, lies stretched out huge in length," holding fast by invisible suckers! Milne Edwards, says that the tænia has no distinctly formed mouth—(la tête ne présente pas de bouche bien distincte.—(Zool.)

M. Beaude, (Méd. Dict. 2 vols., 1849,) described the head, suckers, and hooks of the tape worm as indiscernable by the unassisted eye; they can only be seen (appréciables seulement,) by a lens.

Prof. Cruveilhier, (Entozoaires, Dict. de Méd.,) says that the head of the tænia cannot be seen without a strong magnifying glass—"le tête est très-petite et ne peut être vue qu' à l' aide d'une forte Loupe." vii, 325.)

Miscroscopic suckers which take a hold so firm that the body will break before the animals will let go their hold! By some writers tænia are mentioned as being more than half a mile long! Thus, Pliny mentions one three hundred cubits long; the Copenhagen Transactions, one of eight hundred ells; Rosenstein one of three hundred feet! Robin, cited by Bremser, relates, that a patient who had died after voiding several feet of tapeworm, there was found a remainder, extending from the the pyloris, to within six or seven inches of the anus. Prof. Stone, of the University of Louisiana, informed the writer of this paper, that on making a post-mortem examination of a Russian, in this city, he found a tape worm, which from his description must have been much longer than that last mentioned.

"Van Doeveren mentions a peasant who, having taken an emetic, vomited up forty Dutch ells of tapeworm, and might have got clear of more of it, 'if he had not been afraid of puking out all his guts, and for that reason bit the worm off.'"

Physiological or teleological anatomy, often a most reliable guide in such investigations, must repudiate these microscopic hooks as instruments by no means adapted to fix so huge an animal to the intestines. It must equally reject the four microscopic suckers as organs unsuited to the alimentation for so large an animal, which has not any structural adaption for this suctorial function.

Comparative physiologists, who assign to the tapeworm the functions of a colossal leech, ought to point out a suctorial apparatus, an intestinal canal, and anatomical adaptions thereunto belonging.

Distrusting my own microscopic and anatomical researches on the tapeworm, I beg leave to call the student's attention to a late learned and reliable authority, namely, Professor Siebold, whose work on the Anatomy of the Invertebrata has recently been translated and annotated by a name dear to science, now no more, the late Prof. Burnett, who died in Boston a few weeks since, and who had already acquired, at the early age of 26, the reputation, at home and abroad, of a profound naturalist.

In this able work, Prof. Siedbold's sixth class, helminthes is divided into six orders — the second of which, cestodes—includes tænia. He thus describes the latter:

"The panenchymatous body is ribbon-like, having often incomplete transverse fissurations; often it is wholly divided transversely into rings.

Digestive organs are wanting. The genital organs of both sexes are combined in the same individual; copulative organs are present; the four cup-like cavities at the cephalic extremity *are imperforate, and not so many oral orifices leading into an alimentary canal.*"

In his first order of helminthes, the cystici, and in the second cestodes, (including tænia) and in the fourth order, the acanthocephali, "*there is neither mouth nor alimentary canal;* in the first two orders there is, however, a system of vessels which may be regarded as a digestive apparatus; but these are designed for circulation, rather than for digestion, since their walls are complete throughout, and have no openings as has erroneously been supposed, which communicate with the suckers of the head; and their contained nutritive material is received by them *through the skin in an endosmotic manner.* It has already been observed that the four suckers of tænia, regarded by Nitzsch as oral orifices, are imperforate at their bottom. Owen (Cyclp. Anat. II. 131) has fallen into a similar error in regarding these organs as mouths,"—III. 112.

"The system of vessels designed for the circulation," alluded to in this passage, is supposed to relate to what has been called the *water vascular system*, not blood vessels proper.

Muscular System of Tæniæ.

Prof. Siebold says there is a distinct subcutaneous layer of longitudinal muscular fibres in the rings of the tænia, forming also sucking cups, cavities and septa.

Bremser, who has seen the tapeworm live for some time after it was passed from the bowels, says the head and neck were in continual movement, and that the body was alternately elongated and contracted.

Prof. McGugin's account of the tænia of the catfish (published in the September number of this Journal) agrees with Bremser's as to the alternating elongations and shortenings of the body of this animal during muscular contractions. These do not, however, constitute the sum of its powers in locomotion; they doubtlessly give rise to the pains and disturbances of which patients often complain, and describe as pinching, gnawing, crawling, &c. These animals can execute the most complex and extraordinary motions. A lady for whom I prescribed kousso, discharged a living tapeworm, which had doubled up a considerable portion of the larger part of its body, forming a symmetrical duplication, around the middle of which it threw a single transverse band, tying therewith a very firm, beautiful bow

knot, which, probably, cannot be untied without breaking the tape-like bundle to pieces.

Nervous System of Tæniæ.

In the order to which the tapeworm belongs, no nervous system has been found. The late Prof. Burnett, in a note to Prof. Siebold's Invertebrata, (1854) says: "Blanchard (1848) appears to have distinctly made out a nervous system in tænia. With *tænia serrata*, there are, directly behind the proboscis, two small medullary nuclei united by a commissure; from these pass off on each side a nerve which is distributed to the lateral parts of the head, and connects with a ganglion situated at the base of each sucker, which sends filaments to the muscles of the last.—Posteriorly there are given off filaments which run parallel to the intestinal tubes. This, however, has not been confirmed by other observers, and Agassiz has made a statement in a private letter to me, which is worthy of notice. He says: 'I believe the nervous system described by Blanchard to be bands of muscular fibres which cross each other between the fossæ of the proboscis; at least, this is so in the new species of tænia, from *Amia calva*, which was observed alive for several hours; and I could discover no nervous threads, but only muscular fibres, which had exactly the arrangement of Blanchard's nervous system.'"—109.

Circulatory Apparatus in Tæniæ.

Prof. Siebold says, "a circulatory system has not yet been found with certainty in the helminthes"—117. The vessels, canals, and capillaries which exist in them, are not blood vessels, but belong to what he has termed "the water vascular system," "and contain some times water only, sometimes a peculiar secretion."

Generation of Tæniæ.

All experience seems to warrant the conclusion that no species of worms which are found out of the body can live long when introduced into the human intestines, while, on the other hand, it is equally true that the parasitic worms which inhabit the alimentary canal, die when they pass out of it.

The late Prof. Charles Caldwell, of Kentucky, maintained that, inasmuch as the parasitical worms, round and flat, are found in the alimentary canal, and as he affirms, no where else, they must be "produced by abnormal secretion," "equivocal generation;" "nature having the power to produce living beings without the instrumentality of specific parentage

all the result of settled laws;" "external nature does not produce these animals; from without they cannot be received; with neither our food nor drink are the animals or their eggs swallowed. Both the tænia and the ascaris lumbricoides, and, I believe, the trichuris also, have been found in the alimentary canal of the fœtus in utero. The tænia moreover has been often detected in the livers, muscles, and other inaccessible parts."

* * * "A worm was found in the liver of Mrs. H., a lady of some note who died in Philadelphia, of an unknown disease. The worm was so large as to occupy a great portion of the viscus that contained it, and was of a species unknown to every naturalist who examined it. The dissection was witnessed by the late Prof. Rush, who spoke of it in his lectures; and the fact is recorded in the American Museum."

Although the experiments of Dr. Crosse, with galvanic electricity, were, as some persons thought, favorable to the doctrine of spontaneous generation, yet, the microscopic *acarus crossii* thus generated does not seem to be at all conclusive in this behalf, as has been argued in this journal by the late Dr. Hort, not to name others; these experiments by no means prove spontaneous generation, because the microscopic germs, or ova of the aforesaid insect, may have been introduced into the solutions, or into the air contained in the apparatus used by Dr. Crosse during his prolonged experiments.

The theory of spontaneous generation, of which the tapeworm has been supposed to be an indubitable instance, is wholly inadmissible. All analogy, all research, all zoological and physiological discoveries, tend to disprove or repel this theory, and to reduce, more and more, the number of supposed examples which have been brought forward to sustain it.

The tapeworm is doubtlessly subject to a series of metamorphoses, alternate generations, the phases of which are to the uninitiated little short of incredible, and to all extraordinary, though recent researches have thrown a brilliant, increasing, and even an unexpected light upon these hitherto obscure transformations of this parasite.

Whether the tapeworm be, as some contend, polyzooic, that is, an aggregation or colony of individuals conjoined to each other; or whether it is monozooic, that is, a single animal, will not be here inquired into.

Recent researches seem to show, that the tapeworm undergoes a series of transformations, called alternate generation, before it is developed in

the human intestines as a squatter; where only it gets the name and form, and assumes the functions now attributed to it. With respect to several other animals, at least, naturalists have ascertained that the offspring does not resemble the parent until after several generations shall have elapsed. Thus naturalists have ascertained that some insects pass through numerous metamorphoses, some of which take place in the bodies of other animals, and yet the offspring bear no resemblance to the parents until after from three to eight generations, whereupon they complete their double cycle of metamorphoses and alternate generation; returning to their original type or form, so that the great-grandchildren alone, and not the intermediate generations, resemble their ancestral primordial types.

To the casual, uninitiated observer one of these animals will seem, in its different stages of parent, child, grandchild, adult, &c., as so many separate and distinct animals.

Metamorphosis and alternate generation, are not antagonistic, but may co-exist, or co-incide in the same individual, so that a series of different metamorphoses through a series of generations may be necessary to reproduce in a gradual and progressive manner the original and perfect animal like that which was its progenitor—each change thus becoming a step towards the perfect finality, though the mode of generation may have been for each stage different—the first may have been by ovulation, the next by gemmation, another by fissuration, and so on. It would be too tedious to quote, on this occasion full illustrations of these principles from the ablest and latest authorities.

"At certain seasons of the year, the Sculpins of the Baltic are infested by a particular species of tænia or tapeworm, from which they are free at other seasons. Mr. Eschricht found that, at certain seasons, the worms lose a great portion of the long chain of rings of which they are composed. On a careful examination he found that each ring contained several hundred eggs, which, on being freed from their envelope, float in the water. As these eggs are innumerable, it is not astonishing that the Sculpins should occasionally swallow some of them with their prey. The eggs being thus introduced into the stomach of the fish, find conditions favorable to their development; and thus the species is propagated, and at the same time transmitted from one generation of fish to another. The eggs which are not swallowed are probably lost.

All animals swallow, in the same manner, with their food and in the water they drink, numerous eggs of such parasites, any one of which, finding in the intestine of the animal favorable conditions, may be hatched. It is probable that each animal affords the proper conditions for some particular species of worm; and thus we may explain how it is that most animals have parasites peculiar to themselves." (Agassiz & Gould. Prin. Zool—171–'2.)

The cause and the condition—the creation and the development of animal life and form belong to different categories. The seed enveloped for thousands of years in the cerements of an Egyptian mummy vegetates when it finds the essential conditions of development, as heat, moisture, earth; yet these do not create or produce the seed itself. Thus the ova of the tapeworm may be taken into the stomach in drink, or in animal or vegetable food, and on reaching the intestinal canal, may find the sole and essential conditions in which they can be developed into the form of a tænia, all other ova being lost or remaining undeveloped.

Prof. Siebold says:

"Although most of the Helminthes propagate by means of genital organs, yet there are a few species which multiply by *fissuration* and *gemmation.*

"The *fissuration* is always transverse, and differs from that of the protozoa and zoophytes in the fact that complete individuals are not produced, there being only a separation of certain organs from the perfect animal, as, for instance, that of the segments of the body in cestodes. This fissuraton is complete or incomplete. In the first case, occurring in the tænia, the segments are detached from the body, and continue to live independently, without, however, ever forming a new individual."—119.

"In the eggs of tænia, the envelop is colorless, and of a very variable, and sometimes quite remarkable form."—123.

Prof. Burnett inclines to the opinion that their development is not from eggs, but buds.—129.

"The entozoa," says Gluge, "must cease to form an especial zoological order, and several of them are now known to be only different stages of development of the same animal. The cysticercus is only a young tænia, and this is only a trematode worm without a digestive apparatus. The acephalocysts and echinocci also appear to be only steps of development of cysticercus."

"Some years ago, Dr. Kuchenmeister, of Zittau, had made use of the cysticercus pisiformis, a species of encysted parasite very common in the peritoneum of hares and rabbits, for a series of experiments, in which he caused these parasites to be swallowed by dogs and cats; in the expecta-

tion that they would become developed into tapeworms in the intestine of these animals. The trial succeeded perfectly with dogs."

In 1852, "Professor Von Siebold made a report on the experiments which were undertaken some months previously, in the Physiological Institute under his direction, for the purpose of showing the possibility of a transformation of the cystic parasites into tapeworms." The following results were obtained from the experiments "with the cysticercus pisiformis: These entozoa, which are usually about the size of a pea, were given to young dogs, mixed with milk, still inclosed in their peritoneal cysts, and in quantities varying from thirty to sixty." These dogs were subsequently killed, at various periods from three hours to eight weeks:

"After eight weeks, the cysticerci in the intestine of the dogs had attained a length of many inches. The largest were thirty-six to thirty-nine inches in length, and their posterior articulations were provided with a perfectly developed sexual apparatus, and contained many mature ova. Several of those a yard long had already thrown off their posterior articulations, with their mature sexual products. Von Siebold was now able to recognize, in this tape-worm, developed out of the cysticercus pisiformis, the tænia serrata of the dog. The cephalic extremity, the form of the articulations, the structure of the generative organs, and particularly the mature ova of this tapeworm, corresponded in the most perfect manner with the same parts in the tænia serrata. There was no longer any doubt that the cysticercus pisiformis of the hare and rabbit bore the same relation to the tænia serrata of the dog, as the cysticercus fasciolaris of rats and mice to the tænia crassicollis of the cat. Furthermore, the tænia serrata is rarely met with in the intestines of parlor and house dogs; but is, on the contrary, very abundant in hunting dogs; no doubt because the latter are often allowed to devour the entrails of hares killed in the chase, swallow at the same time the cysticercus, and so become infested with the tænia; a circumstance that would naturally be less frequent with parlor and house dogs."

Professor Siebold had already, in 1844, expressed the opinion that the parasitic cysticercus (C. fasciolaris,) found in the liver of rats and mice, was nothing else than an abnormal, dropsical tapeworm; and that it was, in reality, identical with the tapeworm of the cat. (Tænia crassicollis.) He maintained further, that the cysticercus fasciolaris, like all cystic worms, was invariably destitute of sexual organs, and could not multiply its species by generation, unless it were transferred to a favorable locality, where it might lose its dropsical condition and develop its sexual organs. These changes actually take place when a rat or a mouse, with a cysticercus fas-

ciolaris in its liver, is devoured by a cat. The parenchyma of the liver, according to Siebold, is digested in the stomach of the cat; but not so the entozoon. The parasitic animal loses only its dropsical appendage, and passes, with the digested food, from the stomach of the cat into the small intestine. It then finds itself in a favorable locality, and becomes developed into a perfect tapeworm, with articulations and sexual organs, (tænia crassicollis.) This idea had been first suggested to Prof. Siebold by the perfect resemblance between the cephalic extremity of the cysticercus fasciolaris and that of the tænia crassicollis; and by the fact that there are often found, in the intestine of the cat, several specimens of the tænia crassicollis, in different stages of development.

Although Siebold's experiments with the other species of encysted entozoa, mentioned above, were not entirely finished, he had yet carried them so far with the "cœnurus cerebralis" as to convince himself that this worm also, which is so much dreaded by sheep breeders, becomes developed, in the alimentary canal of the dog, into a tapeworm. (Cited by Prof. Dalton, Buffalo Med. Journal, 1853.)

"M. Blanchard has investigated the habits and mode of development of the fasciola hepatica, an entozoon, very common in the livers of th eox, sheep, &c. In all his researches, M. Blanchard never found any but *adult animals* in these affected livers. In spring, myriads of their ova could be detected in the biliary canals, and ductus choledochus, and throughout the whole length of the intestinal canal; the nearer these ova approached the extremity of the intestines, the more fully developed did they appear. M. Blanchard considers it evident that the ova undergo incubation in their progress, are cast out of the body with the fæces, and again taken into the body with the food in a farther stage of development; on this latter point, however, he had no data for speaking decisively. In the batrachiæ, whose intestines swarm with entozoa, M. Blanchard has never found any but adult, or very nearly adult trematodes. He considers it very probable that in the human subject, the eggs of tænia, &c., are swallowed with the food; in some countries tænia seem endemic. M. Blanchard has never seen worms in the fœtus—their existence is, however, placed beyond all doubt by M. Grätzer."*—Ranking's Abs.

The British and Foreign Medico Chirurgical Review, (Oct., 1852,) in a critical notice of Van Beneden's work on cestoid worms, holds the following language:

"The labor of such investigations must have been very great. A cestoid worm is not to be found in all its stages in a single animal. Dis-

*Comptes Rendus, p. 355, 1848.

covered in a cartilaginous fish in an adult state, for instance, the prey of that fish had to be found out, and in these animals the search was carried on for the earlier forms; perhaps, again, the worm was to be found, in its very earliest state, only in the food of these;—so that the youngest form of the parasite of a ray might be ultimately traced to a crustacean, to a mollusk, or to an annelid."

"Those alone," says Van Beneden, very truly, "who have worked far from their study, know what labor and trouble these researches cost; how wearied one gets in soul and body, when one has to look to everything oneself; to seek out fish, then to hunt for their parasites; when these are found, to examine, to dissect, to draw, to preserve them; to make one's notes, and to do everything in a short time,—for, generally, they die very soon, and then undergo immediate alteration."

"In whatever manner the young cestoid reaches its proper locality—the intestine—the succeeding changes are the same. The tail-end rapidly lengthens and enlarges; it becomes divided, in most cases, into well-marked articulations, in each of which a complete set of generative organs is formed. The development of the joints takes place, so that the hindermost is the oldest—the others being developed successively *in front* of one another; differing in this respect from the development of new segments in annelids and myriapods, in which the new formation of segments takes place between the last segment and the last but one. To this state, which is that under which the cestoid worms are commonly known, Van Beneden has given the name of strobila, from a just perception of the close analogy between it and the strobila condition of the medusæ observed by Sars.

Finally, the segments become detached and free, the posterior segment usually dropping off very early; and for these detached segments, to which the old writers gave the name of 'cucurbitary worms,' from their resemblance to gourd seeds, Van Beneden retains the name of proglottis, first applied to them by Dujardin. The proglottides become rounded off at each end, and take on a certain rough resemblance to a distoma; never acquiring, however, any trace of the complicated organization of the latter, though they exhibit independent contractions. They are, as Van Beneden himself expresses it, but 'a sort of sheath to the sexual apparatus.'

How long they are capable of maintaining this form is wholly unknown; but it seems probable that, discharged among the fæces of the animal in which they were formed, they become the food of some of Nature's many scavengers,—of some mollusk, or osseous fish, for instance, in whose intestine the eggs are set free, and the embryos developed into their 'scolex' form. In this state, it would seem that they remain until the mollusk, or osseous fish, is devoured by one of the carnivorous plagiostoms, or some other higher animal, when the cestoid worm starts from its wholly buried and half-dead state, into new life.

By the discovery of these extraordinary facts, the problem of parasitism among carnivorous animals seems to be pretty clearly explained."*

Topography of Tapeworm.

The British and Foreign Medico-Chirurgical Review, in an article on the endemic diseases of Sweden by Dr. Magnus Huss, recently published at Stockholm, quotes thus:

"The inhabitants of the coast of the Baltic, at the head of the Gulf of Bothnia, from Tornea to Pitea, are grievously infested with tænia-lata, or tapeworm. Dr. Wretholm states that in Haparanda, the suburb of Tornea, there is scarcely a family, whether rich or poor, of which some of the members are not afflicted with this disease. He has observed it in children at the breast, who had never imbibed anything but their mother's milk. From the coast the disorder spreads inland, following the course of the largest rivers; and it is ever most prevalent where large surfaces of water are found.

Dr. Wretholm observes:—'As the result of the attention I have paid to this subject for several years, I may observe that the prevalence of tapeworm is in a direct ratio to the nature of the soil, and of the water used for drinking. In the higher tracts of land, where the drinking water is derived from springs or from the mountain brooks, tænia is hardly known; but as soon as we descend to lower ground, and especially to the banks of the larger lakes and rivers, where the houses are often built on the former beds of the lakes, and where the water, coming from morasses and bogs, is impure, and derived from wells sunk in the soil, and filled with extractive matter rapidly passing into decay, there tapeworm prevails among persons of both sexes, and in every condition of life.'—(p. 5.)

In Gestrikland (lat. 60°, 61°) Tænia-lata is, again, common as in Lappmark, and the reporting physicians ascribe its prevalence to the great consumption of salmon, a fish much infested with tænia. Is, then, the tænia of the salmon identical with that found in the human species?

The district of Halland is one of the poorest of Sweden; and here, consequently, uncleanliness is seen in its highest degree. Ascarides are likewise common here; and strangers, who have never before been affected by these parasites, soon complain of their presence when they have settled in the country.

In the district of Smaland, children are constantly infested with the ascaris lumbricoides, while tapeworm but seldom occurs. Children of all ages, and all conditions of life, are equally subject to these tormenting parasites; and females often suffer from them during their whole lives."

Helminthologists of the West, in their views of the topography of the tapeworm, seem to attribute too much importance to rivers, swamps, humid-

*"We are heartily sorry to find that such labors have seriously impaired the Professor's health, and that we must not hope for a continuation of these researches from the same learned and conscientious observer. We trust, however, that his prognosis is not so trustworthy as his observations, and that we may yet have a renewal of the long series of contributions with which he has enriched the Comparative Anatomy of the Invertebrata."

ity and low lands. In the East, in the very land where kousso flourishes, tapeworm is immeasurably more prevalent than elsewhere. These lands are high, dry, and to a great degree devoid of rivers, swamps, and depressed basins, as in Arabia, Abyssinia, and the coasts of the Red Sea.—Mr. Surgeon Vaughn, at Aden, Arabia Felix, in a letter to the London Lancet, says almost all, if not the entire population of those regions, are more or less affected with tapeworms, the kousso being a special blessing of Providence—a medicine within the reach of the poorest shepherd, and infallible in its effects.

Tænia is prevalent in Egypt, Russia, Poland, Switzerland, in most departments of France, and in some other countries of Europe, both insular and continental. In America its topography does not seem to excite, as yet, the attention of helminthologists.

The writer can affirm from personal observation that the hilly districts of Western Virginia are incomparably freer from tapeworm than the low lands of New Orleans.

Diagnosis of Tænia.

The diagnosis of tapeworm anterior to the discharge of a portion of its joints, per anum, is often obscure, seldom amounting to more than presumptive evidence. Among the numerous symptoms indicative of the presence of this worm in the intestines, the following are the most common: Convulsive motions in sleep; epileptiform fits; headache; disturbed vision; dark areolæ around the eyes; noises in the ears; itchings in the anal and nasal passages; capricious appetite; morning sickness; water-brash; eructations; vomiting; diarrhœa; intermittent colicky or gnawing or pricking sensations in the stomach and bowels; palpitation; sharp features; debility; emaciation; swooning; febricula.

Tapeworm produces, as might be expected from its enormous size and length, great central irritation, which is speedily radiated to the whole system. It produces irritation, without doubt, as a foreign, inert body in the bowels, and, also, as a living, moving parasite. Not that the animal bites and hooks into the bowels, as credulous pathologists gravely assert When its large body, with angular serrated margins and articulations, distributed through twenty or thirty feet of the intestine, are all put into commotion, alternately contracting and elongating themselves, and even forming convolutions and dense knots, it is by no

means wonderful that the patient should suffer gnawing pain and constitutional disturbances. In one of my patients, who discharged a tapeworm after taking kousso, the worm tied a part of its body into a very artistic not.

The terrible effects attributed to the microscopic mouth and hooks of the tapeworm, are doubtlessly due to the violent contractions and motions of the animal. A late reviewer accounts for the patients' sufferings as follows:

"Consider the common tape-worm, for instance, with the general structure and habit of which troublesome parasite we have been long tolerably well acquainted. It is known to be found in the intestine of vertebrate animals, and of these only. The head with its four suckers and its proboscis, armed with strong hooks, is fixed in the mucous membrane of its unhappy host, whilst its jointed body 'lies stretched for many a rood' in the intestinal cavity."

The following statistical ætiological statement, by a physician who practiced in the Danubian districts, indicates certain precursory and co-existent diseases, with which tapeworm is, by him, supposed to be connected.

"During the period of twenty years, Dr. Wawruch treated two hundred and six persons affected with tænia; seventy-one men, and one hundred and thirty-five women. The oldest person was aged 54; the youngest, 3½ years. Twenty-two were under fifteen years of age; and among these were six girls who had not menstruated. Most of the patients were from fifteen to forty years old. The patients belonged to the middle and lower classes; they nearly all inhabited the district lying along the Danube, or lived in low or damp dwellings. They followed very different occupations, and their mode of life was various. It is remarkable that the same conditions which give rise to scurvy in Vienna also produced tapeworm. The articles of food which seemed chiefly to engender tapeworm, were bad bread, meals of milk, butter, cheese, potatoes, pork and mutton, and bad water. The diseases which preceded the formation of tænia were gastric and cutaneous affections, but especially gastric and intermittent fevers. There were observed forty-three cases of intermittent fever, twenty of gastric fever, sixteen of typhoid fever, ten of ringworm and herpes, forty-two of itch, eight of scarlatina, thirteen of measles, and two of chronic urticaria. Scurvy, syphilis, chlorosis, and other diseases which affect digestion, were also observed to precede tænia; but, in general, few of the individuals affected with tape-worm, had not had lumbrici when young. The influence of hereditary predisposition is very doubtful. Dr. Wawruch only saw

two instances; in one, a mother and daughter; in another, a father and son, had tænia. Irregularity of the menstrual function seemed common; thus, thirteen females had only menstruated at the age of 16 years, twelve at 17, nine at 18, seven at 19, and one at 20. The duration of the disease was from a few months, to ten, twelve, fifteen, twenty, twenty-five, and, in one case, even to thirty-five years. Of the two hundred and six patients, three only, who were foreigners, had *bothriocephalus;* the others had *tænia solium.*"—(Gaz. Méd.)

Therapeuties of Tæniæ.

It is not intended on this occasion to enumerate the various remedial agents usually employed in the treatment of tapeworm, one or two excepted.

Anterior to the actual discharge of one or more joints of the tapeworm per anum, as has already been stated, the diagnostics of its presence in the intestinal canal seldom rise beyond a strong probability. Hence the value of such vermifuges as kousso and pumpkin seed, which are never deleterious to the patient, but to the worm only; while almost all other remedies of reputation necessarily disturb the patient's health, often severely, at least for a time. Hence, upon the empirical or experimental principle, an innocent vermifuge is admissible where the indications are even slight and would not warrant the exhibition of pink root, spirits of turpentine, &c.

A careful analysis of the facts connected with the successful administration of kousso for the last six years, notwithstanding its occasional failure, would clearly establish on a firm foundation, the propriety of the class termed anthelmintics, which some writers on the Materia Medica have called in question or openly denied, so far at least as one agent can serve as the type of a class, kousso, for its peculiar power of destroying tapeworm without causing, at the same, time any marked perturbation in the human economy, presents a valid claim to the rights of classification, as being not arbitrary, or spurious, but fundamentally scientific.

The evidence in favor of the unequaled efficacy of kousso as a vermifuge, derived from oriental travelers and still more from the occidental doctors of Europe and America, is strong, increasing, and incontestable. The Materia Medica scarcely affords a parallel to kousso for the efficacy and certainty with which a given indication is fulfilled.

A concentrated view of the evidence to this effect would prolong this paper too much, but would show that this remedy, unlike most of the new ones, has not been overpraised.

Many of the so called anthelmintics expel worms by their violent purgative action, which is often injurious to patients, who, for the most part are already debilitated from the intestinal irritation and consecutive diarrhœa occasioned by these parasites. At the same time, this class of worm medicine is altogether uncertain, very generally failing to dislodge worms.

Kousso.

Kousso, or Brayera, Anthelmintica (called after Dr. Brayer, who made its virtues generally known in Europe) as met with in the apothecary's shop in New Orleans, appears to consist of broken down calyces, petals, leaves, &c., or of a coarse powder of a dull brownish hue, which, when closely examined by the naked eye, and still more with a magnifying lens, presents many purplish particles, supposed to be disintegrated petals or flowers; the coarser and more predominant portion seems to consist of leaves, their ribs, stems, fibres, &c. This powder* has no well-defined scent, or taste; at first, there is a faintly saltish taste, which is soon replaced with one slightly bitterish, astringent, and acrid, somewhat like rose leaves. The tree belongs to the order rosaceæ, and is said to be about twenty feet in height, occupying the elevated table lands of Abyssinia, Arabia Felix, and the shores of the Red Sea.

Mr. R. Kirk, surgeon to the British Embassy to the Kingdom of Shoa, a few years ago, gave an account of the anthelmintic properties of Kousso, representing the tree as being one of the most picturesque in appearence. The Kingdom of Shoa, extending from 8° to 10° North latitude, consisting of two great platforms of table land; the lower, two thousand five hundred, and the higher, ten to twelve thousand feet above the level of the sea, has, in the day time, a range of temperature from 70° to 75,° with a yearly mean of 55½°. In the colder months ice is occasionlly seen.

The Kousso grows on the table lands of Tigre, and other parts of Abyssinia, as well as in Shoa.

*Will Kousso deteriorate by age? The affirmative is, it is said, maintained in the East, Hitherto the article is said to have retained its virtues for years, in Europe.

Dr. Beke, who in 1841,–'2 passed a whole year in these regions, little known, says that, "This tree is found throughout the entire table land of Northeastern Abyssinia, but appears to require an elevation of upwards of six thousand (perhaps of seven thousand) feet for its growth. Where I found it most luxuriant, was in the vicinity of the source of the river Abai, (Bruce's Nile,) at an elevation of close upon nine thousand feet. Tigre, the Northern portion of Abyssinia, being, on the whole, of lower elevation than the rest of that country, the tree is only found there in a few places."

There can be but little doubt that this tree would flourish on the elevated lands of Southern Mexico, and in Central America.

M. d'Hericourt, of Paris, appears to have enjoyed the monopoly of the kousso trade, until lately. The price, originally about ten dollars per ounce, or five dollars per dose, has greatly declined. This article can now be had in New Orleans at $2 50 per ounce, in phials near the size of the ounce phial of quinine.

It is believed that no kousso has ever been imported into New Orleans direct from Egypt, or Aden, where it is delivered by the caravans. Nothing could be easier than to adulterate this medicine in the state of a powder or broken down petals and leaves. It would, therefore, be advisable to get it in packages of the dried capsules and flowers, as put up in skins, where it is gathered, dried, and forwarded.

The dose is from half an ounce to six drachms, infused for an hour in a pint of boiling water; one-fourth to be taken every fifteen minutes, or half hour, first stirring the mixture, so as to take the substance as well as the liquid.

It should be borne in mind, that the discharge of a portion of the small thread-like anterior extremity of the worm, affords a strong presumption that the head has also been discharged. It is highly probable that the anterior undeveloped end of the worm, when broken into pieces, is not, like the posterior, capable of maintaining an independent vitality in each part; moreover, the head, with the adjoining portion, detached from the main body, in a great majority of cases, owing to its minuteness, would altogether escape observation.

During four of the trials with kousso, which I carefully watched, I was not able to discover any marked physiological, or rather patho-

logical effects, due to the remedy itself. The testimony is very strong, going to show that its effect on the economy is either null, or extremely slight. It sometimes acts gently on the bowels, and has been supposed to occasion nausea, though rarely, if at all.

Case of Madam H.—Kousso.

Madam H., aged about 35, married, mother of a small family, enjoyed vigorous health until about three years ago, at which time she began to emaciate; her digestive functions became impaired; her bowels disordered, and finally, joints of tapeworm were often passed, per anum; whereupon, she consulted some of the medical faculty, and passed through a long course of vermifuges without any advantage. In the winter of 1853,–'4, I was called to see her. She had just finished taking a great bottle of spirits of turpentine. I found her suffering from extreme debility, hot skin, abdominal pain and tenderness, diarrhœa, tenesmus, nausea, headache, muscular disturbances or irritability, quick pulse; symptoms much resembling typhoid fever. She had intermitting pains, of a dull gnawing character, in the right lumbar and iliac regions, particularly in and near the ileo-cæcal valve; from this deplorable condition she gradually, but slowly recovered her former health or *statu quo* as before the administration of the vermifuges, having used opiates, quinine, bitters, wine, and a good diet.

Here recovery from the fever, which latter was probably in a great degree, if not wholly due to the inflammation of the intestines from the vermifuges she had unsuccessfully used, left her still an invalid. For several months her general health, the tone of the alimentary canal, and the muscular forces, were in a more healthful condition. But at length, headache, intestinal pains, debility, and many anomalous and indescribable symptoms prevailed in various degrees from time to time. A course of diet, tonics, opiates, iron, bitters, combined with the milder vermifuges—also Carolina pink root, were tried, but emaciation with intermitting intestinal pains and griping continued. Kousso had been directed, but none could be obtained until after considerable delay; the article was found at Messrs. Sickles'. About six drachms were administered, in infusion, in divided portions during two or three hours, in the morning. No appreciable physiological or morbific action

resulted from the medicine. Early in the afternoon she passed a large quantity of broken up tapeworm—the longest portion of which was tied in a bow knot, and was alive, as was also, the cephalic extremity or head and neck. On the following day two drachms of kousso were given without any effect. After one month an ounce of kousso was again given in two doses on successive mornings. No worm was discharged. From the time that the first dose was given, now more than six months, no symptom of tapeworm has been felt. The lady's health has constantly improved, with the exception of an alarming uterine hæmorrhage three or four months since, which for several weeks reduced her strength. Her health is now restored.

The Pumpkin Seed Cure.

The seed of the pumpkin (*Cucurbita Pepo*) appears to be next to kousso as a therapeutic agent in removing tapeworm, being a true vermifuge, like kousso, expelling the parasite without producing physiological disturbance in the patient's health.

In the use of this remedy I have no experience. But the testimony in its favor is strong and increasing, though by no means equaling that in favor of kousso. As the medicinal virtues of the pumpkin seed emulsion, or powder, is ignored in the most recent American medical formularies, it is proposed to give a part of the clinical evidence extant, in relation to this and several other articles, in the Excerpta of this Journal, as its introduction here would extend this paper beyond the limit intended.

ADDENDA.

Galen (lib. iv, c. xvii) recognizes broad or tapeworms as being more pernicious than the round worms—" atque interdum latos eos, qui pejores," &c.—a distinction he alludes to in the treatment—" si lati sunt," &c.—if they be broad, &c.

Although a great number of the vertebrata are infested with tapeworm, it appears that the alligator is not; at least, I have never found the tapeworm in this animal, in which, however, the long threadworm (tricocephalus dispar) abounds, both in the stomach and intestines.

ART. VI.—WORMS IN THE URINARY BLADDER.

BY BENNET DOWLER, M. D.

April 19, 1828. Mr. B. and his lady, persons of good moral character, eye witnesses of the facts here related, and the parents of the boy whose case follows; at separate interviews stated that their son, aged 13, had been in bad health for a long time, and had undergone several courses of treatment under different physicians without relief. His disease appeared obscure—its seat uncertain. He had been treated by one physician for worms in the alimentary canal, but none had been discharged.

About four months since, Mr. B. having assembled his neighbors for the purpose of "*a house raising*," at which whiskey was drank as usual; some of the men succeeded in getting Mr. B's boy to drink until he became quite intoxicated. A painful and obstinate retention of urine followed, which lasted four days, and which did not give way until he discharged several living worms from the urethra, some of which were fifteen inches long. He has since discharged from the urethra a number of similar worms—in all about twenty; three of which were delivered to me by the parents, inclosed in a phial of whiskey. One of these exceeds ten inches in length and is nearly one inch in circumference for the distance of three inches from the head backward. A longitudinal band which is smooth constitutes the belly.—One fifth of the body next the head has numerous well defined semicircular rings, which are obliterated on both sides as they approach the abdominal band. The middle third of the worm is nearly smooth; the tail becomes rough, wrinkled, and annulated like the undeveloped joints of the tapeworm. The anterior portion of the body is nearly uniform in size for more than one-third of its length; it then abruptly diminishes to less than two lines in diameter; two considerable enlargements occur in the posterior third, or caudal extremity, which latter has a thread-like termination. These enlargements divide the tail into three unequal parts.

The head consists of numerous well defined rings, closely set, which compress the cephalic extremity more and more for a quarter of an inch—from the centre of which projects a proboscis, which at

its emergence is more than a line in diameter, and about two in length formed of rings, which become smaller until they terminate in the blunt proboscis which is studded with strong fangs or hooks. In the interior of the animal, adhering the back or dorsal aspect, two large parenchymatous columns, varying in size with the body, extends its whole length, being like lard in color, but a little firmer in consistence.

The other two worms appear to be of a different species, resembling tænia in structure, being flat and having numerous short annulated articulations with acute margins, like the joints of the tapeworm before development; one worm is five and a half inches in length—the other something less, both being almost uniform in size, scarcely averaging a line in width. All of these worms are white.

These worms, still in my possession, have not been examined microscopically.

Whiskey, for once, seems to have acted beneficially, having doubtlessly, in this case, proved detrimental to this nest of worms. The boy recovered his health rapidly after the discharge of these worms.—He removed, not long afterward, with his parents to the West. About seven years later I met with him in New Orleans, (in 1836,) in good health, pursuing his trade, in the saddlery establishment of Mr. John Hoey, on Tchoupitoulas street.

ART. VII.—REPORT OF A CASE OF VESICO-UTERO-VAGINAL FISTULA,

As treated successfully by M. Jobert, of the Hotel Dieu, Paris.

BY E. M. BLACKBURN, M. D.,

OF NATCHEZ.

Mme. Ponvard, aged thirty-four years, chair woman, was admitted into the ward Saint Maurice, on the 14th March, 1854. This woman, of lymphatic temperament, appears to be of a weak constitution. She has never been sick, however, with the exception of some rheumatic pains at the age of twenty-four years, which readily yielded to proper treatment. She began menstruating at the age of fifteen years, the menses re-appeared, regularly, until she arrived at the age of twenty-four years, at which time she became enceinte for the first time. The pregnancy was attended by no accident, her accouchement was easy.

The infant was born healthy, but died at the age of nine months. The menses re-established themselves five months after this delivery. Since this delivery, Mme. P. has become enceinte four times; her pregnancies and deliveries have always been easy, without any complicated accident whatever, although her fourth labor lasted longer than any one previous; why it did so, the patient can assign no cause. In 1852, the patient became enceinte for the fifth time. This pregnancy progressed happily; labor commenced at the proper time. This one (in March, 1853,) was long and difficult. The labor had already lasted some ninety-six hours, when the forceps were applied with success. The child was delivered dead, and from the statement of the patient was very large. It was not until the fourth or fifth day that Mme. P. perceived that the urine flowed entirely by the vagina, and that she had no inclination to urinate whatever. This infirmity has persisted since that time, and it is to disembarrass herself of this that she now enters the Hospital. The menses re-appeared five months after this delivery; she has menstruated regularly every month since. The menstrual fluid flows out mixed with urine. This accident appears to have greatly affected the mind of the patient; her gaiety has disappeared, and her *embonpoint* is diminished.

May 6th.—Although this woman has been in the Hospital since March, M. Jobert has not yet attempted an operation, wishing her to become accustomed to the wards of the Hospital. She has been employing baths and emollient injections. To-day she was examined before us for the first time.

We find first an erythematous state around the anus, also around the vulva; second, vagina red and erythematous; third, the urethra is a little shrunk; fourth, by the touch we cannot recognise the fistula, we find only that the neck of the uterus is very irregular, and that the anterior lip above all, is formed of two tubercles, which are easily separated, one from the other; fifth, the direct examination discovers an opening at the union of the neck and the vesico-vaginal partition. This opening is irregularly rounded, surrounded by inodulary tissue, and bordered on the central walls by two hard tubercles, which are nothing else than the remains of the neck of the uterus. Its diameter is considerable, enough to admit, easily, the introduction of a female catheter.

The urine flows always into the vagina, no matter what position the patient occupies.

May 6th.—*The Operation.*—The patient is placed in the ordinary position, the posterior wall of the vagina is thrown back by the aid of the speculum-univalve; the labia being held aside by leviers, M. Jobert implants there a strong pair of forceps on the vesico-vaginal wall, immediately before the opening of the fistula; then by the aid of a narrow bistouri, he pares the edges of the orifice, that is to say, he takes away the two tubercles, of which we have spoken, and which represent the remains of the neck of the uterus. Now we can appreciate the extent of the fistula; it presents the aspect of a cleft or gutter, formed, on one side, at the expense of the anterior lip of the neck of the uterus, and on the other by the vesico-vaginal partition. When M. Jobert had finished trimming the edges of the fistula, it presented a diameter of almost two centimetres. He proceeded then, by aid of a Porte-aiguille (needle bearer,) to introduce two points of suture—one in the middle, and one at the extremities of the fistula. As soon as the sutures are terminated, M. Jobert makes three incisions, two lateral and parallel to the orifice, and the third posterior on the grand vaginal cul de sac. He then made two injections of cold water into the vagina, and introduced a tampon of amadon. The patient was then carried back to the wards and placed in bed, with the pelvis slightly elevated, by the aid of pillows. A catheter was then introduced, to remain. Diet; two pots de tisane. At two o'clock in the evening, a small clot of blood was expelled by the vagina. The urine passes entirely by the catheter. Skin hot; abdomen a little tender. The tampon of amadon has been removed. May Seventh, light febrile movements; abdomen soft, not painful on pressure. The patient has felt, during the night, a desire to urinate. The urine is mixed with blood. Two bouillons, two pots de tisane.—Eighth, same state; no fever; the urine passes by the catheter.—Tenth, the catheter became obstructed during the night, and the patient is wet, as before the operation; probably the urine passes out by the vagina.—Eleventh, state satisfactory; no fever.—Twelfth, the urine passes off by the catheter, but the linen of the patient is stained with blood. We see, furthermore, a small clot of blood at the entrance of the urethra, and another more voluminous on the sheets of the bed; these clots are probably produced by the men-

strual fluid. For the patient says it has been five weeks since she menstruated.

May 13th.—The patient was examined to-day; the reunion almost complete. M. Jobert removes the sutures; no accident.

In the evening, the patient feels herself wet by the urine; it has probably passed by the vagina; the menstrual flow has completely disappeared. Fourteenth, the urine passes by the catheter; none has passed by the vagina; the catheter remains. The patient complains of some slight pains in the bowels—*Diete.* Fifteenth, nothing worthy of remark in regard to the urine. The night has been less favorable than the preceeding. Light febrile movement; two liquid evacuations.—Sixteenth, the diarrhœa continues; tongue dry, red on the borders and at the point; abdomen somewhat swollen and tender on pressure.

The catheter is found obstructed during the day; the urine has accumulated in the bladder; none has passed by the vagina. The introduction of a new catheter is a little painful; it brings off a considerable quantity of urine. Diete, portion gommeuse.—Seventeenth, the diarrhœa seems to have disappeared, although during the day the patient had two liquid evacuations; no fever; no accident in regard to the bladder.—Eighteenth, diarrhœa; four operations during the day.—Nineteenth, the diarrhœa persists. In the night preceding four liquid evacuations. M. Jobert removes the catheter. During the day the patient has two or three times had a desire to urinate. The desire is so pressing that she scarcely has time to take the vessel. In spite of this she is found wet. In the evening the diarrhœa appears to have diminished.—Twenty-first, some colic; the laxity has diminished.

The urine flows involuntarily, which renders the patient always wet. It is impossible to know whether this urine passes by the urethra or the vagina. The catheter is again introduced.—Twenty-second, the patient can no longer support the catheter. M. Jobert orders it to be taken out.—Twenty-third the diarrhœa and colic have almost entirely disappeared.—Twenty-fifth, the catheter is again introduced. During the day she has slight colic; furthermore she has two serous evacuations.—Twenty-sixth, the catheter is again withdrawn.

The diarrhœa disappears; general state excellent.—Twenty-seventh, the patient is again wet, but the urine flows out of the urethra by regorgement. The desire to urinate is very frequent, about every fifteen or

twenty minutes, and always pressing.—Twenty-ninth, nothing new; the patient although wet, is much less so than before the operation.—Thirty-first, the patient examined. The cicatrix is complete, with the exception of a small opening about the size of the head of a pin, through which some drops of urine pass. M. Jobert cauterises it with the nitras argenti. A catheter introduced into the bladder does not penetrate more than four or five centimetres, which represents the length of the urethra and bladder, showing how much this organ has shrunk.—June third, the slight diarrhœa which showed itself three days ago, has completely disappeared.

The urine continues to wet the patient, but it passes out by the urethra.—Sixth, the patient is examined again; we see on the lateral walls, two cicatrices, very apparent, under the form of two white lines. In the centre we see another cicatrix formed by the rudiments of the neck of the uterus. This central point appears well cicatrised, although there still remains a small opening through which the urine oozes. We must recollect that the bladder of this woman is extremely narrow. Cauterization with argenti nitras. During the day the patient was a little wet. Ninth, the catheter is again introduced; the urine often passes out between the instrument and the walls of the urethra. General state excellent.—Thirteenth, we still observe a small opening which has not yet cicatrized.

One might say that the fistula was cured; but the patient cannot retain her urine more than an hour. Cauterization with argenti nitras. Fifteenth, cauterization with argenti nitras. She was not found wet during the day.—Sixteenth, the patient is wet. We might remark that the patient is never found wet during the day of cauterization, but always on the day following.

The catheter remains.—Twentieth, we see at the bottom of the vagina two or three drops of urine. In the place where the fistula existed, we find an inodulary tissue, perfectly drawn on all the points. In raising the vesico-vaginal partition, we see again a small tubercle, in the centre of which exists a small opening, which, probably, gives passage to the urine. Cauterization with argenti nitras. In the evening the patient is not wet.—Twenty-fourth, wetting by the vagina. M. Jobert thinks the urine passes through the urethra, and falls back into the vagina. Cauterization with argenti nitras. Catheter retained. Twenty-fifth, the

patient is again wet.—Twenty-eighth, cauterization with argenti nitras; about every hour the patient has a desire to urinate.

The urine sometimes passing involuntarily, after an effort at coughing. July sixth, we still see the small opening at the end of the vagina. The catheter is still retained. Cauterization with argenti nitras.—Seventh, the patient is wet.—Eighth, same state, cauterization.—Ninth, the patient still loses her urine.—Tenth, one small point is only to be seen, where exists an opening almost imperceptible, by which the urine oozes. Cauterization with argenti nitras.

In the evening no urine escapes by the vagina; 13th, cauterization with argenti nitras; since the 11th, the introduction of the catheter has been painful; evidently the mucous membrane of the urethra is becoming inflamed. To prevent this accident, M. Jobert withdraws the catheter.—14th, the patient is wet again.—17th, M. Jobert cauterizes the little opening with the nitrate acid of mercury. No catheter.—18th, the patient is wet.—20th, the patient is left to repose.—22d, cauterization with argenti nitras.—23d, as we find that the different cauterizations have been of little service, the urine still running into the vagina, M. Jobert thinks it necessary to practice immediately, one point of suture on the small opening before mentioned.

This he performs in the same manner as in the previous operation. Immediately after he introduces a catheter to remain. In the evening no urine has escaped into the vagina. The urine flows abundantly through the catheter and is perfectly clear.—24th, during the preceding night the catheter was found obstructed twice; in spite of this the patient was not wet. The desire to urinate was strong enough to awaken her twice during the night.

To-day no urine is found in the vagina; we cannot appreciate the length of time she is able to retain her urine as the catheter remains in the urethra.—25th, the patient is a little wet, but we are convinced that the urine flows through the urethra. The catheter is still retained.—27th, the urine flows freely through the catheter; during the night the patient had slight colic. The urine is somewhat mixed with blood—M. Jobert thinks that this blood is that of the menses, flowing through the urethra; as the neck of the uterus not existing, the menses must pass through the bladder.—28th, the hæmorrhage has almost disappeared; the urine passes freely. At the entrance of the vagina and opening of the urethra we see

two or three clots of blood.—29th, the patient is examined to-day; after the second operation, we see at the bottom of the vagina a lineary cicatrix, in the middle of which we see the loop of the suture thread. M. Jobert extracts it, and remarks that not one single drop of urine passes out.

The cure is now complete, nevertheless the catheter is continued.—31st, the patient is perfectly dry, but during the preceding night she was seized with considerable diarrhœa. The catheter is withdrawn. To-day the diarrhœa has diminished; abdomen somewhat tender.—August 2d, diarrhœa has completely ceased. The urine is retained without difficulty for nearly two hours.—3d, we see at the extremity of the vagina a surface perfectly smooth and dry; at the place where the fistula existed is an infundibuliform cavity.

The patient retains her urine more than two hours which proves that the bladder has developed itself.—5th, the patient is examined, and we find a streak of inodulary tissue where existed normally the neck of the uterus. Complete disappearance of the neck; no traces of fistula; around the point where the neck existed we see a radiation of the walls of the vagina. The width of the vulvo-uterine duct does not appear to have diminished; the length is a little less. The recto-vaginal wall makes a slight bulge, having the appearance of a prolapsus. The patient retains her urine at least four hours.—8th, M. Jobert examines the patient for the last time and finds the vagina perfectly dry. The catheter introduced into the bladder brings off one hundred and fifty grammes of urine perfectly clear and limpid. This shows that the bladder has taken great development at the bottom of the vagina, and to the right of the lineary cicatrix the remains of the neck of the uterus make a small bulge on the ancient fistula. Finally the patient can retain her urine a little over four hours. She makes her exit *completely cured.*

Paris, Aug. 10th, 1854.

ART. VIII.—LETTER ON YELLOW FEVER.

BY M. MORTON DOWLER, M. D.

To the Editor of the "New Orleans Medical and Surgical Journal."

Agreeably to promise, I sit down to write you a word or two, in relation to the health of the Fourth District, for the last two months. I had intended, in the coming number of the "Journal," to have given,

somewhat at length, my views of the ætiology, pathology and therapeutics of yellow fever; but I have been prevented by business engagements. I hope to have my article prepared for the January number.

I may set out by saying that the front of the Fourth District has been scourged by the yellow fever, in its most malignant form. In every direction, the unacclimated have been indiscriminately smitten. Our District, is the great resort of the German and Irish immigrants, particularly of the former; and in my ample experience, which extends from 1837, to the present, I can safely say that, considering the comparatively small number of persons, who were subjects for the disease this season, I have never seen a more ghastly display of deaths doings. In the square bounded by Jackson, Tchoupitoulas, Josephine, and the Mississippi, I am informed that no less than twenty-five persons have died, besides which a number have been sent off, in a moribund state, to the Charity Hospital. There have been about thirty cases, several resulting fatally, in four houses, near the corner of Tchoupitoulas and First streets. From one house on Josephine street, near Tchoupitoulas, in which the German immigrants were literally piled and crammed, no less than twelve persons were carried out dead; and from the same house, a number were sent to the Hospital. From the German boarding house, at the corner of St. Andrew and Tchoupitoulas streets, a favorite stopping place for German immigrants, I learn that a great number have been sent to the Hospital, not a solitary immigrant escaping the disease. In the Irish districts of St. Mary and St. Andrew streets, none of the unacclimated have escaped. In the German portions of the District, the same thing is seen. Amongst these destitute people, black vomit, hæmorrhages and death have constantly occurred, since about the first of August. Those amongst the sufferers who could command the comforts and necessaries of life, and who early underwent treatment, have generally passed safely through the disease. The cases I have taken charge of, have been principally of this class. The filthy and destitute immigrants, to which I have been summoned, I have caused to be sent to the Hospital.

There is no change in the character of the disease. It is what it was last year, what it has ever been, and probably ever will be, one and identical; a pestilence, peculiar, specific—distinct from all other diseases, as non-contagious as the colic, and as little liable to importation or exportation. It stands out in bold relief, and alone, amongst fevers. In

many cases there is no "fever" whatever, but mere simple apyrexial debility, the morbid agency having acted so lightly that the organism is not aroused, and there is depression instead of excitement. I saw last year numerous *walking cases* of yellow fever, and this year I had a remarkable one. Yet some pretend to say that yellow fever is but a high grade of bilious remittent! I will give you some idea of the general course of treatment, I have pursued. I hold that yellow fever requires a treatment different from that of any other disease with which we are acquainted.

It should never have the treatmeut of an acute inflammatory disease, under whatever form it may appear; it should not be treated as a bilious disease, nor as a congestive disease. Some morbific agency has disturbed the functions of the body. That agency is a something which amongst other conditions produces disturbed vascular action, painful innervation, morbid temperature. A man is apparently in perfect health, and in less than an hour he is laboring under the severest symptoms of yellow fever. Here there is nothing but functional derangement. There are amongst other things three grand indications to be fulfilled, namely: to allay and relieve pain, to reduce the temperature if excessive, and to quiet the tumult of the heart and arteries. We must, at the same time, bear in mind that the organism has been assaulted with a noxious agency which has but a few hours of potency, which if left to nature must either give way to convalescence, or destroy the patient. All three of these indications, if the disease be taken at its very onset, can be fulfilled by a single agent and that is opium in sedative doses. In the epidemic of 1847, I satisfied myself of the incalculable value of large doses of opium at the very onset of yellow fever. The first case in which I administered the remedy was in that year in the case of a stout fat plethoric German woman, æt. 22. She was most furiously assaulted with the disease. There was burning heat, panting, surging of the carotids, universal pain, red eye, thirst, restlessness, delirium. In such cases I had in former years tried the lancet, I had caused my patients to be marked with the lived rings of the cupper, I had used purgatives and refrigerants, I had used foot-baths, I had used warm-baths, I had sweat my patients with air-tight bed-clothes, and all had too often proven unavailing. In that year I had used the sulphate of quinine in doses of from twenty to thirty grains, in many cases repeating the same, and though the remedy has a very marked

salutary influence on the disease when administered early, it took but an imperfect hold on the symptoms in which there was but a treacherous pause. Quinine and relapse too often go together. I at once gave this patient five grains of opium; in two hours I revisited her; she was sleeping, but touching her wrist awakened her; she was covered with a single blanket and was literally flooded with perspiration, her pulse at first 98, was now 70; pain entirely gone. The heart and arteries were calmed; the nervous system was relieved, the proper temperature restored. The case went rapidly on to a favorable termination. During that year, I treated numerous cases of yellow fever with sedative doses of opium, when called early in the disease, with entirely satisfactory results. In the epidemic of 1853, I attended an immense number of cases of yellow fever. Either alone or in combination with quinine, I have as a very general rule, in the early stage of all cases requiring treatment, given powdered opium in decidedly sedative doses. I generally give from three to five grains in from twenty to thirty grains of quinine at a single dose, in a table spoonful of water, repeating the dose in six or eight hours if necessary. The quinine though it acts well in combination with the opium, acts a subordinate part. If the opium fall short of its full sedative effect, it produces injurious stimulation; hence in stout, robust, plethoric subjects when the onset is severe, four or five grains should be given at once, in combination with the quinine; in such subjects I have frequently repeated the five grain dose of opium. It is one of the most powerful of all sudorifics; it puts an end to the pain, it quiets the throbbing, lowers the exalted temperature. Its effect on the mind is one of its precious advantages.

The fear of death, which harrasses the panic-stricken patient, and often renders his case intractable, is assuaged under the calming influence of the medicine. Fear is a deadly debilitant, and should never be lost sight of in the treatment of disease, but should be met by some remedy which modifies nervous excitement. It is a most remarkable faet, that opium, thus administered, at the onset, in the febrile state of the disease, is very rarely objectionable on the score of these idiosyncracies, which so frequently show themselves under its use in other diseases, and in the latter stage of yellow fever.

But, perhaps, on mere theoretical grounds, I may be told that I am

endangering the life of my patient by administering sedative doses of opium in the furious excitement of yellow fever; and that I have nothing to expect but apoplexy, fatal coma, effusion, &c. If practical facts, exhibited in several hundred cases, in 1853, and not a few cases this year, show that no such accidents happened, in my hands, I leave antagonistic theories to their own worth. One of Molière's doctors was informed by the servant, that a certain patient of his, was dead. The doctor said it was false, and quoted Hippocrates—the time for the crisis not having arrived. The servant replied that Hippocrates might say what he pleased, but the man was most definitively dead. Theoretically, the man was living, but for all practical purposes he was done for. If the furious pumping of the heart and arteries be allayed and softened down, if the agonizing pains of the head, back, limbs and joints, &c., be relieved, if the skin becomes flooded with perspiration, if tranquil and non-comatose sleep be produced, then we have a right to infer that the patient has taken a salutary agent, a powerful febrifuge. But the theorist, cannot, from anything which can be said of opium, make out even a *prima facie* case against the treatment.

In yellow fever I have laid aside the lancet; I have dismissed the cuppers and leechers, *in toto;* I allow no hot mustard foot-baths, or body-baths, as there is generally more than enough of heat. I never make an effort by piling bed-clothes on my patient, to force a perspiration; on the contrary, if the patient is heavily covered, and there is a hot sweat upon him, I prefer to throw off the clothes, and cover him merely with a sheet, and allow the skin to dry and cool. A mortal injury is constantly done to patients by the *bed-clothes diaphoretic.* If the *calor mordax* persist, as it often does, under every known mode of treatment, we must be unremitting in our efforts to get rid of the heat. The patient must be stripped naked in the bed, and diluted alcohol, cooled to the utmost with ice, must be applied *á capite ad calcem,* the patient turned again and again, till the skin is effectually cooled. A couple of blankets should then be thrown over the patient. The hand of the nurse should frequently be thrust under the blanket to mark the returning heat, which should be the signal for renewing the application. Some patients will require two or three days, of this kind of nursing. There are cases in which the patients' life depends upon it. But the opiate generally produces an approach to the natural temperature, and a free

perspiration, without the forcing aid of bed-clothes. In the excessive perspiration, the patient is generally clamorous to get rid of the saturated under-clothes and sheets. I invariably allow these to be changed frequently. Shirts and chemises may be thrown off two or three times a day. Under all circumstances the air should frequently be admitted under the bed-clothes. The foul custom of allowing a patient to lie for five or six, or more days, in the same sheets and under-clothes, for fear of relapse, cannot be too strongly reprobated.

The opium itself, in powder, I prefer to any of its preparations. It far exceeds any of them in sudorific and febrifuge power. I have observed the sedative doses sometimes cause the urine to be retained in the bladder. In some instances the urine appeared to be increased in quantity. Retention of urine, however, often occurs in cases in which there has been no medical treatment, opiate or otherwise. The state of the bladder requires strict attention.

I wish here distinctly to say, that in a very great number of cases, to which the physician is called, the use of sedative doses of opium is wholly inadmissible. The same may be said of quinine or any other sedative agent. The opium must be employed early in the disease, or not at all. If the case has gone beyond the second day, all idea of such treatment must be in most cases abandoned. A dose or two taken on the day of the attack, which would produce the happiest results, might, if taken two days later, destroy life. There may be equal hazard in the latter case, whether the febrile symptoms have continued or subsided. In such case, if the fever has persisted there may be complications and lesions already existing which would render opiatism pernicious to the last degree, and if the pyrexial symptoms have subsided, whether the case put on a favorable aspect or otherwise, opium as a sedative, is out of the question.

When the case is, to all appearance, going on favorably and safely, and the febrile symptoms rapidly disappear, there is often great uncertainty of the patient being out of danger. In an hours' time there may be a total change in his case. The fever may again flare up; or in the midst of a most insiduous and treacherous *apyrexial* state, in which he makes little or no complaint, the fatal black vomit may be lurking in his stomach. Even in cases in which no formidable symptoms arise, after the subsiding of the febrile phenomena, the patient has often a

great deal of uneasiness and suffering on entering into convalescence. There is very generally an uneasy painful sensation in the pit of the stomach, accompanied with tenderness, on pressure. This sensation frequently extends up the sternum, producing a disagreeable sense of constriction in breathing. The latter affection throws the patient into a perfect night-mare during sleep. A light stimulant dose of the *liquor opii sedativus*, a free dose of the ammoniated tincture of valerian, or what is perhaps better, a dessert spoonful of brandy, given occasionally, relieves the patient. Mustard may be applied for the abdominal tenderness, or in case of nausea; but I have entirely discontinued the use of fly blisters in yellow fever. If applied early they produce injurious stimulation, and whether early or late, they induce a vile hæmorrhagic appearance, excessively annoying and irritating to the patient, and sometimes discharging blood. Last year I saw a case in which even a mustard plaster produced a bloody oozing from the skin.

Fly blisters, in fact, are extremely objectionable in yellow fever, and in all hæmorrhagic diseases, on account of their action on the kidneys, ureters and bladder. The strong tendency to hæmorrhages, from every mucous surface in this disease, should, of itself, banish their use. Cantharadism in the urinary organs, in yellow fever, very generally produces hæmaturia, and it no doubt serves the renal and cystic epithelium in the same manner it does the external cuticle.

The administration of quinine, in large doses, at the outset of yellow fever, has often been found to produce such modification of the organism as to induce a sudden apyrexial crisis and rapid convalescence. Opiatism brings about the same condition with greater certainty. This sudden arrest of the febrile symptons by quinine, has been called by the absurd name of *abortive treatment*, showing obviously, how we may bamboozle ourselves and others with a mere shibboleth. We will carry out the obstetrical idea. In some cases there is simple apyrexial debility from first to last. Here there is no abortion but sterility. In other cases the febrile symptoms spontaneously disappear on the same day, and here is an abortion without an abortionist. The same patient may relapse into a conception, or when the fœtus is at first expelled by the ergotism of quinine, and the patient deemed effectually delivered, a new conception may cause him to die in a subsequent parturition. No rational physician should ever think of attempting to cast out devils in this manner. I have

never thought of striking at the cause of the disease, because that is unknown. I look to the prevention, repair, and moderating of injuries at the hands of an unknown enemy. Words often weigh heavily in medical science. Broussais treated his patients with gum water, leeches, and acid slops, and called his treatment the "physiological system." The leeches and gum water were but a small affair, but the term, physiological treatment, conveyed an idea of science, and turned the honest heads of physicians who had grown gray over calomel and jalap.

Whatever plan may be adopted in the treatment of yellow fever, (and our Materia Medica is abundant in resources,) our remedial means ought to be such as will be attended with the least possible expenditure of the patient's strength. It is not a disease which admits of severe and exhausting medication. It is not a disease for blood-letting, cupping, leeching, mercurialization, vesication, antimonials, or rattling cathartics. I tried them well, many years ago, and speak from experience. Yellow fever is strongly neuropathic in its character, and the physician who thinks himself justifiable in looking to the pyrexial character of the fever alone, and draining the system of his patient, greatly mistakes his case. Take blood-letting for instance :* Some years ago *V. S. ad deliq. anim.* appeared to be the panacea, in the Charity Hospital, if sufficiently often repeated. I tried that panacea. The patient is robust, and the fever furious. I bled him according to the above flippant formula. Oh! what a change. The patient is in raptures; for the pain, the heat, the excitement, are gone, and the perspiration drips from the surface. In less, perhaps, than two hours the whole phenomena have returned, and the throbbing is as though a previously bound demon had been let loose in the heart and arteries. Let the prescription be repeated. Fresh gratitude, and a renewal of the symptoms, with a quick, irritable pulse. Here the Sangradian, if he doubt, will recede; but if he be enthusiastic, he will

* Dame Nature has the greatest abhorrence of hæmorrhage. She spares effusion of blood in all her vital actions, save only in the urgent and indispensable case of providing for the continuation of our species. *Ovulation* and *Parturition* are her only acts which result in bloodshed; but in these conditions she acts with the greatest economy, and holds fast to her own tourniquet. She seals up and stops all the leaks that she possibly can, and stands by the surgeon, tying a living cord by the side of his threaden ligature. She seals up all the blood openings in the walls of the largest abscesses, which she causes to point, thin out, and burst without loss of blood. Though mortification may throw off at once its "pound of flesh" in one slough, she, by separating the living from the dead parts, allows "no jot of blood." In this respect she is parsimonious to the last degree. One of the strongest characteristics of yellow fever is the tendency to hæmorrhage, and thus to make war on the habitudes of nature. The Sangradians first assault her with their active hæmorrhages of the cup, lancet and leech, and then turn her over to tender mercies of passive blood-letting at the hands of the pestilential bleeder. I do not deny the virtue of blood-letting as a remedial agent, but let the Sangradian ponder well when he is dabbling in nature's food, and throwing it by bowlfulls to the dogs.

bleed the third or fourth time. The chances are, that he will, in either case, discover, in due time, that he has, with the best intentions, established a triple alliance between the lancet, hæmorrhagy, and debility. In any case, he has put his patient in additional bodily danger.

In the advanced stages of the disease, the circumstances that govern the treatment, are very diversified. Some cases may be greatly benefited by direct stimulants; others may require cooling salines, or alkaline remedies. Some patients may require, for a time, to not only abstain from all medicine, but even from drinks. The state of the bowels demands attention, and should they not be in an open condition, an aperient should be administered. One of the best, and at the same time least used of these, is the *sulphate of soda.* It is one of the mildest, and least objectionable aperients, and should take the place of that most disgusting of medicines, castor oil.—Should black vomit occur, we must give nature all the glory, if the patient recover. Nevertheless, we should never turn our backs on black vomit patients. If we do not continue to attend and "cure" them, perhaps some good old woman may; as has happened. The *vis medicatrix* smiled on four of my patients in 1853, and gave me the credit of the cures. One of these cases is so extraordinary, that I will glance at it for a moment: Miss L., æt. 18, sanguine temperament, light complexion, flaxen hair; tendency to *embonpoint*, was attacked with extremely urgent yellow fever symptoms, on the afternoon of the 14th of August, 1853. She took, immediately, gr. iijss of opium with gr.xx of the sulphate of quinine. The excitement abated, the pain was relieved, and there was free perspiration. I saw her next morning at 10 o'clock, at which time she took gr.ijss of opium and gr.x quinine, to meet a slight increase of fever. The febrile symptoms disappeared at the end of the third day, and there was a calm on the fourth day, which might have deceived the uninitiated into the belief that all was doing well. But there were breakers ahead. The eye, which was almost bloodshot from the beginning, showed now coagulated blood beneath the conjunctiva. Blood began to ooze from the gums, causing the tongue to look as though it had been dabbled in blood, and there was hæmorrhage from the nose. These hæmorrhages, however, soon ceased. Blood at one time escaped from one of the eyes. The sclerotica became of a very dark greenish yellow. On the fifth day she threw up large quantities of the genuine matter of black vomit; some of

it heavy, flaky, and settling to the bottom of the vessel, and some half diffused through the fluid vomited. She took the following:

R—Pulv. Aromat.
Sodæ Exsiccat. ā ā ʒ ss;
Ferri. Pulv. (u. s. p.)
Pulv. Kino compos. ā ā gr. xii;
M.ft. pulv. no. vj.

One to be given in a little water after each effort at vomiting. The prescription appeared for a time to quiet the action of the stomach; but the black vomit soon re-appeared. I then ordered her as follows:

Argent. Nitrat. gr. vj.
Micæ panis, q. s.
M.ft. pil. no. ix.

One every three hours.

She took the whole with evident improvement, the morbid irritability of the mucous membrane of the stomach appearing to be greatly diminished. The medicine even appeared to have some effect in arresting the sanguineous oozing, doubtless taking place from that membrane. The patient's skin soon after put on a singular aspect. In some places it was of a deep yellow, in others greenish yellow, in others of an orange hue, mixed with violet, in some places a *black-and-blue* appearance, the whole putting on the appearance that results from severe contusions, or from a thrombus which has discolored the arm from awkward blood-letting. Elevated tumors appeared in various parts of the body, indicating plainly that there were numerous subcutaneous hæmorrhages, lodged in the form of coagula in the cellular membrane.

There was a low, irritative fever, delirium, and tossing from side to side. The patient continued pretty much in this state to the 14th day. The hæmagastric symptoms having ceased, she had taken a little broth, milk and rice, and had also occasionally taken a little milk-punch, as a stimulant. The darker colors of the body began to fade, leaving the whole body a kind of greenish yellow. Some of the tumors which were principally about the breast, neck, and sides of the chest, began to put on a dusky red color, others did not differ from the color of the surrounding skin. I opened one of the latter, and found it to be a simple coagulum of blood, and also one of the former, and found it to be the same thing, surrounded by a sanguineous pus. I gave them all time for a slight sup-

puration and then discharged their contents. The patient began to convalesce, and at the end of five weeks quit her room. She lost her hair, which was speedily re-produced. She is now in the most perfect health. I gave her during her convalescence, the impalpable powder of metalic iron twice a day, which produced the happiest effect on the blood.

A good deal has been said in diagnostics about the proper color of the yellow fever patient, and a particular shade of yellow has been set down as pathognomonic of the disease. This case goes to show: First, that the patient may have every possible hue in the disease, from jaundice to purpura hæmorrhagica; Secondly, that the discoloration, whether the disease be yellow fever or not, is of the nature of hæmorrhage, ecchymosis, or bruise—some kind of bloody endosmosis. In what manner soever the blood may be affected in yellow fever, there is, as we know full well, a leaky condition of the solids, and often they will not hold blood.

In severe cases of yellow fever there is a remarkable tendency to abrasion of the cuticle in the scrotum of males. A large bloody vesicle appears on the posterior surface, which soon carrying away the whole cuticle, leaves a large cherry red or black surface, which is very formidable; sometimes there is hæmorrhage. A still more formidable condition appears in the *sinus pudoris* of females. In a hæmorrhagic case you will sometimes be told by the female or her friends, that her courses have come on, though her *pudeur* may prevent her from complaining of suffering in the vulva. Inquiry discloses the latter fact, and on examination you find that the whole epithelium has been carried away as far as you can explore, the whole being of a dark cherry red, and there is an exudation of an offensive sanguinolent discharge. The vulva is tumid, and the labiæ everted by the intervening tumefaction. The denudation of the urethral *meatus*, renders it impossible for the patient to urinate, and the catheter must be used, which gives the patient acute pain. I saw a case of this kind in a child, seven years old, recently.—An Irish girl, aged eighteen, who died with black vomit* a few days ago, had this most formidable lesion. I saw several such cases last year; they were totally unconnected with any venereal affection. This hæmorrhagic condition of the vaginal mucous membrane may give us an idea of the terrible condition of the

*She passed about half a gallon of black vomit *per anun*, in bed, just as she expired.

gastric and intestinal mucous membranes during life, and which we are often unable fully to realise after death.

I have had only five cases of negroes, as yet, in the epidemic of 1854. I treated them without reference either to free-soilism or the ultraism of your world renowned contributor, Dr. Cartwright, who, whatever he may expect from the laity, cannot expect any medical man, be he fire-eater, unionist or abolitionist, to swallow his paradoxes with regard to negroes. My first case, a woman, was the only one of interest. She was the fat, stout, lusty slave of Counsellor P. The fever was furious, and there was considerable hæmorrhage at the nose. Here was a case for Dr. C.'s "hot sun," his "oil," his "strap," his "turnip greens," his "corn bread" and "fat bacon." I, however, took my "*hot and black knee-bender*," and treated her on the same principles I would have treated a golden-haired daughter of Japhet. The opiate acted in the most salutary manner. She showed no repugnance to "white folks' diet," but, on the contrary, during convalescence, her disposition to substitute the kitchen for the sick room amounted almost to a "*drapeto-mania.*"

I have spoken of *apyrexial* and *walking cases* of yellow fever, in which the pestilence has lightly touched the patient. I have, however, said nothing of those cases which occasionally occur, in which there is little or no febrile excitement, owing to the excessive and overwhelming malignity of the attack. The patient takes his bed, and the uninitiated would suppose that little or nothing ails him, there being little if any febrile excitement. But the disease has overwhelmed him at its inception, and the organism is unable to react. I have at the present time, (Sept. 25,) in one house, in Jefferson City, three cases in one room which will serve for illustration in a remarkable manner. Eight days ago, the daughters of Frau M., Marianna and Wilhelmina, the former æt. 20, and the latter 18, all German immigrants, were attacked with the pestilence. Wilhelmina was attacked with a simple debility and perspiration, and slight cerebral and articular pains, without febrile symptoms, from which she has not yet completely recovered. I left the case to nature, diet and good ale. During her attack she has menstruated naturally. Marianna was attacked with great depression, slow pulse, skin nearly natural, injected eye; made little or no complaint; started up in a querulous manner when spoken to. She has been free from febrile excitement from first to last. During the last three days there have been, occasionally, nasal hæmorrhage,

and bleeding from the sides of the tongue, and in the mean time, though her menstrual period had not yet arrived, she has had a copious sanguineous discharge from the vagina, and there is some epithelial abrasion in the external parts. To-day she is threatened with black vomit. In none such cases can either quinine or opium be administered. Frau M. took the disease three days ago, with the highest febrile excitement, which was promptly put an end to, and a favorable crisis was brought about by the administration of opium. In 1853, I saw a deadly *walking case* of yellow fever. The patient, a German servant girl, about eighteen years of age, had attended to her work for two or three days, all the time feeling very unwell. I was requested to see her, in order to determine whether it was necessary to send her to the hospital. She came to meet me, looking extremely ill—denied having had any fever. I ordered her to go and lie down. She died the same day with black vomit. How peculiar this pestilence! How different from all other *fevers!* Mr. P. sent for me two weeks ago, to prescribe for his wife, who was lying ill with the disease.—He was resting by the side of his wife on the bed. "I wish you to prescribe for my wife," said he, "she has got the yellow fever." I looked at him, and told him that he was as bad off as his wife. He gazed at me with an incredulous stare, and said: "Not at all, sir; I have only a slight cold. I require, and will take nothing." There was only a slight excitement of the pulse, very little pain, heat scarcely increased, yet it was easily seen that the man was in the deadly coils of the serpent. His case proved completely intractable, and he threw up astonishing quantities of black vomit before the fatal termination, on the fourth day.

There are a large number of hæmagastric cases, in which the patient does not, and cannot, throw up the black vomit. There are generally retchings, straining, a thrusting forward, and acute pointing of the tongue; what is called, in the Hibernian dialect, *a dry boken.* Mrs. L., whose case was noticed in the September number of this Journal, was affected with these symptoms, in connection with nasal hæmorrhage. If there be a large and sudden accumulation of the black vomit in the stomach, it sometimes boils out of the patient's mouth, as he lays on his back, or it may be squirted across the room with considerable force. On the contrary, if there be a slow and partial exudation from the mucous membrane, all the conditions of black vomit may exist, without any ejection of that fluid from the

stomach. Hence, a much greater number of real hæmagastric cases recover, and a much greater number occur, than is generally believed.

There are certain severe cases that, having passed the ordinary crisis of the disease, fall into an anæmic, jaundiced, decayed, adynamic condition, in which there is a protracted tenacity to life, which is most unaccountable. There is a slight fever, which appears to be all that is left of life. I have, every epidemic year, seen samples of these breathing corpses. I have invariably here pursued the alcoholic treatment, and many times with the happiest results. I have had several remarkable cases in the course of my practice. I will glance at one I had in the epidemic 1853: I was called to see S., coffee-house keeper, a stout, athletic German, æt. 40. He had allowed the sixth day to pass without any medical treatment. He was of an intensely deep greenish yellow, all over his body; there was extravasation of blood on the sclerotica, which had a deeper color than the skin. He was retching and straining to throw up, and ever and anon he raised a small speck of the black vomit. I gave him eight grains of the nitrate of silver, in broken doses, at three hour intervals, in pills. He took, also, simultaneously the following:

℞—Confectionis Aromat., ℥j;
Aq. Cinnamomi, ℥iv;
Creasoti, gtt. iv;
Liquoris opii sedativ, ℥ss;
Syrup. Acaciæ, ℥iiss;
Misce Optime.

Take a dessert spoonful after each spell of straining and retching. These symptoms were mitigated; otherwise his case was as bad as possible. There were debility, delirium, and subsulsus. I temporised with him for several days, during which time he appeared to be neither better nor worse; I then began the alcoholic treatment. His urine being scant, and resembling spoiled porter in its appearance, I preferred gin as an alcoholic, owing to its diuretic properties. He took a table-spoonful five or six times a day, which was continued for several days, taking small and often repeated portions of tender animal food. He took, also, night and morning, a few grains of the impalpable powder of metallic iron. He improved daily, and left his room after about four weeks of sickness. I have generally treated cases of this kind with brandy. Alcoholics, as a luxury must be

condemned by every well regulated mind, but they can often be wielded to the greatest advantage in the practice of medicine.

The yellow fever presents some specialities with regard to children, of the greatest interest. The epidemic of 1853, has thrown further light on a fact of which I was long since convinced, namely, that the children born and stationary in New Orleans, all undergo what is called acclimation, and take the yellow fever, either at the breast, as they now are doing, and as they did in 1839,–'41,–'43, &c., when we had an epidemic every alternate year; or at all ages under five years, as they did in the epidemic of 1853, when five years had elapsed since the great epidemic of 1847. The term *acclimation*, as defined by Dunglison, is totally inapplicable to the process by which immunity is acquired against yellow fever. That process is not a thing of *climate*, but of mere locality. There is no difference between the climate of New Orleans and that of all the Southern parishes of the State; yet the city has been regularly smitten, while the parishes have been exempt from yellow fever. The creoles of these parishes have been born, have lived and grown gray there, and never were attacked till 1853. It is not our climate that is chargable with the disease. The patient takes the disease but once, and his protection is good for all climates; and that protection the patient can only acquire by having once undergone the disease. The term *acclimation*, if thus construed, I do not object to. One word is as good as another, provided the proper idea be attached. Now it was apparent to all that during the epidemic of 1853, the native and fixed population of New Orleans, over five years of age, did not suffer from yellow fever, whilst those under that age were almost indiscriminately attacked. The children in New Orleans, under five years of age, were nearly all "unacclimated;" whilst old and young, who were born and resident in our parishes, out of this yellow fever locality, were last year in the same predicament. The yellow fever is now prevailing amongst children under one year old. My own child, Henry Summers, born last November, had the disease a few days ago, and is now "acclimated." My two other children, born here, became "acclimated" last year; both born since the epidemic of 1847. I am now accosted on this wise constantly, in visiting patients: "*I wish you would look at the baby, it don't seem well,*"—

the infant having nothing more nor less than the yellow fever. Infantile yellow fever is, happily, a light disease; in ordinary cases, it requires little, if any, medical treatment. Nature espouses the cause of the little patient, and the case generally goes on well. The body may be sponged with cold spirits, and an aperient administered if necessary. We must, however, be on our guard; if the febrile action be high, convulsions may come on, and black vomit may attack very young children. Last year I saw convulsions, black vomit, and death, in case of a native born child, of 18 months. I saw the same thing, with recovery, in 1841: A boy, six years old, born and resident here, was attacked with black vomit last season, and was a month recovering. The same principles of treatment should be applied to children as to grown persons. Common sense will teach caution in the use of quinine and opium in such cases, as they strongly affect children. In most of the cases nothing will be required. A simple refrigeration of the body with cold spirits, and wrapping the child afterwards in a blanket, will put an end to the febrile symptoms.

I cannot close this communication without noticing one characteristic of yellow fever which should never be lost sight of by either physician or patient. The visible, cognoscible symptoms of yellow fever are often speedily put an end to, either by nature, or through the agency of medicine. The patient is to all appearance perfectly liberated from the yellow fever agency, whatever it may be, or the patient may have, on the contrary, gone through a tedious and dangerous attack, and have attained this condition. Nevertheless, a *relapse* may take place, and the identical or still worse symptoms may afterwards attack the patient. Some have advanced the monstrous absurdity that it is not right to put an end to the symptoms too soon, for fear of relapse, and the same argument might be addressed to Nature when she "cures" the patient at the very onset. We shall not stop to discuss such inanities. We merely wish to notice the undoubted fact, that when the visible and cognoscible symptoms of yellow fever disappear, by what means soever this may be brought about, the yellow fever agency, whatever it may be, still remains with the patient in a latent, innocuous and inactive state in all cases, for a considerable length of time. I have known a mortal relapse to take place in five weeks after the subsiding of all the visible symptoms of a protracted first attack.

No physician who is at all acquainted with yellow fever will pretend that there has been an interregnum in the yellow fever agency, and that this is a separate and distinct attack. There is no second attack in yellow fever, according to my experience of eighteen years in this city. I would sooner expect a second attack of small pox. I could cite many instances to show that this latent condition may continue for weeks, and that the above morbid symptoms may again arise. That there is no separate, independent attack, is perfectly conclusive. If, then, the yellow fever agency is thus persistent, how solemnly are the physician and patient warned to guard against all causes that may tend to awake the sleeping destroyer in his lair!

But I draw this epistle to a close, already too extended.

Rousseau Street, Fourth District, New Orleans, Sept. 25, 1854.

Part Second.

EXCERPTA.

ART. I.—*Dr. Smith, of Warsaw, on the Hydrochlorate of Ammonia, and its Therapeutic Uses. Translated from the "Revue de Thérapeutique Medico-Chirurgicale:" By* M. MORTON DOWLER, M. D.

The discovery and therapeutic application of the hydrochlorate of ammonia, muriate of ammonia, sal ammoniac, *hydrochloras ammonicus, sal ammoniacum, ammoniacum muriaticum, chloretum ammonici*, may be traced back to a very remote period. It is said that its name, *sal ammoniacum*, is derived from that of the province of Ammonium, in Lybia, in which stood the temple of Jupiter Ammon. Blancard ("*Lexicon Medicum Renovatum, editio tertia Hallæ*, 1739) expresses himself in these terms: *Ammoniacum sal nativum, Cyrenaïcum, veterum a majoribus descriptum, sub arenis Lybicis concrescebat. Ex notis bonitatis quas Plinius indicavit, nostro hodierno factitio similium apparet. Verum, nostralibus nonnisi artificiale cognitum est. Quod nihil aliud est, quam sal factitium, compositum, volatile, sublimatum, ex urina et sale communi, vel sale gemmæ et similibus. Tale et circa ignivomos montes crustatur variis terræ locis. Vesuvi optimum censetur hodie. Eligitur candidum ac purum, vel nisi satis nitidum fuerit, purificari potest.—Præstantius habetur Antuerpianum et Venetianum. Alchimistæ in suis libris varia et partim monstrosa nomina, prodesignando hoc sale adhibent, quibus referendis supersedere liceat. Nominis ratio obscura. Forte ab arena, quia olim credebatur ex urina jumentorum in arenosis Lybiæ condensari, aut a Jove Ammone, ad cujus templum per arenosos illos tractus iterfaciendum est. Dicetur et sal mirabile et clavis metallorum quæ fluere facit. Signum ejus chimicis est asterismus.* At any rate, the primitive manufacture of this article was in Egypt, and hence it received the name of Sal Ammonici Ægyptiacum. In Germany, it was manufactured for the first time in 1759, at Braunschweig. At present, it is manufactured in various ways, as by the dry distillation of bones, and the combination of ammonia which they form with hydrochloric acid. The salt which is used for medicinal purposes should be purified by being dissolved in boiling water, and by repeated crystallizations. The hydrochlorate of ammonia crystallizes with great facility. Its crystals are prominent hexaëdral prisms, of a white color, and have a sharp, saline taste. It dissolves in three parts of cold water; boiling water dissolves still more. It is but little soluble in alcohol.

Action of the Hydrochlorate of Ammonia on the Animal Economy.

Locally, the salt acts as an irritant, but not to the same extent as the carbonate of ammonia. Internally administered in small and medium doses, its action resembles that of all the neutral salts, acting on the intestinal mucous membrane, and producing a cooling effect analogous to that of cold water. It neither acts on the nerves nor vascular system, nor does it sensibly affect the pulse. Nevertheless given in doses of eight grammes, (℥ ii) and continued for several days it brings about (as do all the ammoniacal salts) a sensible increase of the cutaneous transpiration, increased secretion of urine, and in a special manner a notable increase of the bronchial mucus. The pulse undergoes little or no change. But if the doses be still further increased, and if they be for a long time continued, the nervous system becomes affected. The head then suffers, and there is cephalalgia vertigo, and in the mean time, the stomach and intestines secrete a greatly increased quantity of mucus. The digestion becomes simultaneously deranged, and there are vomitings. The patient acquires (although rarely) a sickly color, the cutaneous transpiration, the urine and the bronchial secretion strongly increase, while the animal heat undergoes no sensible change, so that sal ammoniac appears to act in general on digestion and nutrition in the same manner as the alkalis and the other salts.*

Given in large doses, it produces irritation of the *primæ viæ*, and violent vomiting, diarrhœa, diuresis, copious perspiration, and all the symptoms of inflammation of the stomach and intestines.

Gumpert † has observed, that as a result of the administration of sal ammoniac, given in quantities of from 15 to 24 grammes (℥ ss to ℥ vj) in 24 hours, in the treatment of strictures of the urethra, indurations of the liver, chronic cystitis, &c., (in which case it is necessary to give 250 *grammes*, (℥ viii) at this average, in the space of about four weeks,) it produced a combination of symptoms which simulated the pituitous diathesis. The patients swallowed with a kind of repugnance ; their eyes were heavy and acquired a peculiar lustre. The whole body had a sense of fatigue, lassitude, and depression ; the the tongue was white, the patients felt a continual biting sensation between the skin and the muscles. They coughed continually, owing to a sense of tickling in the throat, without, however, being able to expectorate much mucus. Their stomachs, though empty, loathed food. Ordinarily the perspiration is increased, and the sweat breaks forth every time the patient moves. The urine flows abundantly, has a strong ammoniacal smell, and sometimes becomes putrid, without losing, in consequence, its ordinary clearness. The stools contain a glairy and glassy mucus, though this is not, nor is diarrhœa constant attendants. This state does not continue long. The patients have an attack of fever, which ends in an abundant perspiration. After the paroxysm, which resembles that of an ordinary intermittent

* Wibmer (Beitraege z. Heilkund t. 1, 1849.)

† Schmidt's Encyclop., t. 1, p. 116.

ever, there is a complete remission, during which time the symptoms, before mentioned, successively disappear. There remains only for a long time, a loathing of sal ammoniac, followed by a notable improvement in all the morbid phenomena. When this remission does not take place the consequent paroxysms assume the septenary type. They manifest themselves so long as the system is sufficiently under the influence of sal ammoniac, and they cease or become more feeble, so soon as this state of saturation disappears. To produce this state, Gumbert found that he never had to administer more than from 250 to 300 grammes (ʒ viii to ʒ ix). In every case it appeared that the sal ammoniac given in large doses, passed into the mass of the blood, unchanged, and that it was excreted by the skin, and especially by the kidneys, without undergoing any alteration. *Thinefeld* poisoned rabbits with strong doses of sal ammoniac, and he found in their urine ammoniacal crystals.

Therapeutic Application.

The hydrochlorate of ammonia is one of the most heroic agents of the whole Materia Medica. The physician may give it without apprehension at the outset of disease in cases in which he has not yet been able to determine either the kind or character of the malady to be treated.* It essens the irritability of the vascular system, augments and favors the secretion of the gastro-intestinal and bronchial mucous membrane, rouses the functions of the skin and kidneys, and it very often suffices alone successfully to combat the disease without disturbing in any manner the critical efforts of nature. In many cases it is preferable to mercury, to iodine, to antimonials, being one of the best alterative medicines of which the Materia Medica can boast.

The long continued action of this agent produces a serosity of the blood, and may even coagulate it. *Wibmer*, *Fischer*, *Oesserlen*, *Koesch*, and others maintain, on their own experience, that this great solvent can determine the resolution of membranous and glandular tissues in hypertrophies, indurations, and scrofulous infiltrations into different organs and tissues; as in tubercles, schirri, induration of the

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*"Le médicin peut le donner sans crainte dans tons les cas où il n' a pas encore saisi dans les premiers jours ni le genre ni le caractère de la maladie," which, being very literally translated, *mot-à-mot*, would read thus: "The physician may give it without fear in every case in which he is understood in the first days, neither the kind nor character of the disease." While we have given a very free translation of the author, in order to avoid broken English, we have maintained strict regard to his meaning. How our Polish physician, who appears to be a man of eminent attainments and extended experience, should have given the absurd license to prescribe sal ammoniac, contained in the above sentence; it is not easy to conceive. We should say positively, and *a priori* that no physician should give his patient sal ammoniac or any other such potent agent, "sans crainte," unless he has "discovered the kind and character of the disease." That is a singular *sine quâ non*.

So vast a tribute to the merits of sal ammoniac falls little short of that which was made to this agent in "Peregrine Pickle," by the erudite old doctor, who was so wonderfully carried away and captivated with the whole of antiquity. He resolved to get up a dinner to a large circle of distinguished friends, and to have the whole repast served up according to the manner of the ancients, to enable him to show off by contrast, the handy-work of one who was regularly *Mageiricos*, with the foppery and superficiality of the modern gastronomers. Amongst his other *barreries* of dishes and condiments, he concluded to invoke the assistance of Jupiter Ammon to help his guests to a good digestion, and to this end, he seasoned his numerous viands with a table sprinkling of sal ammoniac. The classic feast naturally terminated in a general stampede; and the novelist gives a graphic description of the effect of the condiment on the healthy organism which its emetic properties is exhibited to the highest advantage. [*Translator.*]

prostate, of the spleen, liver, &c., in blenorrhagia of the bladder, (vesical catarrh,) induration of the mammæ, and even in hypertrophy of the pylorus and œsophagus. This resolvent virtue that we have ourselves observed, has led to its very general employment, especially in Germany and Poland, in place of the alkalis, the antimonials, iodine, and mercury. It is but little employed in France and England. In my practice, which extends through twenty years, I have been able fully to appreciate this salt, and the frequent use I have made of it, enables me at present to designate the affections in which it is really efficacious. All that I advance has been consigned to my journal, and rests in rigorous observations and experience.

Pituitous Fevers,—Mucous Fevers.*

The pituitous fever frequently appears in the localities where intermittent fevers prevail. This disease makes its appearance sporadically, during warm, humid summers, especially amongst the country people, whose diet consists of farinaceous food, milk, water, &c. The period of the first appearance of the fever is often difficult to ascertain, and it is not till after an attentive inquiry, that it can be made out, for, in general, the patient has not recourse to the physician, till the eighth or ninth day from the invasion of the disease. In no year have I had to treat so great a number of diseases of this sort, as in the years 1843, and in 1852,–'3; cases which I treated in connection with intermittent fever.

The following are the symptoms of this fever: After eight or ten days of initiatory symptoms, which exhibit themselves in loss of appetite, regurgitation, indigestion, orbitary cephalalgia, a tongue charged with a whitish mucus, with a clammy taste, eructations, diarrhœa, a light chill every evening, traversing the back and thighs, followed by a sweat, which greatly enfeeble the patient. The pulse becomes gradually frequent; the teeth, gums, and tongue are covered with a thick mucus, and the fever has exacerbations and intermissions, which are so slight as to be scarcely discernible. The patient has a bloated and leuco-phlegmatic appearance, and the eyes and mouth are encircled with pale bluish rings. Towards morning there is vomiting or retching, coughing up of saliva and mucus, slight thirst, great depression, urine red and clouded, diarrhœa, nocturnal sweats, a miliary eruption, insomnia, slight delirium, showing that the nervous system has fallen into the disease. I have observed amongst many young and robust subjects, attacked by this disease, that it took on a sub-inflammatory character; that hepatitis, bilious fever, and especially passive pneumonia and accute pharyngitis, complicated the malady, and

* The Poles appear to suffer severely from the *pituitous* diathesis; also the French people. Sue makes two of his characters, in the "Mysteries of Paris," fight a duel on the strength of it. M. Robert demands of the Duc de Lucenay if it be true that he has asserted that he, M. Robert, has phlegm. M. le Duc makes it a point of honor, to neither avow nor disavow that he has so asserted. M. Robert challenges the duke, and a shot is exchanged. The duke then comes forward, and frankly declares that it had never entered into his head, to either think or say, that M. Robert had phlegm. M. Robert's feelings overpowered him, at the manly avowel of the duke. [*Translator.*]

that it became transformed into a quotidian intermittent fever, or into a masked intermittent, with cephalic symptoms. Ordinarily the cure was slow; if the patients had long delayed resorting to medical aid, they had great difficulty in re-establishing their bodily and mental forces. I have observed, at the same time, that during its prevalence this year, colics, diarrhœas, and cardialgias frequently made their appearance.

The plan of treatment pursued, and which was successful with me in all of my cases of pituitous fevers, was as follows: 1st, to increase the fluidity of the mucus, and to thus facilitate its expulsion by the exhibition of sal ammoniac: 2d, to expel the mucosity by purgatives: 3d, to fortify the digestive organs, and subsequently attack the cause of the disease, by appropriate dietetic and pharmaceutical means: and 4th, to endeavor to bring about a favorable crisis.

A servant, aged 34 years, having been for eight days complaining of a clammy state of the mouth; cephalalgia; lassitude; depression, with whitish tongue; red urine, colic, thirst; pulse ninety, coughing continually, night sweats, chills, and flushes of heat. I gave him sal ammoniac in the quantity of eight grammes, (℥ ii,) in two hundred grammes, (℥ vi,) of tilia water, and thirty grammes (℥ j) of oxymel, of which he took a table-spoonful every hour. Two days after, the patient insisted on taking an emetic, and it was given him in compliance with his request, but it produced only the ejection of a glairy fluid, and an operation by stool. On the fifth day, the above ammoniacal mixture was resumed, with the addition of 0,05 centigrammes ($\frac{3}{4}$ grain) of tart. antim. On the seventh day, the tongue was much coated; gums and teeth covered with thick mucus, pulse 87, thirst, depression, insomnia. (emeto-cathartic.) On the eighth day bilious vomitings, and eight glairy stools, tongue whitish,—sensible improvement; the same pituitous stools as on the seventh day, (laxative decoction, which produced ten muco-bilious stools.) The tongue, on the thirteenth day, was clean on the edges; the mouth less clammy, pulse ninety, urine red and highly charged, perspiration abundant; rested well at night, bronchial expectoration abundant. I then prescribed a decoction of acorus aromaticus, with infusion of valerian, and compound tincture of cinchona; a strengthening regimen; and, at the end of twenty-four days of this treatment, the patient's health was entirely re-established.

All the other observations that I have made in pituitous fevers accord with the above case, which I have taken indifferently from amongst a large number of others, excepting that I have seen some cases in which it was necessary to repeat the sal ammoniac from two to four times before prescribing purgatives. The indications for the repeated employment of the salt are based on the state of the tongue, the mouth, the taste, and the continued cough. Here the sal ammoniac acts principally as a resolvent, especially on the mucus membrane. Its effects on the abdominal viscera are very marked, and this effect is subsequently propagated to the lungs through the medium of the ganglionic nerves. In gastro-bilious and rheumatismo-catarrhal fevers, and even in inflam-

matory affections of special organs, we must not expect the sal ammoniac given *alone* to act as an evacuant; for in the dose in which I administer it, the salt never produces evacuations. It only regulates and excites the mucous secretions, and under these circumstances it is often preferable to calomel. Bennius, Mager, Raimann, Sarcone, Sobernheim, Oesterlen and others extol highly its efficacy in the afore-cited fevers, and I can add that I have made extensive use of this medicine, which I believe to be extremely efficacious in gastric, and especially, in mucoso-bilious and rheumatismo-catarrhal affections.

Pituitous State of the Stomach and Intestines.

One of the morbid conditions in which sal ammoniac renders the most important service, is the mucous or pituitous state, *(status pituitosus, blenhorrhœa ventriculi et intestinorum)* which is very frequently met with in practice, in an inveterate chronic form. This malady is often seen in aged phlegmatic subjects. The following is a case in point: Mr. Riehl, agriculturalist, aged forty-two years, presented the following symptoms: foul tongue, covered with a layer of thick mucus, augmentation of the mucous secretion in the mouth and in the throat, loss of appetite, nausea in the mornings, continual cough, bad, gluey taste in the mouth, dull pains in the epigastrium, a feeling of constriction in the belly, which exhibits swelling with meteorism, and is affected with slight colic pains, obstinate constipation, stools covered with thick transparent mucosity in great abundance, pulse regular and slow, (68), no thirst. This state had continued already many weeks, and the patient became greatly emaciated. Before I saw him he had had leeches applied over the pit of the stomach, and on the abdomen. As these symptoms plainly denoted an inveterate pituitous diathesis, I prescribed sal ammoniac in the following manner:

℞—Ammon. Hydrochlor.
Succ. Glycyrrh. ā ā ʒ ii
Aq. Fœniculi ʒ vj

A table-spoonful every hour. Give nutritious broth, with wine and water. This mixture repeated three times in six days. On the eighth day the following:

℞—Tart. Antim. gr. ii
Ipecac ʒ ss

Divided into four parts, and one to be taken every four hours. The medicine produced bilioso-mucous vomiting, and abundant pituitous stools. On the sixth day medicines discontinued, appetite returned; gave nourishing broth, and a little roast beef, with horse-radish, red wine or good beer. On the seventh day, the preceding sal ammoniac mixture; pulse continues at 70; no thirst. On the eighth and ninth days, a purgative of senna, rhubarb and phosphate of soda, followed in an hour after by a cup of strong coffee, and a strengthening regimen adopted. On the following day there was a gradual disappearance of all the before-

mentioned symptoms; appetite, sleep and spirits returned, and I then prescribed for the patient as follows:

℞—Tinct. Cinchonæ Compos. ℥ i
Ext. Aloes Aquos. gr. iij
Tinct. Humuli ʒ iiss
Tinct. Rhei Aquos. ℥ ss

Three dessert-spoonfuls daily, strengthening regimen, meat, food slightly spiced, horse-radish cooked, good red wine or beer, exercise in the open air. The patient, after having undergone this treatment for four weeks was perfectly restored to health.

Generally this pituitous, or gastric diathesis, is not dangerous; but as it very often puts on a chronic character, it has for the greater part of the time an intermittent and periodic character. It breaks out according to the seasons, with much less intensity, and becomes extremely serious to the patient, in bringing about a dropsical condition.* In these cases, the sal ammoniac, by reason of its primary resolvent effect on the mucus, dissolves, liquifies, and evacuates, downwards the glairy contents, exalts the ganglionic system, and prepares the way to other medicines—at first purgatives, and then tonico-excitants—that this pituitous diathesis demands. In all these cases the strengthening regimen is indispensable to a complete cure, and to the prevention of relapse.

Intermittent Fevers.

As intermittent fevers are very often complicated with the mucous diathesis, especially during the spring season; the administration of the hydrochlorate of ammonia has been recommended by various practitioners, not as a febrifuge, but as a prelude to the administration of quinine. Before the discovery of sulphate of quinine, the sal ammoniac was mixed with Peruvian bark, and with opium, and administered an hour or half an hour before the ordinary time of the paroxysm. *Jacobi* and *Vogel* prescribed the hydrochlorate, sometimes in powder, and other times in mixture. Meanwhile, it is well known, that if the attacks of intermittent fever are frequent, or if the paroxysms are too rapidly arrested, the primæ viæ are generally in a state of disturbance, and that abdominal viscera are left in a state of inaction; but in administering the hydrochlorate at first, during three or four days, we obtain a more complete apyrexia, throw off the crudities from the digestive organs, and thereby render the sulphate of quinine more efficacious. We have lately treated a great number of spring intermittent fevers, tertian and double, complicated with gastric symptoms, in which there was not a free apyrexia, and we have never prescribed the sulphate of quinine in these cases, without preceding it by sal ammoniac.

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*I have at present a little negress under treatment, strongly affected with ascitis and anasarca, resulting from a previous attack of scarlatina. She has now taken sal ammoniac for three days, in doses of two grains every four hours, dissolved in water. It has acted strongly on the skin, and especially on the kidneys, and there is a great diminution of the dropsical distension.
[Translator.

Pleuro-Pneumonia.

The employment of the hydrochlorate of ammonia in inflammation of the respiratory organs, has given a high reputation to the medicine, which is fully justified by the eminent service it has rendered in the hands of those by whom it has been administered. It is especially after the employment of antiphlogistics, that its well marked action on the bronchia and pulmonary parenchyma is observed.

It augments and facilitates, at first, the mucous secretion, renders respiration and expectoration more free, the cough less fatiguing, promotes diaphoresis and diuresis—crises by which the disease comes to a favorable termination. Even in the course of other diseases complicated with bronchitis or pneumonia, as in typhoid fevers, acute exanthemata, in inflammations of the liver, &c.; or, where the cough is dry, where the pain in the breast is light, where the expectoration is difficult, and even where there is diarrhœa, the use of this salt is not contra-indicated. In such cases we prefer giving the medicine in the decoction of senega.

Asthma and Bronchitis.

In humid and pituitous asthma, in chronic bronchitis, chiefly of old persons, the working bakers, millers, carpenters, &c.; when after an attack of the characteristic dyspnœa, the patient is seized with a dry cough, which later in the disease becomes humid, the sal ammoniac is of the greatest utility. A baker, Oct. 26, has an attack of this kind two or three times annually. At each attack his expectoration is mucous, gluey and even purulent, intermixed with striæ of blood. I always have succeeded in the space of from eight to ten hours, in these attacks, by administering sal ammoniac and sulphur; and afterwards administering bitter expectorants.

We ought to remark that this agent, administered for a long time, and especially to patients already feeble and aged, that may be attacked with asthma or chronic bronchitis, may become very injurious. For if on the one hand it favors the formation of bronchial mucus, and facilitates expectoration, experience on the other hand proves that its prolonged use is dangerous when the expectoration is more or less difficult, or even impossible.

Gum ammoniacum, camphor, ipecacuanha, oxymel of squill, and the antimonial preparations are indicated as first; and it is only when the mucus is tenacious, and the *râle* is mucous and abundant, that the employment of sal ammoniac is indispensable.

Phthisis Pulmonalis.

The ancients as *Lentin*, *Mar;* and more recently *Cless*, *Most*, *Doerfelt*, *Roesch*, *etc.*, recommend sal ammoniac in pulmonary consumption. Meanwhile there is a diversity of opinion, not only in relation to its efficacy in this disease, but in relation to the proper period in the disease in which it should be administered. Thus *Kortum* and *Roesch* praise the medicine in the crude state of tubercle, whilst others prefer it when the object is to soften the tuberculous masses, and to favor their expectoration. The

latter view, the most natural, and which is confirmed by the most satisfactory observations accords with mine. It agrees with my experience extending through twenty years, in the midst of an industrial population, (manufacturers of cotton tissues,) where I had an opportunity of administering this salt, and of observing its effects on a great number of consumptives. It is true I have never seen any case of this kind cured by it, but its action was strongly marked in cases in which the object was to soften the tubercles, allay the fatiguing cough by the expectoration of the tuberculous mass and even to arrest the purulent secretion in the vomicæ. If we further reflect on the softening action of the sal ammoniac, of which we have already spoken at the beginning of this memoir, it will not appear astonishing that its effects extend themselves to the tuberculous masses, and favor their expulsion. It is only in cases in which the intercostal pains, have been removed by appropriate antiphlogistics, when the pulse has become less frequent, and the cough less dry, that the use of sal ammoniac is indicated. It is then that the dyspnœa disappears, the respiration becomes more free, and the patients obtain a respite for a greater or less time.

Laryngeal Phthisis.

In laryngeal consumption this salt enjoys some degree of reputation. *Neumann* recommends here sal ammoniac in powder with an equal part of powdered liquorice root. He even contends that this combination is sufficient to effect a cure, being given at the very beginning of the disease, and that by virtue of its stimulant action, it has the power of subduing this affection. His conclusion is in some measure true, for in the treatment of all the chronic phlegmasiæ, one of the objects to be obtained is to produce a new activity in the part affected. Thus in laryngeal phthisis, we ought to employ means which strongly excite the throat, and which by that means transmit the irritation to the larynx. Here the sal ammoniac, given in powder, is in a state to produce the desired result. It is right, notwithstanding, that I should acknowledge, that in many cases of laryngeal phthisis, I have been obliged to suspend the employment of the medicine, owing to the irritation, to the cough, and to the suffocation that its contact with the throat produced; the patients generally refusing to continue it, and it becomes necessary to resort to demulcents and lubricatives.

Croup and Pseudo-membranous Bronchitis.

Dr. Hirtz cites a case of inveterate croup, (*Gazette Médicale de Strasbourg*, 1853,) in which the administration of sal ammoniac in powder, succeeded better with him than any other means. I have, myself, treated the case of an infant two years old, affected with croup, in which this salt, after the application of leeches, and the administration of emetic, did not produce any effect. In the case of a young girl aged sixteen years, attacked with pseudo-membranous bronchitis, the sal ammoniac has succeeded better in my hands. This girl was of a robust sanguine constitution, and had been laboring for two days under a hoarse cough, with crowing inspiration, and acompanied with

aphonia. After having passed the night in a cold room, she remained an hour and a half at church, and returned to her house almost suffocated. The cough became more hoarse and low, the inspiration dry and hot, and hissing; pain in the larynx and trachea, the patient was almost aphoniated, constant tightness and suffocation; pulse frequent, face lived-red. She had been bled before my arrival, I gave her gr. iiss of tart. antim., which vomited her, and the debris of tubular false membrane, was observed to be thrown up. After the application of leeches to the anterior region of the neck, I prescribed sal ammoniac in a mixture for six days. This mixture, by its solvent effect on the bronchial membrane, accompanied by a copious diaphoresis, succeeded in re-establishing the health of this young girl in the space of ten days. This is the only case of acute pseudo-membranous bronchitis, that I have observed in my practice of twenty years.

Angina.

The catarrhal angina, called pituitous, lymphatic, œdematous, false, watery, serous, mucous, is the affection in which sal ammoniac produces the surest results. If we reflect that the disease sets in with a sense of dryness and constriction in the throat, in consequence of the suppression of the buccal secretion, by pricking in the neck, and in the tonsils, by great dysphagia; that later in the disease the secretion of mucus becomes more abundant, and that on an attentive examination, there are discovered small white suppurating points in the interior of the isthmus, that a disagreeable odor is exhaled by the patient in ejecting an abundant stringy saliva, that in fine, the inflammation arrives at this point that the patient is affected to suffocation, when the tonsils are entering into suppuration, we may readily presume that the sal ammoniac is a most heroic agent in controlling this affection. The use of this salt is more efficient than either leeches or blood-letting, from its acting directly on the disease, and removing the dryness, producing an abundant secretion of the buccal mucus, and acting more effectually on the disease in either the resolution or suppuration of the inflamed tonsils. It is after a severe spell of cold weather, during the spring and autumn, that we meet with this kind of angina, and an asthenic and typhoid character has been erroneously attributed to it. The best means that can be used in these cases are cooling diaphoretics, and especially, the sal ammoniac. A butcher was attacked with a severe sense of chilliness, and the result was that catarrhal angina appeared with all its violence. The patient had the severest suffering in making the least attempt at deglutition, and he had ear-ache, enormously swelled tonsils, discharge of a liquid, stringy, limpid mucus, tongue coated, dyspnœa, cephalalgia, thirst, pulse 110. This state had existed for three days. I ordered the following mixture:

℞—Am. Hydrochlor.
Succ. Glycyrrh. ā ā ℥ iiss
Aq. Tiliæ ℥ vj
Tart. Antim. gr. 5-6
Syrup. Simp. ℥ j

Every hour a table-spoonful. Under the use of this mixture there was a notable diminution of the difficulty of swallowing, the buccal mucus secretion was re-established, and on the fifth day one of the tonsilar abscesses discharged its matter, and on the next day the other. Astringent gargles soon completed the cure. I have treated a great number of quinsies that have occurred in my practice by the sal ammoniac alone. In case the above mixture should be very repugnant to the taste of the patient, the dose may be given in a cup of barley water, which will render the taste less disagreeable. In these cases the hydrochlorate of ammonia, as a light irritant, favors the resolution of the tonsillar inflammation, hastens by its contact the suppuration and opening of the abscesses, and counteracts the production of the chronic morbid condition that, as a result, may set itself up in these glands; furthermore, it facilitates the expulsion of the tenacious mucus that accumulates in the throat and mouth. *Loeffler* and *Freyer*, (Materia Medica, Warsaw, 1817, t. ii, p. 350,) *Hauser* and *Hornurg*, (Hufeland Jour. t. xliii, p. 2,) have corroborated the efficacy of the medicine in quinsy, and affirm, as my own experience proves, that it has never been found inefficient in catarrhal and œdematous anginas.

Diseases of the Bladder.

A. F. Fischer was the first who recommended sal ammoniac in induration and swelling of the prostate and bladder, vesical catarrh, in suppuration of the bladder. *Lately*, *Hufeland*, *Crammer*, *Blum*, *Kuntzmann*, *Most*, *Bultner* have verified by repeated observations that this salt undoubtedly cures affections of the vesical mucus membrane. I have had occasion to treat many cases of vesical catarrh, and I have now before me a case of an officer, æt. 70, to whom I prescribed this salt with great success. This subject was affected with gout, but had never labored under any symptoms which would have given rise to the presumption of disease in the bladder till within the preceding three weeks he perceived that his urine was deranged, and charged with mucosity, that the sediment which fell to the bottom of the vessel was tenacious, whitish and yellowish, inodorous, and often in a considerable quantity; that micturition was more frequent, attended with violent pains, which extended to the prostate. As the secretion of morbid mucosity increased, the micturition was proportionably slow, the urine escaped *guttatim*, and with great effort, and he very frequently having to arise from fifteen to twenty times in the night. The mucus became thicker, more stringy, resembling jelly. After having treated the irritation of the bladder by emollient remedies, to which I added opiates and extract of cicuta, I prescribed the following mixture:

℞—Fol. Uvæ Urs.
Radicis Ononis Spinosæ
Fol. Diosm. Crenat. ā ā. ℥ ss
Boil with Aq. Font. q. s.
Strain, and reduce to ℥ ix; add
Sal Ammon.
Succ. Glycyrrh. ā ā. ʒ ii to ℥ ss

Take six or eight table-spoonfuls daily. The effect of this mixture on the mucous secretion of the bladder was most remarkable. Regularly as the patient took the medicine, the urine, at first highly colored, became clear, the sediment daily decreased, as also the uric acid, the mucus less thick, and the micturition less frequent and less painful. At the present time the urine is almost entirely natural, and the patient is in an excellent state of health. I am at present giving him oil of white pepper, with the tincture of cynorrhodon,* a prescription extolled by *Rademacher*. I have since obtained a like success in the case of two other old men, afflicted with chronic cystitis, and who have also undergone the sal ammoniac treatment. The rationale of this treatment in vesical catarrh will readily occur to all when we refer to what we have already said, touching its action on the mucous tissues of the organism. If it modifies the diseased suppurating surfaces, the gastro-intestinal and bronchial mucous membranes, we might naturally expect a still more remarkable effect on the mucous membrane of the bladder, for, given in large doses, and often repeated, it enters the mass of the blood, and from hence it is found anew in the urine, and in direct contact with the mucous membrane of the bladder.

I respectfully invite my esteemed *confrères* to make a trial of this mixture in the aforenamed cases, and they will be enabled to realize the progressive changes which take place in the mucoso-purulent discharge from the bladder. They cannot fail to be convinced of its efficacy in diseases of the urinary passages, which so often attack aged men. It is useful also in urinary gravel.

Its effects in various affections.

This salt is highly extolled in indurations of the liver, spleen, ovaries, stomach, and rectum. It is proper that I should say, that in these affections I have derived from it but little advantage. *Ellinenger*, (Schewitz Zeitchrift, 1847, p. 202,) speaks of producing by this remedy, the absorption of purulent effusions into the lungs and brain. *Fischer* has cured a stricture of the anus, by the remedy, (*Casper's* Wochenschrift, 1834, No. 329.) *Rademacher*, the new empirical reformer in Germany, has, for the last twelve years, recommended it in *catarrhal dysentery*, and in catarrhal inflammations of the bladder and urethra. *Schlussel* prescribes it in leucorrhœa and gonorrhœa, in doses of two grammes (thirty grains) every hour, dissolved in water, with bitter extracts. It has been extolled in anasarea and ascitis, resulting from suppressed perspiration; and also in ulcerations of the vagina and uterus. As it is incontestible that the suppurating surfaces have a great analogy to one another, it should not be considered astonishing that this agent should be of great utility in cases of ulcers and suppurating surfaces in these organs. *Rust*, *Fischer*, and *Van Oye*, praise it highly as a resolvent, in affections of the prostate, and especially in cases of induration. I have treated only one case of this affection, which appeared to be very greatly modified, by the administration of sal ammoniac, in large doses.

*Rosa canina, dog rose.

External employment of Sal Ammoniac.

Externally we employ sal ammoniac as a cooling and soothing medium, a resolvent, a gentle excitant; a promoter of resorption. It is especially beneficial in contusions, infiltrations, glandular swellings; in hernia; fractures; in apoplexy; in light burns of the skin; in hydrocele; and warty excresences. The cold which is developed during its solution in water, resembles that produced by the action of water on common salt. In all cases, *Hauser*, *Rust*, *Graefe*, and *Schmucker* obtained excellent results from fomentations with this salt, in the form of saturated solution, in rheumatic hydrarthrosis of the knee, and in hydrocele and chilblains. In angina, where there is a great accumulation of mucus in the throat, a weak solution of sal ammoniac is extremely useful. In mammary abscesses, and in engorgements, this agent, according to *Vogel*, and other authors, is of undoubted utility. Fifty grammes of this salt (℥ iss.) should be dissolved in five hundred grammes (℥ xvi) of water, with the addition of sixty grammes (℥ ij) of alcohol; and with this a tepid fomentation should be made for the breast, through the medium of flannel. This salt is also used to recall suppressed perspiration to the feet. It further modifies the condition of gangrenous and malignant ulcers, when applied to their surface, in the form of powder; and, finally, it is given in the form of enemata, in asphyxia and syncope, as an excitant.

The best mode of internal administration is in the form of powder with *succ. Glycyrr.* in solution, or in pills. In administering it, we must avoid the simultaneous employment of the fixed alkalis, lime-water, acids, calomel, or alum.

FORMULÆ.

I.

R—Amm. Hydrochlor. ʒ ij—8 grammes.
Potas. Nitratis, ʒ j—4 grammes.
Aq. Framboes. Destil., ℥ vi—200 grammes.
Syrup. Corticis Aurant. ʒ vijss—30 grammes.
M. ft. mist.

A tablespoonful to be taken every two hours, in the early stages of rheumatismo-catarrhal fever.—(Clarus.)

II.

R—Amm. Hydrochlor.
Succ. Glycyrrh. ā ā. ʒ ij—8 grammes.
Aq. Tiliæ. ʒ vj—200 grammes.
Tart.Antim.gr. $\frac{3}{4}$ to gr. iss—0.05 to 0.10 centigrammes.
M. ft. mist.

Every two hours a tablespoonful, in the pituitous diathesis, in the commencement of catarrhal fevers, in the second stages of pneumonia, in augina, and asthma.—(Smith, of Warsaw.)

III.

R—*Anti Emetic draught of Rivière*, ℥ ij—60 grammes.
Aq. Fœniculi, ℥ v—150 grammes.
Am. Hydrochlor. ʒ ij—8 grammes.
Succ. Glycyrrh. ʒ iss—6 grammes.
Tart. Antim., gr. ¾—0.05 centigrammes.
M.

Take a tablespoonful every hour, in pulmonary consumption, to promote the expectoration of tuberculous masses.—(Smith, of Warsaw.)

IV.

R—Ammon. Hydrochlor.
Succ. Glycyrrh, ā ā gr. xv—1 gramme.
Ext. Leon. Tarax. (*pissenlit, piss-a-bed ;*) q. s.

Make into a bolus. Take a bolus every two hours, in cases of induration of the prostate, bladder and ovaries; strictures of the urethra, œsophagus, stomach and intestines, with good wine or beer, and nourishing regimen.—(Fischer.)

V.

R—Ammon. Hydrochlor. ʒ ij—8 grammes.
Aq. Rutæ. (*eau de rue,*) ℥ viij—250 grammes.
Acet. Rutæ, (*vinaigre de rue,*) ℥ iv—120 grammes.
Tinct. Arnicæ, ʒ ij—8 grammes.

For fomentations in contusions, hydrocele and extravasations.—(Clarus.)

VI.

R—Ammon. Hdrochlor. ʒ ij—8 grammes.
Assofœtidæ.
Aloës Socot. ā ā ʒ i—4 grammes.
Succ. Glycyrrh. q. s.

Make into pills of the weight of 0.10 centigrammes, (gr. iss.) Give from six to ten pills daily—in obstinate obstructions of the liver and spleen.—(Radius.)

Art. II—*Ovariotomy Performed with Success.*—Translated from Rev. de Thér: By M. Morton Dowler, M. D.

Professor Langenbeck has performed ovariotomy with success, in the case of a maiden, thirty-four years old, who was afflicted with a tumor of the ovary. The tumor had existed for five years, and had become so largely developed, that the abdomen appeared to be that of a woman at the full period of utero-gestation. The patient was operated on under the influence of chloroform. Professor Langenbeck began by making an incision in the parietes of the abdomen, on the *linea alba*, to the extent of two inches and a quarter; and proceeding from below upwards and commencing a little above the symphysis pubis. The cyst laid

bare, and, fixed in the wound by means of a crochet, was punctured. There flowed out about twenty-eight litres, (19 pints,) of a limpid albuminous fluid. During the flow, the cyst was drawn outwards to its pedicle. The wound was then closed with pins, and the cyst excised on a level with the skin, and afterwards the suture was applied. In nine weeks the wound was cicatrized; but during this time the patient complained constantly of colic pains.

The extirpated tumor comprised, in addition to the ovary, other cysts, of the size of hazel nuts, and also the fallopian tube. The parietes of the cyst were thick, and presented, interiorly, numerous folds. The liquor was strongly albuminous, and exhibited, under the microscope, numerous scattered epithelial cells. This operation is the seventh of the kind performed by Professor Langenbeck. Of these seven, three died, and four recovered.—*Deutsche Klin.*

ART. III.—*Good Effects of Belladonna, in a Case of Spermatorrhœa.*—Translated from Rev. de Thér: By M. MORTON DOWLER, M. D.

A young man, of the purest morals, and of entire continence, came to consult M. Lepri, in consequence of nocturnal pollutions, which, re-appearing every night, and even many times in a single night, began to exhaust his strength, and to effect his moral and intellectual faculties. These pollutions had existed for some weeks, and had not been relieved by any medicine, nor by any of the hygienic means applied, such as reposing on a hard bed, awakening in the night, and the cold affusion, &c. Two years previously, this young man had an attack of miliary fever, which had left, as one of its effects, nocturnal incontinence of urine.

Some months had passed, in which the patient had been sometimes better, and sometimes worse, when, all at once, this vexatious infirmity disappeared.

After having applied a blister to each thigh, advised a tonic and corroborant treatment, and, also, treated the patient with camphor combined with laudanum, alum, &c., without the least success, M. Lapri came to the conclusion that the spermatorrhea, in this case, might have some connection with the previous incontinence of urine; and recalling to mind the good effects of belladonna, in the latter affection, he prescribed this remedy for the patient in the evening, on retiring to bed, and in the morning on rising. At the end of a few days, every trace of the malady disappeared.

ART. IV.—*Translated from Rev. de Thér. Academy of Sciences, sitting of May* 29, 1854*: On the efficacy of ice used in connection with compression, in reducing strangulated hernia, and in combatting consecutive peritonitis:* By M. MORTON DOWLER, M. D.

Mr. Baudens read a summary of a memoir bearing the above title. The treatment that we adopt, says he, with so much success in cases of traumatic lesions—the application of ice, with or without the addition of common salt—we have extended to the treatment of strangulated hernia. In sixteen cases of hernia complicated with strangulation, when all the ordinary means of reduction had failed, the whole number was successfully treated by the application of ice, conjoined with methodic and permanent local compression.

In order to reduce such herniæ, it is evident that first of all, the attention must be directed to the reduction of the excessive development of the tumor, occasioned, as it is well known, by the arrest of the capillary circulation, and by the production of sanguineous congestion. To effect this result ice is of all agents with which we are acquainted the most efficacious. We have two things to expect of it, which it never fails to confer, namely; first: the driving back of the fluids with which the hernia is engorged; and secondly, to put an end to the inflammation, which soon brings about gangrene, if measures are not interposed to prevent it. The effects of ice on a strangulated hernia, are, to drive back the fluids, to allay pain, to condense the intestinal flatus, to efface the hernial tumor, to remove the danger of inflammation, and especially the danger of peritonitis, so often fatal.

Should the ice produce only the latter result, and afford time to arrest the rapid march of danger, should it render precipitancy on the part of the surgeon unnecessary, and enable him without danger from temporising, to exhaust the milder resources before resorting to the knife, a most valuable agent is presented, and one which we are justified in placing the greatest reliance.

The repulsion of the fluids, and the condensation of the expanded flatus, superinduces in the herniaëd parts, a diminution of volume, which gives full power to the taxis, previously powerless.

Spontaneous reduction may take place under the influence of refrigerants alone. We can cite three examples of this. But when it does not thus take place, we must aid in bringing it about, by assisting the ice with a compressing bandage. This joint action has always, up to the present time, succeeded in our hands.

When the tumor is so painful that the least compression cannot be borne, we most always begin with the ice alone. The pain once assuaged, we can, if we judge proper, apply a spica bandage, which is to be covered with pounded ice. In order to give this bandage a continued contractile action, analogous to the fingers, we must provide an elastic band of caoutchouc. This special bandage we call the *spica-taxis.*

We know that the ordinary taxis can be but little applied when the

tumor is inflamed and very painful. We know that there is danger of bruises and even lacerations, however cautious we may be, for the intestine is much less resistent in a state of inflammation. The compression of the taxis, preceded and accompanied by ice, made by an elastic band, does not expose the patient to this danger. To efficiently assist, the pelvis ought to be elevated, in order to facilitate by their gravity the return of the protruded viscera.

It is under these circumstances, after having exhausted, in vain, the whole list of means recommended for the reduction of a hernia, that the surgeon has no other resource except the knife.

In sixteen analogous cases, being the whole number up to the present date, in which we have had occasion to resort to our treatment, we have been enabled to avoid celotomy; and all the patients have obtained prompt reduction of the hernia. These cases are detailed in the memoir presented, and of which we here present a summary.

We are thoroughly convinced that ice is destined to produce a complete revolution in the treatment of traumatic lesions. During twenty-four years, it has constantly responded to our efforts. If it has not yet triumphed, it is owing to the fact that nothing is so difficult to eradicate as prejudice, and especially error. We leave it to the Academy to determine whether or not we have put forth a well grounded and certain guide, and established the truth of the principles we have adopted.

Art. V.—*Animalcular Origin of Epidemics.*

Joseph Leidy, M. D., Professor of Anatomy in the University of Pennsylvania, says (Smith. Contr. Knowl.) that many important diseases have been supposed to originate from parasitic animals and vegetables.

The former are not the true entozoa, for these are too large, and may be detected by the naked eye; but they are considered to be animalculæ so small that they cannot be discovered even with the highest powers of the microscope. But, independent of the fact that the existence of such entities is a mere suspicion, none of the well-known animalculæ are poisonous. At various times I have purposely swallowed large draughts of water containing myriads of *Monas, Vibrio, Euglenia, Volvox, Leucophrys, Paramecium, Vorticelli,* &c., without ever having perceived any subsequent effect.

The production of certain diseases, however, through the agency of entophyta, is no longer a subject of doubt; as in the case of Muscardine in the silk-worm, the Mycoderm of Porrigo favosa in man, &c.; but that malarial and epidemic fevers have their origin in cryptogamic vegetables or spores, requires yet a single proof. If such were the case, minute vegetables and spores, conveyed through the air, and introduced

into the body in respiration, could be detected. The minutest of all known living beings is the *Vibrio lineola* of Müller, measuring only the 36,000 of an inch, and the smallest known vegetable spore is very much larger than this, whilst particles of inorganic matter can be distinguished the 200,000 of an inch in size.

I have frequently examined the rains and dews of localities in which intermittents were epidemic upon the Schuylkill and Susquehannah rivers, but without being able to detect animalculæ, spores, or even any solid particles whatever. I have examined the air itself for such bodies, by passing a current through clear water. This was done by means of a bottle, with two tubes passing through a cork stopper; one tube dipping into the water, the other reaching not quite to its surface. By sucking upon the latter tube, a current of air passed through the former, and was deprived in its course of any solid particles. Ordinarily, when the atmosphere was still, early in the morning, or in the evening, neither spores nor animalcules could be detected. When piles of decaying sticks or dry leaves were stirred up, or the dust was blown about by the wind, a host of most incongruous objects could be obtained from the air; none, however, which could be supposed capable of producing disease.

To assert, under these circumstances, that there are spores and animalculæ capable of giving rise to epidemics, but not discernible by any means at our command, is absurd; as it is only saying in other words that such spores and animalculæ are liquid and dissolved in air, or in a condition of chemical solution. That the air may be poisoned by matters incapable of detection by the chemist, is proved by the emanations from such plants as the *Rhus vernix, Hippomane mancinella,* &c.—*Va. Med. and Surg. Jour.*

Art. VI.—*Vermifuges.*

Inasmuch as but few practitioners can solely from their own experience deduce either the ætiology, pathology, or therapeutics of verminous *maladies,* it has been thought advisable to give a considerable portion of the present number of this Journal to helminthology, with the expectation that the data submitted to the reader's consideration, imperfect though they be, will not only prove interesting, but serve as incitants to further investigations into this neglected department of medical science.

Vermifuge properties of Pumpkin Seed.

The seed of the pumpkin (*Cucurbita Pepo*) a highly valued remedy of tapeworm, and one that has been the most fully tested, though not

discovered by Americans, seems to be altogether ignored by American compilers of Dispensatories and Formularies.

The pumpkin seed remedy is often mentioned as being a new one, and as having been first introduced into practice in America. It appears, however, that this article was first introduced to the notice of the medical world by Dr. Mongeney, of Bordeaux, about three years ago, at a meeting of the Medical Society of that city; he then and there declared that for thirty years he had used with great success for the expulsion of tænia, a paste of pumpkin seed (la pâte de citrouille;) 90 grammes of fresh seed mixed with twice as much honey—a dose which in seven hours without producing any unpleasant effect, dislodged the worm. Dr. M.'s confrères of that city, among whom were MM. Brunet, Sarramea, and others, had in a great many instances used this article with the most complete success—some of whom gave 45 grammes of the skinned seed (semences dépouillées) united with the same amount of sugar. One of the physicians, who had suffered extremely for two years, from an almost constant pain in the lumbar region, excessive debility, indigestion—symptoms which he regarded as due to a disease of his nervous system. He finally, having voided some flattened fragments which he thought might be portions of tænia, took by the advice of M. Sarramea, 30 grammes of pumpkin seed pounded with 10 grammes of sugar; he suffered all night from a violent fever and great agitation; by morning, twelve hours after having taken this dose, an injection simply of water brought away seven mètres, or about twenty-three feet of tænia.

As suggested by M. Rollet, measures were taken to convey to the Council of the Administration, a recommendation that this valuable remedy should be admitted into the pharmacopœia (*codex.*) (See Jour. des Connais. Méd. Chir., June 1, 1852.)

Cure for the Tapeworm.

Procure sufficient seed of the pumpkin (those grown in the West Indies are the best) to make two ounces after removing the outside shell of the seed; put them into a mortar and add half a pint of water; pound them well up, and make a liquid orgeat of them, which strain through a cloth. Drink this mixture in the morning on a fasting stomach. If it does not operate in the course of an hour and a half, take one ounce of castor oil. Drink all the time as much fresh cool water as the stomach can bear or contain; that is, drench yourself with water. After taking the orgeat, if the stomach is well rubbed

with ether, and an injection of about sixty drops of it is taken, you will find it an assistant to the orgeat, but this may not be necessary. Should the first application of the remedy not answer, repeat it the next morning, and there is no doubt your complaint will be removed. The worm will leave the patient all at once, and probably entire. This can be ascertained by finding the small end or head of it, which tapers almost to a point.

The New York friend, from whom I received the recipe, of which the preceding is a copy, in March, 1848, remarks, in support of his opinion of the efficacy of this remedy, that Capt. —— says he did not have to take the injection, although he took two separate doses of the seed, (the first not operating sufficiently,) which relieved him at once, and since which time has cured probably a dozen different persons afflicted with the tapeworm, who had been given over by the physicians. The worm from him was about thirty-four feet long, each link about one inch. He rubbed the stomach with ether after taking the orgeat. It may be advisable to use the forenamed remedy under the advice and with the assistance of a physician. I have only to add that the suffering lady in this city, for whose relief the writer's aid and influence was solicited by her husband, has been restored to perfect health, after years of prostration and efforts for relief; and in thankfulness for the interest I had manifested in the case, sent me a glass jar containing a large part, if not the whole of the worm that had been her tormentor for several years.—*Boston Med. and Surg. Journal.*

F. W. Cragin, M. D., says: "On reading an article on pumpkin seeds, in a late number of the Boston Medical and Surgical Journal, I recommended it to an intimate friend, who had, two months before, discharged about four yards of that detested parasite, a tapeworm, and who was sure there was "more of the same sort left." He, in three days afterwards, showed me the bottle, since left at your office, containing what was formerly discharged, together with the tapering part of that which was removed (in all about four yards) by the remedy.

His statements, which may be implicitly relied on, are, that for want of West Indian or other pumpkin seeds, he took undried acorn or marrow squash seeds, and proceeded, *secundum artem*, following the orgeat, in about one hour and a half, with about six drachms of castor oil, taken in two spoonsful of Holland gin. He drank very little water twice; drank and ate nothing else till noon, when the only effect of his faith and practice was manifested "in one liquid discharge containing the squirming worm; at one end about one-third of an inch broad, and tapering down to nothing."

If this remedy should continue to prove as efficacious as in this and former instances, it is to be hoped a specific has been found for one more of "the ills that flesh is heir to;" which remedy should never be lost sight of."—*Ib.*

On the Treatment of Tapeworm by the Oil of Pumpkin Seeds: By the late Prof. Henry S. Patterson, M. D.—In the Medical Examiner,

for October, 1852, I reported a case of radical cure of tænia by the use of an emulsion of pumpkin seeds, after the Ol. Terebinth. and even Kousso, had signally failed. Several other cases have been reported before and since mine, all going to establish the efficacy of this new remedy. Should it prove as generally successful in expelling the worm as the cases indicate, it will become a valuable accession to our means of treatment in a troublesome and often obstinate affection.

The seeds of the common pumpkin *(cucurbita pepo,)* consist of a leathery white envelop, inclosing an oily albumen of a slightly greenish tinge. They are inodorous, and have a sweetish, mucilaginous taste. Rubbed up with warm water or milk, and sweetened, they form a very pleasant emulsion; and this is the way in which they have generally been administered. They abound in fixed oil, which is readily yielded on expression, and appears to be the only constituent of any importance. Conceiving this oil to be the anthelmintic principle, I determined to use it in the first case of tænia I should encounter. A quantity was obtained, by cold expression, by our accomplished pharmaceutist, Mr. Frederick L. John, of Race street. From four lbs. of the seeds he procured ℥ xiv of oil; but, he has no doubt that, if the operation were conducted on a larger scale and more carefully, the yield would be from thirty to forty per cent. The oil is clear, transparent, of a light brownish green color, with a slight oily odor, and a perfectly bland taste, like that of the oil of sweet almonds. It has now been kept some ten months, in well stoppered bottles, and is perfectly sweet and bland.

No case of tænia has occurred in my own practice; but in May last I learned from Mr. John C. Lyons, an intelligent member of the medical class of Pennsylvania College, that a poor woman in his neighborhood (Kensington) labored under the disease, and had asked his advice. I requested him to use the pumpkin seed oil, which he did, with the happiest results. Causing her to fast rigidly for twenty-four hours, he gave her ℥ ss of the oil in the morning, and in about two hours ℥ ss more. This produced a slight disposition to looseness of the bowels. In two hours after the last dose, ℥ i of castor oil was given, and purged freely, bringing away a considerable portion of the worm. From that period until the present, (September) she has remained entirely free from any symptom of verminous irritation, and there can be no doubt that the worm was altogether destroyed.

Tænia is of so rare occurrence with us, that no individual practitioner sees enough of it to enable him alone fairly to test any medicine. I therefore beg leave to call the attention of my medical brethren to a remedy readily obtained, cheap and pleasant, and which I believe will be found quite efficient.

The same gentleman reported the following case:

Failure of Kousso—Successful Use of Pumpkin Seeds.—The subject of this case was for some time under my care, in consultation with my colleague, Dr. Darrach. I can aver that he was most tho-

roughly put through the entire routine of tapeworm remedies before he left Philadelphia. He tells his own story so well that I prefer to give the following extract from a letter announcing his restoration to health: "In the early part of January, 1836, I was rather suddenly attacked with what seemed to be an alarming diarrhœa, which continued for some weeks, resisting the usual remedies. My symptoms had been peculiar for some time previous to the attack. Indeed, I had all the prominent symptoms of tænia as laid down in the books, viz: dizziness; occasional false vision; variable appetite; pain in the lumbar region; pain in the knee joint; swelling of the abdomen; hesitancy of speech; restlessness in time of sleep; unusual drowsiness during the day; variable strength, being sometimes quite strong, and then again quite feeble. Somewhere about the middle of February of the same year, I discharged at a morning stool, about nine yards of the tænia. From that time onward, for six years, I was more or less under medical treatment continuously. I took large quantities of the spts. turpentine, (once or twice two ounces at the dose;) also, the male-fern, calomel, and jalap, and Jayne's vermifuge; and was several times under homœopathic treatment. I took also iodide of potassium, iodide of iron, decoction of pomegranate, and the 'kousso.' I discharged large quantities of the worm, but no head could be perceived. When the kousso failed, I began to dispair of being cured at all, but my sister, Mrs. ——, sent me in December last two numbers of the Boston Medical and Surgical Journal, containing two several accounts of the cure of tænia by the use of pumpkin seeds. Having previously abstained from usual food for a day, on the 10th of January last, I took, at 8 o'clock in the morning, two ounces of the kernels of pumpkin seeds pulverized with two tablespoonfuls of white sugar, and commingled with a half pint of boiling water, making a very pleasant drink for a fasting man. I kept my bed, drinking frequently of cold water, and at 9½ o'clock, I took an ounce of castor oil. At 10½, I drank a cup of hot black tea, and, in about two minutes discharged about eight yards of the tapeworm, *with the head.* O, how I wept for joy that I was again a free man, after a servitude of six sad years to this awful complaint. Since then I feel like a new being in a new world. My life had often been a weary burden, and yet I grew fleshy and looked healthful. For months in succession I had discharged the worm daily in pieces of six to eighteen inches, and also in gourd seed form. I suppose that without any over-estimate, I discharged, during the six years of my affliction, about *four hundred yards!* The remedy is very simple. Were I a practising physician, I would never administer turpentine for tapeworm; I sometimes fear that I have experienced irretrievable harm to my kidneys by using it. There is virtue in pumpkin seeds, doctor, even if it be a *Yankee notion.*"

In the Northwestern Medical Journal, for May, 1853, Dr. J. McCreary Sudduth, gives the following case:

B. H., male, aged 28 years—occupation farmer, volunteered in 1847 to go to Mexico. In good health when he left home (Ky). Sickened while on the gulf, Nov. '47. Landed and went as far as the City of Mexico; though unfit for duty, and on the sick list during his entire stay in Mexico. Discharged and returned to Kentucky early in the summer of 1848, quite weak and much emaciated. In August of 1848 began to pass by stool small portions of what proved afterwards to be joints of tapeworm. Shortly after his return to Kentucky, he applied to a number of physicians for advice; receiving no benefit from any article recommended, except those used by one doctor whose prescription brought away eight feet of tape worm. Patient thinks the articles used by him were cal. and Dr. Jayne's vermifuge. Shortly after passing this piece of worm, finding himself not relieved, and knowing now what it was that troubled him, he went to Louisville and consulted a number of physicians of that place, offering one hundred dollars to any one who would rid him of his troublesome companion. He however received no benefit from any articles used by physicians of that place. Soon after leaving Louisville, he came to Illinois, still poor and in bad health. He consulted with all the doctors that came in his way, using all articles by them recommended, as well as all the patent medicines that he saw extolled for the removal of tapeworm, (and their name is legion) in newspapers, almanacs and receipt books, &c.; however all failed. When he consulted Dr. Smick and myself, he presented the appearance and symptoms as follows: Was lean in flesh, and ænemic, judging from the appearance of *the surface* and color of his lips. Abdomen somewhat distended, troubled much and especially at night by a moving, creeping sensation in the abdomen. Variable appetite, at times voracious, at others none at all, passing daily by stool a number of joints of the worm. We advised him to eat the paste of pumpkin seed and honey, ʒ iij, one ounce at a time, with an interval of two hours. Six hours after taking the last portion he passed twenty-two feet of the worm, though in three pieces, the longest of which was eighteen feet, and bearing the head of the parasite; showing the superiority of paste of pumkin seed and honey over all known articles for the removal of this troublesome, if not destructive parasite. All symptoms of worms have disappeared.

In the American Journal of the Medical Sciences for July, 1854, D. Leasure, M. D., of Newcastle, Pa., says: "Mary——, aged twenty-eight, unmarried, has been delicate all her life, and for fifteen years subject to severe cramping pains of the abdomen, accompanied sometimes, by obstinate vomiting. About ten years since, she noticed that she passed portions of tapeworm, of lengths varying from a single joint up to many feet, and if the statements of the patient and her mother are to be relied on, sometimes half filling an ordinary chamber mug. Her mother had also, at an early period of her life, been a victim to a tapeworm, which had been expelled by a secret *vegetable remedy*, probably *male fern*, given to her by a worm doctor.

My attention was called to Mary's case some time in last February, while in attendance on her sister, for another disease; but from causes not necessary to mention, I did not prescribe till last week. I had intended to use the male fern or kousso, or both; but not having access to either of them in a fresh state, I determined to wait till they could be procured from Philadelphia. While thus waiting, I noticed in one of the journals a report of a case of tænia expelled by the use of emulsion of pumpkin seeds. Curiosity, more than the expectation of success, prompted me to gvie it a trial. I directed a pint of the bruised seeds to be infused in three pints of soft boiling water, and left over night, the whole to be taken during the next day, the patient fasting in the meantime.

On the morning of the 9th of May, the patient commenced its use, and in the afternoon experienced the most violent cramps and pains in the bowels for several hours; and on the morning of the 10th, she passed eleven feet of the parasite, including the head, as proved by observation under the microscope. The animal was entirely dead when voided from the bowel, and is a most beautiful specimen of a perfect tænia.

Kousso.

The three following cases, treated by Dr. Budd, who determined to give kousso a trial, having often found ineffectual the remedies in common use, as turpentine, pomegranate bark, &c., which create disgust in the patient, as well as, in some cases, severe constitutional symptoms. This plant, as long ago as the beginning of the year 1847, had been brought under the notice of the Academy of Medicine, at Paris, who appointed a committee to inquire as to its properties. It was in consequence tried, under their direction, in several hospitals, and the result of their experience was such, that they were induced to return a very favorable report. The Academy of Sciences were equally satisfied of its efficacy.

The parts of the plants used are the flowers, which, being reduced to a fine powder, are macerated in lukewarm water for fifteen minutes. The powder and solution are taken either in one, two, or three doses, quickly following each other. It is recommended that lemon-juice should be taken freely before and after the kousso. The patient must be prepared by low diet for a day previously, and the medicine taken on an empty stomach, before breakfast. The clear infusion has the color, and a somewhat similar taste, to a very weak senna tea. It rarely causes any annoyance or uneasiness, except slight nausea, and this but seldom.

The first woman to whom the kousso was given had generally enjoyed good health. From her account it is probable that she had not been troubled with tapeworm previous to her coming to London, a year and a half ago. When the first symptoms of it came on, twelve months since, she took oil of turpentine and castor oil, under the use of which a large portion of tænia passed. She had, at first, a ravenous appetite, which passed away, leaving a constant feeling of flatulency in the stomach. Languor, general debility, incapacity for work, and nausea, were her chief symptoms. During the four months preceding her admission, she was

constantly taking various remedies, such as turpentine, &c., under the direction of her medical attendant; but with none of them has the worm passed. When admitted into the hospital she was ordered half a drachm of jalap and low diet, and subsequently other purges, but without bringing away any joints of the worm for four days, when the kousso was administered on an empty stomach, which in the course of the day brought away a large worm. Its head could not be detected, but the narrow portions which seemed to have been joined to it came away. During the same and following day, there was considerable diuresis, but afterwards the urine became scanty. The motions were loose and dark. Her general state improved, and she left the hospital apparently cured.

The next patient was also a woman, aged about 44, who had apparently got the worm at Fort Beaufort, in the Cape of Good Hope, at which place she resided for some time. She began to pass joints in the year following that on which she went to that place. Worms are very common among the natives, who are in the habit of taking infusion of pomegranate bark, turpentine, and also, a scraped root called "Cacay." Of all the remedies which she has used, the pomegranate was the most effectual, which has not, however, cured her, as she continues to pass joints. Her symptoms are gnawing pain, and constant feeling of sinking in the epigastrium, pain in limbs, general lassitude, dimness of sight, loss of appetite, short, dry cough, and a sensation as of the movement of the tænia. She took kousso as the other patient; it was followed by slight nausea for a quarter of an hour. Its taste, she says, is very much like pomegranate. A tapeworm of very large size was passed four hours and a half afterwards, and subsequently, some isolated joints. The head could not be found; but there is no doubt that it came away, on account of the narrowness of some of the pieces. During the same and two following days, numerous joints, apparently long dead, and partially decayed, were passed.

The other was a delicate, anæmic looking woman, who had had the usual symptoms of tapeworm for some time, but did not to her knowledge pass any until three weeks, and again one week ago. Is a native of Norfolk, but has latterly resided in Soho, which neighborhood is supplied with water by the New River Company. She had the kousso exhibited in the same way, followed by a dose of carbonate and sulphate of magnesia. A portion of worm passed with every motion. Both these patients have left the hospital, improved in health, and apparently free from any symptoms of tænia.[—Ranking's Abstract.

To the Editor of the Lancet.

Sir:—I have much pleasure in forwarding for the Lancet the particulars of a successful trial of the kousso. Mr. B., residing in Cheapside, a delicate young looking man, had been troubled with tænia for some years, and had taken the usual remedy, turpentine, with partial success, having at times seen parts of the worm only. I obtained a bottle of kousso from my druggist, which my patient took on Sunday morning, the 15th; after waiting two hours, with the aid of seidlitz powder, the monster was expelled, *téte et col* complete, measuring twenty-one feet.

I need not add that my patient was highly delighted at the good effects of the kousso, and has presented me with the largest specimen of a tape worm I have ever seen.

I am, Sir, your obedient servant, THOMAS SMITH.

In former numbers of the Lancet, (March 16, 1850, and April 20, 1850,) cases were noticed in which the kousso was found very efficacious for procuring the expulsion of the tænia solium. This plant is now acknowledged to be so useful in tapeworm, that it seems almost unnecessary to adduce new cases; we shall, however, just sketch a few of those which were lately benefited by the kousso, as they present various features of interest.

The first case, as taken from Mr. Jordan's notes, runs as follows:— Rebecca R., aged 22, is a native of Wapping; she went to Devonport when seven years of age, but only stayed there about a fortnight; with this exception she has constantly lived in town, generally at Wapping, but about eighteen months ago she spent a year at Peckham. Patient's sister, who has been dead nine years, also suffered from tapeworm, which remained upon her to the time of her death. Patient likewise knows of a neighbor of hers in Wapping, close to her own home, who suffers from the tænia. This latter person and the above-mentioned sister are the only people she knows to be thus affected. The water is supplied by the New River Company to the whole neighborhood.

Patient was quite healthy until about two years ago, since which time she has had great pain in the side and stomach; her appetite was good, but she used to feel sick on first getting up; she had, however, no idea that she harbored a tapeworm until a week before Christmas, when she first passed joints of it, and from that period, such joints have been passed almost every day.

Twice since she first noticed the joints she has passed long pieces of the worm, once after opening medicine, the other time without any such agency. She has never taken any turpentine nor any other remedy expressly for the worm.

Patient was admitted under the care of Dr. Budd, and took the kousso at half-past nine in the morning, the day after her admission; and, after taking a dose of castor oil in the middle of the day, the worm was passed with a motion at a quarter to five in the afternoon. This entozoon was nearly three yards in length, and the narrow segments approaching to the head were attached to it, though not the head itself. The medicine gave patient a slight feeling of sickness, which soon went off again. Her appetite was bad on the day she took the kousso, and she felt weak. With the exception of the tapeworm patient seems to have generally had good health; she has only a slight cough. Her mother and sister died of phthisis, but patient's appearance is remarkably florid and healthy. The day after admission, this woman left the hospital in good condition, without passing any more of the worm.

The second case was admitted under the care of Dr. Todd. The subject is a young woman, native of Scotland, *four months advanced in*

pregnancy. She complained to Mr. Steele, the house-physician, that she was in the habit of passing long, *round* worms, but when she brought the joint which she had lately evacuated, they were found to be pieces of the tænia solium. When the nature of the worm was ascertained, the patient was admitted into the house and took the kousso in the morning; at seven in the evening, she went home, and a quarter of an hour after she had reached her residence, she passed five yards of the worm.

The third case was sent to Dr. Todd from the country. The patient is a middle aged woman, residing at Bow, who took the kousso at three o'clock in the afternoon, and left the house immediately afterwards, promising to bring the worm as soon as she should evacuate it. The next morning she brought a tapeworm measuring about six yards in length.

The fourth case, who was admitted under the care of Dr. Budd, is that of a man, about forty-six. His health has, in general, been pretty good; last winter, however, he was attacked by cholera, and treated in King's College Hospital. Whilst laboring under this disease patient did not pass any joints of the tapeworm, though, previous to his being visited by the epidemic, he had now and then evacuated portions of the tænia. When convalescent, he took some oil of turpentine, and by the agency of this medicine he voided a few joints. From that period he continued passing joints, and was admitted under the care of Dr Budd, May 3, 1850. Patient took the kousso in the morning, and had two doses of house medicine in the course of the day. At six o'clock in the evening, he passed a tapeworm of a very great length, since it measured nearly ten yards. The next day he voided a piece six inches long, which came evidently from very near the head. It is to be regretted, as we stated before, that this medicine is so expensive; still, when it is considered how rapidly and effectually it promotes the evacuation of the tænia, the 17s. 6d. can hardly be looked upon as a high price; the more so, as in hospital practice, the patients need stay in the house but a short time. It will be extremely interesting to keep an eye upon these patients, in order to ascertain whether the benefit is of a lasting or temporary kind.

The late Dr. Frydenger, of New Orleans, in this journal, about two years ago, gave a very interesting case illustrative of the efficacy of kousso in destroying tapeworm, from which it appears that a young gentleman was treated in both Europe and America by the entire routine of vermifuges, and particularly, by the terebinthinate preparations, with so much energy that his general health had become impaired, and his life endangered. It may not be improper to recall the attention of the reader to the following paragraphs of Dr. F.'s paper, and the more so, because kousso has been treated with neglect in the South:

"I have treated him for the last three years, at intervals, with varied

results, but invariably gave him some relief for a time; at times he would discharge from a few separate joints to several hundred, at others, portions of the worm measuring in length from three or four inches to seventy or eighty feet; and I may here state, that since I commenced treating him, he has discharged over a *thousand feet of worm.*

In February last it annoyed him very much, and I determined to try kousso on him; accordingly, half an ounce was administered in water at bedtime, followed in the morning by a seidlitz powder. In a few hours his bowels were moved, but no signs of worm or the kousso could be discovered in the evacuations. During the following night the kousso came away, bringing with it a large quantity of the worm, broken and torn and mangled to such a degree that it was some time before it could be recognized in the mass of matter. Such portions as selected from the mass, of sufficient size and form, worthy of preservation, can be seen at my office, with specimens of the entire worm from the same person."

The late Professor H. S. Patteson gave Miss W., aged twenty-two, long a sufferer from tapeworm, "six drachms of kousso at once, the patient having fasted from the previous day. It excited some nausea but no vomiting. It was followed in a few hours by a dose of castor oil, which brought away a tapeworm several yards in length, but which unfortunately, was not preserved for more minute examination. There can be no doubt, however, that the entire worm was expelled, as the patient rapidly convalesced, and has been in the enjoyment of uninterrupted health since that period."—[Med. Exam.]

Prickley Ash Bark.

The late Prof. Henry S. Patterson relates the following case:

Successful use of Xanthoxylon fraxineum.—For the following curious case I am indebted to my friend Dr. Thomas J. Turner, of Port Richmond. J. R. æt. 41 years, is a workman in a chemical laboratory. In Dec., 1847, whilst a private in the British army in Ireland, he first perceived that he was afflicted with tapeworm. He states that he passed fifteen to twenty joints at almost every stool for a time, and on several occasions as much as thirty feet at once. The surgeon of his regiment treated him with Ol. Terebinth. ℥j every other day. He also took tin-powder, male fern root, and "every other article he ever heard of." He finally abandoned the hope of a radical cure. The symptom most prominent was a sense of gnawing and beating at the epigastrium in the morning. He was obliged to eat before rising, as he otherwise became faint and "had all sorts of queer feelings." His appetite was insatiable. While at Port Richmond in the autumn of last year, he was attacked with a tertian intermittent, for which he was recommended to take an infusion of prickly ash bark in brandy—a popular domestic remedy. He digested an ounce of the bark in a pint of brandy, and drank the whole during the apyrexia. The result was a most copious diaphoresis, as usual,

and also some purgation, bringing away the entire worm. He has remained perfectly well since."—*Md. Exm.*

Tannic Acid.

At a meeting of the fellows and licentiates of the College of Physicians in Ireland, Prof. Osborne mentioned that he had been lately led to use tannic acid for the destruction of tapeworm by several considerations, and especially from its action on gelatine, as well as on albumen. Having ascertained in the usual way the presence both of gelatine and albumen in these parasites, it is to be inferred from analogy that the former is in their integuments, and if so, that in tannic acid we have the desideratum of an agent acting chemically on the worm, but not irritating the stomach or bowels of the patient, and, moreover, capable of being retained long enough to produce its full effect. He stated that in the case of two patients whom he had under this treatment, the appearance of the ejecta indicated that the worms had suffered from a chemical irritant, being in some instances curled and contracted, and in others friable and broken down, when expelled by a purgative.—*Dublin Quarterly Journal, Nov.* 1853, *p.* 457.

Quicksilver and Zinc as a Vermifuge.

The following case, reported by the late Professor Caldwell, of Kentucky, authenticated by his own direct testimony and by collateral evidence, is reproduced, not for its therapeutic value, but for its physiological, pathological, and chemical suggestions. The Professor says that "in the treatment of tænia, an amalgam of zinc and quicksilver is a valuable remedy, but that in the following case the dose was too large:"

"In the year 1818, J. P., of Louisville, was attacked severely by the tapeworm. After the employment of sundry remedies, by which portions of the worm were, from time to time expelled, he was advised to the use of an amalgam, composed of equal parts of zinc and quicksilver. Of this he was directed to swallow, every hour, a dose of an *ounce weight*, until he had taken *twenty-four doses*. He commenced, intrepidly, the formidable process, and proceeded in it regularly, until he had swallowed sixteen ounces of the amalgam, in sixteen hours. By this time, an uncomfortable and increasing weight (he called it very correctly, "a dragging down") in his bowels, admonished him to stop. He did so; and, on the second day afterwards, took an active purgative, by the aid of which he discharged a much larger portion of the worm than he had ever done before. But, of the amalgam, he discharged very little. By means of repeated cathartics, taken subsequently, at periods of time not very remote from each other, he conceived, at length, that the worm was entirely removed; and that he was now well, except that almost the whole of the amalgam was still in his bowels. Feeling no great inconvenience from this, he made a voyage to New Orleans. Perceiving, during his absence, that he still

retained some remains of the worm, he swallowed, on his return to Louisville, in four successive hours, four ounces more of the same amalgam, having now taken, in all, *twenty ounces.*

From the worm affection he was soon afterwards relieved; but not from that produced by the remedy. A large portion of the metallic mass still remained in his bowels, and began to excite in them considerable uneasiness.

In the summer of 1820, I saw him for the first time. His health was much shattered, his person emaciated, his countenance sallow, and his whole appearance strikingly hypochondriacal. The amalgam had been in his bowels nearly two years. When he related to me his case, I considered the story a morbid fiction, and belived his intellect somewhat deranged. But on further examination I discovered my mistake. Near to the umbilicus was a hard, round, floating lump, of considerable size, which, by the requisite management, I could grasp in my hand, and which appeared to weigh about a pound. The patient himself was confident it weighed more, for he asserted, that, of the twenty ounces of amalgam swallowed, he had never discharged more than two.

The account he gave me of the effects of this movable ball on him was, that if in walking he made a false step, or in any other way jarred himself, it gave him great uneasiness; and that it entirely disqualified him for riding on horseback. When he attempted to exercise in that way, it created, by its irritation, a constant and painful *erectio penis.* He could not, without inconvenience, bear even the motion of the easiest carriage. He was exceedingly anxious for the removal of the foreign body, and urged its extraction by a surgical operation, if he could not be relieved from it in any other way. From this resolution he was dissuaded for the present, and advised to bear with his misfortune as long as it should be tolerable, reserving its removal by a surgical operation for the last extremity. In the mean time, a suitable diet and regimen were prescribed, with the use of occasional laxatives or purgatives, as circumstances might require, with a view to guard as much as possible against the bad effects of intestinal irritation.

In the autumn of 1823, the patient died of the malignant fever, which prevailed that season in Louisville. The metallic mass had now been in his bowels nearly five years. On opening his abdomen, the quantity found amounted to about eight or ten ounces. My belief had been that it was in one of the cells of the colon, where nature had formed a cyst around it to retain it, and keep it from moving along the intestine, analogous to the capsule which she throws around a leaden bullet, when buried in muscular or cellular substance. But I was mistaken."

The facts of this case, so far as they had transpired anterior to 1821, were published in the Philadelphia Medical and Physical Journal several years before the patient died.

The original prescription is thus:

℞.—Hydrargyri, } ā ā
Stanni, } ℥ xii.
F.—Amalgama.

Action of Vermifuges.

Dr. Budd, of Kingsbury College Hospital, 1850, says: "Tapeworms are very tenacious of life. They are seldom voided entire without the aid of medicines that act especially upon them. Single joints often come away, and pieces two or three feet long are often then voided, but it very seldom happens, unless after medicine, that a portion comprising the head is thus passad. This portion remains and grows again, so that a person is often troubled with the parasite for years. One of our patients, Sarah Wheeler, had been so plagued for sixteen years, during which, she told us that she had seldom gone a week without passing joints of the worm. She had taken turpentine, and the bark of the pomegranate root, which brought away large pieces of the worm, but the head and the portion near it remained, and the worm grew again. The worm was expelled after the kousso in different portions, and she has since had none of the symptoms which she attributed to it.

Another case that shows more strikingly still the tenacity of life of the tapeworm, is that of Samuel Payne. He first passed joints of the worm seven years ago. In September last, he was brought into the hospital with cholera, and on the day of his admission passed a portion of the worm two yards in length. He remained in the hospital three weeks on account of the cholera. Some time after this he again passed joints of the worm, and to get rid of it came to the hospital as an out patient. Turpentine and castor oil were given him, and brought away a long piece of the worm. The ailments which he attributed to the worm were much relieved for a time. They then became again more severe; joints of the worm were again passed, and in January last he came for the second time to the hospital to be rid of the worm. Turpentine and castor oil were given him as before. A long piece of the worm was again expelled, but the creature was not destroyed; so that here the worm had existed seven years, had kept its place during the terrible commotion and flooding of the intestines in cholera, and had escaped destruction by two doses of turpentine. Payne took the kousso on the third of May, and the next morning voided the worm, which was ten yards long.

In all the cases in which the kousso had been given in the hospital, the head of the worm, or the taper portion near the head, has been found, so that there is reason to suppose that the creature has been entirely destroyed.

Dr. Budd, as shown in these extracts, seems to regard the discharge of portions of the tapeworm as proving the tenacity of life in this animal, as if the dropping away of the fully developed articulations might

be expected to cause the death of the entire animal, whereas, it appears probable that the detatchment and discharge of these articulations are consistent with the normal condition of this parasite, and may even be proof of its high state of vital development.

Dr. Küchenmeister, of Zittau, has examined the various vermifuges by immersing the living entestinal worms of fowls, cats and dogs, in albumen at a temperature exceeding 77° Fahr., and adding the anthelmintics in the form of infusion or of powder. In some cases, a mixture of warm milk and water was substituted for the albumen. The experiments were not continued for more than from forty to forty-eight hours, if the worm had not been killed before the expiration of that time. Dr. Küchenmeister made use of electricity as the most delicate re-agent for proving the occurrence of the death of the worms. In the first place, electricity cannot be considered as a vermifuge. The author subjected a female *Heterakis vesicularis*, taken from a partridge that had been killed, to the action of a rotatory apparatus, which was kept up with longer or shorter intervals during an entire day. The animal was not destroyed by the experiment. He next tried the remedies employed for the removal of tæniæ, and first tested kousso in the following manner:—A living *Tænia crassicolis*, procured from a cat, was placed at four o'clock in a mixture of albumen and dolichos pruriens. The worm appeared to be perfectly well in this mixture, and at two o'clock on the following afternoon exhibited the most vigorous movements. The tænia was now transferred to a vessel containing a mixture of infusion of kousso and some of the infused as well as some of the fresh powder with albumen. The temperature of the mixture was 30° R. (99.5° F.) On its introduction, the worm quickly extended itself; after some time it was found to be dead, its color having changed to a dirty reddish yellow. Two *Tæniæ serratæ* were placed at about half-past one in the afternoon in a mixture of albumen and kousso; at two o'clock they were dying, and at three completely dead. Two *Tæniæ serratæ* from the same dog were brought in contact with kousso and milk at half-past one in the afternoon, and at two o'clock were dead. Two *Tæniæ serratæ* were placed at half-past one in the afternoon in albumen, mixed with decoction of pomegranate root and with some of the powdered root: they died in three hours. Two others were placed in milk mixed with the decoction only: they died in three and a half hours. A *Tænia crassicolis* was put into a mixture of albumen with ethereal extract of male fern: it died gradually in three hours and three-quarters. A number of *Tæniæ cucumerinæ* were placed in a mixture of albumen and oil of turpentine: they were dead in an hour and a quarter.

A number of the same were put into a mixture of albumen and castor oil; they appeared lively at first, but were dead in seven hours. Similar worms were put into a salad, composed of pieces of unwa-

tered herring, boiled potatoes, large pieces of onion and garlic, albumen, vinegar, and a large quantity of oil, They died in eight hours. Lastly, the author tested the vermifuge powers of the brown oxide of copper; fifteen grains were administered in the course of four days to a strong cat. When the body was opened, the entire intestinal canal was found to be full of fluid, yellow, flaky fæces; the intestine was softened, and denuded of epithelium, especially at the termination of the ileum, where the adjoining Peyer's glands were much swollen, particularly in two situations, one of which was an inch and a half long by one-third of an inch broad; the other was nearly circular, and its diameter one-third of an inch. The cat had been purged. The tæniæ and ascarides it contained were lively. It would hence appear that this substance is both inefficacious as a vermifuge and dangerous to the system. The following table contains the results of the above experiments:

In milk boiled with kousso tæniæ died in half an hour.			
In a mixture of oil of turpentine and albumen, in	1	to 1¼	hour.
In decoction of kousso with albumen.....	1½	to 3	hours.
In decoction of pomegranate root with milk....	3	to 3½	"
In decoction of pomegranate root with albumen,	3		"
In ethereal extract of male fern with albumen,...	3½	to 4	"
In castor oil with albumen,.....	8		"
In salmagundi with garlic and onions,.....	8		"

Kousso would therefore seem to be the most efficacious remedy against tæniæ. When pomegranate bark and male fern root fail, their failure may be owing to the habit of administering a laxative in from four to six hours after the exhibition of the vermifuge, by which the latter may be carried beyond the worm. With regard to pomegranate root, it must be observed, that in large doses it occasions diarrhœa. The same remark applies to castor oil. The author also alludes to cold water, strawberries, dolichos pruriens, and filings of tin. When tæniæ are placed in water containing ice, they are instantly benumbed, and if allowed to remain in it, they will always be found at the end of ten hours to be quite dead. Strawberries may be useful as a mild remedy in cases of tapeworm; if large quantities of them be taken on an empty stomach, entire portions of the worm will often be passed. Dolichos pruriens, with which the author tried many experiments, appears to possess no power of destroying worms. The author has also minutely studied the medicines recommended for the removal of round worms. In albumen, these worms behave as the tæniæ; in water, at about 77° F., they live for some days, but swell, stiffen, become longer, thicker, and more sluggish; they lose their power of suction, and their motions become slow and only partial —they resemble leeches which have gorged themselves. In general, however, the males and young neutrals resist the effects of water longer than the mature, impregnated, egg-bearing females, which become quite rigid and inflexible, and swell considerably. Milk and whey affect the

worms like water. The following are the medicines, the effects of which were tested:

1. Camphor. An ascaris lived from eighteen to twenty hours in albumen into which some camphor had been introduced. 2. A mixture of oil of turpentine and albumen killed some ascarides which were placed in it from two and a half to six hours. 3. Ascarides lived forty hours in albumen and wormseed, whether the latter was employed in the form of powder or infusion. 4. Some ascarides were placed in albumen mixed with santonine; they did not die in it, nor did they die in a watery infusion of santonine. When santonine was dissolved in oil, especially in castor oil, and mixed with albumen and ascarides, the latter died in ten minutes. An injection of santonine and castor oil was thrown up the rectum of a cat, and produced numerous motions containing dead worms; and on killing the cat, the entire of the lower portion of the intestinal canal was ascertained to be free from worms, while four were found near the stomach quite rigid and extended, and retaining but little life. A *Tœnia crassicolis*, however, was found in the intestines, and appeared to be quite uninjured and very lively. 5. A mixture of albumen and aniseed, with a strong infusion of the latter, killed the worms in about twenty-four hours. 6. Parsley, mixed with albumen, killed ascarides very slowly. 7. Flour of mustard and albumen destroyed them in about four hours. 8. In rue the worms lived upwards of twenty-four hours. 9. The same was the case with millefoil. In contact with tansy, valerian and camomile, great numbers of them lived for twenty-four hours. With onions and garlic they perished in from ten to fifteen hours. A decoction of cloves, with or without albumen, killed them in twelve hours. In an infusion of ginger, with or without albumen, they lived about twenty-four hours. Petroleum, mixed with albumen, killed them in less than six hours, as did also oil of cajeput and albumen.

A series of vermifuges, taken from the class of balsamics, was tried in like manner, namely, assafœtida, ammoniacum, balsam of Peru, extract of juniper, and Venice turpentine. In all these the worms lived more than twenty-four hours. Of the class of empyreumatics (brenzlichen stoffe) the following were tried:—Oleum chaberti, [a mixture of four parts of oil of turpentine, and one of the animal oil of Dippel,] oil of amber, castor oil, tar water, creasote, wood-vinegar, and wood-soot. In these, for the most part, the worms lived from twenty-four to forty-eight hours; except the wood-vinegar, in which they lived rather more than twelve; and creasote, in which they died within two hours. Of bitters, the author tried aloes, gamboge, ox-gall, wormwood, myrrh, gentian, quassia, hops, bitter orange, and acorus calamus; in all these the ascarides lived from twenty-four to forty hours. Of astringents, pure tannic acid, pomegranate root, kousso, extract of walnuts, cinchona bark and quina, elm bark, willow bark, the flowers and stalks of meadow sweet, oak bark, dragon's blood, catechu and kino. In these the worms died in from twenty-four to thirty hours, with but two exceptions, namely, tincture of galls and pomegranate root, both of which killed them in the

space of eleven hours. Of saline preparations, sulphate of soda, chloride of sodium, and the roe of the herring, were tried. In the first the worms died in from fifteen to eighteen hours; in the second, in from two to six; and in the roe of the herring, in four hours. The following metallic poisons were experimented on:—Arsenic, calomel, corrosive sublimate, and the salts of tin, of lead, and of copper. Corrosive sublimate alone destroyed the worms in so short a time as two hours; all the other metallic salts required a much longer period. From these experiments it would appear that santonine, mixed with oil, is the most powerful vermifuge, then chloride of sodium, the roe of the herring, garlic, onions, &c. The author advises that santonine should be given as a vermifuge; mixed with oil, in the proportion of from two to five grains to an ounce of castor oil. This solution should be given in the doses of a tea spoonful until the effect is produced. As auxiliary treatment, chloride of sodium, herring-brine, mustard, onions and garlic, may be employed.—*Virginia Medical and Surgical Journal.*

Part Third.

MEDICAL INTELLIGENCE.

Art. I.—*Sketches of the Epidemic Yellow Fever of* 1854.

The following sketch of the yellow fever of 1854, founded in part upon published letters, telegraphic dispatches, documentary publications, editorial remarks in the public journals, and the writer's personal experiences and observations, is, of course, imperfect. The whole evidence has not as yet been elicited. The events themselves, yet in progress, have not reached their *dénouement*—the drama has not yet terminated—the curtain has not yet fallen.

Unfortunately, the medical journals which have as yet been received in New Orleans, either ignore the existing epidemic altogether, or barely allude to it, giving no details.

The meagre tableau now offered has been undertaken with reluctance yet with a view to meet the supposed wishes of the readers of this Journal; and although this sketch may fall short of their expectations, it has required no little labor: Take the will for the deed.

It must not be forgotten that delay instead of bringing authentic records of a desolating epidemic but too often obliterates from the memory the unwelcome impressions made during its consternating march—impressions which like writings in the sand, the next wave destroys forever. Agonies, corpses, shrouds, coffins and funeral marches from hour to hour for weeks and months in succession, like a terrible

tragedy tend to depress the mind so that re-action is natural. The past is soon forgotten, or replaced with far other concerns.

Man soon turns away from the repulsive realities of death to the attractive illusions of life, and pursues wealth, power, fame, or pleasure which either elude his grasp or disappoint his expectation, or at most can be enjoyed only for a short period until "death, great proprietor of all who treads out empires and quenches the stars," shall beckon him hence.

Happily humanity was not made to mourn, but wrapping itself up in sublime selfishness pursues its illusions less grieved to hear that the expedition to the Crimea, or all China, has fallen in battle, than that its own little finger must be amputated on the morrow. New Orleans is not addicted to unavailing sorrows; its revulsion from an epidemic catastrophe to commercial activity and cheerfulness is as rapid as it is salutary. "To enjoy is to obey."

To the rigid Æsculapian bent on statistical tables, therapeutic formulas, and post mortem examinations, these remarks may appear as the veriest of platitudes. But let him bear in mind as an extenuating circumstance that the New Orleans Medical Journal has some readers —thanks to the liberality of the public, who do not practice venesections nor administer castor oil, or other drugs (with which this number is replete) but follow other avocations.

The writer's experience (which however, may not have been sufficient to characterise the epidemic completely as a whole) justifies him in believing that, in no former epidemic for eighteen years has yellow fever yielded more readily to timely medication. Had he hitherto been a sceptic in the efficacy of medicine in the treatment of this disease, the present epidemic would have afforded grounds for faith in this behalf. Of faith however, real or affected, there is no lack in the medical world, be the mode of treatment what it may. It is proper, however, to state that, with few exceptions, he received early calls, that is, within from three to twelve hours from the attack. The violence of the disease, that is, the great pain, heat, and so forth, generally subsided on the second day. Hæmorrhages from the tongue, gums, nose, bowels, vagina, and scrotum, usually so portentous, by no means boded a fatal result in 1854; and, although he has had but one recovery from black vomit, some practitioners have probably witnessed several such cases. The comparative frequency of recovery after black vomit in the late epidemics of 1853–'4, is the most salient point of symptomotology in which they differ from those of former years. It is remarkable that black vomit is not only becoming less fatal in, but less characteristic of yellow fever, owing to its late extension to some other maladies. The writer may be permitted to mention a single case which he treated, and which occurred during the present epidemic, namely, that of Mr. Heiner's child, born in New Orleans, aged fifteen months, which was under treatment, at intervals for about two months for cholera infantum. During the last day of the child's disease, a febrile state supervened followed by black vomit and death.

Among the auxiliary causes or conditions favoring the development, aggravating the symptoms, and enhancing the dangers of yellow fever, may be mentioned severe labor, exposure to the sun, damp, filthy, unventilated and crowded lodgings, late hours, a bad diet, and intemperance; many ignorant of the danger or reckless of the consequences of this malady, neglect medical aid until death impends and medication becomes unavailing.

With regard to the various modes of treatment and their comparative successes, nothing will now be said. No compliment that one can bestow upon himself—no self-advertising formula yet invented can equal the statistical. He who cures ninety-nine in the hundred must, of course, be a better doctor than one who cures but forty-nine. Figures cannot lie. One who is his own statistician, and is addicted to mental reservation, *suppressio veri, ex parte* evidence, selfish biases, and to the practice of reporting an incredibly low ratio of mortality, should not forget that quacks have in all ages used this same kind of argument, namely, figures whereby to establish their superior skill and the infallibility of their panaceas. In this kind of arithmetic the most competent and conscientious physician may not mingle, without more or less suspicion, being no match for the quack whose lies are broad, yet deep and full to overflowing.

Weather in New Orleans, in 1854.

The weather is a most convenient, if not an edifying topic in conversation, and tends to non-committalism. In this point of view it is rarely referred to by the grave expounder of the causation of yellow fever. Whether this topic, valuable enough to the agriculturist, should not be placed on the conversational platform so far as the causation of epidemics is concerned, or whether it should remain, as heretofore, the inexhaustible staple for dogmatism, sophistry, pedantry, platitudes and book making, is questionable, seeing its failure to account for yellow fever. For example, of all the supposed causes of this disease, temperature, would seem the most conclusive and important, but in reality it cannot be identified as the cause of a single epidemic; that is, no special maximum, minimum, or mean heat has been ascertained as yet even as an invariable antecedent or forerunner of this malady; nay, a probable prognostication* cannot rest upon such a sandy foundation. In neither the temperate nor tropical climates of the Eastern and Western hemispheres do similar temperatures produce similar maladies.

During the epidemic of 1853, the yellow fever declined in New Orleans before cold weather; in some interior towns it prevailed in the face of heavy frosts with undiminished force, for weeks.

It is not intended to give elaborate tables of the daily mortality, weather, rain, thermometer, barometer, and so forth, which indeed the limits of this Journal will not admit at present; nor is it likely that

*Æsculapians, who have prophesied the advent of epidemics have been oftener wrong than right, which is surprising where the chances seem equal.

such enumerations replete with decimal fractions will afford much light in the pathology and treatment of the yellow fever.

The year 1854, as to wetness and dryness may be thus summed up: But little rain fell until near the close of February; the early and lat ter days of March brought forth several rains; the 14th April inundated the city; May committed no excesses; June and July afforded from five to six timely showers; August and the early part of September, though very arid, were not without light showers. As the equinox drew near heavy rains with winds prevailed for several days:

> "The wind, 'tis true
> Was somewhat high, but that was nothing new,
> No more than usual equinoxes blew "

The equinoctial storms along the littoral of the maritime States of the South, were scarcely ever paralleled in violence, duration, and destructiveness. Before, during, and after this war of the elements, the epidemic raged unchanged; at least it did not recede, but followed its usual course.

Texas, Georgia, and South Carolina, which suffered most from this hurricane, suffered also most from the epidemic. In Savannah and Charleston the storm preceded the equinox two weeks, and in Galveston as many days. In New Orleans it was by no means violent, but it introduced the musquetoes. During the summer these insects had not been annoying.

Writers on yellow fever bent on discovering its cause, as every one should be, lay great stress on stagnant air, though this latter is rare in the present yellow fever zone. This disease like the storm "goeth whither it listeth." The great epidemic of 1837, gradually traveled from the lower to the upper confines of New Orleans in about six weeks, during which, that is, on the 6th and 7th of October, the memorable storm which dashed the waves of the lake to the centre of the city, prevailed, without having exerted any marked influence upon the pace of the epidemic, then declining below Canal street. On the 12th of October frost was reported; but during the whole of that month the fever prevailed in the upper part of the Second Municipality and in all Lafayette. Although the storm stranded vessels in the cypress forest, sunk boats, made Rampart street navigable, demolished Milneburg, and killed or drowned people (one of whom the writer saw covered and crushed beneath the ruins of her house) it proved an utter failure as an extinguisher of the epidemic, thereby bringing into contempt many of the wise men of New Orleans, medical and non-medical who had declared that the epidemic was done for. Returning from this digression to 1854, let it be observed that October set in rainy, but soon cleared up pleasantly, being as yet, only ten days old: compared with other epidemic seasons, 1854 was wetter than 1837, dryer than 1839, and fell short of the timely rains usual in most epidemics in this city. The year 1854 in regard to cold and heat appears from Lillie & Co.'s Meteorological Journal, which has been directly consulted, on this wise: January, coldest 32½°, hottest 79°; Febru-

ary, coldest 37°, hottest 89°; March, coldest 41°, hottest 83°; April, coldest 47°, hottest 83°; May, coldest 63°, hottest 91°; June, coldest 71°, hottest 95¾°; July, coldest 77°, hottest 94°; August, coldest 79°, hottest 95°; September, coldest 75°, hottest 94°; the first ten days of October, coldest 69°, hottest 84°.

Filth in New Orleans.

The question of the filth of New Orleans, in 1854, has given rise to contradictory statements—one affirming that the city was never more filthy—the other that it was never more cleanly. Those who believe that yellow fever is caused by filth would find their theory damaged by admitting the prevalence of a great epidemic in the absence of the cause aforesaid, and as usual, they charged the city authorities—not with being accessories, but principals in the causation of the epidemic. As the Lord liveth they should be indicted by the Grand Jury of the Parish of Orleans, if they have done this great evil to the people.

On the other hand, those who steadfastly believe that yellow fever is a foreigner, always imported from some unknown territory *(terra incognita)* in which it is born and bred, and from which it feloneously emigrates to New Orleans, and that as there is no fundamental law in the constitution by which it can be naturalized and become a native of these States, there can be but little danger to their theory by the filthiness of the city in this behalf. The general opinion, however, by the majority who have no theories or selfish ends to support, probably is, that the city in 1854, has been less filthy than usual, notwithstanding the interregnum in the sessions of the common council, while the epidemic has been severer than in any former year with the exception of the last, and perhaps, that of 1847. The council is as guiltless of the epidemic as it is of the equinoctial storm.

If the Æsculapians of the importation school can be believed "yellow jack" has no country, is a native of no locality, is generated nowhere, is a *filius nullius*, the son of nobody, since the importationists of every place unanimously, and most indignantly disavow his paternity as Lear did that of Cordelia:

> "By the sacred radiance of the sun;
> By all the operations of the orbs,
> Here I disclaim all my paternal care,
> Propinquity and property of blood,
> And as a stranger, hold thee forever."

No *Ignis Fatuus* of the shaking prairies, black crocodilian swamps of Louisiana will claim propinquity with the aforesaid pestilent yellow jack. Until the importationists find out the place of his nativity he might as well retire to the Hartz Mountain and join the witches' chorus—

> "The limits of the sphere of dreams,
> The bounds of true and false are past;
> Lead us on thou wandering gleam—
> Lead us onward far and fast."

Mortality.

His Honor Mayor Lewis, on the 3d of Oct., 1854, in his Message to the Common Council of New Orleans, says:

"At the date of your last meeting the health of the city was in a most favorable condition; the mortuary calendar, for a city like ours, was very light; but I am sorry to inform you that, shortly after your adjournment, the yellow fever appeared in our midst, assuming, as I am informed, a similar type to that of last year; the weekly reports of deaths from yellow fever within the last ten weeks have ranged from 12 to 341 per week; the total deaths reported by the different sextons of cemeteries, of all diseases, during that time, up to the 1st October inst., were 3,513, of which 1,805 were of yellow fever."

On the 8th of October the official report for the week, gave this result: total 358, yellow fever 207; total for the period commencing with that mentioned by the Mayor, to the 8th of October, 3,871, of which 2,012 is the number of deaths from yellow fever alone—of the 2,012 who died from yellow fever, it is presumed, that, as usual, nearly all were whites aged above fifteen years. If the deaths from the epidemic during the season yet remaining shall reach a total of 2,400, this will more than double the whole mortality of the whites aged fifteen years and upward in New Orleans, Lafayette, and their environs from the 1st of January to the 18th of October, in 1845, as given in Mr. T. Stringer's compilation, from which it appears that 1,183 died from all causes in the nine months and eighteen days of that non-epidemic year.

The epidemics of 1837, 1839, 1841, 1842 and 1843 averaged 1,100 deaths; the epidemic of 1841 gave the highest number (1,800) and that of 1842, the lowest. The mortality from yellow fever, in 1847, which was the highest which up to that time had ever occurred, (2,600) and the mortality of 1853, afford the only two examples that surpass the mortality of the year 1854, although these estimates are liable to the objection that they do not regard the numerical ratio of the deaths to the fluctuations of the population.

Towards the close of June sunstroke prevailed epidemically, so to speak. The yellow fever scarcely could be called epidemic until after the middle of August; public opinion hardly recognized it as such until September.

The first official notice of the Howard Association appeard on the 22d of September offering relief to the indigent yellow fever patients of New Orleans; subsequently the Association extended its benevolent operations both as it regards pecuniary and personal aid to the cities of Galveston, Savannah, Charleston and Augusta.

The following data represent a more detailed view of the weekly mortality of New Orleans, in 1854; from the 25th of June to the 8th of October; the first figures denote the total deaths for each week—the second, in parentheses, the number of deaths, if any, from yellow fever for the corresponding week: 329 (0); 191 (0); 129 (0); 159 (0);

122 (0); 212 (29); 207 (43); 258 (118); 380 (186); 393 (185); 484 (284); 530 (340); 504 (341); 423 (269); 358 (207); total deaths in fifteen weeks 4,629—total deaths from yellow fever 2,012.* The great mortality of the first week in this enumeration ending July 2d, was occasioned by sunstroke; 83 deaths were reported from that cause, and 22 from apoplexy; but it is probable that 100 may be attributed to the former, which suddenly appeared, and as suddenly subsided, as did the usual mortality, so that "heat and health" were regarded as synonymous for a time.

Those who affect statistics and sanitary wisdom—who disparage the city authorities and the salubrity of New Orleans, and undertake to prove that the English or some other people are the healthiest and the longest lived in the world on account of their supposed sanitary improvements, know little of what has yet been done in the premises, notwithstanding their fallacious figures, decimal fractions, and vital statistics. The military element in France, and the commercial in the United States, have led to some reliable statistical results. But the British, have failed fundamentally failed in every census, unless, perhaps, that of 1851. Their plan has been excellent, its execution faulty. The hasty comparisons in favor of the health of British towns and against New Orleans, are insufficient, for this plain reason, namely, that a sufficient time has not yet elapsed, even if the boasted improvement had as yet been actually made, to afford reliable deductions—as for instance, the average duration of life and so forth, inasmuch as the sanitary questions concerning the public health of towns in England were not broached until the year 1839, even by petition to Lord John Russell, from the Poor Law Commissioners. The next year the House Lords petitioned Her Majesty's Government on this behalf. In 1840, a Committee of Investigation was appointed by the House of Commons. In 1842, the Poor Law Commissioners reported. In 1843, the Government Commissioners were appointed; their first report was presented in 1844; their second, in 1845. The National Cyclopædia, of 1851, of London, says that "the large towns have already began to make improvements. Of fifty towns visited by direction of the Commissioners, only eight were found to be in a tolerable state as to drainage and cleansing; and as to the supply of water, the reports were still more unfavorable." These reports tend to show that the improvements, with all the means of the nation, appear to enhance the salubrity of towns; but one or more generations must elapse before such a question as the average duration of life can even be compared with that of the preceding generation. Moreover the British Empire has not been recently, nor is it now healthful in a remarkable degree.

Her Britanic Majesty, with her immense treasure, her willing Parliament, has not improved her good city of London in a ratio comparable to the government of the much abused city of New Orleans; which

* Since the above enumeration, the mortality of the week ending October 15th has been announced—total 272 (134); giving a grand total of 4,901; including yellow fever which latter gives a mortality of 2,146.

latter has within a few years reclaimed more than half of the city from the water fowl and the alligator. These sanitary measures as the filling of lots, the paving of streets, drainage and so forth, are set at naught, because they accord with mother wit, and are not based on astrology or something too subtle for common sense.

The researches of the vital statisticians of modern times, tend to prove that the physical comforts promote health, longevity, and the increase of population, while on the other hand, extreme poverty, unwholesome food, insufficient clothing, bad lodging, imperfect ventilation, filth, overcrowding, excessive labor, and exposure to the inclemency of the weather, have a contrary tendency. If, however, facts are to guide the sanitarian, he will be compelled to accept, what few would expect, the negro population of the Southern States of the North American Republic, as the most perfect types of increase of population, health, and long life. No nation, no class how favored soever it may be, from the dwellers in the deepest mines and cellars to the most gorgeous thrones—no town how clean soever it may be, can, as yet equal the negro slaves in these fundamental criteria, as all reliable vital enumerations, including six censuses of the United States (1790—1850) will show. The difficulty of accounting for this postulate cannot invalidate it in the least.

Another inexplicable fact, opposed to the sanitary theory of crowding, the cholera of 1854, has developed, namely, that this epidemic prevails in an inverse ratio to density of population.

It appears from *Revue de Thérapeutique*,* of September 15th, 1854, that cholera which has prevailed in France for nearly a year, has been most fatal where the population has been the least dense. It is believed that the United States will furnish a similar result.

Among the large cities, New York has suffered most, but had the mortality from cholera in that city been in a ratio similar to some small inland towns, villages, and thinly populated settlements, more than 100,000 would have perished during the last summer. Had New Orleans been affected with cholera as severely as a few isolated plantations in the South have been, perhaps 50,000 would have died in this city. Even Marseilles and Naples have not suffered in an equal ratio to many small towns and villages in Europe and America. But it is time to return to the yellow fever: early in the season yellow fever appeared, though to a very limited extent in Vera Cruz and Havana. Before the close of summer, it passed the Tropic of Cancer, in an epidemic form, to New Orleans, Galveston, and Key West. It soon doubled the cape of Florida, and having concentrated its greatest force upon Savannah, reached Charleston subsequently; with few exceptions it spared the intervening towns. Lastly, it visited several inland cities on the Savannah, Alabama, and

* This Journal says that the "cholera of 1854, presents this characteristic, namely, *that the proportional intensity of the scourge, is in inverse ratio to the population; the villages being more severely dealt with than the cities; the hamlets, than the villages; and certain farms more still than the latter.* In La Meurthe, on the banks of the torrents, the disease prevails more strongly than in the vicinity of the large streams In the Vosges it is found, also, that the smallest places are the most frequently attacked. Mirecourt has lost 250 persons out of 5,000, that is to say one twentieth; Dompaire 180 in 1,800, or one tenth; Bouzé 100 in 500, or one-fifth; and to crown all, a village of the environs of Saint Dié has lost 49 in 100; or one-half."

Mississippi rivers, where it produced great consternation, but little mortality.

A few remarks concerning some of the places visited by yellow fever, will close this sketch.

Key West.

Key West, was the first portion of the Republic invaded by yellow fever in the epidemic of 1854. A correspondent of the Charleston Courier, in a letter dated at Key West, September 8, 1854, affirms that the town had then been exempt from yellow fever for six weeks, though, at the military barracks it still raged terribly, having attacked almost every soldier and destroyed one-third of the entire force.

This island, the gift of the tiny coral insect, so unique in its medical topography and interesting to the ætiologist, is, as all know the most Southern portion of the United States, the town of Key West being situated 24° 32′ North latitude. The whole island constituting Monroe county, Florida, has independently of the troops, nearly 3,000 souls, chiefly wreckers, and scarcely affords an area of 200 acres.

Its general elevation above the level of the sea, six or seven feet, no where rising to twenty feet, almost the whole surface during hurricanes being washed by the waves of the sea. An outlier of the Peninsula of Florida, forty miles distant from the Continent, a mass of coral having but a slight stratum of soil, washed by the ever rolling Gulf stream, fanned by the sea breezes, having a delightful temperature, never very hot nor cold, distant from Morasses, Cypress swamps, crowded filthy cities, alluvial deltas, and malaria, and yet this gem of the sea, with its handful of earth, its tropical flowers and shrubs, its clean sands, its variegated shells, its coral strand laved by the waves of the sea, has been visited by yellow fever notwithstanding the general salubrity of the island. Upon the theory of contagion, this coralline ought to be the centre of imported yellow fever from Havana, New Orleans, Vera Cruz, and towns lying on the Gulf of Mexico. By what telluric, astral, or Neptunian sub-marine entity its late visitation or its general exemption from yellow fever may be owing, has not yet transpired. The Æsculapian oracles are either silent, or *Philippise*, to use the Demosthenian idea as to the satisfactory responses which Philip of Macedon, got from the Pythia at Delphi.*

The annual mean temperature of Key West, the highest in the Union, is for nine years 77.1°, about ten and a half degrees above that of New Orleans (67.5° for eighteen years—Smith. Rep. 1853); yet in many northern cities the heat for a portion of the season is greater than in the

* A few years ago the Academy of Sciences, at Paris, appointed a commission to ascertain and report on the prevailing diseases and meteorology of the seasons in Paris. This report drawn up by the illustrious Arago and the distinguished Dr. Double, based on ten preceding years, quarter by quarter, concludes that, no connection can be traced between the characteristics of the seasons (*caractères des saisons dominantes*) and the prevalence of epidemics; though the weather serves to guide the practitioner in the treatment of the sick, (Comptes Rendus, T. IX. 515.) The Academy, however, appear not to understand the word *Buncombe*, which, like "the invention of sleep covers one over like a garment."

former. The arid rock of Gibraltar, and the almost submerged island of Key West, though subject to yellow fever, are devoid of its assumed causes in a most remarkable degree.

The first case of yellow fever which appeared in Charleston, in 1854, occurred on the 11th of May, according to the official report, in the person of a passenger directly from Key West; the second case two months later, arrived from the same port.

Savannah.

Savannah, 32° 04' 56" North latitude, contained in 1852, a total popu lation of 18,801; of which the colored amounted to 6,122 (free 624—slaves 5,478) or about one-third—whites 12,679; by the United States census of 1850, it appears that the total population is but 15,312.

This city on the Savannah river, eighteen miles from the ocean, was founded about ten years after the foundation of New Orleans.

The white population of Savannah during the existing epidemic has been estimated variously at from 3,000 to 5,000, consisting chiefly of the unacclimated; according to some published estimates the unacclimated reached eighty in the hundred of the whites.

The mortality from the epidemic in this city is already, perhaps, unparalleled in the history of yellow fever in the United States. The week ending August 29th, gave a total mortality of 92—yellow fever 60; the week ending September 5th, total 123—yellow fever 74; the four weeks ending September 12th, total 492—yellow fever 305; for five weeks ending on the 19th September, total 681—yellow fever 436. Now taking this last figure of the total mortality as a divisor, and the total white population then remaining in the city at 5,000, it will be found that nearly one in seven perished in five weeks. The mortality before and after that period was great; a ratio of mortality which in little more than half a year would have left none alive.

The mortality among the medical faculty of Savannah has been appalling. While the epidemic was still progressing, the deaths of Drs. Welles, Wildman, Harris, Schley, Ellis, Gordon, and it is said four others, were announced. Estimating the physicians of Savannah as being in the ratio of one to every thousand souls, and reckoning the population at about 15,000 as given in the last United States census about two-thirds of the physicians must have died before the decline of the epidemic.* EDITOR.

NEW ORLEANS, October 10, 1854.

Art. II.—*Letter on Yellow Fever, (continued from page* 380*:)* By M. MORTON DOWLER, M. D.

[To the Editor of the N. O Medical and Surgical Journal.]

In my communication addressed to you, dated ten days since, I spoke of the epidemic in relation to the front or river section of the Fourth

* Owing to a mistake of the printer, discovered too late to be remedied, it was found that the space in this number of the Journal was less by eight pages than the editor expected, though the subscribers will receive the full amount of matter. Hence, several short communications intended for this number, as well as several editorial notices of the yellow fever at Galveston, Charleston, &c., were crowded out. The editor regrets the mistake, not on his own account, but because the space allotted to the friends of the Journal had been unintentionally pre-occupied.

District; (formerly Lafayette) and detailed a portion of what I had seen and known. At that time I had seen but two cases in the rear of Magazine street. During the last ten days, however, there has been no portion of the Fourth District, nor of Jefferson City, in which I have not been called to yellow fever patients, and I may say the same thing of a that portion of the First District above Race street, and about the Melpomene Canal, and the Gormley Canal and Basin. The epidemic is, I believe, quite generally diffused throughout the whole city, there being no section of it in which the disease has not been seen. It has never been surpassed here in malignity; never has there here been seen a greater proportion of hæmorrhagic and black vomit cases.

The utmost attention has been paid in this district to the enforcement of the ordinances in relation to cleaning the streets, and the removal of nuisances. In no season for the last eighteen years has there been a more satisfactory state of things in this respect. I believe the street commissioner has effected this season what no Board of Health ever did or ever could do. Notwithstanding the denunciations which is usual when epidemics prevail have been fulminated against the Common Council, I hope that neither they, nor any future city government will ever again organize another of those useless debating societies called "*Board of Health.*" They never have nor ever can meet the approbation of the people of New Orleans. But even on the supposition that a Board could do all this, never was, nor ever will any Board here be furnished with the pecuniary means to effect any measures of importance. The subjects regularly entertained by such Boards, as for instance, filling of lots, draining of swamps, paving of streets, cleansing of privies, and the general purification of the city, to say nothing of the millions to be expended in "confidence," and quarantine, all implying an unlimited access to the public purse with independent functions, are totally out of the proper jurisdiction of any power save the direct legislative and responsible authority alone. Never yet has any measure emanating from any Board of Health in this city, warned us of an approaching epidemic, prevented its extension, shortened its duration, or given the least idea of its origin. The Health Committees of the Common Council duly empowered, co-operating with the Street Commissioner, constitute all that is necessary in any emergency. The Common Council should never throw off its proper legitimate authority, and delegate the same to any subordinate, secondary, and irresponsible Board. The fact that Boards of Health, do very well in Berlin, London, or Paris, affords no argument in favor of their applicability here. In the countries in which these cities are situated every thing is done to render government as complicated as possible, and to clothe every department of a complicated system with ample powers and pecuniary resources, and there is little or no responsibility to the people. Here the very reverse obtains. Government here is simplified to the utmost, and everything in relation to the public interest is always botched completely, if not done under the direction of the immediate authorities themselves. We have borrowed many valuable things from European institutions, but

Boards of Health are not amongst the valuables. On this subject I may have something to say in a future number. I may merely remark that our streets and city generally have never been in a good condition; that we have had the best possible Board of Health, namely, an efficient body of laborers led on by an able and efficient street commissioner; and that had such "Board of Health" been placed at the head of affairs during the epidemic of 1853, both the money and reputation of our city would have been greatly husbanded. As it is this season, the gutter sends up no intelligence in relation to the origin of the epidemic, and the ship hold maintains a profound silence.

A word in relation to meteorology. It is a science which has shed but a feeble and unsatisfactory light on the ætiology of epidemics generally, proving the same proposition to be true and false at one and the same time. In relation to that of yellow fever it has shown itself to be absolutely worthless. It shows the following truisms, and nothing more; and I call on any one who can, to make anything further out of it. When the barometer shows either a dense, a lightly, or extremely raried atmosphere; when the hygrometer shows either a gentle moisture, reeking wetness, or an intense desiccation; when the thermometer indicates either a mild, airy, pleasant temperature, a breathless, heated atmosphere, or one modified by a protracted chilly breeze; when we behold either a deep, cloudless, sunny sky, or a horizon overcast with impenetrable clouds, blotting out the sun; in all or any of these conditions, all we can make out or infer is, *that the yellow fever may or may not appear as an epidemic; or if it has already thus made its appearance, that it may or may not become more or less general or malignaat; or that it may or not disappear!* The meteorologist points out to you and classifies the clouds. He shows you his *cirri*, his *cumuli*, his *strati*. He points out the *cirro-cumuli*, the *cirro-strati*, the *cumulo-strati*, and the *cumulo-cirro-strati*, or *nimbi*. He gives you a learned disquisition "about the weather," and yet all he can tell you at last is that in relation to yellow fever "fair is foul and foul is fair," He can neither warn you of an approaching epidemic nor tell you what atmospherical conditions will put an end to it; nor what conditions will render it more or less malignant. I would respectfully call on the meteorologist if he knows any thing more about the ætiology of yellow fever than his own coachman, to come forward and publish it to the world. Nothing has yet appeared that every drayman does not know to be without foundation.

The yellow fever agency cannot be transported from one place to another through the medium of *fomites* nor can it be retained in our city in such manner. On examining the mortuary records of 1853, it will be seen that the epidemic was virtually at an end about the middle of September; for during the week beginning at that date the daily average mortality did not exceed eighteen per day. Fresh immigrants from Europe, and especially Germans, poured into the city. The weather was burning hot, and frost distant, and it is palpable to every one that the disease did not cease for want of subjects. Of yellow fever subjects there were thousands left. But the disease did rapidly disappear here, while it was ravaging other places. If the morbid agency then can possibly nes-

tle itself into, and attach itself to goods, merchandize, houses, &c., it follows that every house in New Orleans was at that time but a receptacle of poisoned articles. The yellow fever and death had touched every thing. But none of the newly arrived suffered from eating, drinking, wearing, or touching any article in New Orleans. Still less is the likelihood that any person did or could suffer from any article sent to a distant city after perhaps a three months voyage. If the newly arrived immigrant can sleep with impunity on a cotton bed which has passed through the pestilence, there can be no possible danger to the people on the shores of the Mediterranean or elsewhere from a free reception of New Orleans merchandize. If the propagation of the disease by *fomites* or trade be true, it is incumbent on every conscientious dealer to fumigate and disinfect every article in his store, and never to presume to send any article into the country from New Orleans without the use of ozone, chlorine, or some other disinfecting agent. No such article should be received without the certificate of the *fumigator general* of the city. If there be any possibility of infecting our trans-Atlantic brethren, how much greater the danger of domestic infection! The idea of transporting and carrying the yellow fever agency from New Orleans, is a thing in which every person practically displays his disbelief. All experience shows its utter and entire falsity. If the disease can be thus transported, then the sum total of all the goods in the city of New Orleans is, on this sixth day of Octotober, 1854, but a mass of yellow fever *fomites*, for there is not at present in our corporate limits a house or lot in which an unacclimated person would not take the yellow fever. What becomes of the conscience of a sincere contagionist and importationist when he sends away his goods into the country by the ton without fumigation, or disinfection, and thus sends poison to millions of his countrymen? Our commercial interests and the cause of truth require that every person whether physician or layman should with one accord come forward and give the lie to so monstrous a doctrine. Yet there are those who believe that the yellow fever can be corked up, and the vial of wrath poured on a distant city!

The peculiar morbid phenomena to which we give the name of yellow fever, are the effects of an unknown noxious agency. It is to the assemblage of the effects manifesting themselves on the organism to which we attach this name; and the person laboring under these effects has received an injury from an unknown agency. The injuries he has received he cannot communicate either directly or indirectly to another. A vulnerable bystander incurs no additional risk from being in contact with the injured party. The latter has been assaulted and his injuries show themselves by the resulting pain, heat, thirst, delirium, hæmorrhage, black vomit, &c.; but the injuries he has sustained, the effects which have been produced, are no more communicable to the looker on than are the wounds, contusions, and injuries received in an affray. The same agency which produced the yellow fever injury in one person may and does produce it in another, but one person cannot communicate his symptoms and sufferings to another. Hence we see the unacclimated husband and wife occuppying the same bed, one of them escaping entirely, and the other being smitten. Or husband, wife and children may all be attacked

at nearly or quite the same moment of time without all or any of them having been in contact with the injured, proving clearly thereby, 1st that the injury was not communicated to them by other sufferers; and 2d that they were suffering independently of the bodily condition of each other, or indeed the unacclimated husband while in perfect health, nursing his sick neighbor with impunity, may be summoned home in haste to find his wife and children who may never have approached a yellow fever patient, all attacked at the same moment showing that those who are least exposed to contact with the sufferer are often the first to suffer. Mrs. L. died on the 12th of June last, with the disease, none of the rest of the family suffered at the time, nor did the unacclimated neighbors. I did not see another yellow fever case for about five weeks, when I was called to attend on two ladies—one in the clean, retired, aristocratic portion of the Fourth District, and the other in a fine pleasant house in the front of the same, both attacked with yellow fever. Even the filthy and destitute immigrants had hardly yet begun to suffer from the disease when these two patients were attacked, strangers to filth and to yellow fever contact.

New Orleans stands on the latest of geological formations. The interminable and irreclaimable swamp is seen in the distance at every point of the compass, and in many districts the sun rises and sets, to the eye, in the waves of a sea of gigantic grass rooted beneath the water, yet there is nothing here to explain the causes of yellow fever. Cities resting on secondary rocks and high tertiary formations, are visited annually by the yellow fever in its most malignant form. No one has ever brought forward a single satisfactory proof that our swamps have any thing to do with the production of yellow fever, nor is there any reason to believe that if the swamps of lower Louisiana were at once obliterated, and New Orleans rendered as high and dry as Gibraltar, she would be otherwise than as Gibraltar herself or Havana, still liable to the yellow fever. New Orleans stands on an alluvion but in regard to this pestilence she is none the worse off for that. Drainage is good, filling up is good, reclaiming the swamps is good, but no one need ever expect that all this will have any influence on the appearance of yellow fever. If as some think the yellow fever is generated in the swamps and wafted into the city by the winds, then it is plain that the cause can never be removed by any thing we can do in and around New Orleans. Nothing short of draining, filling up, or reclaiming the whole of lower Louisiana—requiring all the gold of California to effect the object, would be of any avail. The marshes of Louisiana are literally made up of decayed vegetable and animal matter by which they are gradually elevated. The sudden draining and drying up of millions of acres such as these may be considered *anceps remedium*. One thing is certain, we have a population residing on live oak platforms called *chenières* wholly surrounded by those marshes and our city people deem it a privilege to reside amongst them in the summer season *for health*! In such places yellow fever was wholly unknown till the disastrous season of 1853.

The filth theory of yellow fever has been totally annihilated by the events of the past season and the present epidemic. In May and June we

had rain and open sluices admitting the water of the Mississippi into the swamp. June 1854 was not unlike June 1853 in meteorological character. A few straggling cases of yellow fever appeared in June and July last, but so few that the disease was as usual ignored. In the latter month never was there felt in this city a more heated and becalmed atmostphere. Though we have by no means, so filthy a city as New York, it is quite impossible for the most vigilant authorities to entirely rid our city of filth. Every privy, gutter, basin, canal, unfilled lot, square, excavation, or cemetery sends up its exhalation. The swamp and the common exhibit the remains of poisoned dogs, sun-struck horses, dairymen's cows, and bullocks and sheep that have proven themselves too debilitated for the meat market. The evaporation that took place in July was very active. The cypress platform in the rear of the city which had been flooded with rain and river water was as it were boiled to dryness. The Gormley excavation after presenting its festering waters for a long time, now lay *anhydrous* to the sun. Every impurity was in a state of ebullition. Yellow fever, however, had been but barely seen. The mortuary records of the city never read more favorably save only in relation to the effects of the sun, man and beast having expired from insolation in every direction. Filth, it is true, was very active as it ever is, whether yellow fever prevails or not.

The cause of yellow fever is, we know, in an altogether essential degree, subjective or internal. There is, however, an external, or objective cause or agency which is absolutely essential also to the production of the disease. The objective agency, so far from being a thing which is movable and floating in the air, is no doubt an agency as fixed and immovable as the very streets themselves. It yields no obedience to the winds; and it has this season stood as firm as our alluvion before the tempest. Like the cholera it countermarches the whirlwind and the storm. On the first of October this agency was assailed by a hurricane of wind, and a cataract of rain, and the stern disease smote the people in this District during the two succeeding days with increased violence. I saw more new cases during those two days than I have seen in any other two during the present epedemic. So far as the atmostphere is concerned, I have not the least doubt that it may and does exist in its almost purity, alike in the absence or presence of a yellow fever epidemic. Whoever discovers the objective or external cause of yellow fever, must look beyond the crude dealings in gases, animalculæ, cryptogamia, quarantine, filth, and meteorology which are now being exhibited before the world. The mystery lies deeper than all this rubbish. It may or may not be in the power of man to grasp the secret, but if ever that is done new laws must be investigated and new problems solved. A higher scientific era will have been inaugurated.

The ætiology of epidemics and especially of yellow fever and cholera, is a subject in which the public have always taken the deepest interest. The mass of the people ever enthusiastic and hopeful, have generally been rather inclined to receive every new theory with more or less favor, and a like spirit has animated the legislative mind. A singular spirit has in the meantime pervaded medical writings on the subject. There are com-

paratively few of the profession who do not believe that little or nothing is known in the premises, and even the people at large are beginning to ridicule the entire array of "causes" which have so long been paraded before them. Nevertheless, when we consult the greater part of the treatises on the practice of medicine, and the special books, papers, and pamphlets touching on the ætiology of yellow fever, we find a most notable display of causes. The writers cautiously make out these by giving a little "meteorology," a little "medical topography," a little "geology," a little "putrefaction and exhalation," together with a very circumspect reference to the organic, microzoic, cryptogamic, electric, calorific, gaseous, and filth theories, and so forth. A very specious appearance of real knowledge is thereby kept up, and an undoubted display of impartiality. All that the medical reader can make out is, that nothing can be known; and all the non-professional one can discover is that he cannot understand the scientific method of finding out how it happens that nothing is known. It does not comport with the dignity of authorship or professorship to say, "I don't know." It must be said gradually, and with the appearance of knowing everything. Speaking for myself, I may say that I have been a student and practitioner for twenty years; have read and reflected much on this subject, and I have never yet obtained any explanation of the causes of either one or other of these diseases, to which the least importance should be attached—any theory worthy of belief or of legislative attention.

Art. III.—*Discovery of Viviparous Fish in Louisiana.*

"What have we here? A fish—a strange fish."—*The Tempest.*

In the month of October, 1854, through the politeness of J. C. B. Harvey, M. D., of Tchoupitoulas street, I received a small osseous fish, caught in the New Orleans canal, which connects the city with Lake Pontchartrain. This fish had been placed in a basket containing crabs, one of which wounded it slightly in the abdomen near the cloaca, thereby exposing several fœtal fish enveloped in a delicate membrane. The parent fish, which had been rudely thrust into a narrow mouthed phial of spirits, retains after immersion for two weeks, the original *rigor mortis*, and the same remark applies to the fœtuses, though they have been soaked in water; some of them have been forcibly straightened. On the 17th of October, in the presence of, and assisted by Drs. J. Hale and M. M. Dowler, I enlarged the wound and proceeded to dissect a somewhat globular mass of fœtuses bounded by the intestines before, and separated from them by an indescribably thin, diaphanous membrane; this mass was further bounded above by the spine and ribs, below and behind by the posterior inferior abdominal walls, bulging backward of the anal orifice and fin. The exterior envelope of this oblong globe consisted of a very thin, pelucid, extremely delicate and apparently laminated and

flocculent membrane, like the amnion of the human embryo in the early state; it did not form a simple sack, but consisted of many duplications like the arachnoidal reflections among the sinuosities and convolutions of the human brain, sending its prolongations as the hyaloid membrane does, through the vitreous mass of the eye.

This uterine membrane (ovisac it may not be termed) contained twenty-two fishes. It is probable that the inner surface of the uterine membrane sent forth a still more delicate membrane which enveloped each fish after the manner that the peritoneum envelops the abdominal viscera; but the parent fish, and still more its inclosed organs, were too minute to admit of full demonstration during a necessarily hurried examination; moreover, the wish not to mutilate the parent fish very much prevented a fuller dissection of the fœtal mass *in situ*.

Each fœtal fish was doubled laterally, sometimes to the right, sometimes to the left, into the globular form, the caudal fin which is inclined to the lancet shape, though blunter, overlapped one eye and one side of the mouth; each fish *in situ*, and even after forcible extraction from its bed was infolded in a sack; some were drawn out united by pedicles to a common stem, somewhat like an umbilical cord.

These fœtal fishes presented a perfect example of close packing. A perceptible force was required to dislodge them from their beds. The concavity left by their extraction appeared to be lined with a smooth, black, peritoneal membrane.

The intestines which were very minute were crowded forward by the rounded mass of fœtuses which occupied the greater portion of the abdominal cavity. No ova were discovered.

The maternal fish not being much mutilated, is reserved for a more detailed technical description, which my leisure and the limits of this Journal will not admit of at present.

Without attempting fully to describe even the dermal skeleton, I may observe that this tiny fish is a most symmetrical one. Its minuteness may be imagined when I state that after the removal of the inclosed fœtuses it weighed only seven grains, though not disembowelled. Thorough desiccation would probably reduce its weight about half or more. The fish exposed for two hours in the shade on a damp day, was but slightly desiccated. It was weighed by Mr. Macpherson, apothecary, in my presence; but fearing a mistake I had it weighed a second time, with the same result. If each fœtus should weigh but one grain, the aggregate would be more than three times greater than that of the mother!

Measurements in inches: Length including the caudal fin 2 inches; greatest circumference $1\frac{3}{4}$; width vertically $\frac{1}{2}$; length of thoracic fin $\frac{1}{4}$; the caudal fin does not expand from its base or proximal end, but terminates ovally, its length $\frac{1}{2}$; the anal but little expanded $\frac{1}{4}$; the ventral is too minute for convenient measurement, being almost invisible without a lens; the dorsal which is single, has but a slight vertical width, arising from a base $\frac{1}{4}$ of an inch, nearly opposite, though a little forward of the anal.

The teeth are advanced, nearly ranging with the lips, being very numerous, close and small, though scarcely discernible without a magnifying

glass. Lips thin, the under one slightly projecting; angles of the mouth not depressed; eyes medium size; head flattened at the frontal bone; operculum much expanded. The branchiæ largely developed in three great arches, densely fringed with thick tufts, the outer and inner rows inclining to the central, having also, one, perhaps more rows behind, which are shorter.

The predominant hue of this fish is a tawny or fawn color; the opercula silvery; head metalic gray; muzzle blackish, slightly projecting.

There are six rows of rather quadrangular black spots, more particularly marked in the posterior half of the body, averaging twenty-five spots for each row. These black spots, resting on a tawny ground, leaving intervals something larger than themselves, give a picturesque appearance, forming stripes of alternating hues, the three upper of which slightly curve corresponding to the arching back; but each becomes straighter, the fourth and fifth being nearly straight; the sixth or lower row follows the abdominal curve, and disappears at the anal fin; the other five rows gradually converge without coalescing at the origin of the caudal fin. At the origin of this fin the spots are displaced out of line. By this arrangement the six rows of alternating black and tawny leave in the longitudinal direction six other continuous tawny stripes, all of which except the two interrupted ones, are lost at the anal fin, and converge without mingling in the tail, all being about equal in length. The colors fade somewhat into a greyish yellow around the thoracic fins, which are nearly central between the dorsum and abdomen, being on a level with the eyes, and about one line from the opercula.

There are six or seven rows of scales. The spinous rays of the fins are about twenty-five caudal, twelve anal, fifteen dorsal, ten thoracic.

These fœtuses are half an inch long, all alike, exactly resembling the maternal form and proportion, with the following slight exceptions, namely: their bodies are more slender and compressed laterally; their heads are comparatively larger, and their eyes more prominent; their colors are less variegated, and paler; a still greater difference appears about the middle of the abdomen, where there is attached to each fœtus a whitish, faintly yellowish, placental-like irregularly formed mass of considerable size, having a broad base, being apparently implanted in or blended with the abdominal integument, possessing considerable strength, and constituting what may be termed the umbilical prominence; perhaps, it may turn out upon further examination that this mass may be not placental, but an adherent mesenteric mass of convoluted membrane.

These fœtal fishes were probably sufficiently developed at the time of the parent's death to live independent of the mother.

It appears from the proceedings of the Academy of Natural Sciences of Philadelphia, for 1854, that Dr. Gibbons, of the Academy of Natural Sciences of San Francisco, "claims priority of description of viviparous fish," in behalf of the gold-shimmering waters of California, and consequently, that State takes precedence over Louisiana. Agassiz, whose sounding (fishing) line has passed the living waters to the most ancient palæozoic rocks, says, in regard to the California viviparous fishes, that "a country which furnishes such novelties in our days, bids fair to enrich science with many other unexpected facts."

B. Dowler, M. D.