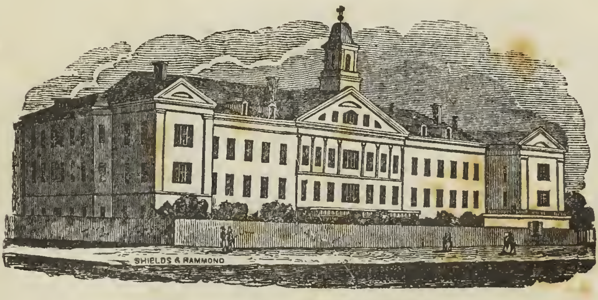
THE NEW ORLEANS MEDICAL AND SURGICAL JOURNAL,

DEVOTED TO MEDICINE AND THE COLLATERAL SCIENCES.

EDITED BY

W. M. CARPENTER, M. D.
E. D. FENNER, M. D.
J. HARRISON, M. D.
A. HESTER, M. D.

"Summum bonum Medicinæ, sanitas."—GALEN.



NEW-ORLEANS CHARITY HOSPITAL.

VOL. III.—FOR 1846-47.

NEW-ORLEANS.
PUBLISHED BY S. WOODALL, 49, CAMP STREET.
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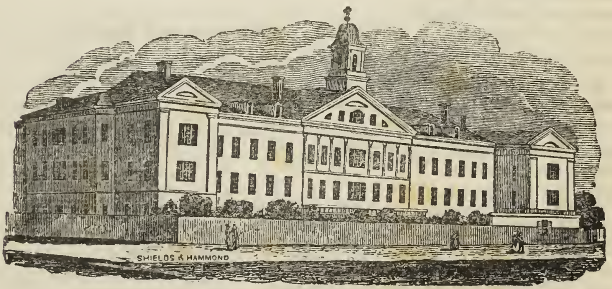

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JULY, 1846.

NEW-ORLEANS.
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TO READERS AND CORRESPONDENTS.

Communications are on hand from Drs. Cocke, Wilkinson, Plough, Hogan, and Ford.

We have received several letters from our colleague, Dr. Fenner, but too late for insertion in this number.

From Dr. Lewis, of Mobile, we have received a valuable meteorological table, which will receive our earliest attention.

We regret that a severe illness has prevented Prof. Wedderburn from reporting his case of operation for tying the external iliac artery.

The following works, in addition to those noticed, have been sent us for review:

1. *Fordyce on Fever*, *Bell's Select Library.* Barrington & Haswell, Philadelphia.
2. *Johnson on Tropical Climates.* S. S. & W. Wood, New York.
3. *Elements of Surgery.* By Robert Liston, &c., with copious notes and additions, by Samuel D. Gross, M. D., &c., of Louisville, Ky. Barrington & Haswell, Philadelphia.
4. *A Manuel of Chemistry.* By Richard D. Hoblyn, A. M., Oxon. &c. S. S. & W. Wood, New York.
5. *Elements of Physiology, &c.* By W. B. Carpenter, M. D., F.R.S., &c. Philadelphia, Lea & Blanchard.

We have received or had access to the following Foreign Journals:
British and Foreign Medical Review. (April.)
Medico-Chirurgical Review, for April, 1846.
Edinburgh Medical and Surgical Journal. (April.)
London Medical Gazette. (March and April.)
Northern Journal of Medicine.
Provincial Medical and Surgical Journal. (1st, 8th, 15th and 22d April.)
Medico-Chirurgical Review.
Annales de La Chirurgie.
Journal de Pharmacie et de Chimie. (February and March.)
Journal de Chimie et de Physique.
Journal de Chimie Medicale.
Annales de la Chirurgie.
L'Experience.
Annales des Sciences Naturelles.
Archives du Museum.
Gazette des Hopitaux.

We have received the following Journals, regularly in exchange:
American Journal of Medical Sciences. Philadelphia.
American Journal of Science and Arts. Newhaven, Conn.
Boston Medical and Surgical Journal.
Western Lancet. Lexington, Kentucky.
Western Journal of Medicine and Surgery. Louisville, Kentucky.
Missouri Medical and Surgical Journal. St. Louis.
Illinois and Indiana Medical and Surgical Journal.
New York Medical and Surgical Reporter.
Buffalo Medical Journal.
Medical Examiner. Philadelphia.
Medical News and Library.
St. Louis Medical and Surgical Journal.
Southern Journal of Medicine and Pharmacy. Charleston, S. C.
Southern Medical and Surgical Journal. Augusta, Georgia.
Bulletin of Medical Science. Philadelphia.

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THE NEW ORLEANS

MEDICAL AND SURGICAL JOURNAL.

JULY, 1846.

Part First.

ORIGINAL COMMUNICATIONS.

I.—*An Examination of the Riddellian Philosophy.* By Albert Welles Ely, M.D., of New Orleans.

1. If it is true that men are always loth to abandon the venerable philosophic opinions in which they have been educated,—a proposition which seems to be opposed to the fact, that in all ages of inquiry one new theory has succeeded another in rapid succession, thus showing no *very* deep reverence for existing systems of philosophy,—it is equally true, that the great and besetting sin of philosophers in general, is *too rapid generalization.* Such is the proneness of the human mind to pretend to be able to assign an adequate cause for every existing phenomenon or effect, that it is not surprising that the history of philosophy should present so many revolutions of opinion—so many highly ingenious theories, one rising upon the ruins of another—so little real veneration for venerable philosophic opinions. No very affectionate tenacity for old theories certainly can be traced. This affectionate tenacity is observable, and lamentably so, in the history of religious and political opinions; but the charge, that theories of physical science have been, and still continue to be, pertinaciously adhered to, for no other reason than because they are old, is quite untenable. Philosophers have not been, generally, so unphilosophic. This new philosophy which I now propose to examine is only another notable exemplification of the truth of this; for assuredly that can be called no very affectionate tenacity, no very *great* veneration for the theory of the immortal Newton, which could venture, after the approval of La Place, and the profound admiration of a host of renowned philosophers, to pronounce it an absurdity!

2. It is assumed, in this new philosophy, that the theory of Newton affords no satisfactory explanation of the physical phenomena of nature, and it proposes to supply this great defect—to give an explanation of them which is satisfactory, because comprehensible, and in every respect in

accordance with known truths. But the grand error of this new philosophy is, 1st, that it assumes that to be an absurdity which is not; 2d, that it generalizes from too few facts, and often from facts which are not facts; and, 3d, that, finally, it falls into the very absurdity which it charges upon the Newtonian philosophy. In my examination of the Riddellian Philosophy I shall consider—

I. What was the great absurdity of Newton?

II. What is the system which takes the place of that absurdity?

III. The objections to the Riddellian Philosophy; and,

IV. If admitted, will it explain phenomena?

3. I. The Newtonian absurdity is discovered to be involved in the doctrine of Universal Gravitation, which Newton declared to be an influence by which every body in the universe, whether great or small, tends towards every other, with a force which is directly as the quantity of matter, and inversely as the square of the distance. Gravitation was believed to be a force acting through matter. Newton did not pretend to say what this force is; he only knew that such a force existed in nature, giving rise to all the great phenomena of the heavenly bodies. He believed it to be a force by which bodies were enabled to act on one another *at a distance;* not a quiescent force, as is stated by Professor Riddell, but essentially an active force, constantly acting, never quiescent. What, then, is the great absurdity, in all this, by which the Newtonian theory is rejected? It lies, we are told, in supposing that one body can act upon another at a distance. This is declared to be altogether inconceivable and absurd. It is admitted that it is incomprehensible—a profound mystery; but is it therefore absurd? To answer this question in the affirmative, would be *itself* a palpable absurdity; for, since there are many things in nature which we cannot at all comprehend, it would follow that they are all absurdities. We cannot comprehend how the particles of a body cohere, so as to form a solid mass, or how they act on each other by impulse, or how the different properties of matter reside together. In what way do impenetrability and other properties co-exist? These are all mysteries which we cannot fathom; but are they absurdities? Reasoning after the manner of Professor Riddell, they are. The position assumed by Professor Riddell is, that whatever is inconceivable, is an absurdity. It is needless to say how fatal to all existing systems of philosophy this principle would be, if adopted. Every thing is an absurdity which the poor, feeble, finite mind of man cannot comprehend. Mind itself would be the greatest of all absurdities. Of the nature of mind—of the nature of its connection with matter, and of its power to act upon it, we absolutely know nothing. Can Professor Riddell tell us, does he comprehend how the slightest muscular action can be produced through the agency of mind! He cannot; and therefore such action, produced by such a cause, must be an absurdity! If gravity be an inherent quality, pent up and *quiet* in matter, how can it produce action at a distance? Newton did not suppose, nor does any one suppose, that gravity is a force pent up and *quiet* in matter, any more than that mind is pent up and *quiet* in the body. It is necessarily an *active* quality; and how it acts we will tell him when he tells us how mind acts, or how impenetrability and other properties joined to extension produce matter.

4. We are told, that the admission of attraction, as an inherent quality, precludes all rational inquiry. By this is it meant, that we must never pretend to assign an ultimate cause? There is no case in which all rational inquiry can cease, except where an ultimate cause is assigned; and can Professor Riddell avoid ultimate causes? Has he avoided them in his theory? When we come to unfold that theory, we shall see. We know that the pride of man is such, that he is unwilling to rest any thing upon a final cause. He loves to think, that there is nothing in nature which the mind cannot finally comprehend. Hence the many theories—vain, poetical, scientific dreams, that have arisen ever since the days of Epicurus and Democritus. What strange cosmogonies have arisen! What ingenious attempts to shut out final causes have been made! Just at this moment has the world before it, perhaps, the most able attempt, and yet a very feeble one, that has ever appeared; I allude to the theory of the unknown author of the Vestiges of Creation, by which he attempts to show, that the entire work of creation is entirely comprehensible by man, and that the whole superstructure of the universe, and every thing in it—man, animals and plants, have all been erected by the natural and unassisted development of the inherent qualities of brute matter alone. To this conclusion he has arrived, by being resolved not to let rational inquiry be shackled by final causes. The idea is of Pagan origin, for it may be traced to Epicurus, and found announced in the writings of Lucretius. In different forms has it been advocated by various modern authors; and this may also be said of Professor Riddell's theory. Newton himself did, for a time, promulgate such an idea, but he afterwards abandoned it. Descartes maintained similar ideas; and Euler, the very same theory itself as that of Professor R. The theory of impulsive attraction, which Professor Riddell now maintains, is only an old theory revived. It was invented and advocated in the time of Newton and Descartes, and during that time discussed and exploded. The theory is clearly announced in the works of Euler, and by him defended. It is now revived. To verify these statements, we quote from Euler: "But in attempting to dive into the mysteries of nature," says he, "it is of importance to know if the heavenly bodies act upon each other by impulsion or by attraction; if *a certain subtile invisible matter impels them towards each other;* or if they are endowed with a secret or occult quality by which they are mutually attracted. Some are of opinion that this phenomenon is analogous to an impulsion; others maintain, with Newton, and the English in general, that it consists in attraction." Here are the two rival theories distinctly announced. Again: "The English maintain that attraction is a property essential to all the bodies in nature, causing them to approach one another. Other philosophers consider this opinion absurd, and contrary to the principles of a rational philosophy. They admit that powers exist, causing the reciprocal tendency of bodies towards each other, but they maintain that they are foreign to the bodies—that they belong to the ether, or the subtile matter which surrounds them, and that bodies may be put in motion by the ether, just as we see that a body plunged into a fluid receives several impressions from it. Thus, according to the first, the cause of attraction resides in the bodies themselves, and is essential to their nature; and according

to the last, it is out of the bodies, and in the fluid which surrounds them." (Euler, Letters on Nat. Philosophy, chapters 54 and 68.)

5. How *new* Professor Riddell's theory is, then, can be seen from the above extracts. We are told by Professor Riddell (¶ 3) that the whole history of science shows that men are always loth to abandon venerable philosophic opinions,—favorite doctrines, and systems of philosophy. The above extracts would seem to prove this conclusively.

6. Having thus shown, I will venture to say, beyond dispute, that the Newtonian theory of gravitation is by no means an absurdity, and that Professor Riddell is only advocating an old theory, long since exploded, I shall proceed to consider—

II. What is the theory advocated by Professor Riddell, in the place of that of Newton, which he calls an absurdity?

7. It is, that all the vast empyrean of nature is filled with a species of extremely subtile matter, or ether, as Euler calls it, or, giving it its new name, used by Professor Riddell, *ponderefacient* matter, by virtue of which all bodies have weight. This ponderefacient matter constitutes a new medium, of which there are many, called the *ponderefacient medium.* Next, it is supposed that this medium, in which all the celestial bodies are suspended, is traversed by right-lined impulses of motion in every direction, and, of course, acting upon every thing contained in it. A body, then, upon which these impulses of motion act in every direction without obstruction, will be perfectly at rest; but if another body intercepts the impulses of motion, in any direction, the two bodies will immediately begin to approach one another. Thus, all motion of the heavenly bodies is made to depend upon the impulses of motion acting upon them, being intercepted, or cut off, by the interposition of some other body or bodies. A body, for example, approaches the earth or sun because the impulses of motion are intercepted in the direction of the earth or sun by those bodies, and thus the opposite impulses are left to act alone upon the body.

8. This theory is conceived to depend on a theory of the constitution of matter involved in the following propositions:

I. Matter is any thing real, which occupies by itself length, breadth, and thickness in space.

II. Matter exists aggregated into spheroids or atoms, forming, in respect to the dimensions of the different terms of atoms, an indefinite series, probably geometrical, in which each atom is composed of an aggregation of an indefinitely great number of atoms subordinate in the series in respect to size. Fixing the attention upon one atom of each term, they present, in their relative dimensions, a decreasing or increasing series, whose ratio is indefinitely great or small, and whose number of terms above and below any assumed point is perhaps infinite.

III. Around each material atom or aggregated sphere there lies a sort of atmosphere or medium, consisting of diffused atoms belonging to the subordinate terms in the material series.

IV. Matter is inherently inert, or possesses what has been called *vis inertiæ;* by which is meant, that matter can neither of itself begin to move, nor cease to move when set in motion.

V. Matter is indestructible.

VI. Matter is inherently and necessarily possessed of no qualities, unless its extension, mobility and inertness be called qualities.

VII. Motion existing in time, of which it is the cause and measure, is the translation of matter through space.

VIII. Motion is the source of all qualities, and the proximate cause of all phenomena which matter exhibits.

IX. Momentum is physically indestructible and uncreatable.

X. Momentum is transferable from matter to matter solely by impact or collision.

XI. Every present material motion has resulted from exactly equivalent antecedent motions.

XII. Every present material motion must be followed by exactly an equivalent of consequent motions.

XIII. Impulsive motion travels in all possible directions, through all possible points in space.

9. Such are the propositions which Professor R. supposes to be established, and upon which this theory rests. Before a theory can ever be received, two things are indispensably necessary; first, that the data upon which it rests are established beyond all question; and, second, that the theory accounts fully and satisfactorily for all the phenomena which it is intended to explain. How far this old theory, which Professor Riddell has revived, satisfies these conditions, will appear, in considering—

III. The objections to the Riddellian Philosophy.

10. The first proposition is admitted by all; but the second is manifestly a wild, extravagant, scientific phantasm, just as rational and demonstrable as Plato's idea, that fire is a pyramid tied to the earth by numbers, and that the world is a figure consisting of twelve pentagons. It is a pure hypothesis, which admits of no possible means of verification, from the fact, that the human faculties cannot take cognizance of the intimate nature of matter, no data being furnished, by observation and experience, by which we can prosecute any satisfactory investigation of it. The problem of the intimate nature of matter has been in the hands of philosophers ever since the first dawnings of science, and it is now precisely where it was then. This is admitted by all philosophers of the present day. How absurd, then, is it to predicate anything upon a fancied solution of this problem. The truth of this proposition is an indispensable element in the theory which Professor R. undertakes to defend. The proposition involves the idea of a number of different media, such as the luminiferous, the ponderefacient, of which the latter is the *sine qua non.* This moves all the stellar systems of suns, planets, satellites and comets. Now, of the existence of such a medium there is no satisfactory evidence whatever. The only evidence of the existence of such a medium is that by which some have attempted to establish the existence of a resisting medium retarding the course of Encke's and Biela's comets; but the observations on the two comets give contradictory results, although made with the greatest care, and under the most favorable circumstances. They establish nothing definite; and the supposition, for it has never been any thing more, of the existence of such a medium is now any thing but established. No writer speaks of it as established, but as a matter of doubt. "Observa-

tions upon Halley's and Biela's comets," says Professor Walker, "have only involved the idea of a resisting medium in new difficulties." It is found that no single estimate of the density of such a medium, or of the law of its resistance, will satisfy the observations of Halley's, Biela's, and Encke's comets, which are most liable to its influence. "It appears," says Professor Walker, "that the theory of a resisting medium does not rest upon demonstrative evidence, and that the difficulties introduced into the science, by the discovery of the three periodical comets, cannot be completely removed, whether we receive it or reject it." To this I might add the observations of Professor Mitchell on the subject, as given in his recent course of lectures. He undoubtedly possessed the latest information, and his doubts expressed on the subject will be recollected by all who heard him. He attempted to explain the retardation of the comets by an entirely different hypothesis—that of the comets encountering, in their orbits, strata of nebulous matter revolving about the sun.

11. Such, then, is the poor foundation upon which this second fundamental proposition of Professor R. rests—a proposition upon which hangs his whole theory. The supposition of a ponderefacient medium must be established, before he can find any support whatever for his theory. In all this philosophy it is a fundamental assumption, that no force or influence can act, except through the interposition of some medium; and with this idea it is, that the mind itself does not, according to some, exert its influence directly upon the body, but through the medium of what is called animal electricity. Men have the right to indulge, to any extent, in flights of fancy and imagination, but they have no right to make their dreams the substrata of grave theories. It is not with a little surprise that we find it announced, by Professor Riddell, in his second paragraph, that "the existence of a subtile inter-planetary resisting medium has been established." It would seem that his zeal, in defence of certainly a very ingenious theory, must have led him, in making this announcement, to adopt the mere opinions of authors for truths. Nor has he dwelt upon this all-important subject at all. Let him take up this subject; and if he can establish the existence of a resisting medium, he will then have *some* foundation for his theory, for, assuredly, it is thus far founded on bare assumption.

12. In regard to the luminiferous medium, we are to bear in mind, that although it affords a very satisfactory explanation of the phenomena of light, its existence is not proved, and therefore we cannot assume its existence as an argument in favor of the existence of other media.

13. Proposition third, so far as it asserts the extreme porosity of bodies, is true, but no further. We shall have occasion to refer to this again. Propositions fourth and fifth, sixth and seventh all admit. In regard to the eighth proposition, it would be difficult to show how motion, in our present *actual* knowledge of the constitution of matter, could give rise to all of the qualities of matter.

14. Proposition ninth, that momentum is physically indestructable and uncreatable, is one which Professor R. has labored hard to establish; but his demonstration is worth nothing, since it is founded upon proposition second, which is not yet established. The doctrine inculcated is, that motion may be transferred from matter to matter, but can never be

lost or destroyed. It is also believed by Professor R. (see our discussion in the New Orleans Bulletin) that motion is the *sole* quality of matter. This is equivalent to saying that matter necessarily moves. This doctrine of the indestructibility of motion has been invented to account for the impulses of motion which are supposed to be constantly traversing the ponderefacient medium. Here a motion must be incessantly kept up, and hence the effort to prove that a medium, once thrown into vibrations, can never cease to vibrate, notwithstanding that the particles of matter composing the medium are all inert. Here, then, if we receive Professor Riddell's demonstration of the indestructibility of motion, we are furnished with a mathematical demonstration of the *perpetual motion*, pronounced by him an absurdity. Nature furnishes no analogies by which we can arrive at the conclusion that a medium of dead, inert matter, once thrown into vibrations, can never cease to vibrate. The very reverse of this is shown every where. The fact is, that the vibrations would continue to become less and less, until they finally ceased; unless we can suppose that dead matter is capable of keeping itself in motion. The effort is manifestly one to assert the independence of matter, and to discover in it certain efficient, creative, natural agencies, adequate to the production of all the forms and phenomena of matter. Motion, in being the sole quality of matter, must be a necessary consequence of its creation. God, then, did nothing but create matter; motion has done the rest; for all qualities, we are told in proposition eighth, such as extension, form, color, are the result of motion. Are we, then, really to believe, with Professor Riddell, that the universe, although composed of inert matter, is a *perpetual motion*, an *eternity clock*, that can never run down?—that the hand of the Deity is never required to clear it of the cobwebs of time, oil its wheels, watch over it, and occasionally wind it up?

> " ——— Credat Judæus Apella,
> Non ego."

Such philosophy is as old as the days of Epicurus and Chrysippus. It characterized the Epicurean school. The gods reposed with sublime indifference in Olympus: the world took care of itself. Horace frankly avows his belief in the doctrine, when he says—

> " ——— namque deos didici securum agere ævum;
> Nec, si quid miri faciat natura, deos id
> Tristes ex alto cœli demittere tecto."

15. Professor Riddell's tenth proposition, that momentum or motion is transferable from matter to matter solely by impact or collision, is a plain *petitio principii*. The very question in dispute is, whether matter acts upon matter at a distance or not? Professor R. adds, that the transference of motion, by impact from matter to matter, is a rational effect from a rational cause. Does, then, Professor R. comprehend how motion is communicated? This is one of the greatest mysteries in nature. Can he tell us why motion must result, even from positive impact? Can he define the *nexus* between cause and effect? Sequence in time is the only connection that can be traced. What we call a cause is but an antecedent, of which the consequent is called the effect. The former is but the sign, and not the cause of the latter. We could

not determine, by any *a priori* reasoning, from an inspection of the antecedent, that the consequent would follow. So that motion, as a consequent of impact or collision, is just as inexplicable, just as much a mystery, as motion which takes place at a distance. And yet we are told, that motion, by impact, is a rational effect from a rational cause. By rational I understand, agreeable to reason; but reason cannot touch the case.

16. We are reminded, in proposition third, of the wonderful porosity of bodies; and this fact leads us to a curious conclusion. It is highly probable, that the particles of matter in bodies are not in contact at all. If they were, they could not be compressed. There is no limit set to the extent to which we can compress bodies. We know of no degree of compression which could not be greater by the addition of a greater force. It was Sir Isaac Newton who advanced the opinion, that our earth might be compressed into the compass of a cubic inch. (Brewster's Life of Newton.) In order to establish his tenth proposition, it will be necessary for Professor R. to show that the particles of matter in bodies of all kinds are actually in contact; and, for his particular theory, it will be necessary for him to show, that that is actually the case in even the most attenuated media. But if it is highly probable, as the extreme compressibility of all known substances clearly shows, that even the particles of matter in the most solid bodies are not in contact, much less must there be contact in ordinary gaseous bodies, our atmosphere, for instance. And if there is no contact in the particles of our atmosphere, how much less must there be contact in the luminiferous medium? And infinitely less must there be contact in the ponderefacient medium; for the influence of gravitation is transmitted instantaneously. If it were transmitted with a measurable velocity, the rate of velocity would sensibly affect the secular variation of the mean motion of the moon. The calculations of La Place would make the tenuity of the ponderefacient medium fifty millions of times greater than that of the luminiferous medium. Professor R. quotes him as saying one hundred millions. Either of these numbers will answer my purpose. The latter number, preferred by Professor R., makes the particles of matter in the ponderefacient medium one hundred millions of times farther apart than those in the luminiferous medium; and yet the extreme tenuity of this latter medium is almost, if not quite, inconceivable. Who, then, can suppose that there is contact of the particles composing the ponderefacient medium? And if there is no contact there, what becomes of the doctrine, that the heavenly bodies are propelled through space by means of a ponderefacient medium, which transmits motion by impact or collision of its particles?

17. By what process of reasoning Professor R. can show that the particles of matter of any of his media are in contact, we know not; but he must show this, or his tenth proposition, and his whole theory, are good for nothing. All experiments and observations, thus far, upon matter, show that there is no contact of the particles. Speaking of the extreme porosity of bodies, Professor R. himself says, that "probably, in the immediate constitution of visible bodies, as iron, gold, water, &c., the actual molecular nuclei, in reference to their intervening spaces, bear some such proportion as that borne by planetary and stellar bodies,

to the immense intervening wilds of ethereal space." It is undoubtedly no exaggeration.

18. But it is objected, that if we suppose the $M\infty^{\infty}$, or ultimate particles of matter not in contact,* the transmission of impulses of motion would require time, whereas, on the contrary, that transmission takes place instantaneously, and therefore there are no intervening spaces. This objection is not valid, for it assumes as established the very question in dispute; it supposes no action ever to take place except by contact. When motion is transmitted through a body, there is no proof that the spaces between the particles are diminished; but that there are intervening spaces, even in the most solid bodies, Professor R. has asserted in the amplest manner. So far as observation and experiment extend, the particles of matter always maintain, in the body of which they form the mass, the same relative distance from one another. Variation of temperature may vary this distance, but uniformly; and when motion is transmitted, no change takes place in the particles. It is then philosophically true, that the head of a hammer, for instance, does not necessarily come in contact with a body before motion can be transmitted through it. When one body impels another, the latter begins to move the moment the former arrives within a certain distance. In the experiments of Professor R., given in sections 27 and 40, there is nothing showing that the spaces between the particles are diminished. The particles in the extremity of the bars being moved, those next to them also move, without suffering contact (at least we have no evidence that they do), and thus motion takes place throughout the whole bar; and whether the bar be crooked or straight, it is the same. It is just as clear a case when it is bent into half a dozen U's, as when it is straight.

19. Since, then, we have ample proof that in all material aggregations there is no contact of the particles, and no evidence that there is, even when motion takes place, the grand conclusion to which we arrive is, that ALL ACTION IS ACTION AT A DISTANCE, and that, consequently, Professor Riddell's tenth proposition is false.

20. Proposition eleventh, that every present material motion has resulted from exactly equivalent antecedent motions, is one dependent on proposition ninth, asserting the indestructibility of motion; but this ninth proposition is false. Professor R. observes, that, "as millions of clear cases are presented, in which motion is caused by precedent motion, and momentum produced from preëxisting momentum," *ergo*, *all* motion and momentum are the result of previous motion and momentum. This is using the *ergo* rather too soon, for there are also millions of clear cases in which motion takes place without any apparent antecedent motion whatever; and cases of this latter species are infinitely more numerous than the former. Leaving out of view the doctrine of gravitation, the number of cases of motion which can be traced to antecedent motion is comparatively small; while, on the contrary, there is almost an infinite number of cases of motion for which we can find, not only no antecedent motion, but no cause whatever. Nor does the doctrine of gravitation, even, help us out of the difficulty; for there are very

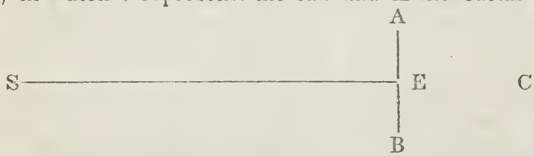
* See this Journal, vol. ii., p. 595.

many extremely interesting motions constantly occurring before our eyes, which find no explanation in the doctrine of either attractive or impulsive gravitation. Proposition eleventh, then, is not established. Proposition twelfth, that every present material motion must be followed by exactly an equivalent of consequent motions, also rests on the doctrine of the indestructibility of motion, and therefore falls with it. The idea that perfectly dead matter can move eternally, without the constant renewal of some force, although the dead matter is constantly jostling other matter, is absurd. The force which, in the first instance, threw matter into motion, unless renewed, must be finally destroyed.* We need not refer to the mechanics of collision to prove this. Professor Riddell's demonstration of the indestructibility of motion, being founded on his second proposition, which cannot be established, is radically defective.

21. The last proposition, that impulsive motion travels in all possible directions, through all possible points in space, is neither proveable nor probable. It is dependent upon the existence of a ponderefacient medium; but as the existence of such a medium is not established, or even rendered probable, this last and thirteenth proposition is good for nothing. We will now proceed to the last branch of our subject, in which we are to consider—

IV. Whether the Riddellian philosophy, if admitted to be true, will explain phenomena?

22. The greatest of all phenomena, the motions of the celestial bodies through space, cannot be accounted for by this revived philosophy. This I shall proceed to show. The Newtonian philosophy accounts for phenomena just as well when there are supposed to be only two bodies in the universe, as when there are fifty or any other number. Let us take the simple case of two bodies, as in the following diagram, in which S represents the sun and E the earth. Now, sup-



posing, according to the theory, that the whole universe is filled with a ponderefacient medium, traversed by impulses of motion in every direction, the sun and earth are acted upon by these impulses on all sides, and neither of them could move except in that direction in which the impulses are intercepted or cut off: the earth at E, then, could move neither towards A, B, or C, in consequence of the resisting impulses; nor could it move in a line perpendicular to the plane A E S, for the same reason. There is, therefore, but one direction left in which it could move, namely, towards S. Circular motion, then, it is plain, could not exist. The earth, if Professor Riddell's theory were true, would drop into the sun, and it would be the same with all the other planets; for the mass of the sun infinitely exceeds the mass of all the others put together. In order that the earth might move in any other direction than towards the sun, towards A, for instance, there would be required the presence of a third body constantly intercepting the impulses in the direction of A; for we cannot suppose that the earth could continue long in that direction, or any other, against the resistance of Mr. Riddell's impulses of motion, which, supposing them to exist, would exert a power equal to that of gravitation.

23. This fatal objection to Professor Riddell's theory, which I have before pointed out in the columns of the New-Orleans Bulletin, he tries to avoid, by saying, that the motion of the planets in their orbits would be kept up by the rotation of the sun on its axis. But he is a little guarded in his expressions; for he says that the projectile force created by the sun's rotation would be "*probably* adequate to make up for the slight resistance offered by the attenuated material medium through which the planet moves." He is not positive about this, and, indeed, it would be very singular and original if he were; for it would be saying that the planets and comets owe their movements simply to the rotation of the sun on its axis. It is universally admitted, that the doctrine of a resisting medium, if established, leads to no other result than the destruction of the solar system. As the planets now revolve about the sun, the centrifugal force just balances the centripetal; but, according to Professor Riddell's theory, the centrifugal force must not only equal the centripetal, but also be sufficiently great to overcome the entire force of gravitation on the planets. There remains, then, only his supposition, that the rotation of the sun on its axis generates a force equal to the force of gravitation. The centrifugal force is just equal to the centripetal; but before Professor R. can make the planets revolve, upon his theory, he must show that the centrifugal force is *double* the centripetal; for the force constantly acting against the planets, on the new theory, is the force of gravitation.

24. In section 58, Professor R. attempts to demonstrate, that the force of his impulsive attraction must vary inversely as the square of the distance. The demonstration which he has given depends upon the sectional area of a body; whereas it must depend upon the mass or quantity of matter alone. The sectional area of a body has nothing whatever to do with its mass; for it can be shown that there may be an indefinite number of sectional areas, all corresponding to the same mass. It is only the old proposition, that the apparent area of the body varies inversely as the square of the distance; and this is all that his demonstration proves. His reasoning is this: the sectional area of a body, whose mass is the constant quantity a, varies inversely as the square of the distance; therefore, the force of impulsive attraction must vary inversely as the square of the distance. In this attempted demonstration of the law of impulsive gravitation, we think that Professor R. has greatly deceived himself; for if he will examine the matter with a little care, he will perceive that there is no connection whatever between the law of his impulsive attraction, and the sectional area of a body. According to the theory, the matter of the ponderefacient medium is so extremely subtile as to penetrate all known substances. Now, does it follow at all, that the force of the impulses of motion which pass through the body whose mass is a, must vary inversely as the square of the distance, simply because the apparent area of the body thus varies? There is no connection between the two things. Admitting, even, that the *amount* of impulses intercepted by a given mass from a given point

must vary as the apparent area, it by no means follows that the *velocity* or force of those impulses must thus vary. The mass of a body can only determine the *number* of impulses intercepted; but the *velocity* of the impulses will remain the same. We must suppose that the velocity of the impulses traversing the ponderefacient medium is uniform. A variation in the mass would vary the number of impulses, but not the velocity. Professor R. claims it as a superiority which his theory alone possesses, that it enables one to assign a reason why the force of gravitation *must* vary inversely as the square of the distance. But this is not at all apparent from his demonstration.

25. In section 61, Professor R. informs us, that "the ponderefacient impulses must require time for their transmission through space. Assuredly, then, they cannot account for the phenomena due to attraction, since the force of attraction is instantaneous.

26. Professor Riddell's ponderefacient medium was invented to explain the phenomena of attraction and gravitation: let us now see how the new theory explains gravitation, gravity, or weight. Since we can conceive of no matter destitute of weight, although we may not be able to weigh it, it follows that the ponderefacient medium itself, which is matter, must have weight: so that Professor Riddell invents one species of matter to give weight to another. A third species of matter, we suppose, would be necessary to give weight to the ponderefacient matter, and a fourth species to give weight to the third; and so on *ad infinitum*. Professor R. seems to have forgotten that his ponderefacient medium was matter. The great object of Professor Riddell's theory was, to explain action at a distance; but it is sufficiently evident from the preceding examination, that his theory, instead of affording any explanation, only substitutes one action at a distance for another. He endeavors to arrive at the cause of universal gravitation; and he thinks that he has done so by the invention of his ponderefacient medium; just as though assigning that to be a cause which itself requires a cause, were a satisfactory explanation.

27. Professor R. expresses a great dislike to inherent qualities, and says, that the Newtonian philosophers are incessantly obliged to resort to them to explain phenomena. We confess that we have studied this new (old) philosophy with great attention, without being able to discover, as yet, how even Professor Riddell avoids inherent qualities. He informs us that motion is the *sole* quality of matter, (¶ 3 Bulletin). Motion, then, must be an inherent quality. If it is not, what is it? And I would ask, as I have done before, how Professor Riddell's theory explains these phenomena, which, he says, the Newtonian philosophers can explain only by resorting to inherent qualities? He says (¶ 35), "The system of philosophy which I have espoused furnishes common sense ideas of the proximate causes of cohesion, chemical attraction, light, heat, electricity, galvanism, magnetism, the unexhausted and unexhaustible luminosity of the sun, and the unobstructed motions of the planets—all resulting from the harmonious relations of material atoms, in accordance with the clear and necessary laws of motion." This is certainly making high pretensions. His theory is a potent key, which unlocks all nature. Would it not be well for Professor R. to make good these high pretensions, by explaining, by his philosophy, independently

of inherent qualities, the phenomena of cohesion, chemical attraction, magnetism, electricity, &c.? This he has not yet done. His philosophy affords no explanation of the unobstructed motions of the planets, as I have shown, and his demonstration of the probable causes of the luminosity of the sun is founded upon the unestablished proposition of the indestructibility of motion. Indeed, he only thinks it himself "tolerably satisfactory." (¶ 108, 109). He can only explain the above phenomena by assuming as established some of those propositions which I have shown to be unestablished. For the sake of science, we trust that Professor Riddell will give the public an explanation, founded on his philosophy, of the above-mentioned phenomena; and also show how an explanation, founded upon those peculiar propositions of his philosophy which I have shown to be unestablished, is more satisfactory than the explanations usually given. If it is impossible to comprehend the *modus operandi* of inherent qualities, we confess that we cannot perceive how phenomena are rendered any more comprehensible by resorting to a complicated machinery of various subtile media, the very existence of which is not established, self-acting, though inert matter. There is nothing absurd in resorting to inherent qualities; nor can Professor R. himself escape them. The incomprehensible prevails just as much in this new philosophy as in the popular philosophy of the day. Even admitting that motion is consequent only on impact, impulse thus created is no more comprehensible than attraction.

28. Before closing this article, I will take the trouble to correct an error fallen into by my distinguished opponent, in regard to the laws of Kepler. In his last article in the New Orleans Bulletin, he endeavored to refute my argument, by laboring to show that I had fallen into a contradiction. Any one who will take the trouble to examine sections 49, 50, 51 and 52 of my last article in the New Orleans Bulletin, will find no difficulty in comprehending my meaning, and in perceiving that there is no contradiction. In section 50 I have shown the manner of deducing the law of attraction from the laws of Kepler, which I have stated to be independent of the theory of universal gravitation. I might have used a different phraseology, but I supposed that my meaning could not be mistaken. Those laws, as I stated in section 52, were independent of universal gravitation, only in being determined independently of it; and this is all that I intended to express. Kepler determined his laws independently of the law of universal attraction, since he knew nothing of that law. Kepler had established the truth of his laws by long observation; and by these laws, as I have shown (¶ 50, Bulletin), we can arrive at the fact, that every planet is retained in its orbit by an attractive force residing in the sun, and varying in intensity inversely as the square of the distance at which it acts.

29. I have shown, in the foregoing examination, that so far from the philosophy of Professor R. being true, every one of the propositions *peculiar* to his philosophy is either absolutely false, or, at least, unestablished. I am at a loss, then, to discover how we can receive the new philosophy, even if we were disposed. Every one of his thirteen propositions, except those common to the Newtonian philosophy, is either false or unestablished. While, then, I cannot but admire the zeal, in-

genuity, and great ability of my learned opponent, I am compelled to say, that his philosophy does not stand the test of rigid examination, nor present any superiority over that of Newton. It involves principles equally mysterious, and has not the advantage, even, of greater simplicity. We do not say that the Newtonian philosophy is absolutely true, in all its parts; but that, since it explains phenomena better than any other theory yet introduced, and since it involves no absurdity, we must adhere to it until something better is offered. What the labors of philosophers may reveal we know not. Doubtless great discoveries, and at no distant day, will be made; and we trust that the profound researches of Professor R. may result in the discovery of important physical truths, and give much honor to himself. The only fault we can find is, that he has engaged in the work of deduction rather too soon. The corner-stone of his philosophy is not yet laid, and the various parts of the edifice are composed of too frail materials, and often too slightly cemented together.

II.—*The Remedial Powers of the Sulphate of Quinine.* By Thomas D. Mitchell, M.D., Professor of Materia Medica and Therapeutics in the Medical Department of Transylvania University.

The first paper I wrote on the sulphate of quinine was published in the year 1825, in the *American Medical Review and Journal*, edited by Professors Eberle, McClellan, Smith, &c., a periodical that was not continued beyond the second volume. The article had then been a very short period in use, and it met with violent opposition. I had seldom given the Peruvian bark internally, and had relied exclusively on the preparations of arsenic, in all periodical affections, until experience assured me of the superior value of the sulphate of quinine.

In the paper referred to, several points were stated and defended, as follows:

1st: It cures the forms of disease in which bark has ordinarily been given, more speedily than any other medicine.

2d: It is more easily administered to all classes of patients than almost any other article employed in the treatment of fevers.

3d: It is a perfectly safe medicine, whatever be the age of the patient.

4th: It is less frequently followed by bad consequences than either bark or arsenic.

Lastly: It is cheaper than bark, and many other articles employed in the treatment of intermitting and remitting fevers.

The plan of treatment advocated for intermittents was that which has ever since been successful, viz., the exhibition of the medicine during the intermission, in such quantity as to prevent a return of paroxysm. To guard against relapses, I directed a grain or two of the sulphate to be taken every day, for five or six weeks, and this plan almost invariably succeeded.

I quote again from my paper of 1825: "But the opponents of the sulphate of quinine have proscribed its use, chiefly, in the *remitting* fevers that lately prevailed so extensively. And here I feel myself authorized to say, the medicine manifested its superior excellence more decisively, to my mind, than in the ordinary *intermittents.* So confident was I in the powers of the sulphate in the remittents (called the *bilious* remittents), that if a choice of fever cases had been presented to me, I would have taken all the remittents, in preference to the intermittents. The course of treatment was plain, easy and successful. If called in immediately, I cleansed the primæ viæ at once by calomel and ipecacuanha, and then, by means of spiritus mindereri, set up a free perspiration, and so obtained a palpable remission. Occasionally, it was needful to precede the cal. and ipecac. by a moderate bleeding, especially if there was obvious cerebral determination. All this seldom consumed over twenty-four hours, and the favorable opportunity then presenting, I gave the sulphate as rapidly as possible. In two hours, I generally had the patient under the influence of a sufficient quantity of the medicine to prevent a return of fever, or to postpone it for hours, or to abate its severity, so as to insure its subduction, wholly, by a second use of the sulphate. Thus have I seen many cases (I might safely speak of hundreds) which at first seemed to be alarming, completely checked in a few days, so that the patient was about in less than a week, while others similarly attacked, perhaps under the same roof, but badly treated, were confined to their chambers from six to eight or nine weeks. In short, I did not meet a single case of fever, during the last season, that gave me any serious trouble, excepting such as had been previously under the care of a neighbouring practitioner, who decried the sulphate as poisonous, and whose practice was inert, and consequently unsuccessful."

I quote again from the same paper: "In short, I hesitate not to affirm, that no state of the system whatever can present a reasonable objection to the use of the sulphate, unless it be decided febrile action. Nor am I willing to concede, that even febrile action is an insurmountable barrier to its administration. I was called to see a young man who had been extremely ill of a remittent for two or three days, under the care of a neighbouring physician. The case had been treated with spt. minder. and ant. wine, in the proportion of half a tea-spoonful of the former to five drops of the latter, every two hours, unaided by other means, excepting active purgation. I found the boy almost speechless, with hot dry skin, dark brown furred tongue, suffused eyes, great uneasiness, frequent, weak pulse, approximating the typhoid character. I gave him active sudorifics, followed by blisters to the wrists, and yet the skin was hot and dry. I then ordered ten grains of the sulphate in solution, to be given during the night. On the next morning I found him evidently better, and a repetition of the same medicine carried him beyond the point of danger."

I have made these extracts from my paper of 1825, to show how the sulphate was employed at that date; and also because I design to refer to them in the present article, which has been prepared at the request of the East Tennessee Medical Society. It is proper to add here, that such was my conviction of the mal-practice then prevalent, that I read a paper in the winter of 1825–'6, before the Philadelphia Medical Society, on *Artificial Typhus*, the chief aim of which was to prove, that the typhus or typhoid fevers, about whose fatality so much was then said, were, most commonly, the result of bad practice. I thought so then, and my opinion is about the same now, in reference to what is called *typhoid* fever, just as it was a few years since, in respect of that most *popular* disease called *congestive* fever, which seems to have passed away, in some unaccountable manner, and to have given place to a successor, so exactly like itself, that the difference is hardly greater than that between tweedle-dum and tweedle-dee. When I first came to Kentucky, in 1837, the chief mortality was from *congestive* fever (so called), which so closely resembled the remittents I had treated extensively and successfully with the sulphate of quinine, that for the life of me I could not perceive any real difference. Again and again did I urge the prompt use of the sulphate, after a suitable evacuation of the stomach and bowels, because I felt that *periodicity* was there, as I had seen it in hundreds of cases, and that the sulphate was its proper antidote. But the response was instinctive, "*that the time for quinine had not yet come;*" "the tongue and the stools call for more cathartic medicine;" and thus the patients often fared worse than if they had been a thousand miles from the touch of a doctor.

We venture to indulge a little, just here, in the way of episode, to inquire, what has become of the terribly fatal *congestive* fevers that once desolated this region? True, we are told, that those fevers were falsely named, and that they were, in fact, what are now called *typhoid* fever. Certainly, we never hear a physician speak of having a case of *congestive* fever on hand, now, in Kentucky; but, in every quarter, you meet with its *substitute*. The present typhoid fever, then, is precisely the same with the former congestive fever; and while the latter was very fatal, by reason of excessive purgation, and neglect to use the sulphate of quinine, the latter is equally mortal, because of the belief, that a do-nothing practice is essential to success.

I have said, that the congestive fevers from 1837 to 1842 were exact likenesses of badly treated remittents, as I first saw them over twenty years ago; and I now affirm, that the typhoid fevers which have technically supplanted the congestive, are, in no essential feature, different. There has been a change of name, and a change of practice, to some extent, but I am not able to discover any favorable change in the results: in other words, the mortality is about as great from typhoid fever, as it was, a few years ago, from congestive fever.

But, says one, what will you do with the congestive fevers of the South? I reply, that the best writers on the spot, and familiar with the disease, have proved conclusively, the common origin of congestive, intermittent and remittent fevers; and their practice shows, most lucidly, that the very worst forms of congestive fever are paroxysmal, and that periodicity is the common basis. Hence, although there be diversity in respect of the use of the cold dash and stimulants, there is a concurrence as to the semi-omnipotent energy of the sulphate of quinine, to prevent a return of paroxysm, and so to save the patient. Those who feel disposed to cavil at the doctrine of the common origin of such fevers, and who tell us that intermittents are never seen in England and Ireland, will do well to read the statements of Dr. O'Brien, in the

Dublin Medical Transactions, vol. iv., p. 545. "In the spring of 1817, intermittents (rarely seen for years) began to prevail in Dublin, pretty generally, whilst the ordinary continued fever (typhus) showed a strong tendency to assume the intermittent and remittent forms; and these three orders of fevers were converted into each other, in a new and extraordinary manner."

I care not a straw for any argument to be deduced from post-mortem examinations of fever cases. Experience and observation have proved, long since, that no dependence, for successful practice, can be placed on such a foundation; while both equally proclaim, that the true mode of meeting all fevers that are stamped with *periodicity*, whether more or less palpable, is by remedies, that so affect the organism, as to meet precisely that state of things, and to these remedies we have wisely given the title of *anti-periodics.*

With these preliminary remarks, I shall proceed to notice, more particularly, the uses of the sulphate of quinine, as a remedial agent; and, as my design is to present a practical paper, I wish to be understood as eschewing theory and speculation as far as practicable.

It is proposed to view the sulphate of quinine as a *tonic*, *anti-periodic*, and *sedative*, as we can present all that is desirable on the subject, most advantageously, under this arrangement.

On the *tonic* power of the sulphate, it is not needful to be at all prolix. We give it, in convalescence, as a remedy for pure debility, from any and every cause. We say it is a tonic, because it obviously augments the general tone and vigor of the system. It displays this feature, very frequently, in the stomach, before we perceive any decided constitutional manifestation. The appetite is revived, the digestive powers excited into more healthful action, and the work of assimilation is more complete and natural. It might be affirmed, that the remedy in these developments is truly an alterant, because it is gradually effecting most happy changes. We are aware, that every tonic is necessarily an alterant, and the persistent use is essential to the desired result. Hence, we give the sulphate, as a tonic, not for a few days, but for weeks, in small doses at first, and then gradually augmented. Ordinarily, we prescribe from half a grain to a grain per day, or oftener, according to circumstances; and slowly increasing, we give it, at length, in doses of two or three grains. This course of procedure is safe and successful, in ordinary cases.

As an *anti-periodic*, we hold the sulphate in much higher estimation. Does any one inquire, what we mean by anti-periodic? We have already eschewed theory and speculation, and shall reply by reference to palpable facts. Ague and fever is held to be a periodical disease, or a disease of periodicity. Nobody doubts this. A man has it one day, is well the next, and sick on the third day, and so the chain continues for months, unless you strike out a link or two, by remedial agency. On the well day, the man takes sulphate of quinine, and is cured. A thousand do the same, and all get well. The facts prove that periodicity was the grand basis of the morbid action, somehow or other; and they equally prove, that the sulphate of quinine has broken the morbid associations, and this is exactly all we mean by an *anti-periodic.* The term is applicable to all periodic diseases, that are curable by medical means. If any still inquire, what is the essence of periodicity, we reply, that

Hippocrates knew just as much about it as the wisest man now living, and that we have no valid reason to hope for more certain knowledge on the subject. The facts are all we know; they are readily tangible, and we have an undoubted right to use them for the best practical ends, whether we fully comprehend them, in all their bearings, or not.

The anti-periodic power of the sulphate is variously developed. Where the morbific agency is comparatively feeble, and the diseased associations not bound as by an iron band, small doses generally answer all desirable ends. But where the opposite state is present, the success of the medicine will depend very much on the size of the dose, and the quantity introduced into the system, in a given time. I say, *introduced into the system*, for the purpose of embracing the *endermic* use, as well as that by the mouth and rectum.

The following case furnished by my son, B. Rush Mitchell, M.D., now of St. Louis, Mo., is in point. It occurred in his practice at Madison, Ind., in the summer of 1845.

"Mr. S——, aged 35, returned from the South to Madison, and was attacked a week after, with a slight chilly sensation, succeeded in a few minutes by an intense and long-continued chill, little subsequent fever, and no perspiration. I did not arrive while the patient was in the paroxysm, but learned from those present, that throughout the whole of it there was scarcely any pulsation at the wrist. The countenance was cadaverous, tremors constant, a total lack of motive power, and the entire surface of an icy coldness.

"The aspect of the case was very much that of a dead or dying man. Calculating that it was a case of severe intermittent, I left twenty grains of the sulphate of quinine, to be taken in five-grain doses, every hour. I was summoned on the next day, with the tidings that the paroxysm had recurred with great severity; and finding the symptoms much more appalling, I became alarmed for his safety. I determined to remain all night, as the paroxysm began about 10 P.M.; and fearing another would be fatal, it was my fixed purpose to put the sulphate to the test. At 5 in the evening, I began with thirty-grain doses, and repeated them every half hour. As no untoward effect was manifest, these were continued until 9 P.M.; and now, having administered 240 grains, I thought if the patient was not safe from another fit, medicine could not save him. I discontinued the sulphate, and watched the case till the dreaded hour passed by. There was no recurrence of paroxysm, and I retired to rest. On the next day, the sulphate was renewed in small doses, and continued for three days, with the effect of complete restoration to health."

I regard the foregoing case as one of vast interest. In about *four hours*, *two hundred and forty* grains of the sulphate were administered, with no developments of bad symptoms, and with the effect of perfect recovery from a most alarming grade of what would be called, in many parts of the South, *congestive fever*. The case is a good illustration of some points made and forcibly expressed by the late Dr. Johnson, in his excellent work on the diseases of tropical climates. The patient, from residence in the South, had acquired a fixed predisposition to fever. The elements, thus far, were incorporated with his being, and all that was needful to display morbid phenomena, was a sufficient exciting cause. That was realized in a change of residence to a region decidedly more salubrious. Had the man remained in the South, it is more than probable he would have escaped the severe attack which nearly destroyed him.

The case teaches, moreover, that whenever the evidence of the paroxysm is vastly above the ordinary grade of intermittents, we can scarcely give too much of the sulphate during the intermission. To the same point, is the testimony of Dr. Flint, of Buffalo, and the army surgeons, as reported in the journals. So that, if we never know what is the essence of periodicity, we know what is far more important, viz., the practical fact, that sulphate of quinine is the grand anti-periodic, with which no other agent can be compared, but to be consigned to insignificance. That the action is, somehow or other, on the nervous system, through the stomach, we cannot doubt, although we have no positive knowledge in the premises.

We may also cite the case reported by Dr. Corbin, of Virginia, in Dunglison's Medical Intelligencer, of August, 1841: it has a very direct application.

The patient had an ague of eighteen months' standing, that had resisted various remedial efforts. Dr. C. applied a blister to each extremity, and one over the epigastrium, an hour before the expected paroxysm. To the denuded surfaces, an ointment was applied, made by rubbing five drachms of the sulphate of quinine with four ounces of simple cerate. The whole of this was spread on cloths, and laid on the blistered surfaces, eight hours after the blisters had been applied. These dressings were not disturbed until the parts had healed. The result was a perfect cure, and that, too, without a single inconvenience.

But let us turn our attention to the agency of this medicine, in the treatment of remittents, from the mildest grade up to the severest form, usually called yellow fever. The extracts from my paper of 1825 show the success of the sulphate in mild and more aggravated cases. In this region of country, I have put the same medicine to the test, most triumphantly, in the cases of medical pupils, who were brim-full of typhoid fever notions, and who sent for me to manage them, for that disease. Several cases of this kind have fallen under my care. The first salutation, on entering their chambers, was, "*typhoid fever.*" I laughed at the idea, and annihilated it, as far as I was able, treated the patients vigorously with the sulphate of quinine, and had them in the lecture rooms in a week. These very cases, I feel confident, could have been made as veritable typhoid fever specimens as are to be seen every where in the West.

As a palpable counterpart of the practice of many physicians, let me here refer to the Medico-Chirurgical Review for July, 1845, at p. 212. The writer treats of *malarious remittents.* "I found the little boy, W. J., with an ardently hot skin, face almost scarlet, sharp pulse, too rapid to be counted, and delirium; in fact, every symptom of fever, so intense as, in the minds of some of the pupils who were with me, to justify most active antiphlogistic treatment; and yet, the next morning, a remission having occurred, he bore the sulphate of quinine, in two-grain doses, and in five days was convalescent."

Again: "When the remission was not very distinctly marked, an emetic generally seemed to produce this effect; and then the sulphate was given, and invariably cured the disease."

In the New Orleans Medical Journal, vol. i., a writer (Dr. Beugnot) speaks of a *syncopal* remission, induced by copious bleeding in the onset of yellow fever, after which the sulphate of quinine was eminently successful.

I have long since maintained the doctrine of *cutting short* febrile diseases, and I am daily more convinced of its truth. My own experience in the use of the sulphate of quinine in remittents, as reported in 1825, contrasted with the eight weeks' treatment of those who adopted an inefficient course, is amply to the point, to convince any reflecting and unprejudiced man. The case reported by my son, in which 240 grains of the sulphate were given, successfully, in four hours, is evidence wholly irresistible. But for that rigorous treatment, the man would probably have died in the next paroxysm. The means adopted *cut short* the disease, as certainly as you snap a pipe-stem into two pieces when you break it in the centre. The very fatal yellow fever kills ordinarily in three or four days; but if, by proper treatment in the first twenty-four hours, I save the patient, what do I effect, but the *sudden arrest* of the febrile attack? If the man is saved at all, it is, for the most part, by what is done in the first twenty-four hours.

Well, but, says one, how would you cure a case of typhoid fever, even conceding your views of its remittent nature, after the lesions of the small intestines were established, or, in other words, dothinenteritis set up? Would the sulphate of quinine be successful then, and if so, how? To this I respond, that the query embodies an assumption which I do not admit, because it is absolutely without proof. I know there are some men who, on finding such lesions in the small bowels of a subject in a dissecting-room, of whose history they knew not an iota, would swear, that typhoid fever had killed the patient. But I am not quite so highly enlightened as these gentlemen, and do not pretend to see quite so far: yet I do honestly believe, that the bowels may be the subject of exactly such lesions as are made, by some, infallible proofs of typhoid fever, where not a symptom of that disease, as recognized by its special advocates, had existence. I have said, that I had seen precisely what is now called typhoid fever more than twenty years ago, in the hands of men who had very badly managed the prevalent remittents, and I say so now. It is my abiding conviction, from experience and observation, that nine-tenths of the cases called typhoid fever were curable in the first week, by the sulphate of quinine, properly administered. If the favorable period for its administration be wasted, as I know it often is, and the patient harassed and worn out, either by irritating cathartics, or by a do-nothing policy that allows the secretions from the bowels to become depraved, and inflammation and ulceration to follow, thus involving the patient in a sympathetic fever, that might have been prevented by an early resort to proper means, I have only to say, that I cannot thank any man for the honor of calling me to his aid, as consulting physician in such a case. For the very same reason, I have often refused to take such cases when then the attending physician had been discharged, or had discontinued his visits.

It would be no difficult task to cite many authorities, in proof of the efficacy of the sulphate of quinine, in what was called typhoid fever, by virtue of its anti-periodic power. The cases were truly remittents, and the remedy succeeded because it annihilated the periodicity. Doctor Waton, of Montpelier, in France, and Dr. O'Brien, of Dublin, who speaks of Typhus instead of Typhoid, in this relation, are in point. Dr. Kennedy, of Ohio, whose letter was republished in the Western Lancet, of August last, is equally forcible, and the whole, with many other proofs, equally cogent, are corroborated by my own experience.

And then, let us bear in mind what mischief has been done, by dissociating the idea of typhoid fever with the doctrine of remission, and abandoning that disease to the expectant plan of treatment. The names of physicians could be given by the score, who can see nothing but typhoid fever in any case, if it be a febrile attack, in any sense; and, as a consequence, they overlook not merely the more obscure remissions, but even palpable intermissions, and treat, not unfrequently, with blue mass or nothing, cases that imperiously demand the interposition of the sulphate of quinine. To such gentlemen, let me say, *that the fevers of the United States are essentially periodical; and, if you would treat them successfully, the doctrine of periodicity must never be forgotten; no, not for an hour.*

A few weeks since, a very intelligent graduate of Transylvania, who has been engaged in active business some eight years, detailed to me several cases, fully confirmatory of all that is stated above. He assured me, that in his region, the physicians can see nothing but typhoid fever, go where they may. Their practice is actively cathartic for the first three or four days, and then the cases are left pretty much to nature. He avers, that under this delusion, cases of pure intermission, calling loudly for the sulphate of quinine, are often mismanaged, and that remittents are constantly treated on the same erroneous principles. The proper time for the salutary action of the sulphate of quinine is thus lost, the bowels having been needlessly irritated by repeated cathartics, the strength wasted, and no recuperative means attempted, the patient sinks, either from the debility induced, or from a sympathetic fever set up by lesions of the small bowels, it may be, which, if seen after death, are put down as the cause of all the mischief, when, in truth, they are frequently the legitimate fruits of bad management, and had no agency in the production of the original fever.

I am not unaware of the argument in favor of the totally diverse character of what is called typhoid fever, typhus and remitting fever, based on alleged symptomatic and post-mortem differences. But I should like to be told, what all these diversities amount to, as an argument for dissimilarity in essential nature, that will not, with greater force, operate to make the mildest form of scarlatina, the malignant variety, and that in which every stereotyped sign of the disease is absent, separate and distinct diseases, rather than mere varieties of scarlet fever. There are practitioners in Lexington, who hold to the doctrine of specific difference between typhoid, typhus and remitting fevers, who, a few years since, pronounced several cases to be real scarlatina, in which there was neither cutaneous nor anginose evidence to justify such a decision, and their opinion rested on the naked fact, that scarlet fever had recently been prevalent. Nor were those physicians singular in this matter, for the very same thing can be found in some of our standard works.

We feel as confident, as of any other position, that all fevers, not characterized by obvious local lesion, are mere varieties; and that *in* all, periodicity can be detected, and is the most tangible basis for successful remedial efforts. We mean just what we say, and are content to take *pure typhus*, as a case in point. We know that, by many, this is regarded as a *bona fide*, *continued fever*, in which, of course, there are no remissions, and consequently, no periodicity. But this is precisely what we deny; and although we have seen a good deal of this true typhus, in early life, we prefer, in order to escape cavil, to rely on authority that cannot be called in question.

The *Dublin Medical Transactions* contain the fullest and best history of *typhus* fever that has ever been recorded, and we rely on its statements, on the present occasion. In the fourth volume, we find an excellent paper by Dr. O'Brien, from which we shall make some extracts. "In two cases," says the Doctor, "the effect of the sulphate of quinine was decisive, and the recovery as rapid as in intermittent fever. In three cases, the result was less rapid, but not less effectual. The patients improved gradually, and ultimately recovered, without relapse." (P. 370.) "The rapid recovery effected by the medicine (the sulphate of quinine) in both cases, excited a suspicion, that the fever was of a *remittent* type." (P. 374.)

Let it be borne in mind, that Dr. O'Brien is writing the history and treatment of *Irish typhus*, recognized as such by all his professional brethren, and by many attributed to contagion. Indeed, the faculty there seem, for the most part, to be wholly ignorant of intermittents or remittents, excepting as matters of history.

Again: "There is reason to believe, that the important class of *remittents* has been too much overlooked and disregarded in this country, and, by a loose and general classification, *confounded with typhus*." (P. 376.) "With respect to the stage of fever most appropriate for commencing the sulphate of quinine, the period of direct debility or collapse appeared to me the most suitable." (P. 377.) It is quite easy to understand, if we reflect on the almost total want of familiarity with remittents in practice, that the *period of direct debility* spoken of, was the period of remission.

Can it be matter of astonishment, that the physicians of America should regard typhus fever as a positively *continued* febrile affection, with *no remission*, when the medical men of Ireland, who have seen so much more of the disease than any others of the profession, have almost uniformly fallen into the same error? We are too much the slaves of fashion in these matters; and in the face of all the fashion, here and elsewhere, I affirm that there is not and never was such a thing as *absolutely continued fever*, *without remission*, for five days, in unbroken succession. It is not in the nature of the human economy, nor of disease, to have such morbid persistence. Remission, more or less perfect, is a law of nature, and may be detected, with care, in what are called typhoid and typhus fevers, just as certainly as in the mildest remittent. But the grand error lies, in mistaking the meaning and nature of remission, and in looking for positive *intermission* in its stead, as *the* basis of the right use of the sulphate of quinine. Here is the rock, on which hundreds of practitioners, otherwise well informed, are daily making shipwreck. The physician perceives no obvious remission, and he takes the word of the nurse, who perhaps was asleep more than half the night, as proof that there was no remission at all. Such is the basis of professional opinion, and consequently of professional blunders. And even though the physician remained with his patient during three whole days and nights, if he had no just views of the difference between remission and intermission, (and many have not), his apparent care would result in no practical advantage.

These doctrines could be sustained by a vast amount of testimony, if it were needful to swell this paper by so doing; but we presume enough has been said already. The subject is a favorite one, and we have been in the habit of teaching thus for years, and shall continue to do so, until new light shall discover our error.

A few remarks on the sulphate of quinine, as a *sedative*, and we shall dismiss the whole subject, for the present.

We have not met with facts, in our own experience, that would positively sustain the sedative action of the remedy. Some intelligent practitioners think it acts thus, even when given in large doses, in the stage of high febrile excitement; they suppose it abates the excitement, and thus controls the morbid action. Others regard it as a sedative, when it succeeds in allaying high irritability of the stomach, as seen in ordinary remittents, and in the most malignant form, known as yellow fever. That it has acted most happily in such circumstances, there can be no doubt; and it would seem to operate then either as a counter-irritant, or contra-stimulant, or sedative. Nausea and vomiting have been arrested by it, as occurring in the more ordinary remittents, and even the black vomit of yellow fever has yielded to its potent sway.

In reference to the sedative agency of the sulphate of quinine in yellow fever, I am inclined to believe, that there is co-operative, at the same moment, the anti-periodic power. In a paper, *On the Yellow Fever on board the Volage*, contained in the New York reprint of the London Lancet, for March, 1846, we find the following remarks:—"Some of the German and Spanish physicians at Havana give the sulphate of quinine at a very early period of the disease, as soon, in short, as they observe any thing like a *remission*, and they speak favorably of its operation when thus exhibited." And again: "The sulphate of quinine was freely given, and with a success so marked, that I feel justified in recommending it as an invaluable and essential adjunct." Nor is it at all difficult to reconcile the declarations of those who believe in the power of the sulphate to allay the gastric irritability of yellow fever, with the statements of others who laud the value of the remedy, when given in the earliest remission perceptible in that dreadful malady. We have only to suppose, that the sedative and anti-periodic powers co-operate, to bring about the happy results.

The facts cited by Professor Harrison, of New Orleans, in his paper on yellow fever, published in the medical journal of that city, in November last, are supposed by some, to prove the sedative operation of the sulphate. I am not able to come to a like conclusion. Certainly, the experiments of Dr. Hunt, as detailed in that paper, do not establish a sedative action. They were performed on convalescents, who, of course, were debilitated. The reduction of pulse, under the use of the sulphate, was, no doubt, accompanied by a corresponding augmentation of volume, and was precisely what we usually observe to follow the administration of tonics. But the cases of yellow fever, noticed by Professor H., in which the sulphate was given at the onset of the fever, presented a widely different state of the system; and although, after a general or local bleeding had induced a remission, the sulphate cut short the fever most astonishingly, I do not regard the facts as developing a sedative, but rather an anti-periodic agency. "The breaking up of the whole condition upon which the morbid actions depend," (the language employed by this writer), is an effect so like the usual action of the sulphate of quinine, as an anti-periodic, that I can refer it to no other power. It is true, the concentration of the poison in yellow fever, changes the intensity of the phenomena, or exalts it, so as to disguise, to a certain extent, the ordinary operation of this heroic medicine.

As a specimen of the practice in these cases, we quote from a report of Dr. Mackie. "A stout young man, aged 28, was seized with yellow fever, at 1 P. M. At 6 P. M. of the same day, he was under treatment. Ten cups were applied to the epigastrium, thirty grains of the sulphate of quinine were directed to be taken by the mouth, immediately after the cupping, and forty grains to be given by injection." Says Dr. Harrison, "He was cupped before I saw him, but had not taken the sulphate. His pulse was 120, full and strong; he had great heat of skin, great pains in the head, back and lower extremities, tongue a little furred, eyes heavy and a little injected, great restlessness on account of the pains. I saw him next morning, between 6 and 7 o'clock. He was perfectly free of pain; pulse 84, skin cool, and every vestige of disease gone. From prudential reasons, he was kept in the hospital four or five days longer, but there was no return of disease."

This and similar cases give some reason for assigning to the sulphate a sedative action; but I feel more confident by far, that the secret of arrest of the fever is to be found in the anti-periodic agency.

I cannot but think that Surgeon McCormick, of the U. S. army, is in error, in asserting, that excitants are never given when the system is highly excited, and that, *therefore*, the sulphate of quinine, given by him in that condition of the system, must necessarily have acted as a sedative. The premises are not so, and of course the inference cannot be sustained. Opium is conceded to be an excitant, in doses of one to three grains, and has thus been given, a long time, in the hot stage of intermittents, and is now employed by very respectable physicians, in inflammatory diseases. I do not say, positively, that the sulphate may not act as a sedative; but I am satisfied, that the argument of Surgeon McC., as employed in the New Orleans Medical Journal, does not settle the question. I have never exhibited the remedy in the same range of inflammatory diseases, it may be, with that gentleman, and therefore cannot decide the point. If he has not been mistaken, there would seem to be ground for conceding the sedative quality contended for. A perusal of the essay, however, will discover an acknowledgment of the need of the lancet, in some cases, as the *first* means, immediately after which the sulphate was employed in 30 or 40-grain doses. Now, the lancet is, unquestionably, a powerful sedative; but why resort to it at all, if the sulphate possess the very same power, to such an omnipotent degree, as is claimed for it? The region where the practice was carried out was highly malarious; and we incline to the belief, that the lancet induced just so much of remission, in the given cases, as secured more effectually the complete anti-periodic action of the sulphate. We do not doubt the correctness of the practice, and differ only as to the explanation.

In reference to the question, yet mooted by some, of large doses of the sulphate of quinine, it is due to truth, as developed by an immense amount of experience, to declare emphatically, that it is not only a safe remedy, but decidedly more often successful than when administered, as originally, in very small doses. Let it be borne in mind, however, that the force of the medicine, when properly given in very large doses, is spent on the morbid action to be subdued, in the absence of which, the same dose would, most probably, be injurious.

I may add, in reference to intermittents and remittents, that their prompt arrest, by the liberal use of the sulphate of quinine, is rarely, if ever, followed by dropsy; an event often seen, when those diseases were treated with feeble doses of bark and snake-root.

III.—*Practical Remarks on the Epidemic of Yellow Fever which prevailed at Opelousas in the years* 1837, '39, and '42. By T. A. COOKE, M.D.

Having read many interesting essays in the Medical and Surgical Journal on the subject of Yellow Fever, I have thought that some account of the epidemics of that disease, which prevailed in the town of Opelousas in the years of 1837, '39, and '42, might not be destitute of interest to those, at least, who seek to explore its many mysterious phenomena.

But before entering on the subject, permit me to congratulate my country *confrères* particularly, on the establishment of your journal in the city of New Orleans. Of its merits, the very important matter which it has been the medium of conveying abroad, affords a flattering proof; and of its success, none who are acquainted with the zeal, energy, and talents of its proprietors can for a moment doubt. Its establishment constitutes an era in the medical profession, in the South; and affords an opportunity to the profession of the city, of liquidating those just claims which they owe to the physicians of the country. Many able contributions have already been made to the pages of your journal, —articles which afford satisfactory proof that only an opportunity, with fit incentives to exertion, is wanting, to elicit from the resident physicians of the city, whether native or foreign, evidences of acquirement and capacity, commensurate with the vast requirements which so great a city, presenting, as it does, diseases more violent and varied than any other in the world, exacts from the votaries of the medical profession. Enjoying, as they do, the advantages of union, of a constant intercommunication of ideas, and of hospital or clinical studies, it not only becomes them, as guardians of the honor of the profession, to watch over it and promote its purity, by discouraging that fatal charlatanry too prevalent over the land, but also, by continued investigation and experiments, to scatter over the land the results of their labors, and the fruits of their experience.

The many interesting communications from the country, highly creditable to their authors, afford a flattering promise of what may in future be expected; and surely there is no want of subjects which merit the attention of physicians, in the complaints of an endemic and epidemic nature to which the country is subject. It is in the South, particularly, that we witness the immense influence of local causes in modifying the character—the form, and treatment of disease. No one is ignorant of the fact, that there is a diversity in the effects of the same remedy in different years, in the fevers of the country: it is confessed, that yellow fever assumes different forms, and that the effect of remedies is modified thereby in different years. When we read, in the histories of yellow fever, and in the description of particular epidemics, opposite modes of treatment recommended, (at least, so to be considered, according to the general explanations of the modus operandi of medicines),—remedies by some extolled as the safest, if not indispensable, and condemned by others as either uncertain or dangerous, we must conclude that such diversity of results arises from the modifications of the fever in the different circumstances under which it has been observed; and if such be the truth, another conclusion is established, which is, that in every epidemic, the first duty of the physician is to modify his treatment according to the form of the disease,—to adapt his remedies to what is called the type of the fever. In a subsequent part of this paper, this important subject may, perhaps, be more particularly developed.

I proceed now, in an irregular manner, to make some remarks on some of the facts or phenomena of yellow fever, and to consider some of the remedies generally regarded as the most important in the treatment of the affection; and I wish it particularly understood, that these remarks will relate exclusively to three epidemics of a formidable character which it was my fortune to witness in the town of Opelousas, in the years '37, '39, and '42. Any comments which I may be led to make, will have reference to these epidemics, being suggested by the facts which were witnessed. The symptoms of the fever on the three occasions alluded to were much the same. I am of opinion, that all three presented very nearly the same form or type. In 1837 and 1839, the attack came on suddenly, rarely with premonitory symptoms, and, in a large majority of cases, between the hours of midnight and daybreak. As regards severity, no difference was observable, as a general rule, between the cases commencing at night or during the hours of the day.

As regards duration, the fevers of '37 and '39 were the same. The cases which formed an exception to those of confirmed convalescence, or of a fatal termination before the completion of the fifth day, were few. But the disease of '42 was frequently protracted to twice the duration of the two previous fevers, and presented, in some cases, pictures of human suffering, and of the most appalling symptoms which ever fell within the observation of physicians.

The first fact which appears to me worthy of mention, and which no one will deny, is, that in all cases drastic medicines were fatal. Too many instances, unfortunately, of fatal results, occurring in individuals both old and young, of both robust and feeble constitution, attributable clearly to drastic purgatives which had been taken by the patients themselves, may be cited. The general effect of the operation of such medicines was a great reduction of the fever, so far, at least, as a full and strong pulse, and hot skin, are evidences of fever; but, at the same time that the pulse diminished in force it increased in frequency, and was accompanied by a cool skin, with clammy and offensive perspiration; the tongue would become redder, more pointed, and in some cases would clean off; the stomach, the seat of great irritation and nausea, accompanied with belching of gas; very frequently there ensued an impossibility of discharging by stool, in consequence of that singular and mysterious state of the colon sometimes observed in yellow fever, which consists in its contracting on itself, in some cases to the extent of obliterating its cavity, and thus rendering the passage of matter through it utterly impossible; and then comes on, at an early period after this purging, a coldness of the end of the nose, fingers and toes; an indescribable sensation of fullness in the epigastric region, with constant nausea, would continue to increase until about the fourth or fifth day, when black vomit would ensue, and death soon close the scene. Notwithstanding the excessive purging which such medicines never failed to produce, and which sometimes resulted from the administration of a single dose of oil, that sinking-in of the abdomen, seen frequently in our autumnal fevers, was never witnessed, the bowels always feeling full and doughy to the touch.

The fact alluded to, viz., the fatal effects of drastic purgatives, is so constantly seen, as not to admit of an exception in confirmation of its truth; and it appears to me, of itself, sufficient to show, that between yellow and bilious fevers there exists no affinity or relationship whatsoever; and yet, by more than one writer, the one is regarded but as a higher grade of the other.

All who are conversant with autumnal bilious fevers in the South know the fact, that drastic purgatives are commonly resorted to, and that speedy cures frequently result from the perturbating effects of such remedies. In some regions of our country, the famous preparation of Le Roy, remarkable for its violence of action, supersedes all other medicines in the treatment of our common fevers. Emetics also, condemned in yellow fever, are liberally employed, by experienced physicians, in many forms of our bilious fevers.

These facts go to show, that the cause of the one fever is different from that of the other; that the lesions of tissue are different, and that the organs are differently affected by the causes in these different affections.

I now proceed to notice a circumstance, which, though it may be said to have been universal in the epidemics of Opelousas, has not been observed generally, and never to a similar extent, elsewhere, that I am aware of—I mean, an arrest of the secretion of bile. Of all the facts or symptoms of yellow fever, this appeared the most fixed—the only never-failing one. In the only two dissections which were made (and it is to be regretted that the great prejudices in the country render autopsies almost impossible), no bile was found in the intestines; and in the gall bladder, only thick mucous, tinged with bile, was found; and it was also universally observed, that simultaneously with a favorable change in the disease, there was a return of the biliary secretion, shown by the presence of bile in the evacuations. As far as I have conversed with the physicians of this place, the fact stated has been confirmed, with one exception, in which there was a vomiting of matter mixed with bile during the rigor or at the onset of the attack. I have never seen bilious vomiting in cases of yellow fever, from the beginning to their termination. So constant was this condition of the liver on the epidemics of Opelousas, that some of my medical friends, as well as myself, considered it pathognomonic of the fever, and would hardly have been disposed to give its name to any case which presented a free secretion of bile: we regarded it, in fact, as an essential element in the nature of the fever. I am well aware, that, in many accounts of particular epidemics of yellow fever, as well as in professed treatises on the subject, copious secretions of bile has been cited as a common, if not a constant condition, nor do I wish to be considered as denying its frequency; I desire only to specify a fact as appertaining to the epidemics of Opelousas.

The secretion of bile was a certain sign of returning health. In the fatal cases, it never re-appeared: hence, the most painful anxiety was experienced until the presence of bile in the evacuations showed the restoration of the hepatic function.

No account has ever been published of the epidemics of Opelousas. During the wide-prevailing maladies of the years 1837 and 1839, the physicians were too incessantly occupied with their cases, even to allow them to take notes; and I presume this circumstance must have prevented several of my esteemed colleagues from presenting for publication an account of the disease. For my part, relying, as I do, upon some disjointed memoranda taken shortly after each epidemic, aided by a memory too vividly impressed, not to be faithfully retentive of the leading facts of the disease, I offer no apology for the absence of all system or method in the previous or following pages.

Each of the epidemics was preceded by a fatal case of yellow fever in individuals who had contracted the disease in New Orleans.

In a population, white and black, of about 1400, there occurred, in 1837, about 250 cases; and in 1839, the disease spared none who remained in town, except only those who had previously had it: amongst these, embracing very many who, two years before, had been attacked, and also others, who had, a great many years ago, contracted the disease in different parts of the world, there was not a single case of it. It attacked white and black—old and young. Its duration was from four to seven days. The exceptions, to a favorable or unfavorable change about the fifth day, rapidly progressing towards health or death, were very few. Black vomit, in nearly every case, was thrown up before death; and there was no recovery after its appearance. In one instance, though none had been discharged before death, the stomach and small intestines, on a post-mortem examination, were found full of it, and in many instances it flowed after death, from the mouth and nose, though none had been ejected before. In one instance, I observed passive hœmorrhages from the mouth, anus, nose and eyes, flowing in immense quantities, and which nothing appeared capable of arresting. Such hœmorrhages were common towards the end of the disease, and, though not a fatal, was considered a very unfavorable symptom. The disease sometimes commenced with a chill or rigor, during which, some patients manifested a peculiar perversion of the mind, which passed off as the fever came on. Delirium occasionally was seen, but not often before the fourth or fifth day, unless that derangement above mentioned as occurring during a chill, deserve the name. There never was a recurrence of the chill; indeed, I could not satisfy myself of the existence of decided remissions. The pulse, generally strong, full, and very frequent, would gradually sink in force and strength. In some cases, it did not exceed eighty-five or ninety strokes in the minute. The countenance bore an expression peculiar to the disease, indicative of great anxiety, of physical *malaise*, and as the disease advanced, sometimes, of neither of the above conditions, but rather of what may be termed a reckless "*insouciance*." All complained, in the beginning, of the most distressing sensations in the head, eyes, back, and limbs;—sensations which were undoubtedly painful in many instances, but in all very different from those aches or severe pains so often seen in autumnal fevers.

It is unnecessary, in an article of this nature, intended simply as a medium of communicating some of the most striking facts of the epidemics of which it treats, to describe all the symptoms of yellow fever, in its various stages and modifications; in the different works on the subject, they will be found amply pourtrayed: I will simply remark, that there is something difficult to describe, but visible to the experienced eye, in the general physiognomy of the disease, more expressive of the affection, than the *tout ensemble* of all the symptoms, black vomit only excepted. It is also frequently observed, that, as death approaches, the timid and the resolute, old and young, will evince a strange indifference to death. I do not recollect having seen manifested sentiments of grief, or feelings of anguish, at the idea of death, and of leaving behind family and friends; and all this, apparently connected with a conservation of the mental faculties, deserves to be considered one of the most inexplicable phenomena of the disease.

Relapses were not frequent: I can recall to mind but two, one of which proved fatal. In some cases, after confirmed convalescence, jaundice appeared, but yielded readily to diet, sanatives, and preparation of iodine. My remarks hitherto relate almost entirely to the attacks of 1837 and 1839. That of 1842 commenced late in the fall, and was terribly fatal. Its ravages were not extensive, undoubtedly from want of fuel. Unfortunately, medical aid was rarely called in before it was too late to effect any good. Of some twenty-five cases which occurred, one-third, at least, died. Notwithstanding an immense number of black people took the disease, particularly in '37 and '39, and in every case apparently in its severest form, I am aware of but one death amongst them. In 1837, Dr. Mores Settel, who had practised the profession in this place for some twenty-five or thirty years, with great credit, fell a victim to the fever in its worst form. He alone, of the physicians, many of whom were subsequently attacked, died of the disease; and if any circumstance could increase the universal regret which his death caused, it was the courage and philanthropy which he manifested in resuming the practice of the profession at a period subjecting him to constant fatigue, and full of peril to life itself.

In the treatment of yellow fever, calomel, particularly in former times, has been advocated as a most important agent; nor, at the present day, are there wanting those who place the greatest trust in its remedial effects in the disease. Having already said, that, according to the general observation of the physicians, in the epidemics of Opelousas, an arrest of biliary secretion constituted one of the most characteristic phenomena of the disease, it may be inferred that this remedy early attracted particular attention, and that its virtues were fully and fairly tested. To the question, whether it was useful or not, I wish it understood that I offer the results of my own experience, when I answer, that however employed, whether in small, moderate, or large doses, repeated or not, instead of being useful, it appeared to me detrimental. In the epidemic of 1837, observing the absence of bile, calomel was administered in every possible manner; some patients were almost wrapped up in mercurial ointment; and I must aver, that I never saw a recovery under such treatment. Unquestionably, not only in 1837, but in the subsequent epidemics, a great many patients recovered to whom calomel had been, more or less, freely given; such results occurred in my own practice: but from general experience, my belief is fixed, that such recoveries, instead of depending on, were rather retarded by, the mercurials. Both in 1839 and '42, I continued occasionally to give, in moderate doses, mercury, either in the form of calomel or blue mass, in the hope of deriving benefit from it; but it invariably had the effect of increasing the nausea, and of causing secretion of gas in the stomach, and general restlessness. In 1842, I am satisfied that I was very near losing a very interesting patient, a girl of about eleven years, from the administration of simply five grains of blue mass, about the beginning of the third day. The medicine was suggested as one suited to move gently the bowels; but the result was, most distressing nausea, a constant retching or effort to throw off air, a hot dry skin, in place of a pleasant and moist one, and sensations of general distress and constant restlessness. These distressing symptoms luckily yielded to repeated mustard pediluvia, emollient cataplasms over the bowels, and to saline mixtures, combined with anodynes and alkalies: and now I repeat my conviction, the result of a long and painful observation, that in the treatment of the yellow fever of Opelousas, I never saw decidedly beneficial effects from the employment of mercury in any form, mode or manner — on the contrary, invariably, most marked injurious effects. I will not now attempt an explanation of the entire impotency of the medicine, in favorably affecting the functions of the liver. I had conceived that the prostration of the functions of this organ, in the epidemics of Opelousas, constituted the most essential element of the disease; that the poison, or first efficient cause, exerted a particularly malign influence over the liver, and by arresting its functions, and thereby blocking up the most important outlet for the elimination of the morbid elements of the blood, tended on every point of view to aggravate the violence of the disease. In warm climates, functional derangement of the liver is, in my opinion, more frequently observed than a deranged condition of any other organ. In common bilious fevers, the secretion of bile is generally excessive. In the congestive and other dangerous forms of these fevers, it will be observed, as a general rule, that there will be a great diminution, if not an entire arrest of biliary secretion.

Mercury, emetics, and drastic purgatives being condemned, what remedies were most to be relied on? According to the general experience of our physicians, general and local blood-letting deserved to be ranked as the most important of all the remedies.

In 1837, I was called about one o'clock in the morning, to see a patient suddenly seized with all the most violent symptoms of the disease. At my arrival, the fever was succeeding a moderate rigor: he complained of most distressing feelings in the head, back and extremities; his eyes were red, and burning hot; though a large man, of sanguineous tamperament and strong constitution, his pulse was small and frequent. He could not control himself, in the agony of his feeling. About thirty ounces of blood were drawn from his arm, without mitigating very visibly his acute suffering; and a spoonful of castor oil was administered. Next morning I called, and, much to my surprise, found him nearly entirely relieved from his suffering—with slight fever and moist skin. Perceiving an immense puddle of blood under the bed, I ascertained that, in his restlessness, the bandage had slipped from his arm, and that he must have lost, in addition to the blood I had drawn from him, at least a gallon more. Not an untoward symptom interfered with the rapid recovery of this man: mild saline cathartics, with occasionally an anodyne, and a light farinaceous diet, were prescribed. He was a carpenter, and lived with some eight or ten others in the same house, several of whom, with their employer, died with black vomit, some previously, some subsequently to his attack.

This case, as it may be supposed, made on me a strong impression: by it I was much influenced, ever afterwards, in the treatment of yellow fever. Previously esteeming blood-letting very highly, it necessarily tended to strengthen my opinion of its efficacy in our epidemics. In 1839, it proved indispensable. Its salutary effects, when freely employed, were visible to the eye. The quantity drawn was regulated by the constitution of the patient, and relief from suffering; sometimes repeated three or four times, and amounting to eighty or ninety ounces in the first twenty-four hours. Very frequently, from bleeding at the nose in the first stages of the fever, no inconsiderable quantity of blood was lost. These active hœmorrhages from the nose were not considered unfavorable. Though venesection was resorted to sometimes as late as the third day, its beneficial effects appeared the more manifest the earlier it was employed. Blood drawn from the arm never exhibited the buffy coat. Besides, wet cups were freely applied to the temples, behind the ears, and along the spinal column. The mildest purges, such as sweet oil, a small dose of castor oil, or manna and rhubarb, with the addition of magnesia or carbonate of soda, were administered at the start. A full dose of castor oil very frequently produced hypercatharsis, and when such a result ensued, it was a most unfavorable sign. The sup. carb. of soda, with the addition of a small quantity of the muriate of soda and nit. potass, freely diluted, was administered regularly in the form of a drink. As an anodyne, the extract of lettuce, when good, acted often like a charm. So soon as the restlessness and pains had been mitigated by mustard foot-baths, venesection, and purging, a full dose of lactucarium was administered, and always with the happiest effects. Besides myself, several of my colleagues, in the epidemic of 1839, used this remedy, to the almost entire exclusion of preparations of opium.

Our experience in Opelousas fully confirms the opinion of the Honorable Ashbel Smith,* formerly of Galveston, as regards the superiority of an infusion of manna and rhubarb, as a purgative in yellow fever. The addition of some soda I considered an improvement. During the course of the disease, it was necessary occasionally to repeat the laxatives. Mustard foot-baths certainly are a most important remedy: they promote perspiration, tend to relieve the head, and to tranquilize the nervous system. A uniform temperature should be observed in the sick room: the patient should not be permitted to leave his bed under the operation of medicine. Measures which tend to promote and preserve constant action of the skin—perfect tranquility—avoidance of every thing calculated to excite or disturb the mind,—such circumstances should be strictly attended to.

Notwithstanding the violent and frequently fatal character of yellow fever, it seems to demand from the Materia Medica fewer medicines than many less violent affections. On one occasion the chloride of soda was employed by me with the happiest effects. I saw the patient on the fifth day for the first time. He had been previously bled, and purged with mercurials: he seemed to be bordering on a state of putridity or mortification; passive hœmorrhages from the mouth and bowels were copious: he laid in a comatose condition. As an experiment, I recommended the free exhibition of the chloride of soda by the mouth, and by injection. He got well, and I could not help attributing his recovery to the medicine employed As regards quinine, it was employed sparingly, and never in the heroic doses so much vaunted in late years. The absence of every thing like periodicity, or intermittence of symptoms, presented no indications for the use of the medicine. Believing it possessed of a great influence in modifying the nervous system, and, through it, of effecting the secretory organs, I have no doubt of its efficacy, particularly in the congestive or typhoid forms of the disease. As regards acidulous drinks, they have universally appeared to me to do harm in all fevers of the South. Being pleasant to the taste, they produce, for the moment, agreeable feelings, but subsequently increased restlessness, frequently nausea and heat of skin supervene. Warm mucilaginous baths were sometimes resorted to, but always under circumstances adverse or unfavorable to their good effects. In some circumstances, no doubt, they are very efficacious in promoting perspiration, and therefore should not be neglected as a remedy in the disease. Of all maladies, none is more likely to prove fatal without the intervention of medical aid, than yellow fever; and there is a universal concurrence of opinion, that success depends upon the early employment of remedies. A few hours' delay will frequently suffice to establish so completely the morbid actions throughout the whole system, as to render utterly inefficient the remedies which may be given. Then will generally be witnessed great derangement of all the secretory organs; vitiated contents of the bowels, at first of greenish, then of a blackish, earthy appearance, soon terminating in a thin sanguineous fluid, containing sometimes matter like coffee grounds, sometimes flakes of mucus. As the disease made

* See Dr. Smith's Treatise on the Epidemic of Galveston in 1839, containing many excellent views of the nature and pathology of yellow fever.

its progress to a fatal termination, the urinary secretion was invariably arrested; and with regard to the liver, the fact of the disappearance of bile in all the fatal cases has been already more than once cited. To superficial observers, the appearance of the patient, a short time before death, frequently inspired the most sanguine hopes; there would then ensue an entire freedom from restlessness, from nausea, from complaints of all sorts; the conversation of the patient would appear to be rational, and he would apparently enjoy the most refreshing sleep. The physician would be hastily sent for; and finding the pulse very weak—sometimes slow—the extremities cold—deep sighing, all that he could do would be to disabuse friends of their fallacious hopes, and prepare them for a speedy termination of the case. In every case that I saw after death, the skin was jaundiced—the depending parts of the body always became black or marbled. The yellow color of the skin rarely occurred before, but rapidly after death.

The symptoms, the general duration, the presumed cause, the effects of remedies, the condition of the tissues, and of the blood, as shown before and after death—all seem to unite in establishing the character of yellow fever as a pyrexia. Inflammations may supervene during the progress of the disease, and tend to hasten, or may of themselves produce, death. Any sensible person who has ever had this fever, can never be persuaded that it is symptomatic of inflammation, and that it should be classed among the phlegmasiæ. It is true, that in some of its forms—and in the epidemics of Opelousas such was the case—the term inflammatory is applied to the disease, but, by it, is understood no special or local inflammation, but a general condition of excitation of the heart and arteries, and of the capillary actions, or molecular movements of the tissues.

Blood drawn in the height of the fever, instead of being fluid, as is seen in the phlegmasiæ, was of a much darker hue; and it coagulated very imperfectly later in the disease, the clot being soft and grumous, with very little serum.

Before seeing any cases of yellow fever, I had adopted the humoral doctrine, which attributed the disease to a poison or an infected state of the atmosphere, acting primarily on the blood; and, as a consequence, I came to the conclusion, that those remedies which excited the secernent and absorbent vessels should be mainly depended on. The important question appeared to be, How can the blood be renewed, and its noxious elements removed? I had heard of the injecting of immense quantities of fluid directly into the veins, but I mistrusted very much the adequacy of such a mode, either to neutralize or destroy the morbid elements of the blood, or to supply, in proportions compatible with health, its supposed absent ingredients. I conceived, that if we could arouse the nutritive functions, the actions of those vessels which preside over the process of supply and waste in what is called the Capillary tissues, we should attain a grand desideratum in a therapeutic point of view. Hence, blood-letting suggested itself, as the most important of remedies in the disease, upon the admitted principle, that absorption increases in activity in proportion as you abstract blood. It is admitted that it promotes the absorption of medicines administered; and why should it not also—(a matter of, perhaps, equal importance)—excite the vessels of nutrition to feed upon the solids of the body also; and thus conduce to a healthy condition of the blood. In highly vitiated conditions of the fluids, we cannot expect healthy secretions. The improvement in the nature of the secretion will, as a general rule, increase as the blood is freed from its morbid elements, or as it approaches its healthy constitution. If it be admitted, as in the present state of our knowledge appears reasonable, that the cause of this fever is a poisoned or infected state of the atmosphere, primarily acting on the blood, and finally rendering it putrid in the fatal cases, or utterly inadequate to excite the healthy stimulation of any organ, the necessity of an early attempt to reorganize this fluid, or to arrest the farther process of vitiation, must be apparent to all who reflect on the subject. As a means promotive of this great object, blood-letting, in the epidemics of Opelousas, appeared to me most instrumental. Its other effects, however important, such as the relief from pain, the prevention of local congestions, &c., were considered as of secondary utility. During the whole treatment, the avoidance of medicines where the effect was irritation or congestion of the intestinal mucous membrane, was shown to be proper in all cases, for the results were always disastrous, if not fatal, even when the mildest purgative was followed, as frequently happened, by hypercatharsis.

The different modes of treatment recommended by different writers afford not only evidence that the disease presents itself in modified forms, but also that the true nature of its cause, and of the various derangements of tissues which ensue, are but little understood. By some its cause is considered native, and the disease regarded as endemic in its character; by others, it is thought to depend on a foreign cause, the introduction of which may be prevented by proper civil regulations. Some highly recommend mercurials in its treatment; others condemn them, without appeal. Blood-letting, advocated as the most important of all measures by one class, is very sparingly resorted to, or entirely condemned by the other class. In fact, the young physician, in his first intercourse with the disease, finds himself more embarrassed than if he had never heard of it. The conflicting opinions of its pathology and treatment place him in a position of incertitude, the horrors of which are by no means softened by the fatal results which too frequently attend his enthusiastic efforts to uphold the credit of his profession, and to rescue his friends and fellow-beings from an untimely grave.

In the epidemic of 1839, the mortality attributable to the disease was very inconsiderable. It is impossible to arrive at a perfectly accurate estimate, the difficulty being in ascertaining the exact number of cases. I am disposed to place it at about $4\frac{1}{2}$ per centum; and it should be observed, that amongst the fatal cases, there were many individuals far advanced in years, and whose recovery would have been doubtful from any kind of violent disease.

In 1837, the mortality, particularly in the commencement of the epidemic, was much greater, in consequence, no doubt, of the nature of the disease being for a considerable time unknown both to the physicians and the people,—to the absence of early medical aid, and to the injurious effects of frequently-repeated doses of calomel. In this year, the first fifteen cases, I believe, without an exception, resulted in death; but, happily, the subsequent mortality was not great.

It is one of the admitted facts, and, indeed, one of the proofs of this fever, that the chances of cure diminished in proportion to the length of time elapsing before proper aid is obtained; hence it is the duty of all to impress upon the people the indispensable necessity of early medical aid, and to discourage the use of quack medicines, so criminally scattered over the land by charlatans, huckstering pedlars, picayune storekeepers, and even by regular druggists.

It is to be presumed that the fevers of the country would differ in their type from those of the city; but there is no more reason to anticipate the continuance of the same type or form in the one situation than in the other; and as in subsequent epidemics, a form of the disease differing from those which preceded may demand a different treatment, the importance of paying particular attention to the subject cannot too often be impressed on the attention of physicians.

The epidemics of Opelousas were preceded by fatal cases of fever with black vomit, in individuals who fell sick immediately on, or a short time after, their arrival from the city of New Orleans. From this circumstance, the belief is generally entertained, that the disease was imported from the city. Professor Carpenter, in his pamphlet in favor of quarantine laws, has mentioned an instance occurring many years ago, the authenticity of which is fully supported, of the importation of yellow fever into this village directly from New Orleans, through the medium of fomites. A keel-boat, with goods from the city, then afflicted with yellow fever, arrived at the nearest port to Opelousas, now the town of Washington. The goods belonged to two merchants of Opelousas, one of whom, at the instigation of the chairman of the town council, opened and ventilated his goods before bringing them to his store; but the other brought his goods directly to town, and of four individuals present at their opening, three soon fell sick and died, with all the symptoms of the fever, and the fourth narrowly escaped with his life. In consequence of the low stage of water in the interior bayous, the communication between this place and New Orleans during the summer and fall is frequently stopped; and it is an exception to the general rule, if goods are ever imported at this season of the year from the city.

In 1837, the disease commenced at a particular spot, in a house adjoining the hotel, in which the subject, a Frenchman, from New Orleans, had died. Here seemed to be the focus of the infection, which did not appear to extend beyond well-defined limits. All residing within these limits, Creoles and foreigners, who had not previously had the disease, contracted it; and the instances were very few of any in the habit of frequenting this part of the town escaping the fever.

In 1839, the disease pervaded indiscriminately every part of town. No one portion—scarcely a family was exempt. People from the country either year incurred great risk in visiting town. Many most estimable individuals, after a visit to the place, both years, were, on their return home, stricken with the disease, and, with few exceptions, they died with black vomit.

In no instance am I aware of the disease being communicated by contagion in the country; and it must be observed, that among our creole inhabitants, more than common solicitude in the welfare of one's neighbours is evinced, by crowds of visiters calling to inquire, and to offer aid in case of necessity. In many of the country cases of the fever, there had, to my knowledge, been no contact or personal communication with the sick in town. Aware of the danger, they remained but a short time, and visited no house in which there was or had been cases of the disease. Some of our citizens, to escape, retired to the country; but, having the seeds of the disease in their systems, some days after fell sick, and died with black vomit. In these cases, notwithstanding they left town after the breaking out of the fever, and carried with them what they required for a long stay with their friends, yet there was no communication of the disease. In 1837, I knew of some individuals living with their families at a considerable distance from the infected limits of the town, who had contracted the disease, and died in the midst of their family, with its most aggravated symptoms, and yet there was no communication of it. This same year, a short time before the occurrence of the fatal case at the tavern, in the family of Judge King, senior, there was a well-marked case of yellow fever, in a relative who had left New Orleans after the disease had broken out there, and with the hope of escaping it. There was no extension of the disease in this instance, the entire family being exempt from it this year. The residence is situated on the outskirts of the town. In 1839, no member of this large family, black or white, escaped, save the judge and his venerable lady, both of whom had previously had the disease.

These facts certainly militate against the opinion of the contagiousness of yellow fever. But, again, it must not be forgotten, that every epidemic in this village, which, for many years past, has made no very noticeable progress in the path of civilization, is attributable to an infection contracted in or imported from New Orleans, or immediately preceded by such cases. The fact of the importation is as positive as any other in the history of the disease. Does it constitute an example of mere coincidence, a mere accidental circumstance, or is it to be regarded in the light of a cause? Would the disease have broken out as an epidemic, in the years 1837, '39, and '42, had not, each year, a fatal case been imported from New Orleans? It cannot be objected, that these individuals may have contracted the disease in Opelousas; for, in 1837 and '39, the two first arrived sick, and it was some two weeks after before any disease whatever appeared in the town, and in 1842, the third fell sick in less than forty-eight hours after his return from the city.

In works written expressly on this subject, the number of facts recorded in perfect accordance with those stated, to show that the disease in Opelousas was dependent on that of New Orleans, are very numerous. I simply state what was observed in this place; and the important question arises, Is it true, that the epidemics in question were the effects of an infection which would not have existed, had not individuals who contracted the disease elsewhere died in the town? An affirmative answer to this question, settles, for all practical purposes, that of the importation of the disease. Such an answer seems both natural and reasonable, when we consider that the town has, under other circumstances, ever enjoyed an immunity from the disease; that, with the exception of the four occasions mentioned, there never has been a case of the fever; that, on each of those occasions, it appeared after it had prevailed in the city; that, in the first instance, the fever, which, in consequence of the absence of favoring circumstances, did not extend to any more than the cases which subsequently proved fatal in the country, was the direct consequence of imported infection; and that, in the three subsequent visitations, it was positively preceded by fatal cases of the disease contracted in the city. In the present state of our knowledge of the causes of diseases, it appears to me as easy to conceive a peculiar infected condition of the atmosphere, dependent on or produced by such a cause as the one in question, as to assign bilious fevers to marsh miasm, or to heat and moisture prevailing with easterly winds. The occurrence of this fever in Opelousas, resulting from the importation of woollen goods from New Orleans, without spreading beyond the individuals directly exposed to the imported poison, considered in connection with the subsequent epidemics, show that the simple introduction of the poison, without the aid of other concurrent circumstances, is not sufficient to produce a prevalence of the affection; and it is worthy of observation, that such concurrent circumstances are as unknown as is the poison of yellow fever itself; for, in the years of our great epidemics, there was a remarkable immunity, not only in the country, before and during the epidemic, from disease of all kinds, but in the town also, from all affections save the prevailing fever. For the production of every disease, there must exist adequate causes. Long-continued heat and moisture certainly merit the most serious consideration, as predisposing causes of our autumnal fevers. They are generally attributed to a miasmatic origin. Some years, both our country and town are almost entirely exempt from disease of all kinds; as, for example, this present year, remarkable for excess of heat, and continued rains: other years, without apparent causes, the country is terribly afflicted with the common fevers. The causes of fevers are buried in obscurity.

Admitting, though, that there existed adequate causes, in the years of the epidemics, for the production of the worst forms of our country fevers, is it less to be wondered at, that, after the addition to these causes of the poison of yellow fever, this disease should result, than that without it the common bilious fever should have prevailed? What, under this supposition, caused the latter to be supplanted by the yellow fever? The only explanation, all three years, is the addition of the new element of disease, generating a new constitution of the atmosphere, which continued until frost, by purifying the air, rendered the new cause inert. But if these causes did not exist, then the conclusion *a fortiori* results, that the disease must have originated from a union of the poison with those general and local predisposing causes of fever which necessarily exist in the fall of the year in our southern States.

Yellow fever is now very generally acknowledged by all to be a specific disease,—that is, a disease *sui generis*. Thus considered, the doctrine of importation has been objected to, on the ground that the disease presents itself, on different occasions, in different type. Now, it appears to me, that this fact rather strengthens than weakens this doctrine; for, if the poison be importable, and in the absence of certain conditions, or of circumstances which favor its multiplication and extension, does not extend beyond the first attacked, it is presumable that various results might ensue from its union with such condition or circumstances, according to their intensity or character, and that thus different epidemics might rather be expected to present different than identical types. But it is right to observe, also, that the different forms of this fever, observed in different years in our Southern country, may be ascribed to modifications of constitution induced by the ever-changing predisposing causes of fever. It would certainly appear to me more surprising, should the fever, in New Orleans, resemble in type that which might prevail in the Havana, supposing it to be imported from that place, than that it should present a different form of the disease.

In the preceding pages, though it was my intention, at the outset, to record only some of the admitted facts of the epidemics of Opelousas, I have yet been led, almost unknowingly, into comments which, perhaps, had better been omitted. I will now close, disclaiming all pretensions to any thing original, and only hoping to stimulate others better qualified than myself, to pursue the investigation of this very interesting subject.

IV.—*Observations on the Stillingia Sylvatica, or 'Queen's Delight'; with Cases.* By A. Lopez, M. D., of Mobile, Ala.

"Historia *quoquo modo* delectat."

It is remarkable that the therapeutic properties of a valuable remedial agent should have been permitted so long to sleep among the *membra disjecta* of the Materia Medica, and it is equally discreditable that its introduction should be committed to the ignoble though lucrative fate of a patent medidine.

The *plant* which heads this article has received no more than a passing notice from those who speak of it at all, and except its classification and a few general hints as to the diseases in which it has been prescribed, few writers appear to deem it worthy of attention.* Pereira does not mention it at all. Dr. Barton who took much pains to accumulate a history of all southern plants for the completion of his "*Collection towards a Materia Medica of the United States,*" has permitted it to escape his attention. Elliott in his Botany—Nuttal's Genera, and Wood & Bache in their Dispensatory—insert it, but with by far too limited a notice. The amount of information derived from these last named authors may be summed up in what we find at page 637 of Wood & Bache, to wit: "The plant grows in pine barrens, from Virginia to Florida, flowering in May and June. When wounded it emits a milky juice. The root, which is the part used, is large, thick and

* The only monograph concerning this plant which I have ever seen was an inaugural thesis written by a graduate of the Charleston Medical college, and can be found among the bound manuscripts deposited in the library of the Medical society of that city. It evinced a careful research, and deserves publication. I think the author's name was Prioleau.

woody. *We are not acquainted with its precise properties, but understand* that it is much employed in the Southern states. It is *said* to be purgative and alterative, and probably to possess more or less of the acrid quality common to the *Euphorbiaceæ.* It is used in *lues venerea, obstinate cutaneous affections and other complaints* which are usually treated with Sarsaparilla."

This is historically true, and perhaps all that could have been said by those whose allotted labour and researches were thus circumscribed by the inattention to which the curative virtues of the plant has been doomed by those who, living within its reach, were called upon for more extended investigations.

The earliest direct knowledge I have of its being brought into use at the South, was communicated to me by my friend Dr. Thos. Y. Simons, of Charleston, S. C. In a letter received from him in 1834, he refers to its having been adopted in hospital practice according to the recipe established by himself. This assertion however must not be understood to interfere with the claims of any other practitioner—but none such are within my reach.* At the time alluded to, I resided in Georgetown, S. C., and my attention was at once directed to the successful prescriptions of an old negro, who for many years had engrossed the practice of the neighbouring plantations and of this town in venereal cases, much to the curtailment of the fees of regular practitioners. The secret of his remedy was of course religiously guarded—no bribe or entreaties availed to dispossess him of it. Chance, however, favoured my curiosity, and riding through the woods on a professional visit, I overtook the old man, with mattock and spade so busily engaged that I advanced upon him before he could avoid my inquiry. I found him gathering large sacksful of the *Stillingia*—and he surrendered at discretion.

I immediately instituted a series of experiments with the plant, and continued to use it without hesitation while I remained in the State.

I have been unable to discover it in the neighbourhood of Mobile, but entertain no doubt that it is not foreign to the soil, it being indigenous to all analogous localities.

I was much pleased with its effects, sufficiently so, indeed, to continue it among my standard prescriptions—and disclaiming all pretensions of ranking it among the *Panaceas*, I will, if your pages are not better pre-occupied, submit to your readers a few cases selected from my memoranda. I regard it as eminently entitled to a high rank among our best alteratives, and as such I have ordinarily used it, and frequently as an adjunct to other remedies of like character as well as to medicines differing from it in their tendencies.

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* Since writing this article I discover that Dr. Frost, of the South Carolina Medical college, refers to this plant in his Syllabus. He conjoins its use with that of the Sarsaparilla. From his remarks, however, I infer that the Professor ascribes the efficacy of the prescription to the virtues of the Sarsaparilla—wherein we differ, because I obtained influences by means of the *Stillingia* which were not derivable from the use of the Sarsaparilla either alone, or in combination with other approved medicines. Besides, the Stillingia used in powder or tinct. has been equally effective when uncombined. Dr. W. G. Ramsay, of Charleston, S. C., is said also to have prescribed it successfully in the Marine hospital of that city.

Case 1st: *Secondary Syphilis.*—Myrtilla, a free coloured woman, aged 34, of scrofulous temperament, mother of several children, consults me for the following symptoms: ulceration of the throat and posterior nares; entire loss of velum pendulum palati; partial loss of bones of the palate; mercurial erythema of the remainder; great dissonance in speech; ulceration and partial destruction of upper and lower turbinated bones of the nose; excessively fœtid and sanious discharge from the nostrils; emaciation; rheumatic affections of the limbs and pain in the frontal sinus; strength and appetite greatly impaired.

℞ Rad. Stillingiæ Sylvaticæ } incis:
Rad. Sarsaparill. } *aa* ʒ iij.
Fol. Sennæ ʒ ij.
Aq. fontan. ℥ xxxij.

Coque usque ad ʒ xvi. Cola. Sumat ℥ij dulcific. cum mell. opt. ter in die.*

Inject the nostrils and wash the ulcerated parts frequently with the following wash:

Chlor. calc. ʒ i.
Aq. fontan. ℥ ij.

By a steady perseverance in this plan, she left me greatly improved in health and strength, with a sound condition of the diseased parts.

Case 2d: *Secondary Syphilis.*—Martha, a coloured girl. Throat and nose implicated, but to a less extent than the preceding case. She was put on the use of the *Stillingia*, and as her symptoms were more recent, I conjoined the use of Van Swieten's pills of *Oxymur. Hyd. et Mur. ammoniæ*, *aa* $\frac{1}{8}$ gr. three times a day. She was discharged cured.

Case 3d: *Secondary Syphilis.*—G. W. T. Rheumatic pains; nodes on tibia and humerus; nocturnal pains, distressing; daily exacerbations of fever; appetite lost; health and strength rapidly declining; *bubo* not yet cicatrized, discharge sanious. Prescribed *Stillingia*, occasionally interposing *Dover's powder* at night, and black wash to the bubo. Cured.

Case 4th: *Primary Syphilis.*—Sarah. Chancres, and great irritation of external labia. Prescribed *Decoct. Stillingiæ*, Van Swieten's pills, black wash to chancres. Cured.

Case 5th: *Congenital Syphilis.*—Mary, aged 6 months, born of a mother who, during gestation, had been infected by her husband. The case was well defined by extensive crops of syphilitic eruptions on the genitals and gluteal muscles, with occasional patches on the loins, thighs and back. Great emaciation, skin wrinkled and capable of being wrapped around its limbs.

℞ Oxymur. Hydrar., grs. ij.
Brandy ℥ i.

Dose: one drop in a tea-spoonful of sweetened water, three times daily —the dose gradually increased to three drops. *Decoct. Stillingiæ*, according to preceding formula, *one tea-spoonful* sweetened with honey three times a day. Perfectly cured, with rapid amendment of health and flesh. N.B.—During this course I also prescribed for the mother the *powdered Stillingia*, grs. x three times a day, although she had been free from the disease long since.

* This is the formula adopted by Dr. Simons.

Case 6th: *Mercurial Erythema.*—W. W. D., aged 22, consulted me for an affection of his throat. On a visit from home he contracted gonorrhea. A physician gave him pills for several weeks, which, he says salivated him to excess. Examination of his throat displayed a high state of erythematous inflammation, involving the whole of the soft palate, the velum and the tonsils, accompanied with papular eruptions. His voice was considerably affected and he suffered severely from mercurial rheumatism. He had not been tainted with lues at any period of his life. I prescribed the preparation of Stillingia, and at the end of a week he left me for his home, nearly as well as he ever was. I, however, provided him with a supply of the remedy to be continued on his arrival at home. I shortly after received a letter assuring me of his perfect recovery.

Case 7th: *Chronic hepatitis—Enlargement of spleen, with icteric symptoms.*—J. W., aged 40, of bilious temperament, had been subject to repeated attacks af autumnal bilious remittent and intermittent fevers of our rice districts. His treatment being usually of a domestic nature, was consequently irregular and imperfect. He consulted me under the following circumstances: chronic enlargement of the spleen, sallow complexion, jaundiced suffusion of adnata of the eyes, cachectic diathesis, loss of appetite, impaired digestion, much harrassed with eructations, bowels irregular, alvine dejections sometimes pasty and lead coloured, at other times clay coloured, passive epistaxis, gums spungy, breath fœtid.

℞ Mass Hydrarg., grs. v, quaque noct. 3ta.
Decoct. Stillingiæ, ℥ ij ter in die.

Under this treatment he was entirely restored in four weeks, with the exception of the splenic engorgement which was of long standing.

Case 8th: J. M. L.—Consulted me on account of a tumour, which his attending physician had told him was a *dislocation of the ensiform cartilage!* The tumour was the size of a small billiard ball, situated under the skin, midway between the umbilicus and the xiphoid cartilage. It had the feel of a steatomatous tumour, was slightly moveable, gave great pain on pressure and when the body was bent. It was particularly uncomfortable *after a full meal and during digestion.* Its growth had been rapid. His complexion bilious; bowels *habitually costive* rarely having an evacuation oftener than once in eight days; appetite morbid; palpitatio cordis especially urgent during digestion, and usually troublesome when taking exercise.

Treatment: I *cupped* him over the tumour and applied the *tartarized ointment* to pustulation. He was ordered 5 grs. of *blue mass* every alternate night, followed the next morning by a saline aperient. This course did not effect a disappearance nor even a diminution of the tumour. I resorted to the *Decoct. Stillingiæ,* continuing the blue mass. At the end of the first week there was an evident decrease of the tumour, with melioration of all the other symptoms—and for the first time in many years his bowels were brought to a normal periodical condition. By a perseverance for six weeks the tumour gradually disappeared and he never was troubled with it up to the time of my departure from the State.—

Case 9th: *Hemorrhœa Petechialis.*— A. C., a coast captain of irregular and intemperate habits, subject to frequent attacks of acute, supervening upon chronic, hepatitis, sent for me. I found him laboring under the disease at the head of this paragraph. He had been up the river for his load, and being detained by adverse winds, he indulged daily in drinking freely, particularly of porter. He was seized with chilliness followed by fever, which continued high and unintermittingly upon him for three days, without medicine or medical assistance. The wind becoming fair, he returned to Georgetown. While on the passage, he discovered that his body generally, but more especially his arms, legs and feet were covered with what he supposed to be flea bits. His gums were much ecchymosed and spongy, as also the roof of his mouth both discharging blood freely. Complexion thoroughly jaundiced, difficult respiration, *pulse full, strong and quick*, not compressible. Headache—Eyes turgid. Alvine discharges dark, pitchy, tenacious and scanty.

Treatment: V. S., ℥ xvi. Cathartic of Calomel and Jalap.

2d day:—Powder had operated freely, but character of the evacuations not yet changed; pulse softer and more slow; petechiæ still abundant; gums hemorrhagic; sensations of faintiness, or rather oppression, through the past night; pain of right hypochondrium when pressed.—Repeat the cathartic. Habt. *haust. acid nitric.*

3d day:—Much improved in general; petechiæ still out. Complains of much pains in the gums, and I distinguish very unequivocal mercurial fœtor. Upon rigid enquiry I find he had been taken freely of calomel previous to his attack while upon the river.—Continue *haust. acid nitric* and observe caution, as the weather was very cold and damp.

4th day:—Much worse. Contrary to orders he had gone into his garden in thin slippers and occupied himself in trimming a grape arbour. Profuse salivation, accompanied with increased hemorrhagy from gums and slight flow from the legs.—Great mercurial erethism; face swollen and painful; astringent and demulcent gargles allayed the suffering, and I then commenced with the *Decoct. Stillingiæ* according to the preceding formulæ, in addition to which I prescribed *Scruple* doses of *Ext. taraxaci* three times a day. The effects were obvious in a few days. The petechiæ faded and gradually disappeared; the liver resumed its healthy action; the evacuations assumed a natural character; the gums ceased to bleed and he was at his business in a fortnight after.

The preceeding cases are selected from among others and are sufficient to exhibit the various conditions of the system to which the remedy is applicable. It is not infallible. What medicine is or ever has been? But if we possess within our immediate reach a valuable agent, by whose means human suffering can be abridged, and those sufferings appertaining to the most direful and desolating "ills that flesh is heir to," we are inexcusably responsible to the high and solemn obligations we assume as medical men, if we permit this, or any other means of alleviation to remain unnoticed and untried.

V.—*Case of old, unreduced luxation of both Radii, backwards, with bony union of the Radius and Ulna; observed in the dead subject.* By R. J. FARQUHARSON, H.Y., M.D.

In a post-mortem examination of the body of a German, æt. 24, dead of typhoid fever, attention was directed to the condition of the elbow joints, by a marked prominence of both external condyles, with the fore-arm on each side fixed in a state of extreme pronation, the back of the hand corresponding to the front of the elbow joint. Flexion was perfect, but complete extension of the arms into a straight line was impossible. On one side, the phalanges of the fingers were dislocated, forming, by their union, irregular angular lines, accompanied by large cicatrices. Upon dissection, the muscles of the arm and fore-arm were found well developed, their direction and relative position being, however, considerably altered; the insertions of the biceps flexor cubiti and brachialis internus were in immediate contact; the pronator quadratus was as large as usual. The interosseous space and membrane were preserved, being even more extended than normal; this appeared to be brought about by the arched form of the radius, the ulna retaining its straight direction. Upon opening the joint, no marks of laceration of the general capsular ligament were apparent; the head of the radius was found behind the external condyle, lodged in the inter-space between the olecranon and coracoid portions of the great sigmoid cavity; the head of the bone was flattened, and increased in breadth, being covered by a fibrous substance, perhaps the remains of the orbicular ligament. The altered head of the radius formed with the great sigmoid cavity of the ulna, a common articulating surface, upon which the trochlea of the humerus moved in flexion and extension. Appearances almost precisely similar were presented in the dissection of the other elbow joint. A portion of the head of the radius on one side, together with part of the articular surface of the humerus, had a reddish or flesh color, and a diaphanous appearance. Upon making a section of these parts with a scalpel, (which was easily done), there was found within a soft and thin shell of bone, a delicate areolar structure, filled with a homogeneous substance of a dark-red color, transparent, and much resembling jelly. The structure in the condyles of the other humerus was similar, with a thicker coat of cartilage and bone externally, and matter of a lighter color within.

Upon a removal of the soft parts by maceration, the radius was found united to the ulna by bony anchylosis, from the neck down, one-fourth of its whole length: the articular surface of the head of the radius had escaped this bony union almost entirely. The bones of the fore-arm, taken as a whole, are appreciably shortened, but the radius is elongated by an inch or more; the head of the bone being above its normal situation, while the wrist-joint preserves its usual transverse direction. The radius has a decided curve forward, while the humerus is bent inwards in its lower third, and is obviously larger than it should be. That this abnormal state of the parts concerned in the elbow joint was of long standing, will scarcely admit of a doubt: the great change in the form of the head of the radius, the imperfect development of the bones of the fore-arm, the bony union, the return of the soft parts about the joint to a healthy state, all seem to point to childhood as the period when the change occurred. The idea of its being a congenital deformity appears inadmissible, from the following fact, viz., that the portion of the head of the radius, which at the time of death formed no part of the joint, had a smooth articular surface, covered with cartilage. Again: the cicatrices, &c., about the hand would incline us to the opinion of its having been the result of mechanical violence. If this view of the subject be taken, it presents the case in a very interesting light, viz., that of a dislocation of both radii backwards, produced by external causes, and remaining unreduced for a series of years. Sir Astley Cooper never saw a case of dislocation of the radius backward but once, and then in the dead subject, an account of which, together with a drawing, is given in his work on "Dislocations and Fractures;" other surgeons, however, and especially among the French, have been more fortunate, so that, according to the best authorities, we may conclude, in the words of an author, that this luxation, "though it may be rare, has nevertheless been observed several times."* A luxation of both radial bones backward is a much rarer case, such a one is mentioned by Professor Gibson, in his work on Surgery.

But, in opposition to this view, several reasons of considerable force may be adduced: 1st, the well-known effects of rickets in producing distortion of the bones, amounting, in many instances, even to complete dislocation of their articular extremities: now, the appearances of the heads of the bones, in this case, were exactly those described as characteristic of this disease;† 2d, the extreme rarity of such an accident is, in a doubtful case, good presumptive evidence against its occurrence; 3d, the analogy of this case with others detailed by authors, as undoubtedly resulting from rickets. A case given by Professor Cooper, in his "Elements of Surgery," nearly resembles the one in question: "*There is, in the museum of University College, a humerus that has been twisted by the action of the muscles, in consequence of which the ulna has been moved partly into the place of the radius, and the radius displaced. The upper head of the latter bone, no longer having the humerus to play upon, is elongated and altered in shape.*" In the present instance, though we have the head of the radius "elongated and altered in shape," yet it is still in contact with and "plays upon" the external portion of the trochlea of the humerus, the anterior part of the head, however, and not the superior, as in the normal state, being in contact.

Upon the whole, we would, perhaps, be warranted in suggesting, as most probable, the opinion, that the luxation was the result of external violence, its effects being modified by concomitant or subsequent disease of the bones.

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* Sansen—Nouveaux Elemens de Pathologie Medico-Chirurgicale.

† Stanley, as quoted in Cooper's Surgical Dictionary, article, "Rickets."

VI.—*On the Use of Sulphate of Quinine.* By J. B. WILKINSON, M.D., of Louisiana.

Messrs. Editors: The several articles published in your journal upon the subject of Quinine have attracted much attention, and given rise to considerable discussion respecting the modus operandi of that medicine, and of the most advantageous method of exhibiting it. A free and unbiassed opinion upon this subject is much to be desired; for the mind of the enthusiast, although keenly sensitive to imputed error, in the ardor of pursuit may be led astray, and thus defeat the object for which he contends. I do not mean to deny positively the rationality of the views expressed by the preceding contributors to your journal upon this question, but to examine the necessity or expediency of the course recommended and pursued by them. From the large quantities of bark now used to effect what was formerly accomplished by smaller doses, apparently establishes, either that the physical constitution of man is modified, or that the bark is greatly adulterated. The facility of detecting the latter by chemical analysis, would prevent the imposition; and the fact that the human system is regulated by the same forces, influenced in a similar manner by similar substances, as in former days, and presenting external evidence of similar mechanism, indicates that no material change has taken place in its constitution. How happens it, that the virtues of this medicine should have been discovered so many years ago,—had the necessity existed of giving its *active principle* in such large doses? The best bark yields about 2 per centum, making the quantity, as formerly given in the crude state, about 10 grains quinine, during the apyrexia; in this state, too, less soluble, and consequently endowed with less energy than the sulphate quinine. Yet we find that, in the records of medicine, it is adduced as an agent of great virtue and power, obtaining the reputation even of a charmed powder. In the Eastern Continent, where it was first extensively used in fevers, we hear its praises as much sung as they are at the present day. Lind, Clarke, and Balfour, each and all attest its virtues in fevers, intermittent and remittent, yet it is highly improbable that they ever gave as much as twenty grains quinine at any one time. Johnson states, that in the ascent of the Bocca Tigris by the British, intermittent fevers prevailed very extensively, thirty and forty patients coming in daily, and that the use of the bark in the ordinary quantity was entirely successful. We also know that the Jesuits, who first made known this remedy, and who at one time possessed the exclusive privilege of its use, obtained great repute in the cure of fevers: they also used it in the crude state, and consequently could not have given the active principle in large doses. The quantity of quinine now given is, no doubt, often much more than is necessary. In the last three years, during the spring, summer, and autumn, I have treated successfully between 600 and 700 cases of intermittent fever, embracing every variety of form, from the simplest grade, to those cases where complete stupor and insensibility were co-existent, and succeeded to the cold stage, and those cases in which the paroxysm was ushered in by symptoms and appearance so similar to those indicative of cholera morbus, that unless acquainted with the history of the case, an observer would have declared them cases of the latter disease.

I have likewise had my fatal cases, probably, however, not exceeding in ratio the number occurring in the practice of those who advocate the ultra quinine treatment.

I treated the above cases, almost in every instance, with quinine, and occasionally, when the quinine could not be obtained, with the bark in substance, or a combination of the bark and snake root. Generally, the quinine was preceded by other remedies; sometimes, a combination of quinine and some of these remedies, and occasionally the quinine was used at once, and alone. The quantity given never exceeded forty grains in twelve hours; generally, twenty grains in the same time answered every purpose. The doses varied from three to ten grains; rarely, however, as much as the latter: the usual prescription was four or five grains every three hours.

The best period to administer the quinine is, without doubt, immediately succeeding the decline or abatement of the paroxysm, and at intervals during the intermission or remission. In the Blackly Hospital, Philadelphia, it was usual, at one time, to give ten grains of quinine with twenty-five drops of laudanum, about an hour before the expected occurrence of the chill. Impressed favorably with this method, from witnessing its operation at that institution, I tried it in my own practice, but have found it much less successful than when the remedy was given directly after the subsidence of the paroxysm, and during the intermission. It is not uncommon for the chill or fever to return at the first accustomed period after the exhibition of the quinine; but it more rarely happens that a second accession occurs; showing, that the medicine required a greater length of time to affect the system; and hence this is proof that the administration of the medicine immediately after the subsidence of the fever is the best period for its use. How frequently do you see buzzing in the ears, dizziness, &c., take place after giving the quinine, without arresting the chill! Now, if these are the specific effects of the quinine, why is not the paroxysm prevented?

The question that next arises is, can quinine be given with safety or advantage at any other stage of fever than during the apyrexia? Many individuals, at the head of whom is Dr. Dudley, of Kentucky, censure the practice of using quinine at any other period than during the intermission of fever; I am convinced, however, that it may be used with advantage frequently in the other stages, but that proper judgment and discrimination are required, to know when it may be safely and judiciously given. In the hot stage of fever, when a very short intermission or remission is expected, or a violent exacerbation is anticipated at the succeeding paroxysm, quinine may and should be given. Preliminary measures should, however, be always taken, to obviate, as far as possible, any hurtful tendency that it might at the time exert: these precautionary steps are all such as reduce excitement, and counteract local hyperœmia. In this, too, we are seconded by those who give quinine in large doses, since they advise anti-phlogistic measures, pursuant to the administration of the *sedative* doses of quinine. I have undoubtedly seen evil effects result from the use of quinine in the period of excitement, and believe, too, that they were induced by its stimulating properties; consequently, I must deprecate its constant employment in fevers without regard to the period, or other concomitant circumstances.

In the cold stage, I have frequently given it, without, apparently, any decided action. When it is given, in this case, to counteract depression, ammonia, camphor, and warm drinks, are decidedly better.

Associated with the anti-periodic property of quinine, it possesses diaphoretic properties, which latter, no doubt, enhance its value as a febrifuge. It does not, probably exert an immediate action on the cutaneous vessels, but possibly, in overcoming the diseased action, establishes indirectly the secernent function. By combination with diaphoretic medicines, we may frequently improve this property of quinine: where gastro-enteric symptoms did not forbid, I have given it in conjunction with the nitrate of potash, with the effect of increasing diaphoresis, and of lessening arterial action. We have heard much of quinine in yellow fever; but, from the expression of conflicting opinions upon this subject, the medical world has derived no positive advantage. Dr. Mackie, who originated the practice in New Orleans, subsequently stated, that it was a remedy of doubtful efficacy. Dr. Stone, of Woodville, Mississippi, says that, in the epidemic that raged in that village in 1844, the employment of quinine was attended with the most destructive consequences. I have been told, by the highly intelligent house surgeon of the Charity Hospital (Dr. Wederstrandt), that this remedy, in the epidemic that occurred two years ago in New Orleans, met with signal failure. Dr. Johnson, in his account of the Bengal fever, and of the Batavian fever, says, that the use of the bark was very detrimental. The weight of testimony on both sides is very respectable, however, and we must attribute the success or failure of the remedy in this disease to the aspect or type the fever assumed. It is much to be hoped, that as the attention of the medical profession is so engaged with the action of this medicine, in a few years something definite will be established.

Is the exhibition of quinine, in large doses, attended with hazard?

Of the fact, that fatal effects have resulted from the use of quinine in large doses, we have irrefragable testimony. Besides the occurrence of cases in this country, the reports of physicians in Paris, who, a few years since, treated rheumatism with large doses of quinine, make mention of frequent deaths occasioned by this method. I have witnessed bad effects in several instances, which were clearly demonstrable to the action of quinine. In one case, during the autumn of 1843, I was called to see a patient, Mrs. B——, who was affected with ague and fever. She was a woman of good constitution, and had usually enjoyed good health. As the chill occurred late in the day, I requested her to take quinine in the morning of the following day, and at intervals during the day, stating the quantity and period at which it was to be taken. I left her walking about the house, apparently very little indisposed. Early on the following day, I was sent for to see Mrs. B., and upon my arrival found her in convulsions—insensible, with a rapid, feeble pulse, cool skin, and pale face. She continued in this condition about half an hour, and expired. Upon inquiry, I learned from her daughter that she arose early in the morning, expressed herself quite well, and said that she would take all the quinine at one dose, but without weighing it; took out about a table-spoonful from the ounce vial, and, mixing it with water, drank the whole: she then remained in the room a few moments, walked out into the yard, and fell down in a convulsion; the termination of her life, about two hours after she fell in this convulsion, I have already related. The inference is here, plainly, that she died from the effects of the quinine.

Miss ——, whom I attended for chill and fever, after other medicine, was ordered quinine. On my visit the next day, I found she had suffered again from the chill. Her mother informed me, that her head had been so much affected with the quinine, that she had been compelled to desist from its use. I again prescribed the quinine, in four-grain doses. After taking the second dose, the head became dizzy—ached severely—roaring in her ears occurred, and, in attempting to walk to her room, she fell upon the floor. I reduced the dose, and cured her without the return of those unpleasant symptoms. Now, with this susceptibility to the influence of this medicine, might not convulsions or insensibility have occurred from the effect of ten or twenty grains of the sulphate. I attended the father of this young lady, and found his head so much affected with the quinine, that I was compelled to give it per anum. During the past week, I attended a boy aged about 15. He suffered from remittent fever, accompanied with some degree of pneumonia. His intellect was perfectly clear. I gave him a combination of 20 grs. sulph. quinine, 10 grs. calomel, 10 of ipecacuanha, divided into four doses. During the night, he became highly delirious, attempted to get out of bed, and had high fever. I then discontinued the use of the quinine: his excitement of brain subsided, fever abated, and finally recovered, under other treatment.

I could adduce numerous cases illustrating the evil effects of quinine, did circumstances require it. That quinine is a specific for fevers—will destroy a fever, as an alkali does an acid, as some writers wish us to think, I do not believe; neither do I believe that quinine cures fevers, by repairing the losses occasioned to the nervous system by the poisonous cause of fevers, since the action of the quinine is too rapid to admit of its assimilation by the vital organism. It is not at all necessary to employ this hypothesis in explanation of its modus operandi. It may act by immediately decomposing or neutralizing the malaria, or whatever we may consider or term the poison which generates fever: thus, the antidotal effects of ammonia to the poison of serpents and spiders, may be considered as illustrative of its action. It has been observed, that it is a medicine whose action is *sui generis*, altering or modifying the vital forces in a peculiar manner, which tends to restore them from disease to health. It exercises some invisible, inscrutable, mysterious influence on the human system, of which we only know the result. Nature has revealed to us, probably, all that is worth knowing; and the means by which this is achieved, like the elementary principle of most of her laws, will, in all probability, be forever buried in oblivion. I believe, if any medicine deserves the name of a specific, quinine deserves this appellation in the cure of intermittent fever.

In all diseases characterized by periodicity of type, quinine may be used to advantage in some stage of the disease.

With proper precaution, quinine may be used in every stage of periodic fever with benefit; but cases occur in which its use is attended with hazard and bad consequences to the patient. These ill effects sometimes result from idiosyncracy of temperament; sometimes, from inapplicability to the disease itself.

Its primitive action is on the nervous system; having also a reflex action on the arterial system, in different cases producing different results. Its essential action in the cure of disease is entirely unknown.

In the great majority of cases, quinine, given in moderate doses, will effect every useful purpose, and without hazard to the patient; whereas, in 20, 30, or 40-grain doses, although generally successful in its operation, dangerous and sometimes fatal consequences occur.

Its value as a therapeutic agent in yellow fever, remains yet to be proved.

In the catalogue of medicines, to none has nature assigned greater virtues than to the substance in consideration: in every ramification of the civilized world, its benign influence has been felt and acknowledged; hundreds and thousands of lives which would have been sacrificed to the poisonous exhalations by which we are encompassed, have, by its kindly influence, been preserved. All previous discoveries registered in the annals of medicine, sink into insignificance in comparison to the great boon conferred upon humanity in the discovery of this magical substance. The country which we live in — the great South and West, teeming with human inhabitants, animated by manufactures, enriched by commerce, and smiling in agricultural beauty, owes much of its prosperity to this simple powder. How earnest, then, is the call for each laborer in the field of medicine, to co-operate in efforts to elucidate the subtile principles which regulate the perfect action of this medicine, and to reveal to the world its intrinsic excellence and value.

VII.—*A Case illustrative of the beneficial effects of the Nitrate of Silver in strong solution, in Acute Opthalmia.* By O. F. Manson, M. D., North Carolina.

J. N., æt. 40, of sound constitution and robust frame, had been suffering for two days from an eruption of large pustules over the scalp and face, induced by eating enormous quantities of butter of which he was passionately fond; one of the pustules had formed near the margin of the inferior palpebra, from whence the inflammation rapidly extended over the whole eye. When I first saw him he was suffering the most agonizing pain, the pain darting "through his eyes and through his head" as he expressed it, and of such intensity as to elicit loud cries from the patient every minute, the darting pain being paroxysmal. The eyelids were so completely closed and swollen that the eyeball could not be seen by attempting to open them. The patient had high fever, hot, dry skin, furred tongue and fixed pain in the forehead and temples. I bandaged his arm, and bled him upwards of two pounds; when symptoms of syncope appearing the flow of blood was stopped; at bed time, several hours after, 20 grs. of calomel were exhibited, to be followed in the morning by a large dose of Sulph. Magnesia. 10 A. M. next morning, fever has abated somewhat, but the pain and inflammation have not perceptibly declined; eye still completely closed, and can be but very slightly separated by the fingers. Applied 30 leeches in the course of an hour (the medicines have operated well) but without being followed by an apparent diminution of the symptoms. Pain still excrutiating, causing the patient to contort his whole body; even to witness the agony of the sufferer was painful to the bystander. All must own from this imperfect description, that this was a case of the most acute character, and that the practice adopted was well calculated to relieve it; but I am very certain that the patient was suffering as greatly, and that the symptoms of inflammation were as well developed, with the exception of a slight decline in the general exitement, as they were before the treatment was had recourse to. I now determined on using a strong solution of the caustic, and mixed 10 grs. to the ounce of water; four or five drops of the solution were introduced into the external corner of the eyelids, the patient only complaining for a very short space of time of a slight smarting sensation which could not properly be termed pain. *In five minutes the patient expressed himself relieved of pain*, and after the reapplication of the collyrium three or four hours afterwards, the pain entirely subsided to return no more, the patient falling asleep for the first time in 50 or 60 hours. Since treating this case, I rely upon a strong solution of Lunar Caustic alone in the treatment of acute opthalmia. I have introduced it into the eye of my own infant, 16 months old, and can recommend it as a safe and effectual remedy; but at the same time would reprehend a weak solution, except in cases of a chronic character, in which I have found it more useful than the 10 grs. solution.

Granville Co., N. C.

VIII.—*On Magneto-Electricity, as a Derivative and as a Parturient.* By Dr. N. Walkly.

[A Paper read before the Mobile Medical Society, at its meeting in May, 1845.]

I wish to call the attention of the Society to the beneficial effects of Magneto-Electricity as a derivative, in certain cases of cachexia peculiar to females, dependent usually upon uterine derangement.

In cases of uterine engorgement or displacement, and in some cases of uterine or vaginal leucorrhea, we meet with the same train of symptoms, viz., pain in the back and loins, pain in the side, palpitations, difficulty of moving the lower extremities, &c., usually accompanied by depraved appetite; and if her circumstances are such that she is not compelled, the patient will omit the necessary exercise which is requisite to keep up the circulation in the extremities, and, as a consequence, her limbs will become atrophied and partially paralyzed; the blood which should have supplied the lower extremities is withdrawn, thereby increasing the visceral engorgement.

And the excess of nervous action which would have been exhausted by the proper exercise, now exhausts itself, by exciting an increased inflammation of the uterus or its appendages, which, in turn, reacts on the general system, deranging its functions of nutrition, producing this cachectic state, and she lingers out a miserable existence, or goes into a decline, becomes low-spirited—a sympathetic, dry, hacking cough sets up. She commences doctoring for consumption; confines herself to the house; pustulates her chest with tartar emetic, and uses debilitating remedies, until her system is reduced to a state suitable to the production of tubercles, and finally hectic supervenes, and she dies of a consumption of her own cultivation.

In these cases, taken before any organic change of structure of the lungs has taken place, I have found magneto-electricity to be a valuable remedy.

By daily passing a rapid succession of shocks through the limbs, muscular action and circulation are restored, and blood necessary to supply this circulation is withdrawn from the engorged viscera.

From the almost immediate relief experienced by the patient of some of her neuralgic symptoms, she begins to hope that she may be cured, and usually pins her faith to the electrical machine, and if she is cured, is certain that the electrical machine has affected it.

I have before me the notes of a case in point, which will illustrate the mode of application in those cases.

Miss M——, of bilious nervous temperament, aged 18, had been in a bad state of health for about four years; was supposed, by her friends, to be dying of consumption. I found her very much emaciated; lips colorless; had been confined to her bed ten months. She complained of chilly sensation, succeeded by flushing several times a-day, but had no night-sweats, or apparent perspiration of any kind. By percussion and auscultation, no lesion of the lungs could be detected. She complained of constant pain and soreness beneath the left breast. I found a tumor occupying the right iliac region, which, she informed me, increased and diminished in size; that when she became constipated, she had fevers, and at those times it enlarged and gave her much pain, and at that time cathartics had little other effect than to cause vomiting, and increase of pain. She supposed this to be an abscess, which discharged into the intestines, as its giving away, as she termed it, was followed by a diarrhœa, after which she was relieved. This giving away had occurred once a month, with strict regularity, since the total suppression of her catamenia (twelve months). Her catamenia appeared in her thirteenth year, but had never been regular. Her digestive functions were entirely deranged; the stomach very irritable; sometimes the appetite craving, and, if gratified, occasioning great distress; at other times, with no appetite, and the stomach rejecting every thing offered. For a few days preceding each attack of diarrhœa, she suffered much pain on the inside of the right leg, following the course of the sciatic nerve, and in the knee joint, which subsided after the recurrence of the diarrhœa.

When I first saw her, the tumour was at nearly its maximum size, her pulse short and quick, and skin dry. I applied cups over the iliac region of that side, followed by emollient poultices, and administered opiates, to enable her to sleep. The next morning her diarrhœa set in, and afforded her relief: I permitted the diarrhœa to continue two days, and then, by means of chalk julep, suspended it.

On the 6th of June, the day after the checking of the diarrhœa, she had an attack of hysteria, which led me to think that her disease depended more upon uterine derangement than I had suspected. I proposed an examination, for the purpose of ascertaining whether any displacement or tumefaction of the uterus existed, but both her mother and herself objected; the mother "knew that all was right, with the exception of her courses being stopped." I ascertained, however, that since she commenced menstruating she had been irregular, had been troubled with leucorrhœa, and had suffered with dragging-down pains in the back, and was unable to go up stairs during that time except with difficulty. I declined treating her any farther, unless an examination was permitted, and left her. I was sent for that night, and found her affected with hysteric convulsions, which yielded readily to tinct. assafœtida, and the next morning I was permitted to make the examination.

On examination, I found it to be a case of anteversion, with an inclination of the fundus of the uterns into the right iliac region, while the os uteri was pressed into the cavity on the left side of the rectum. Considerable inflammation of the uterus existed, which was so exquisitively sensitive, that no effort at replacing it could be tolerated.

I gave her antimonials, with a view of removing the inflammatory tendency, and directed vaginal injections of warm water, but this rather aggravated the pain. I afterwards used cold water, without any apparent advantage. I then resolved to apply the electricity as a derivative. I placed her feet in warm water, with the negative pole of the battery, while she held the positive over the sciatic nerve, on the upper part of the thigh, immediately below Poupart's ligament of each leg, alternately: the application was continued for about three minutes, and was made twice a-day until the 24th, with manifest improvement of health.

On examination this day, I found the engorgement of the uterus much diminished, and that it had assumed its upright position, though there was considerable prolapsus; it has also entirely lost its sensitiveness.

Her digestive functions were much improved. I gave her, in pill, cit. ferri et quinine, gr. i, to be taken twice a-day, and continued the same application of electricity until the 29th. She is now able to sit up most of the time, and walk about the room. I directed her to sponge her body with salt water every morning, and to continue the tonic pills.

June 30th, called on her for the last time, being about to leave town, and directed her, as it was convenient, to take a cold shower-bath every morning. Found the uterus in place without even a slight prolapsus. She thinks she is well. The catamenia has not appeared. She complains of slight pain in the back. Directed her to use a tepid hip-bath, and to take some warm teas, to induce diaphoresis: this relieved the pain in her back, without bringing on her courses.

On that day I left for Mobile, and did not hear from her until I called on her on the 20th of August. I found her apparently well in health, but looking dejected. Her mother informed me that her general health had been good, but that, on the 1st of July, she was attacked with spitting of blood, and that she spit up about half a tumbler-full each morning for four mornings, and that, at about the first of August, she commenced vomiting blood, that she vomited up a considerable quantity in three days, that they sent for a physician, who gave her salt and water, which stopped it; that she was gloomy at times, thinking that she must be in the last stages of consumption to throw up so much blood. I found that her catamenia had not appeared. I commenced that day applying electricity as before; told her, that I would prevent another attack of spitting blood, by re-establishing her courses. I continued the application, with no medicine, until the 28th, when I directed her to use a warm hip-bath, and immediately afterwards I placed her feet in the bath with the negative electrode, while the positive was placed over the lumbar nerves. I had not continued the application more than three minutes before the menstrual discharge commenced taking place; it continued for five days: she has been regular ever since, and has enjoyed good health. I would here remark, that the immediate effect of each application as applied, in the above case, was to increase temporarily the size of the limbs, by increasing their circulation, so that the calf of the leg measured half an inch more in circumference, after each application, than before; and by thus daily continuing the exercise of the muscles, producing a permanent increase, until they attained their normal size.

I will here read to the society the notes of a few cases which will exhibit the beneficial effects of this agent when applied perseveringly through the extremities as a revellant in those cases of sterility depending upon engorgement, or a tendency to inflammation, of the uterus or its appendages.

Mrs. A—— had suffered from irritable uterus after the birth of her last child (six years), and with attacks of hysteritis at each menstrual period, and with consequent derangement of general health. In March, 1843, I commenced making the application of electricity through the extremities, by placing the negative pole of the electrical machine in a foot-bath with her feet, while she held the positive in both hands. I made this application three times a-week. She had no return of the hysteritis at her next menstrual period, and in a few weeks was relieved of the irritability of the uterus. The application was continued for upwards of three months. Her general health was restored, and she has since become the mother of a pair of twins.

Mrs. W—— has suffered with hysteria and hysteritis for several years, yet has still continued to have children, until her last, which is three years old. After the birth of that child, she was partially paralyzed in the left side, which became atrophied; the limbs but about half their original size: muscular action was not much impaired, and sensibility was nearly normal, nutrition only being seriously impaired. I employed electricity through the affected side twice a-week, in the manner directed in the preceding case, and occasionally through both sides, from July 1st until December of 1843. Nutrition of the affected side was restored, and it regained the normal size, and her health remained good until February, 1845, when she died of puerperal fever, leaving a pair of twins.

Mrs. Y. sent for me on the 2d of September, 1844, to relieve her of a neuralgia under which she was suffering. She had an acute, lancinating pain between the shoulders and supra-orbital neuralgia, together with partial paralysis of the lower extremities, bearing-down pain in the lower part of the abdomen, with considerable enlargement, which had existed from the birth of her child (three months). I applied electricity through the extremities, as in the preceding cases, as well as locally to the supra-orbital nerves, but avoided the lumbar and sacral nerves, on account of the uterine engorgement. This application was made every other day for twenty days, and she was entirely relieved both of her cephalalgia and uterine derangement. She has since remained in good health.

On the 15th of February, 1845, I was called to see Mrs. M——, aged 22. The auditory nerves were paralyzed during an attack of congestive fever, either as an effect of the congestion, or of the quinine administered during her illness in the fall of 1842. I was informed that she had never enjoyed good health since that time, and was exceedingly nervous and excitable. The object in calling me in was to get the hearing restored, if possible. I passed electricity through the course of the auditory nerve daily, by introducing a conductor into the Eustachian tube, while the other was applied to the external meatus. I also occasionally applied it through the extremities, for the purpose of relieving the nervous irritability. This application was made until the 12th of March, with but little improvement in hearing. Her husband informed me, however, that she passed through her menstrual period during her first week's application, without pains, for the first time since her sickness in 1842. Her general health improved rapidly from the the first application, and I am informed that she is entirely relieved of her dysmenorrhœa.

MAGNETO-ELECTRICITY AS A PARTURIENT.

As a parturient, I think this agent far preferable to the use of ergot, for this reason, that the pains are regularly intermittent, as in natural labor, and hence will not be so likely to injure the child or mother. It does not appear to act, in these cases, as when applied to other parts of the body, where direct muscular contraction is produced, but rather appears to resuscitate the exhausted energies when applied in protracted labor, and to induce pains and regular labor, after a short application, through the lumbar nerves, in the last months of pregnancy. I will present a few cases demonstrating its parturient effects.

On the 3d of February, 1843, I was called to see Mrs. G., who was suffering from severe frontal neuralgia. She had suffered an abortion three years before, since which time her catamenia were irregular, and she had suffered under a continued train of nervous symptoms,—had been carried from one watering-place to another,—had been under the care of several physicians, with no benefit. She was, at this time, suffering from the frontal neuralgia above alluded to, affecting the left supra-orbital nerve. When she looked at objects with both eyes, she was troubled with double vision: the object seen by the right eye was smaller than the left, though occupying the same place, owing, probably, to the different focal distance of the two eyes. She could not see small print with sufficient distinctness to enable her to read it.

The abdomen was enlarged so much, as to induce me to think that she must be at least five months pregnant, but both her husband and herself informed me that the enlargement was of more than a year's standing. I applied electricity to the nerves affected with neuralgia, without affording more than temporary relief. At nine o'clock at night, I called, and placed her feet in warm water, with the negative pole of the battery, while the positive was placed on the back between the shoulders, and passed a rapid succession of the magneto-electrical shocks for about three minutes, which produced slight pain in the back. I then left her. The pain continued, with increasing severity, until about twelve o'clock, when she sent out for her usual family physician, who was in the immediate neighborhood. Pains recurred regularly, after his arrival, for about half an hour, when a large quantity of water was discharged from the uterus, and the pains ceased.

On the 7th, I made application in the same way again, which brought on pains, and a further discharge of water, containing floculi resembling the skins of white grapes. I did not see the substance discharged, but have no doubt, from the description, of their being hydatids. I made the same application every third day for six weeks, with an occasional discharge. She rapidly improved in general health, and shortly afterwards became pregnant, and has remained in good health until this time.

In February, 1844, Mrs. ——, in the last month of pregnancy, was visiting at my boarding-house in New Orleans. She was complaining of rheumatic pains in her knees. My landlady having seen me apply it frequently for rheumatism, got a machine from my room, placed one of the poles in the stocking, over the nerve back of the ancle bone, while she held the other in her hands. She applied it in the same way through both of her limbs. In about thirty minutes she was taken in labor, and I arrived, at dinner-time, just in time to attend to her: the labor was very short, occupying only about thirty-five minutes.

In May, 1844, I was called to see a negro woman belonging to Mr. P——, of this place, who was supposed to have the dropsy, by the family. She informed me that her legs were swelled, and her abdomen had the appearance of fifth month of pregnancy. She had not menstruated since the birth of her last child, which was two years old. She had an attack of bilious fever in the summer of 1843, followed by a protracted intermittent, which held on all winter. Spleen was much enlarged. She had no morning sickness, nor any of the usual signs of pregnancy except the enlargement.

On examination, I found but little enlargement of the legs, except that occasioned by varicose veins on the inside of the thighs, and every appearance of pregnancy. But she insisted that she could not be in that condition; and, thinking that possibly the enlargement might be occasioned by suppression of the menses, I applied electricity, as a direct emmenagogue, by placing the negative pole in foot-bath with the feet, while the positive was placed over the lumbar region, and a succession of shocks passed for about five minutes. No pain was produced. I waited a few minutes, and left, directing them to send for me if labor-pains occurred. In about two hours I was sent for, and found that regular labor pains had been recurring at intervals of about five minutes for half an hour. I found, on examination, the os uteri dilated, and a prospect of speedy delivery. I immediately administered a dose of opium, which suspended the pains. I left another dose, in case of the pains returning. She fell asleep, and they did not recur. A week afterwards, motion of the child was felt. She went her full time, and was delivered of a healthy child. Both mother and child are well.

IX.—*An Inquiry into the Existence of a "Vital Principle," considered as an Entity independent of the Phenomena of Life.* By JOHN HARRISON, M. D., Professor of Physiology and Pathology in the Medical College of Louisiana.

[The following Paper has already been published in the Appendix to "An Essay towards a correct theory of the Nervous System." It is republished in this Journal, because we think the subject one of great importance,—one, concerning which, right notions are absolutely requisite to the progress of physiology. We republish it, also, because the writings of Liebig have stirred up the question anew, and invested the subject with much interest. Besides, through the medium of the Journal, it may, we hope, reach those who have never seen the work in which it originally appeared.—In the Excerpta will be found the opinions of Mulder, the celebrated Dutch chemist.]

Physiology is the science of life;* but if there be such a science, it must contain general principles applicable to all living beings,—to the simplest plant as well as to the most highly organized animal.

We say, that the mushroom is endowed with life, and that man is also so endowed:—in what do beings so dissimilar agree?

By observing living beings attentively, and comparing the phenomena they present with those of brute matter, we discover two important facts. First, we observe that all, without exception, appropriate to themselves, by some means or other, certain substances external to their bodies, which substances we term *aliment.* Secondly, we observe, that if this aliment be withheld, there ensues, at a period more or less remote in different cases, a total cessation of all the phenomena of life. We know, besides, that the aliment always enters the system in a fluid state, and as the supply of aliment may be considered constant during life, and as there occurs no accumulation of this fluid in the living being, it follows, as a matter of necessity, that this fluid must undergo some change in the economy, and this change itself be indissolubly connected with the essential actions of life. We know, in fact, that it undergoes a metamorphosis—that it is transformed into solids, and thus becomes a part of the frame-work of the living being.

But if the solids of living beings be thus constantly appropriating to themselves substances from without, it is obvious, that, unless there be some means by which the solids themselves are consumed and removed from the body, there will exist no limit to growth, from the commencement of life to its termination.

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* "Physiology," according to present usage, treats of the laws, organs, functions, &c., of life; "Physics," not so. Now, *quære:* the etymological import of the two words being the same, is the difference in their application accidental and arbitrary, or a hidden irony at the assumption on which the division is grounded? Φυσις ανευ ζωης, ανευ λογȣ; or, λογος περι φυσεως μὴ ζωης εϛι λογος αλογος.—*Coleridge.*

In animals, this consumption of the solids really occurs: in vegetables, with some few cases of exception,* it does not.

For the aliment of plants passes from the fluid state to the solid—from a state of motion, to that of repose, (which endures generally as long as the plant continues to be a living being, and, in many cases, for a long time thereafter) without suffering further change than what occurs from a condensation of its substance, either by loss of water, or the appropriation of earthy bases.

In animals, on the other hand, there is a continual decomposition of the solids going on, at the same time that they are renewed from the aliment.

This consumption of the solids of animals is owing to the influence of oxygen, which they take into their systems by means of the respiratory apparatus. The carbon and hydrogen of the solids unite with the oxygen, forming carbonic acid and water; and at the same time, caloric, so necessary to the vital actions of animals, is generated and evolved as animal heat.

Herein animal and vegetable life totally differ; for the vegetable takes in carbonic acid gas, appropriates the carbon, and eliminates oxygen; animals, on the contrary, take in oxygen, and eliminate carbonic acid gas.

Assimilation, then, or the transformation of the fluid aliment into the solids, is a phenomenon common to all living beings. But when we come to compare one of these beings with others, we are struck with the facts, that they have not the same outward forms; that the aliment of one is not that of another; that the mode of appropriating to itself that aliment differs in each species; that their physical properties and chemical composition are diverse; in short, that in most of these particulars, one may differ from another as much as any two things in existence.

If we now enter the interior of these beings; if we examine with attention their structures, and compare them with one another, the like facts are again brought before us, namely, a striking dissimilitude of one to another: while we find some to possess a heart, stomach, lungs, liver, &c., we find others not possessed of even one of those organs.

In what, then, do beings so unlike as man and a vegetable agree? I have already said, that a transformation of the fluid aliment into solids composing the frame-work of the system, was a phenomenon common to all living beings. This, then, is their *physiological* agreement. Examining their structure, we find them to coincide in two points, and two points only; namely, the material of neither is homogeneous, but is composed of solids and fluids; and the solid portion of both is porous; that is, the fluids can penetrate intermolecularly the solids. Again, if we go back to their origin, we shall discover, that they have not always existed as we find them. The specific form has been built up from other forms: man has become such from an embryo; the oak has grown up from an acorn. This, then, may be called their *historical*

* These cases occur in the flowering of plants, maturation of the fruit, and germination of the seed, in all which oxygen is absorbed, and carbonic acid gas given off.

agreement. And these are the points in which not only man and a plant, but in which all living beings agree one with another. In any other particular they may differ: here they are found to agree universally.*

The nature of life, then, it is obvious, must be sought for in those acts in which all living beings agree; which, as we have seen, are a transformation of the fluid aliment into the solids, and the formation of the adult structure.

From observation, we learn, that the aliment invariably enters the system in a liquid state, and is then termed the *nutritive fluid*. It afterwards may become solid, and in that state form an integrant part of the system. Now, it is amongst the molecules of the tissues that investigation has ascertained this change from the fluid to the solid state to occur; and, of course, if the solid molecule again becomes fluid, and is removed from the system, that change must also occur in the same spot.

Moreover, it is obvious, that there must exist a close chemical relationship between the nutritive fluid and the solids, since the latter have been formed from the former; and we know, besides, that most of the solids are easily converted into fluids. In fact, we find in the nutritive fluid all the chemical elements found in the different solids and secretions, which differ from each other merely in possessing a few elements more or less, or the same elements existing in different combinations.

We have now all the points in which living beings agree, one with another. They agree *anatomically* in the *co-existence of solids and a nutritive fluid* of close chemical affinity, and capable of being resolved into each other; and secondly, in *porosity of texture*, so that the nutritive fluid may penetrate intermolecularly the solids.

They agree *physiologically* in the great phenomenon, *nutritive action* —to which absorption is antecedent, and of which assimilation and secretion are consequences.

In nutritive action, therefore, we must seek the reason why living beings present phenomena so widely unlike those of brute matter.

I commence the inquiry with an extract from the celebrated chemist, Berzelius.

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* I am aware that physiologists mention other points of agreement, but it can be shown that they are all referable to the above. Thus Cuvier,—"Absorption, assimilation, exhalation, development and generation, are functions common to all living beings; birth and death the universal limits of their existence; an areolar, contractile tissue, containing within its laminæ fluids or gases in motion, the general essence of their structure; substances almost all susceptible of conversion into fluids or gases, and combinations capable of an easy and mutual transformation, the basis of their chemical composition."—*Règne, Animal*, tom. i.

It is true that all living beings absorb, but absorption is not peculiar to living beings; it occurs in dead substances, indeed in all porous bodies.

Assimilation, development, exhalation and secretion are consequences of nutritive action; whilst generation consists merely in placing a substance (the ovum) furnished by the parent under such conditions that it can absorb the nutritive fluid, and thus take on vital actions.

Again, birth is the mere separation of the offspring from the parent; and death, but the necessary cessation of one kind of action, and the transition into that of another.

"A living being, considered as an object of chemical research, is a laboratory, within which a number of chemical operations are conducted; of these operations, one chief object is to produce all those phenomena which, taken collectively, are denominated 'Life;' while another chief object is to develope gradually the corporeal machine or laboratory itself, from its existence in the condition of an atom, as it were, to its utmost state of perfection. From this point of utmost perfection, the whole begins to decline as gradually as it had been developed; the operations are performed in a manner less and less perfect, till at length the being ceases to live; and the elements of which it is composed, again set free, obey the general laws of inorganic nature."*

But although it is true that chemical compounds are formed in the living being, and that these compounds may be resolved into the elements which we find in brute matter, still by no means whatever can the chemist work backwards, and again form those organic compounds from the elements which he has set free. Elementary principles, such as carbon, oxygen, hydrogen and azote, may be liberated from fibrine or soluble albumen, and, moreover, the exact proportion of each be ascertained, and yet all the art of all the chemists has never yet been able to make them enter again into combination, so as to reproduce fibrine or albumen. How is this?

Without occupying our time with the "Moving Principle" of Aristotle; the "Anima" of Stahl; the "Archæus" of Van Helmont; the "vis medicatrix naturæ," and a thousand other vagaries of the imagination, let us come at once to the state of physiological science at the present day.

On this subject, physiologists are divided into two sects.

The first of these maintain, that the phenomena of life must be referred to the agency of a *force* or *power*, distinct from, and opposed to, the general affinities of brute matter.

The other sect maintain, that the existence of any such force is a pure hypothesis; that there is no necessity, or even good reason for its introduction; and that all vital phenomena, so far from being opposed to the general laws which regulate the operations of matter, are in reality but so many various manifestations of those laws.

The first support their tenets in this way:—

It is admitted that those compound substances which are formed in living bodies cannot be imitated by the chemist. Let him bring carbon and oxygen and hydrogen together as he please, he cannot, with his greatest skill and all his agents, produce one single compound such as is produced in living beings. He can neither form starch nor sugar, nor lignine, all of which are organic compounds formed of these elements in different proportions. Now, what is the art of the chemist? In forming compounds from elementary substances, what does he do, what can he do, but bring those substances in contact with each other? If they have affinities for each other, he can no longer control them;—they must, of necessity, obey those affinities;—he can neither limit, nor increase, nor diminish their play after it has once commenced.* These

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* Traité de Chimie, tom. v.

† Ad opera nil aliud potest homo, quam ut corpora naturalia admoveat et amoveat reliqua, natura intus transigit.—*Nov. Organum.*—Aph. iv.

elements, then (carbon, oxygen and hydrogen), will not combine, of their own natural affinities, one to another, so as to produce an organic compound. We are, therefore, obliged to refer the formation of these compounds to another *force*, more powerful than the affinities of their elements, one to another. This force we term the "Vital Force;" and we use the phrase to designate some unknown power which we infer to be in operation, from the results we see. In truth, so far from framing an hypothesis, we are, in reality, only adhering more closely to phenomena. And the case is precisely the same with all other subjects of inquiry in physics. There are, in every direction, ultimate facts beyond which we cannot proceed. The word "attraction" is used to designate that tendency which one mass of matter has to approach another mass;—the word "affinity," to distinguish the approach of the particles of a substance to those of another. But in either of these instances, is it possible to go beyond the visible phenomena? Has the unknown cause why bodies approach each other ever been revealed? Assuredly not. The words *attraction* and *affinity* are used merely to represent certain actions in matter, the efficient causes of which are unknown. In like manner we use the words "vital force." We employ the term to distinguish certain other actions in matter, which we see, in their results, to be different from those actions represented by the word *affinity.*

Concerning the intimate nature of this force or power, we feel it to be useless to speculate; just as it is useless in the case of attraction or affinity. Vital actions are ultimate facts, in the same way as chemical combinations, or the revolution of the planets, are ultimate and inexplicable.

Again, we see every day that organic compounds may or may not possess life. Sugar and starch are organic products, and they may exist in the living vegetable or out of it; that is, they may be endowed with life or not. Fibrine or albumen may exist in the vital state, or in that of brute matter: therefore, life must be a principle superadded to common matter; for if it were not, there would be no difference between fibrine and albumen in the living body, and fibrine and albumen out of it.

Moreover, it is idle to talk of Life as the result of organization. Attentive observation of phenomena warrant no such conclusion, but, indeed, rather the reverse. It is true that life is never met with, and cannot be conceived of, apart from organization; but is organization ever met with, which, at one time or other, has not been endowed with life? Is not, in fact, the organization built up by what we call the *vital force?* What two things on earth more dissimilar than the human being in the flush of manhood, and the embryo a week old? Where a greater difference than between the full-grown oak and the acorn? And yet the embryo and the acorn were the primitive forms of these two beings, man and the oak. And what has thus built them up, but the vital force? Abstract this force, or principle, or whatever you choose to term it, from the acorn or the embryo, and what is the consequence? The acorn will never become an oak, nor the embryo a man. *Then* it is, that the usual, the ordinary affinities of matter come in play;—after the removal of this controlling power, the *vital force.*

The chemical elements, no longer subjected to a superior power, obey the general laws of brute matter: particle obeys the call of particle, and the organic fabric is resolved into its elements, or moulded into new compounds.

Such are the arguments used by one class of physiologists. The reasons urged are unquestionally specious, nay, convincing to those who are not accustomed to close and abstract reasoning. But let us look a little more closely at the matter.

Life, in the other view of the case, is a general term, employed to express a great variety of phenomena, which have agreement in certain points, but may be altogether different in others; just as the word "quadruped" is used to designate certain animals, which may resemble each other only so far as they all have four legs: for that this is really the case, one may satisfy himself by solving the question, what are those phenomena common to both a bird and a plant? It will be found that they agree only in possessing an areolar tissue, in which certain actions are constantly going on. The results of these actions being different from what is observed to occur in brute matter, it has been thence inferred, that the chemical elements which enter into organic compounds are forced into combination by a distinct power superior to those general affinities wherewith, observation informs us, all matter is endowed. The reasons advanced we think inconclusive and unsatisfactory.

Because (to commence this subject with a criticism) this thing which has been a *force*, a *principle*, an *agent*, &c., just as happened to suit the fancy of the employer, must necessarily be in one of three categories. It must be either a substance, the property of a substance or substances, or it must be merely expressive of a condition or state of things, which condition may be one either of repose or action—of rest or motion—static or dynamic.

Were life a *material* substance, it must of necessity possess the general properties of matter, and so be cognizable by the senses; and that such is not the case, it is scarcely necessary to mention. Nor can life, with logical strictness, be assimilated to the imponderables. Of the existence of light, heat and electricity, we have the same evidence that we have for the existence of any thing external to us, viz., that of our senses. For the existence of life as an entity *per se*, we have no such evidence. Moreover, we are acquainted with many of the properties of these imponderables; they are the subjects of extensive sciences;—but what properties of life are we acquainted with? Those properties, termed *vital*, such as *sensibility* and *contractility*, are the properties of certain tissues and organs existing under certain circumstances, not of a particular substance called "life."*

* Bichat has no less than five of these *vital* properties. Later authors have reduced them to two, *contractility* and *sensibility*.

Now, what is contraction? It is but a closer approximation of particles, so that the volume of the whole mass is diminished. If cold be applied to the skin, it contracts;—if we prick or galvanize a muscle, or if we exert our will, it contracts. We know, in the last case, that if the nerve leading to the muscle be divided, no contraction will ensue. Hence, in all cases, there is some material cause (whether we have discovered it or not) producing the effect.

Those who would make life an *immaterial* principle, are surely guilty of framing a very vague hypothesis, which, indeed, may be very convenient, as it puts a stop to all further inquiry, and may be acceptable to those who suppose they learn any thing from admitting it, but which we have just as much reason to frame for the solution of any other phenomenon, and just as much reason to insist upon. But, in truth, this is one of those hypotheses which can neither be maintained nor refuted, since it is impossible for the human mind to frame any distinct notion of such a principle. It is a sound, and nothing more.

Admitting, however, for the nonce, that it is possible to attain a clear conception of it, have we escaped our difficulties? Review the great variety of living beings; observe the innumerable diversities of form, and the multitude of organic productions. Has each species of living beings a *vital principle* peculiar to itself? If the answer be *yes*, the difficulty is by no means got over: the admission must extend far beyond this. A separate vital principle must be framed for every tissue and organ, the functions of which are different.* Now, there is a time when the embryo exists without any of the organs being yet formed: where, during this period are their peculiar vital principles?

With regard to the different tissues and organs, and the functions they perform, it may be argued, that though the vital principle is identical in all, yet different effects are produced, because the materials on which it operates are different.† But this very argument carries with it the admission, that there are other causes besides the existence of a vital principle, why gastric juice is secreted from one organ, and urine from another; why muscular fibre is deposited in one place, and bone, &c., in another. After such an admission, the *vital principle* may be rejected entirely; it is no longer necessary to explain vital phenomena.

Again, plants and whole tribes of the inferior animals are capable of propagation by division. By cutting to pieces a single individual, we may make hundreds, each of which will possess an independent life. Have we, in this case, divided the vital principle? How can we imagine the division of a thing which is not material?

Contractility is inseparably connected with the peculiar structure and chemical constitution of the tissue which contracts, and is, therefore, as much a *physical* property as any other.

Contraction, moreover, is not a phenomenon peculiar to living beings: it occurs also in dead substances. Metals expand by heat, and contract from cold; wood expands by absorbing moisture, and contracts when robbed of it. (*a*)

Sensibility, also, can only be termed a *vital* property, in the sense that none but living beings manifest it. But neither do all living beings possess it; nor all parts of the living system of sentient animals. It is, therefore, not essential to life.

* It is considered by many, and perhaps truly, that we are not yet prepared for a generalization of so high a kind, (that is, the hypothesis of life, being a simple principle,) or, at least, that it would be more convenient for the analysis of vital phenomena to consider life as made up of several principles differing in their nature."—*Notes to John Hunter's Principles of Surgery*, by James F. Palmer.

† Such was the opinion of John Hunter.

(*a*) The difference between this species of contraction, and that of the muscles, has been pointed out in the Essay on the Nervous System.—See Chapter 10.

If life be made a separate and distinct property of matter, the enormous difficulty presents itself, for us to conceive how matter can possess a property that is moveable,—that may reside in a certain substance to-day, and, *without the occurrence of any change in its structure or chemical constitution*, be gone to-morrow,—which must occur if death can take place without disorganization, as the vitalists contend.* That the properties of a substance may be dormant, it is easy to conceive, because it may have never been placed under those circumstances necessary for their revelation; but that matter, without a change of form, or of the disposition of its molecules, can take on and put off again a property, is, we humbly think, an impossibility—a proposition irreconcileable with the notions of matter that our minds are constituted to entertain; for, (to come to the pith of the matter,) what is the real meaning of the words, *force*, *power*, *property*, &c.?

In strict philosophy, observation is the only admitted means we possess of obtaining knowledge. Observation, and reflection upon what we observe, are the limits which bound in all human science, and beyond which the human mind cannot advance. "Homo, naturæ minister et interpres, tantum facit et intelligit, quantum de naturæ ordine, re vel mente observaverit; *nec amplius scit aut potest.*" To this aphorism of Bacon, our reason gives assent at once, for it requires no great expenditure of logic to prove that a dream is to be valued but as a dream; that an hypothesis, until proved to be true, is but an assumption; in short, that in leaving the path pointed out, we turn aside from nature, to pursue the meteors of the imagination.

Keeping, then, our senses intent upon facts, what do they reveal to the mind? The existence of unremitting activity; of change—incessant change; of phenomena succeeding phenomena in endless rotation! But for any of these phenomena, where shall we find an ultimate reason? True, we may often take to pieces, as it were, some particular fact, and show that it is made up of a series of more general phenomena. But for any one of these general or *ultimate* facts, as they are called, who shall give a reason? Why may it not have happened otherwise? or exactly the reverse?

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* "I have observed that animal matter may be in two states; in one, it is endowed with the living principle, in the other it is deprived of it. From this it appears, that the principle called life cannot arise from the peculiar modifications of matter, because the same modification exists when this principle is no more. The matter abstracted from life appears at all times to be the same, as far as our senses and experiments carry us."—JOHN HUNTER, *Principles of Surgery*, chap. ii.

"Our ideas of life have been so much connected with organic bodies, and principally those endowed with visible action, that it requires a new bent to the mind, to make it conceive that these circumstances are not inseparable. I shall endeavor to show, that organization and life do not depend in the least on each other; that organization may arise out of living parts, and produce action, but that life can never rise out of, or depend on, organization. An organ is a peculiar conformation of matter (let that matter be what it may) to answer some purpose, the operation of which is mechanical: but mere organization can do nothing, even in mechanics, it must still have something corresponding to a living principle, namely, some power."—HUNTER, *On the Blood, &c.*, chap. i., sec. vi.

These questions none can reply to: but our minds are so constituted, that when we see an event occur, the belief immediately arises, and is irresistible, that the same event has ever occurred, and will again and forever occur, when the co-existing circumstances are the same. This belief is itself an *effect* as inexplicable as any other in the universe.

Therefore, beyond observation, and our instinctive belief in the uniformity and invariableness of events when circumstances are the same, we know and can know nothing. What we term an explanation of a phenomenon, is a mere detail of other separate phenomena, which, occurring in succession, have ended in the one to be explained, but all of which were previously the subjects of observation. When the chemist produces water by the mixture of oxygen and hydrogen gases, he presents us a simple fact—ultimate and inexplicable. If he attempts to give a reason for it, and tells us the union is caused by the *reciprocal affinities* of the two substances, he but repeats the fact in another form, since *affinity* is but a word denoting that very tendency to union which he attempts to explain by it. But when the natural philosopher explains why liquids rise in exhausted receivers, he seems to do something more, since he shows us how the result came to pass. Yet if we look more sharply into the subject, we shall find, that he has done nothing else than to repeat, in a connected order, a series of minor phenomena, with most of which we were already acquainted. He shows us that the atmosphere, like all other terrestrial bodies, tends to the centre of the earth, and therefore presses with a certain weight upon its surface: he then calls our attention to the constitution of fluids, and the hydrostatic fact, that they press equally in all directions: finally, he proves to us that the pressure of the atmosphere has been removed on a portion of the liquid, whilst it is continued on the remaining portion; so that the rise of the fluid in the receiver is nothing new in itself, but is merely a new form in which old phenomena are evidenced.

Such, then, is really the philosophical statement of the subject. We can give no reason whatever for any simple phenomenon. Between two separate events which occur in immediate succession, we can observe no necessary connection; so that no person can predict any event which is single in itself, and has not heretofore been observed: a fact, of which the progress of chemistry furnishes us abundantly with daily proofs. The rest of this subject is, therefore, but a question concerning the use of language—the application of certain words, as, *cause*, *effect*, *power*, *force*, *susceptibility*, *property*, *quality*, &c.

It must be kept in mind, that in all the operations of matter, there is both action and reaction. In any change occurring between two bodies, one is not solely active, and the other merely passive, but both are agents and both patients. When a child puts his finger into the flame of a candle, it is not the burning body only which is active; for although it has produced pain, it has also suffered—the finger having robbed it of a portion of its caloric. Or, perhaps, a better illustration may be drawn from the magnet, which, if it be fixed, will draw a needle to itself; but if free to move, and placed in the neighborhood of a large mass of iron, will itself be drawn; and if we use two magnets, placing, within a certain distance, the north pole of one opposite the south pole of the other, we shall see them *both* moving, and approaching each other. Now, accordingly as we intend our minds on any particular portion of the change, are we disposed to employ the words, *cause* and *effect.* If magnet A be the object of our attention, we would say, that magnet B is the cause of its motion. If we attend more particularly to B, we say its motion is caused by A. The *causes* of any change whatever, lie therefore, in all the bodies engaged in that change; as likewise, the change itself (or effect) is never confined to but one substance.

But in a vast number of the events which occur on our planet, a portion only of the change produced is perceptible, the other portion not being discovered except after much investigation. For instance, in the example already given, every one experiences at once that a change is produced in the condition of his hand when it comes in contact with a burning body; but the change undergone by an incandescent substance is not so readily perceived. It is from this circumstance that the words *cause* and *effect* are employed as they usually are. The burning body is supposed to be the only substance which is active, because we do not perceive the change it has undergone in itself, and we, therefore, say that it is the cause of the pain produced, or more loosely still, that the blaze has caused the pain. We also say, "the fusion of metals is caused by the action of fire," because we attend to the change undergone in the metals, but not to that undergone by the burning body. But there are really causes on both sides, and effects on both sides. This truth is frequently only evident to attentive observers, particularly when the substances remain apart after the change has been undergone; yet, when the bodies coalesce, as in chemical unions, it may be made apparent to every one. Mix sulphuric acid and lime-water: a change will ensue, and we shall no longer recognize either earth or acid. If any one should now say, that the acid is the cause of the change in the lime, another, with equal justice, may say, that the lime is the cause of the change in the acid; since the actions which result in the formation of the neutral salt are reciprocal.

To the same circumstance, namely, that in many cases a portion of the change undergone is not perceived by us, or is unattended to, is owing the employment of the words *power* and *susceptibility.* We use the word *power* more strictly in reference to the producer of the evident change; whilst the term *susceptibility* is employed in reference to the body that evidences the change. "Fire possesses the *power* of melting metals, and metals are *susceptible* of fusion by fire." The change undergone by the caloric escapes our observation, whilst that undergone by the metals is attended and referred to. But, as I have already said, *power* is exerted by both substances, and both manifest *susceptibility* of change.

The words *property* and *quality* mean the same thing, or differ only in their derivation. They are usually employed to denote what will happen conditionally. We say, "that water has the property of dissolving salt; or, that it is a quality in water to melt salt;" and we mean merely, that if water be placed, under certain conditions, in contact with salt, dissolution will take place. By these words, therefore, we refer more particularly to the future, and express by them our belief that bodies, when placed in such or such circumstances, will develope certain phenomena. When, therefore, we enumerate the properties by which any body is known, we speak of the co-existence of certain powers, or capabilities in that body to produce certain effects, and can mean nothing else; whether the existence of these powers is to be made apparent by immediate operation on our senses, or whether they shall affect us through the medium of other bodies.

Force is another abstract general term, and is strictly synonymous with *power;* for the force of any body is that body considered as producing, or as capable of producing, some particular effect. "A cannon ball flies with immense *power*," or, "with immense *force*." In these phrases the words are convertible, and I know of no other in which they are not so.

As *force*, then, is nothing more than a word expressing the action of matter in producing change, and as the changes produced will of necessity vary according to the relations in which the bodies engaged stand with regard to each other, it is obvious, that just as we change those relations, we may multiply forces. Hence it is, that, with regard to different species of effects, we have so many *forces* recorded as having produced them; such as the force of *attraction*, of *repulsion*, of *gravitation*, of *affinity*, of *cohesion*, of *elasticity*, of *inertia*, and hundreds of others. These general terms are of great use when they are taken for what they really are, merely short forms of expression; for without them, it would be necessary to go over in detail all the subjects of science, which, in these terms, are thus concisely referred to.

Matter and force, then, are not entities distinct and separate. Matter, when closely scrutinized, reveals itself to the inquirer as a principle of constant and unremitting activity; whilst force is the manifestation of this activity in the production of change. "Power," says Locke,* "makes a great part of our complex idea of substances;" but the truth is, there is not a notion we can frame of matter in which power is not essentially a component: for there can be no bodies without qualities of some kind; nor, indeed, do we, nor can we, know any thing of bodies except their qualities; and those qualities are but the expressions of so many powers in bodies capable of producing so many various effects.

The *inertia* of matter is a thing inconceivable, if the term be used to signify that matter is *powerless*. The true meaning of the phrase is simply this: "no body can change its condition without the intervention of some other power."† But can we conceive a body, either in motion or at rest, to be absolutely *inert*—void of power? Surely not; since in either state it is capable of acting on us and upon other bodies. Nay, in either condition, it is *continually* exerting its activity; for if it be in motion, its activity is manifested by the motion itself; and if at rest, its particles are bound down, each to each, by the *force* of attraction.

We are acquainted with matter through the medium of our senses. It is, therefore, relative to us; for were our nervous substance altered,

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* Essay on the Human Understanding, Book II, chap. ii, § 7.

† "To say that matter is inert, or has *inertia*, as it is termed, is only to say that the cause has been expended in producing its effect, and that the same cause cannot (without renewal) produce double or triple its own proper effect." —*Discourse on the study of Natural Philosophy, by Sir John Herschell.*

our sensations would be different. In these sensations we experience the effects of something, which, from a fundamental belief, (an inexplicable and ultimate fact in itself), we imagine to exist without us, and to produce them. What that something is, apart from its powers of causing those feelings, it is impossible for us ever to know. But it is evident, from this statement itself, that matter is a principle of activity; for, to produce sensation within us, it must have the *power* of doing so; in other words, it must *act* upon us. And again, when we turn our attention, not to the immediate action of matter upon ourselves, but upon itself, what can be more wonderful and incomprehensible. We see the same occult, mysterious activity exerted when two globules of mercury approach each other and coalesce, as well as in the earth whirling round the sun, or the sun retaining the earth in its orbit. Nay, if we regard closely, we shall find that this activity is exerted unremittingly, unceasingly, at all times, and in all places — as well at rest, as in motion.

Therefore, as force, in its most general sense, expresses *action*, and in a limited sense, a certain *mode* of action; the vital force, in the production of vital phenomena, must be regarded, not as a thing apart from the substances engaged in those phenomena, but as a convenient term used to express a particular sort of action, attended with certain results.*

Without urging other objections, which might easily be found, against the hypothesis of a *vital force*, let us now advance the arguments of the other side. It has already been said, that *life* must be one of three things: it must be a substance; the property of a substance or substances; or it must merely express a condition or state of things. It has been shown that it can be neither of the two former, it must therefore come within the third category.

This condition may be one of action or repose: *vitality* may express the latter; *life*, the former.

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* Inesse corporibus organicis vivis ad unum omnibus peculiarem vim, ipsis connatam, et quamdiu vivunt, perpetuo activam et efficacem, statutam ipsis et destinatam formam generationis negotio primo induendi, nutritionis posthac functione perpetuo conservandi, et si forte nutriate fuerit, quantum fieri potest, iterum restituendi.—*Blumenbach*, *Inst. Phys.*, § 591.

This *nisus formativus* of Blumenbach, (the *vis essentialis* of Wolfe — the *nutritive force — vital force*, &c., of later writers) represents to us merely unknown circumstances. Like the x of the algebraists, we should employ it to represent, not some being existing *per se*, but merely those circumstances in which living matter exists,— which are essential to the phenomena of life, and therefore, the causes of them. These circumstances are — 1st, matter of a certain chemical constitution; 2dly, a peculiar structure in this matter, so that it is rendered permeable to fluids; 3dly, a nutritive fluid, which is to permeate intermolecularly the solid or semi-solid matter, to which it must also bear a close relation in chemical constitution. These postulates being fulfilled, the phenomena of life must necessarily arise out of them. The inherent forces of matter set all in motion, and maintain a particular sort of action as long as circumstances remain the same. The nutritive fluid is absorbed; chemical changes occur between it and the molecules of the solid; and as a consequence, some portions of it are retained in a fixed state, whilst other portions are rejected. In animals, a portion of the solid is also resolved at the same time into the fluid state, and thrown off. In these phenomena we have all the essential operations of life,— absorption, assimilation and secretion.

All living beings, as we have observed, agree in two points; namely, in the formation of certain compounds from their aliment; and in the building up, as it were, from the embryo state, a machine, complicated in many instances, made of tissues and organs, and possessing a certain form peculiar to the species.

It is contended, that the inherent forces of brute matter are by no means capable of producing either of these phenomena. Let us, however, examine this subject more closely, and first, in the formation of organic compounds.

All bodies possess certain properties which are common. Thus, we speak of gravity, extension and resistance, as general properties; because, wherever we find matter, we find it possessed of these properties. But, besides these common properties, there are others which are peculiar, and it is by these peculiar properties that we distinguish one kind of matter from another kind. Thus, gold and silver have common properties, but each of them has also properties peculiar to itself, by which it is distinguished from the other, and, indeed, from all other substances.

But the properties of a substance can be known to us only so far as they have been the subjects of observation. Thus, chlorine was known to possess certain properties; but it certainly could never have been known until the test of experiment, that it possessed that of dissolving gold. In truth, this is a necessary corollary, from the fact, that the properties of bodies are nothing apart from the bodies themselves, but mere words, employed to designate certain phenomena, which will appear, if certain circumstances co-exist. Now, we cannot see any one substance in every possible relation with all other substances, and therefore it is plain, that *all* the properties possessed by a substance can never be known to us. In truth, the properties of chemical elements, as set down by the chemists, are only a few of the most obvious; and chemistry is, from day to day, developing the existence of properties before unknown and unsuspected.

If we examine phenomena with attention, we shall find, that in every change in nature there are opposing forces overcome. Such a thing as matter undergoing *spontaneous* changes is inconceivable and absurd. No body will ever undergo change as long as the circumstances under which it was formed remain the same; for in whatever state we find a body, it is exerting constant, unremitting activity, and to change that mode of action for another, some other power must be introduced. But these opposing forces may be stronger than those tending to produce a change; and hence, it follows that a body may possess many affinities which, on account of these opposing forces, remain quiescent and unobserved. Thus gold, it is possible, may have a tendency to dissolve in water, but from the paramount influence of cohesion exerted among the molecules of the metal, the tendency can never be made apparent. For the same reason, "one salt may have a greater affinity for water than another, and yet be less soluble."*

Again, the properties of a substance being merely the manner in which that substance will act under particular circumstances, it is obvious, that an elementary substance cannot assume or lose a property; its

* Turner's Chemistry—General Remarks on Salts.

actions being the result of evident necessity. Therefore, when two or more elements combine, the properties of the compound can be nothing but the development of the original tendencies of the elements; for a compound can be nothing more than the co-existence of the different elementary molecules in juxta-position; and such being the case, it is evident, that there must occur a composition of properties,—in other words, that the elementary molecules must act with a joint influence on those of other substances with which they come in contact. Hence, the great difference between the properties of a compound, and of its elements, when separate. To give an illustration: an alloy of silver and platinum is dissolved when immersed in nitric acid, whilst platinum, when presented alone, resists the action of the solvent. The reason of this is, that though the last-named metal has some affinity for the acid, the paramount force of cohesion presents a union. The presence of the silver undergoing oxidation, exerts its power over the platinum, breaks up the cohesion, and effects the combination.

Therefore, as we have never observed any one substance in every possible relation with all other substances, by which means only we could obtain a knowledge of all its properties, it follows, that after two substances have entered into combination with each other, ths phenomena that will ensue, when a third substance is presented, are altogether beyond prophecy. The chemist himself would think it the most idle and ridiculous thing in the world for any one to make such predictions. He knows too well that the properties of all bodies must be learned by experiment.

Again, as the number of elements in a compound are greater, it is obvious, that the properties of the compound will be more striking and unexpected.

Now, it is in living beings that these elementary principles are known to be most numerous. The compounds of inorganic matter consist, generally, of two elements; rarely of three; and scarcely ever of more. But the simplest organic compound contains three at least, whilst higher in the scale of life their number is greatly augmented. In man, fourteen or fifteen have been discovered. But if the difficulty of foretelling the properties of a compound, from a consideration of its elements, be insuperable, and the attempt idle, what shall we say, when we remember the effects produced by mere difference of proportion?—when we remember that, by a mere transformation or transposition of elements or proximate compounds, we may produce two or more substances of entirely different properties, yet possessing ultimately the same elements in the same exact proportions?—when we know that the mere *presence* of another substance may produce decomposition, or cause the formation of other compounds?—when we recur to the fact, that a body already undergoing chemical change may exercise a power upon other compound substances, and cause new combinations of their elements, without itself participating in that change,—as in the case of sugar, converted into alcohol and carbonic acid by the mere presence of yeast?

We insist, therefore, that it is altogether from misconception of the nature of chemical combinations, that it is so roundly asserted that matter, operating by means of its own forces, cannot produce an organic compound. But why is it, then, that the chemist cannot, in his laboratory, produce an organic compound? Simply, because he cannot place his materials under those circumstances *essential* to their production. Sugar and gum are composed of carbon and water; but regard, for an instance, the multiplicity of forces, conjoined and operating at the same moment, required to make water and carbon unite. In the first place, the leaf must be of a certain chemical constitution, be possessed of a certain structure, and be well supplied with water: there must also exist a certain temperature; light must be present; and the leaf must be of a green color. If any one of these conditions be withdrawn, the union of the two substances will not take place. Now, will it be contended, that the chemical elements of the leaf being given, with the whole structure of the living plant, together with every other circumstance the same, the carbonic acid of the atmosphere would not be decomposed, and that the carbon would not unite with the water?—that, in short, there must be introduced into the case the agency of an occult, mysterious principle, before the union could be effected? Assuredly, those who thus argue are under the *onus probandi;*—they must prove their assertion, or their *vital principle* must remain a most vague and gratuitous hypothesis.

Is it fair to argue from our ignorance? Because we do not know the precise manner in which a phenomenon occurs, shall we boldly assert the existence of new agencies in nature? Why, by such logic, we may multiply new principles almost *ad infinitum.* For instance, the nature of the diamond is perfectly well known; but can the chemist work backwards, and form diamond from carbonic acid gas? Can he, in short, form any of those beautiful gems found in the mineral kingdom, such as topaz, ruby, &c.? He cannot. Then, will it be urged that these inimitable productions of nature were formed by some force superadded to matter? Will some peculiar principle analogous to that termed *vital* be introduced, to account for their formation?* If so, let us at once go back to an exploded philosophy. Let us believe again that nature abhors a vacuum, and that this is a very satisfactory reason for the rise of liquid in exhausted receivers. Let us believe that the splendid researches of modern geologists amount to nothing; and that the impressions of organized beings, found in rocks, are in truth not those of beings once alive, but the productions of *Vis Plastica* vainly laboring in the bowels of the earth, to evolve the forms of life. In short, let us shut at once the book of Induction, and open that of Hypothesis, which, though it really teaches us nothing, yet gives a pleasing

* "Cudworth's hypothesis of a plastic nature has had, however, some partizans, though rather among physiologists than metaphysicians. Grew adopted it to explain vegetation; and the plastic nature differs only, as I conceive, from what Hunter and Abernethy have ealled life in organized bodies, by its more extensive agency; for if we are to believe that there is a vital power, not a mere name for the sequence of phenomena, which marshals the molecules of animal and vegetable substance, we can see no reason why a similar energy should not determine other molecules to assume geometrical figures in crystalization. The error, or paradox consists in assigning a real unity of existence, and a real power of causation, to that which is unintelligent."—*Hallam, Literature of Europe*, vol. iv., p. 109.

play to the imagination, and gratifies our indolence, by relieving us of all farther trouble.*

But it is asked, how comes it that tissues so various in structure, and of such diverse chemical constitution,—how comes it, such various secretions are formed in different localities, and all from one common substance, the arterial blood? It is answered, that these different formations are owing to the different circumstances under which the blood reacts with the solid molecules.

All the organs and tissues of living beings, if they differ in function, are endowed with characteristic marks, which distinguish them from each other. The leaves differ from the petals of the flower,—the muscular fibre, from the nervous substance, and so on. We must therefore believe the interior structure or chemical composition of these organs to be different, each from each. Again, into all the organs of a living being the same pabulum enters: a homogeneous fluid, formed from the aliment, is distributed to every tissue. The same common sap may nourish very different organs and tissues, and produce very different fruit, as we see in grafted trees. The same homogeneous blood is distributed to every tissue in the human body, from which muscular fibre is formed in one part, bone in another, nervous matter in a third, gastric juice in a fourth, and so on. Now, observation tells us, that it is in the ultimate structure that the nutritive fluid comes in contact with the solid molecules, and that there occur those phenomena which are the essence of life. It is, therefore, maintained that these different depositions in different organs are to be accounted for, either by diversity with regard to their chemical elements, or their chemical elements being the same, by a difference in their secondary and tertiary compounds, and the manner in which they are united to each other. Hence will arise diversity of structure; for structure itself is but a *consequence.* In fact, can we do otherwise than infer that, under such circumstances, there must, of necessity, be a difference manifested in the results? Can any one believe, that if the kidney and parotid gland were of the same exact chemical composition, and possessed of the same ultimate disposition of molecules, one of these would secrete saliva, and the other urine, being nourished, as they are, by the same homogeneous fluid? If any one

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* Gestit mens exilire ad magis generalia, ut acquiescat: et post parvam moram fastidit experientiam: sed hæc mala demum aucta sunt à dialectica ob pompas disputationum.—*Nov. Organum*, lib. i., aph. 20.

Partez d'un principe vague, indéterminé; prenez pour base de votre étude une cause de la vie, un principe vital dont la nature, et partout les attributions, sont inconnues, que rien ne restreint, et par conséquent ne précise, qui se prête à tout, parce que n'ayant rien de connu on peut arbitrairement le douer de toutes les facultés imaginables; alors ce principe, qui vous expliquera tout en paroles sans vous expliquer rien en réalité, qui vous expliquera ainsi les faits les plus extraordinaires, certaines ou douteux, vrais ou controuvés, vous y tiendrez avec d'autant plus d' opiniâtreté qu'il favorisera d'avantage votre paresse et votre amour-propre. C'est ainsi qu'on apprend à mépriser l'anatomie, la physiologie positive et toutes les lumières qui peuvent nous fournir les autres sciences naturelles; c'est ainsi qu'on entrave les progrès de la véritable science de l'homme, et qu'on l'empêche de marcher vers la certitude dont elle est susceptible.—*Dugès*, *Journal de Médecine*, tom. ii., p. 347.

can believe this, I ask him, why it is that, when any foreign cause intervenes, we have very different results?—why it is, that when the nutritive fluid or the solids are altered in chemical constitution, or in the mere arrangement of their particles, we have lesions of nutrition and secretion—a production of substances not found in health? Is the *vis medicatrix* altogether powerless?

From what has been said, it is plain, that to account for the formation of organic compounds, we have no good reason for saying that they are not solely produced by the operations of matter upon itself. The chemist, it is true, cannot form albumen or fibrine from the elements of those substances, and for a very plain reason,—he cannot command the circumstances under which they were originally formed, and which were absolutely necessary to their formation.

In reply to all this, we are called upon to observe and explain the very different phenomena which ensue when life ceases. Now, we consider that the changes which ensue after death, so far from disproving what has been said, give us, in fact, a strong support. Death, we contend, can never take place without disorganization; for as life is essentially a chemical action of a particular kind going on between certain substances, it is plain, that the phenomena attending on, and characterizing that action, will go on until the relation between the solids and fluids, necessary for their subsistence, cease to exist. In order, therefore, to put an end to life, it is but necessary to change the existing relations of the solids and the nutritive fluid. And this may be done in three different ways:—by mechanical violence; by chemical agents, or by the abstraction of aliment. In vain are offered in reply, the instances of sudden death from poisons. These agents *do* produce disorganization; that is, destroy those relations between the solids and nutritive fluid necessary to life, and just as effectually as the mill-stone destroys the organization of a grain of corn. A drop of prussic acid is placed on the tongue of an animal, it is absorbed, and a sudden change is produced in the condition of the whole nervous substance. This is amalgamated, as it were, with the other solids, and no change can affect it which does not affect them.

It is not strange, therefore, that the compounds of an organized being take on other actions when the circumstances productive of them no longer exist. In truth, changes somewhat similar are occurring in animals during the whole course of their living existence, as must be obvious when we recall the fact, that their particles are in a state of constant renovation. And is not this a better and more rational explanation, than to say, vaguely, that the *Vital Force* has been removed? For, (unless we take it metaphorically), if it be removed, whither has it been removed? If it be flown, whither gone? Or, indeed, what is it, or what can it be?

Thus much for the formation of organic compounds. But living things pass through a series of forms until they reach what may be called the adult structure, characteristic of a species. The oak takes its origin from an acorn; man takes his from the condition of an embryo. We must, therefore, explain the general causes of the formation of the different tissues and organs; in short, the building up of the adult organization.

"It is a rule," says Cuvier, "without an exception, that all living beings have adhered, each to a similar being, which is its parent." This proposition, though so positively expressed by Cuvier, is by no means admitted by all physiologists: there are many who believe in what is termed "equivocal generation;" but, for the present, let us assume Cuvier's law as true.*

Let us begin our inquiry with life in its humblest revelations, as in the vegetable. The seed is a product of the organic actions: it has been formed from the parent vegetable. From this parent it is separated in the course of things, and becomes an individual being existing by itself. But we have found that *life* is a word expressing certain actions attended with certain results: therefore, when we present a bean or a grain of corn to a person, and ask him if it be alive, what does he mean by saying it is? There are no changes going on in the bean;—there is no motion of which we are cognizant;—there is no nutritive action—no absorption, assimilation nor secretion. What, then, is meant when it is affirmed that it is living? Surely, merely this; that, from certain appearances, we judge that if the bean be planted in the earth at the proper season, certain changes will take place, the result of which will be the formation of the perfect plant. In short, the bean possesses *vitality*, but, as yet, no *life*.

To place the subject in another point of view, let us suppose two beans gathered at the same time from the same plant. Let one of them be planted immediately, or in the next season: a certain series of phenomena will ensue, the result of which will be the production of a vegetable bearing leaves, flowers, and finally seeds, precisely as in the case of its parent. This series will come to a termination in a month or two;—the plant dies, and is resolved into brute matter. But if we take the other bean, and place it in a drawer, and keep it secure from moisture and insects, what will be the result. Why, that the bean will exist unchanged for any number of years—ten, a hundred, or a thousand, at the end of which, if it be placed in the earth, there will occur the very same train of phenomena that took place in the other a thousand years before.† Here, then, has been a duration of life far beyond that allotted to the growing vegetable. How is this to be accounted for?

It is answered, that, in the never-ceasing course of change, the seed has been formed, and so formed or organized, as to fit it, under certain circumstances, to become a vegetable. But for the *primum mobile* of the necessary changes, it is dependent on two things—heat and moisture. When placed in the earth, it meets with these. It is now merely matter in contact with other matter fitted to act upon it, and a change

* On the subject of equivocal generation consult Burdach, tom. i.

† On dit avoir fait germer des haricots conservés depuis soixante ans, et des graines de sensitive, après cent ans d'existence.—*Dict. des Sciences Nat. Art. Vie.*

Some seeds retain the power of germinating for an indefinite length of time, since the wheat usually wrapt up with Egyptian mummies will often grow and germinate, as well as if it had been gathered the preceding harvest.—*Library of Entertaining Knowledge, Insect Transformations*, vol. i., p. 29.

See, also, De Candolle, Physiol. Veg., tom. ii., p. 618.

is produced in its condition. And this change is produced, not by any force or principle which is superadded to the material of the seed, and which, therefore, can be supposed to exist independently of the seed, but by the inherent activity of the substances composing it, set in motion by heat and moisture. The result which we see — the change that has taken place, is the manifestation of this activity; the effect entirely of the nature and arrangement of the different substances composing the seed, together with the peculiar circumstances in which it has been placed and acted upon.

It is evident, that after the first change in the condition of the seed, it cannot be the same thing it was before that change. It is another thing. Now, besides heat and moisture, it is in the presence of other matter; of the solid materials of the soil; of salts held in solution by water, by all of which it is more or less affected. Therefore, the seed is undergoing in the earth a constant, a perpetual change. Perhaps at no two seconds of time is it the same identical thing. The result of all this is the growth of the seed. It springs, in consequence, above the earth, and is then placed in close relations with other bodies. It meets with light, with the oxygen and carbonic acid of the atmosphere, with a larger quantity of heat, and with electrical currents. It is subjected to impulses on its frame from winds, rains, and so on. From these causes, other changes take place,—developed in leaves, bark, buds, flowers, and finally, (to come back to the point of the circle from which we set out), seeds, fitted, in their turn, to produce the like phenomena under the like circumstances.*

This series of cause and effect may be broken upon, and then we have other phenomena. If the seed in the drawer should there meet with moisture and heat, it would put on the very same changes that first occur when it is planted in the earth; but having gone through these, and not meeting with a constant supply of moisture and those other substances necessary for its evolution, it passes, of necessity, into other changes, and is decomposed. It is thus, by some eruption of the order of nature, that all her monstrosities are produced. In her established order she is always regular, beautiful and harmonious.

In most vegetables, and in some animals, the process of generation is confined to one individual. The male organs supply to the female a certain substance which seems to be essential, and without which the ovum, or seed, is not fitted for further development. The same thing holds where the sexes are in different individuals. The semen of male animals answers precisely to the pollen of plants; as does the uterus of the female to the earth, which is the matrix of the seed. Therefore, it is plain, that the same reasoning will apply to the formation of the animal organization, that are applied above to the vegetable.

But "life cannot be the result of organization, because the organization itself is the effect of life." This is but a poor piece of sophistry. It is as if one should say, "combustion cannot be the cause of the union of oxygen and carbon, for their union is the cause of the combustion." The actions of life result from the inter-penetration of the

* A bien considérer les choses, la nutrition et la génération sont deux modes du même phénomène.—*Massey.*

solids by the nutritive fluid. Certain relations exist between the two, essential to those results which characterize life as differing from other chemical actions. If these relations are destroyed, chemical actions still go on, but they differ from those of life, and they end in different results: for life is not combustion, nor fermentation, nor putrefaction, nor eremacausis, (to borrow a word from Liebig), nor any other sort of chemical action but its own. It is a chemical action *sui generis*, ending in certain specific results, and differs from all other sorts of chemical actions, as all of the above mentioned differ from one another. The results which characterize vital actions are the formation of an ovum in the parent; evolution, or the formation of the different tissues and organs; birth, or the separation of the embryo from the parent; growth until maturity; gradual decay; and necessary death, or the passage from one species of chemical action into another. As long as certain relations between the nutritive fluids and the solids are maintained, life is a necessary act. As long as the organization of the acorn is perfect, it is fitted, if placed in the earth, to become an oak. The embryo is in a condition analogous to the acorn in the earth; and as long as its organization is unimpaired; as long as it receives its supply of nutritive fluid, it will not only grow and live, but growth and life are the inevitable and necessary effects,—just as much so, as an explosion is a necessary result on the contact of gunpowder and fire.

There was unquestionably a time when no living being existed on the earth. From some unknown combination of circumstances that occurred in lost geological eras, carbon united with water, organic matter was formed from inorganic, and life commenced. As these circumstances were general, doubtless many myriads of living beings were at the same time produced: but, accordingly as the circumstances varied in some particulars in different cases—according to the action of external forces upon them—would there necessarily be some modification of existence, and of the directions which the vital actions, once set in motion, would assume. These primary organic beings may be compared to the ova of plants and animals, all of which, to whatever class they may belong, are closely allied in external form and chemical constitution.* But these ova are situated differently; they are evolved under very different circumstances; hence, the immense variety of forms produced. From these general causes, and from the progression of change in the constitution of the earth, those modifications have arisen, which we have generalized into varieties, species, genera, orders, classes, divisions and kingdoms.

This question is not one the solution of which may be a matter of indifference. Why not (we have heard it said) study the science of life as we study other sciences,—collect the phenomena, and arrange them in the order of cause and effect, but leave the question of the essence of

* The nearest approach which the vegetable and animal kingdoms make to each other, is not in the most perfect vegetable and the lowest or simplest organized animal, but in the lowest and rudest forms of *both* kingdoms, and likewise in their ova. To use a metaphor of Coleridge, they (the two kingdoms) are "two streams from the same fountain indeed, but flowing, the one due west, and the other direct east."

life as one we cannot fathom? We answer, that, by so doing, we make a wrong start;—we commence with an hypothesis which is false: because, the very main fact,—the key-fact to the right understanding of all the rest, is not comprehended in the scheme. The hypothesis of a "vital force," as a thing independent of the substances manifesting vital phenomena, is an error, and the parent of innumerable others;—*chimæra chimæram parit.* Well and consistently may physiologists reject the splendid discoveries of modern chemistry, if, *à priori*, be assumed the existence of a force which can and does resist, pervert and control the chemical forces of matter.* All our inferences concerning the processes of digestion, respiration, secretion, nutrition, &c., must be surrendered, for they are literally worthless: we have labored in a barren field. But, in truth, in thus referring vital phenomena to a mysterious principle of which we can form no idea, do we act more philosophically than the Indian who refers all natural operations to the Great Spirit,—or than the old woman who refers every thing to the Will of Providence? Such references may indeed be very true, but certainly have no claim to be considered as scientific explanations.

Error, when it has once sprung up in the human mind, is not easily eradicated; especially, if sown in early life, nourished during our education, and strengthened by the influence of great names. The history of philosophy is a record of this truth. Phenomena were once explained by a *horror vacui* or *quinta essentia.* Astrology survived many years the birth of astronomy, as, likewise, did alchemy that of chemistry. From this last science, also, may we draw an illustration apposite to the matter in hand. All acquainted with the science must remember the doctrine of Phlogiston—the *principle* of levity and inflammability. When a metal, after being burned, weighed more than before, and was no longer inflammable, it had lost its phlogiston;—when the calx, after being mixed with charcoal, was reduced to the metallic state, the metal had regained its phlogiston from the charcoal. Lavoisier overthrew this doctrine, by demonstrating the true causes of the phenomena; yet many, who receive without hesitation the doctrine of a vital principle, can look back with wonder upon the obstinacy of Priestley, who maintained the exploded theory to his dying day.

Of a "vital principle," however, we can frame no more distinct notions than we can of phlogiston. As soon as we separate these principles from the substances which manifest the phenomena, they are words, and nothing more.

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* Il s'en faut beaucoup, à notre avis, que la question que nous venons d'examiner soit une chose oiseuse. Si nous avons cru devoir la prendre au sérieux, c'est que le système que nous combattons, savoir que presque toutes les fonctions dont il à été fait mention dans les précédentes considérations, ne constituent ni des phénomènes physiques, ni des phénomènes mécaniques, ni des phénomènes chimiques; c'est que ce système, repétons-nous, ne tend à rièn moins qu'à frapper d'une nullité anticipée toutes les recherches, toutes les expériences, physiques, mécaniques ou chimiques que les physiologistes pourront entreprendre pour expliquer ces fonctions. Or, nous le demandons, quel serait le sort de la physiologie, sur quel progrès pourrait-elle compter si l'on interdisait à ceux qui la cultivent le champ des expériences de ce genre.—*Bouillard, Journal de Med. et Chirurgie*, tom. iii., p. 464—Note.

A metal, after being burned, loses its "phlogiston;"—organic matter, after undergoing certain changes, loses its "vital principle;"—the calx, burned with charcoal, is reduced to the metallic state, having regained its phlogiston;—organic matter, being digested, becomes a part of the living being, having regained its "vital principle." The parallel is perfect.

Another obstacle to the overthrow of long-received opinions, is our fondness for whatever is mysterious. To some minds, the nakedness of truth is appalling,—the roseate twilight of poetry being far more charming and attractive. It gladdens the imagination to occupy itself with what we deem the judgment cannot reach, and we shall scarcely take the trouble to fathom a subject which is particularly pleasing to the imagination. Superstition soon usurps the throne of reason; and questions, in themselves perfectly innocent,—questions, that it is the duty of philosophy to propound, and, if it can, to answer,—become thus to be regarded with a holy horror. But this factious mystery, with which we enshroud natural objects, is the bane of science, whose object it is to lay bare the train of phenomena in their natural order of sequence. It, besides, diverts the mind from the perception of that great, that eternal mystery, which lies at the bottom of all things. "All is wonderful, or nothing is." The existence of a grain of sand, with its powers and capabilities, is as incomprehensible and as wonderful as the existence of a star. He who recognizes this great truth, will smile at all human efforts to aggrandize what is already infinite, or to render more sacred the awful mystery of the universe. If we must have mystery, let us seek it there—in "that which lies at the bottom of appearance," and remember always, that, *boni viri nullam oportet causam esse, præter veritatem.*

Part Second.

REVIEWS AND NOTICES OF NEW WORKS.

I.—*Elements of Materia Medica and Therapeutics.* By Edward Ballard, M. D., &c. and Alfred Baring Garrod, M. D., &c.; with Additions and Alterations, by R. A. Griffith, M. D. Philadelphia, Hogan & Thompson, 1846; large 8vo., pp. 516.

This work, according to the statement of the authors in their preface, was brought out in England to answer the demands of physicians and students for some work containing the valuable matter embodied in the large works on the subject, unencumbered with extrinsic matter. The writers desire that it should be looked upon as strictly elementary, and in so far as the description of the drugs is concerned, nothing more than a compilation. The authors have introduced the officinal articles from the British Pharmacopœas, and have adopted, under the head of each substance, the same kind of arrangement as that of Edwards and Vevaseur, Pereira, &c.; that is, giving the title, physical properties, preparation, chemical composition, chemical relations, operation, uses, dose, officinal preparations, &c.

After a very brief notice of the general principles and of the grounds of classification, with an interesting notice of the properties of the classes, and of the chemical and other relations of medicinal substances, our authors enter upon the proper subject of their work—the account of particular medicines. The substances are arranged, according to their natural relations, in the inorganic, vegetable and animal kingdoms; an arrangement that possesses the advantages of presenting some interesting relations between substances, often indicating analogies in chemical properties, and suggesting applications which could not be anticipated from an acquaintance with any other arrangement. Take vegetable substances, for example, of which the "activity and kind of operation" may be predicted from the structure and affinities of the plants from which they are obtained. Plants are grouped together from certain affinities in structure, and experience has established the general law, that, when a group is a very natural one, the medicinal qualities of every member of it are very analogous. This seems to depend upon the fact that the external form is dependent to some extent upon the internal organisation, and it is also on this internal organisation that the elaboration of the peculiar principles, that give them their activity, depends. As examples we have the Solanaceæ, Papaveraceæ, Rosaceæ, &c., in which all the species partake to some extent of the medicinal properties which characterize these natural orders. In other families the resemblance, in medicinal properties, between the different species, is less obvious, but still exhibiting analogies in the modes of action, which are often important to be observed by the practitioner. Take the Umbelliferea for example; we there find Caraway and Anise brought into juxtaposition with Asafœtida and other fœtid gumresins, and these again with Conium, &c. This, to those not familiarized with the idea of the natural relations of the species yielding these materials, as employed in medicines, and judging merely from the physical characters of the substances, might be regarded as a most heterogenous group; but in the order of this natural arrangement they present interesting and important analogies in some respects, and gradations of properties from those of the stimulant and carminative effects of the aromatic umbelliferæ to the stimulant and antispasmodic effects of the fœtid gums, and the antispasmodic and anodyne properties of Conium. This kind of relationship or analogy is often sugestive of important therapeutical applications; and, in fact, it is with medicines as with every thing else; the more numerous the points of view we can see them under, the better we will comprehend their relations.

The principal fault to be found with this work is one which must apply to a greater or less extent to all such works. The authors confine themselves too exclusively to officinal substances; and, indeed, there are hardly any introduced that have not received the sanction of some of the bodies constituted by law to give vogue to or reject medicinal substances. When men write books on Materia Medica, it should be to give their own opinions and experience and that of others; and they should not be deterred from the expression of those opinions in favor of any article, merely because it may not have been received into the Pharmacopœas. Either the gentlemen who wrote this book possessed no original ideas at all, or were deterred by undue respect of authority from any exhibition of the kind. There are many substances in use as medicines which have not been admitted into the Pharmacopœas, but this by no means diminishes their utility; and every day is adding to the list of new and useful salts, both organic and inorganic, and it would be a pitiful thing, indeed, if we were obliged to wait their sanction by some authority before they could be admitted into works, by means of which the profession generally can learn something of their properties and applications. This is a circumstance which certainly renders this work totally unfit for a text-book for students, and it can never replace such works as those of Wood & Bache or Pereira, though it will prove interesting in many respects to either the physician or student.

As a sample of the very satisfactory manner in which the authors treat their subject generally, we may be allowed to give the following, which may be regarded as interesting in other respects:—

VALERIANACEÆ.

Valeriana Officinalis.—The Wild Valerian.

Triandria, Monogynia.

An annual herb, with a perennial root. It has a tall round channelled stem, terminated by a close corymbose head of white or lilac flowers. Its leaves are opposite and pinnate, the leaflets being lanceolate and serrated.

Hab.—In wet places in Europe.

Valeriana. U. S. (*Radix.*) L.

Description.—*Form*, a short tuberose rhizome, from which radical fibres come off, three or four inches in length, which are the officinal part. *Colour*, yellowish-brown, when dry. *Odour*, fœtid and peculiar, and not disagreeable in the fresh plant. *Taste*, warm, camphoraceous, and nauseous.

Chem. Comp.—Its most important ingredients are, *volatile oil*, *valerianic acid*, *resinous* and *gummy* matters.

The *volatile oil* is obtained by distillation of the root with magnesia, in order to fix the valerianic acid. It is of a light-green colour, and lighter than water, having the odour of the valerian.

The *valerianic acid*, when set free from the magnesia by sulphuric acid, occurs as an oily liquid, having an odour similar to the oil, but an acrid acid taste. It is probably formed by oxidation of the oil. It can be procured artificially by oxidation of the *oil of potatoes* or *grain spirit*, to which it stands in the same relation as acetic acid does to alcohol. It forms soluble salts, with bases, as oxide of zinc and quinine, which have lately been introduced into medicine. *Formula*, $HO + C^{10}H^{9}O^{3}$.

Oper. and Uses.—Valerian is stimulant and antispasmodic. We have found it of some value in removing the *paroxysms of headache*, which occur in atonic dyspepsia and anæmia. It has obtained a reputation in convulsive affections, as *epilepsy*, *hysteria*, and *chorea*. We have seen it of service in hysteria, but never in epilepsy.

Dose.—℈ j.—ij.

Off. Preps.—*Infusum Valerianæ. L. U. S.*—(Valerian, ʒ ij. [℥ss. U. S.] macerated in boiling distilled water, f ℥ vij. [Oj. U. S.] and strained.) *Dose*, f ℥ j.—ij.

Tinctura Valerianæ. L. E. U. S. (Bruised Valerian, ℥v. [℥ iv. U. S.] macerated in proof spirit, Oij., and strained.) *Dose*, f ʒ j.—iv.

Tinctura Valerianæ Composita. L. Tinctura Valerianæ Ammoniata. U. S. (Bruised valerian, ℥ v. [℥ iv. U. S.], macerated in aromatic spirit of ammonia, Oij., and strained.) *Dose*, f ʒ ß —ij. More stimulant than the two former preparations.

Valerianates.—Salts of Valerianic Acid.

Lately, salts have been introduced into medicine containing valerianic acid, united with various bases, as zinc and quinine. *Valerianic acid* can be prepared in two ways, either from the root of the valeriana officinalis by distilling it with water, when the acid, being volatile, passes over along with the valatile oil, and can be neutralized by an alkali, and the impure salt redistilled with sulphuric acid to obtain the acid pure; or it may be obtained from the oil of potatoes, by heating it with caustic potash. Its composition is $HO + C^{10}H^{9}O^{3}$.*

When pure, it occurs as a colourless oil, lighter than water, having the strong disagreeable *odour* of the valerian, and an acrid *taste*. It is inflammable, and burns with a white smoky flame. Its salts are generally soluble in water and crystallizable. A pound of valerian root yields about half a drachm of the acid.

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* The explanation of these processes is as follows:—1. In the valeriana officinalis this acid exists ready formed, or is formed from some of the constituents of the root, probably the volatile oil; and, being volatile, it passes over by simple distillation with water. 2. Oil of potatoes, called also Fousel Oil, passes over towards the end of the process for obtaining spirit from grain or potatoes. When pure it forms a colourless oily liquid, of a disagreeable odour. It has the composition $C^{10}H^{12}O^{2}$, or may be better represented by $C^{10}H^{11}O + HO$, that is, as a hydrate of an oxide of a radical $C^{10}H^{11}$, which is called amyle. Hence, this Fousel Oil is called the alcohol of the amyle series; and as acetic acid ($HO + C^{4}H^{3}O^{3}$) is formed from common alcohol ($C^{4}H^{6}O^{2}$) by the addition of 2 eqs. of oxygen and the subtraction of 2 of hydrogen, so valerianic acid is formed from the oil of potatoes by a similar change, and has been called the acetic acid of the amile series.

Valerianate of zinc.—This salt is prepared by dissolving *valerianic acid* in water, and neutralizing with freshly-precipitated *carbonate or oxide of zinc*, a gentle heat is then applied, and the solution concentrated, and allowed to crystallize. It occurs in white laminæ, very thin, with a mother-of-pearl lustre, dissolving in 160 parts of water or in about 60 parts of alcohol; little soluble in cold, but soluble in 20 parts of boiling ether. *Composition*, 1 eq. of valerianic acid + 1 eq. of oxide of zinc.

Valerianate of Iron.—Made by adding a solution of the perchloride of iron to a solution of the valerianic acid, saturated with carbonate of soda. A precipitate falls, which must be dried under 70° F. It occurs as an amorphous powder, of a dark red colour. By heat it is decomposed, the valerianic acid passes off, and peroxide of iron remains. *Composition*, 3 eqs. of peroxide of iron + 7 eqs. of valerianic acid + 2 eqs. of water.

Valerianate of Quinine.—Made by adding freshly-precipitated quina to a hot solution of valerianic acid, and then crystallizing. It crystallizes in thin colourless rhomboidal plates, of a mother-of-pearl lustre, or in needles. Not very soluble in water, but more so in alcohol and ether. It has a smell of valerianic acid, and a very bitter taste. *Composition*, 2 eqs. of quina + 1 eq. valerianic acid + 24 eqs. water.

When a solution is heated above 122° F., it gives rise to the formation of a resinous mass, having the composition, 2 eqs. quina + 1 eq. valerianic acid + 4 eqs. of water.

Oper. and Uses.—The valerianates of zinc, iron, and quinine appear to possess the combined properties, both of the acid and the bases. Thus the valerianate of zinc has been extolled in *nervous diseases*, as epilepsy, chorea, tic-douloureux, and other forms of neuralgia; the valerianate of quina, in intermittent diseases. The high price of these preparations must be a serious obstacle to their coming into general use.

Oil of Valerian passes over with valerianic acid, when valerian root is distilled with water. When fresh, it contains no valerianic acid, but consists of several compounds; one called valerole ($C^{12}H^{10}O^{2}$), crystallizable, which yields valerianic acid by exposure to the air, or when treated with an alkali; the second called Bornene ($C^{10}H^{8}$), identical with the liquid from Borneo camphor; and also a crystallizable camphor, like Borneo camphor ($C^{20}H^{18}O^{2}$).

There are some instances, which may, however, be regarded as exceptions, in which important but unofficinal articles are noticed in the work, and when they are introduced it is in a brief but lucid manner, of which the following may serve as an example under the Lauraceæ.

Bebeerina.

An alkaloid discovered by Dr. Rodie, in the Bebeeru or greenheart tree of British Guiana, said to belong to the natural order Lauraceæ.

This alkaloid has not yet been procured in a crystallized state, but as a brown Mass. It is soluble in alcohol, and combines with acids, forming crystallizable salts. The sulphate is an article of commerce, usually found in brown crystalline scales, soluble in water, sparingly so in alcohol, and possessing an intensely bitter taste; the solutions of the salt are neutral, and ammonia precipitates the alkaloid. On analysis, pure Bebeerina gives, in 100 parts, carbon 72·22, hydrogen 6·62, nitrogen 4·30, oxygen 17·02; and its formula is $C^{35}H^{20}NO^{6}$, so that it is isomeric with morphia (*Tilley*).

Oper. and Uses.—Bebeerina was introduced into this country by Dr. Maclagen as a *substitute for the sulphate of quina;* its price being about one-half that of the latter. It appears, from many trials, to possess all the antiperiodic properties of quina, and has been found of the greatest service in intermittent fevers, and the various forms of neuralgia. It is said also not to produce the unpleasant effects which sometimes result from the use of that alkaloid.

Dose.—Of the sulphate of bebeerina, the same as that of sulphate of quina.

To conclude we may remark, that though this work does not appear to us well adapted for the use of students, for the reasons above stated, yet we would confidently recommend it to the profession as one that any member of it may read with interest and advantage.

W. M. C.

II.—1. *The Diagnosis, Pathology and Treatment of the Diseases of the Chest.* By W. W. Gerhard, M. D., Lecturer, &c. Second edition, revised and enlarged. Philadelphia: E. Barrington & G. D. Haswell; 1846: pp. 288.

2. *A Manual of Auscultation and Percussion.* By M. Barth, Agrégé to the Parisian Faculty, &c., and M. Henry Roger, Physician to the Bureau Centrale, &c. Translated, with Additions, by Francis G. Smith, M. D., Lecturer, &c. Philadelphia; Lindsay & Blackiston; 1845: pp. 160.

3. *A Clinical Introduction to the Practice of Auscultation, and other Modes of Physical Diagnosis.* By H. M. Hughes, M. D., Fellow of the Royal College of Physicians, &c. Philadelphia: Lea & Blanchard; 1846: pp. 270.

As the three works, the titles of which we have given above, bear, either directly or indirectly, upon the same subject, we thought it better to notice them under the same head. By this means, we shall be better prepared to compare one with another, and give the reader clearer views of the principles and doctrines laid down by the respective authors. It is gratifying to see men, famous for their learning, engaged in condensing and simplifying heretofore one of the most obscure, and, at the same time, important, branches of pathology. Since the promulgation of Laennec's discoveries touching auscultation and percussion, much has been done to elucidate the diagnosis and pathology of pectoral diseases. Like a builder, who, in order to rear a splendid mansion, must needs plant his scaffolding, to be afterwards laid aside, so Laennec, with a view to illustrate and enforce the truth of his immortal discoveries, said much that may now be omitted, and added arguments and reasonings which are now superfluous. A shorter route is now being carved out by those who follow in the wake of the distinguished founder of auscultation. It is no longer necessary to reason with the profession on the value of this branch of medicine, in the detection and cure of thoracic affections; all, or nearly all, enlightened physicians acknowledge its high claims to their attention, and but few can be found ready to depreciate a discovery which, whilst it reflects the highest credit upon the profession, at the same time confers incalculable benefits upon mankind.

One of the most marked characteristics of the nineteenth century, in contradistinction to all preceding ages, is the readiness and promptitude with which the public mind receives and adopts the new discoveries made in the various sciences. This is eminently true in medical science. This single fact has done more to push onward the institutions of the civilized world than all other things combined. Formerly, when prejudice swayed the great mass of mind, ages elapsed ere the simplest discoveries in science were adopted. This necessarily retarded the progress of the arts and sciences, and thus opposed serious obstacles to the advancement of the human mind. To confirm the truth of the foregoing remarks, we need only refer to the discoveries of Gallileo, Jenner and Harvey, the first of whom did for the sublime science of astronomy what the latter did for that of physiology.

These observations, although somewhat irrelevant to the matter in hand, thrust themselves upon our mind in looking over the concise and beautiful treatises under examination. It is neither necessary nor desirable to give any thing like an analysis of either of the three works; each treats of the diagnostic signs of the thoracic viscera, differing somewhat in the arrangement, yet all point in the same direction, and aim to elucidate the same morbid phenomena. Dr. Gerhard is the only one, however, who says any thing of the treatment of this class of affections.

Taught to observe under the eyes such men as Louis and Andral, and actuated by a sincere love of scientific truths, Dr. Gerhard has thoroughly qualified himself, by reading and hospital practice, to investigate the diseases of the lungs and heart.

Wedded to no particular theory, nor the follower of any sect, Gerhard aims to present facts, free alike from speculation, and that tedious and irksome minuteness which soils the pages of some of our best works.

Dr. Gerhard's work was at first but a manual on the diagnosis of thoracic diseases; the *second* edition is greatly enlarged, and is much better calculated to supply the wants of the profession in this country. An independent observer, he has confirmed the observations of his predecessors, yet he has hesitated to differ with others, and on certain points, especially the pneumonia of children, he has added much to our previous limited knowledge. We have witnessed Dr. G.'s devotion to the study of the morbid anatomy of the lungs, and any thing coming from him on this subject may be received with assurances of correctness. Indeed, without aiming to be invidious, we may not inaptly designate Dr. Gerhard as the Louis of America. Any further commendation of the writings of Dr. Gerhard is unnecessary; we therefore advise every young physician who wishes to become acquainted with the diagnostic signs of chest diseases to study this work; it is equal to any of the day, and brings us up to all the most recent improvements.

The little work by MM. Barth and Roger may be dismissed in a few words. It contains, in the shortest possible space, an exposition of all the signs of thoracic disease. It is the *multum in parvo:* it contains the essence of all that is known upon the subject of which it treats. Buy it: read it.

Of Dr. Hughes' work we cannot speak so favorably: although very satisfactory on some points, yet candor forbids our unqualified approbation of the book as a whole. It may be read with interest and advantage, especially his remarks upon "*auscultation of the heart.*"

A. H.

III.—*The Natural History and Diseases of the Human Teeth:* by Joseph Fox, M. R. C. S., &c. &c. First American, from the third London edition. Remodelled, with an Introduction and numerous Additions, by Chapin A. Harris, M. D., D. D. S., &c. Large 8vo., pp. 430, illustrated with 30 plates. Philadelphia: Barrington & Haswell: 1846.

It is the duty of the physician to familiarize himself with every department of knowledge that has any bearing on the practice of his profession; and he will often have it in his power to obtain most satisfactory results in practice, and to advance his reputation, by attention to things which are regarded by some as not of sufficient importance to deserve their notice. Almost every man in our profession who makes any pretensions at all, aims at the performance of something great and dreadful: nothing less than capital operations will do them; and it is to be feared, that so strong is this passion, that good limbs are often sacrificed, because bad ones do not offer. Every one wishes to tie the carotids, subclavians, or iliacs, and the only reason they can give is, that nearly all those thus operated on, die. The fact is, that it is merely from vanity; from a desire to do something that the vulgar will stare and wonder at; and we too often lose sight of the fact, that the mere operator in surgery holds the same position in regard to the scientific physician, that the operative mechanic does to the skilful engineer or architect. Many young physicians thus devote themselves to the study of diseases and operations, which they rarely have occasion to see; and, perhaps, will hardly have the opportunity once in a life-time of performing operations to which they have devoted so much attention. How much better to attend closely to the study of those things which we are to meet with at every step, and which are in reality most interesting, as they call most frequently and imperatively for relief. Singularity and rareness are too generally regarded as giving cases their principal interest; whereas, the very circumstance of their being common, and of daily occurrence, should be a sufficient reason to induce us to give them our especial attention.

In connection with the subject of the work we are now noticing, there are many points which are generally regarded, erroneously, as of but trifling importance, but which are of the utmost consequence in their hygienic and therapeutic relations. The physiology and pathology of the teeth; dentition, and its various concomitant lesions; the complicated relations of the dental system to the bony and soft parts with which it is connected; its relations to the digestive functions; and, in fact, the reciprocal relations existing between the dental system and other portions of the economy, are all matters of the gravest moment to the practioner, and not unfrequently the chance of life or death is to be decided by the knowledge or ignorance of the physician respecting some of these points.

This, however, is not all. Our physicians are very often referred to in regard to the teeth, in reference to points which do not involve the life of the subject, but which often deeply affect the happiness of the individual, by destroying or jeopardizing health. How often do we see decayed, deciduous teeth torn from young jaws, thus affecting the formative cells, or sacks, which contain, and should perfect the permanent teeth that are to replace those removed, and which will now arise chalky and ready for decay, from absence of the enamel, which the torn sack has not completed! How often, from injudicious interference, do we find the permanent teeth arising crooked and jagged; thus, perhaps, forming for a life-time, standing monuments of the ignorance of some physician respecting the physiology of dentition!

The natural history of the teeth, their structure, the history of their development in their normal state, and of their deviations from this standard, are subjects of sufficient interest to repay any one for the time spent in reading a good account of them; and no physician can understand either the pathology of the teeth themselves, or the symptomatic lesions which are so frequently met with in connection with those of the dental system, without a good knowledge of the physiology of this system. All the methods of preventing and curing irregularities and deformities of the teeth, and the whole theory and practice upon the diseases of the teeth, should be founded entirely upon this basis.

The work of Dr. Fox was first published in 1803; and notwithstanding the rapid progress of dental surgery from that to the present time, it has still occupied a high place in this department of medical literature. Two other editions were subsequently brought out; the third appearing in London in 1833. The present work is on the basis of this last London edition, and the additions by Dr. Harris have brought it up to the state of the science at the present day. It is a work calculated to be of incalculable advantage to the physician, as well as the dentist; for there are so many diseases, especially those of childhood, having intimate relations with the state of the dental apparatus, that this kind of knowledge is as important to the general practitioner as to the dental surgeon, and even more so, as the diseases requiring the attention of the physician to be directed to this system, often involve the general health or life of patients, whilst the dentist deals principally with those producing only deformity or inconvenience.

We will conclude, by recommending this work to the attention of physicians, as one to which they may often make reference with great advantage and satisfaction.

W. M. C.

IV.—*A Compendium of Lectures on the Theory and Practice of Medicine, delivered by Professor* Chapman, *in the University of Pennsylvania.* Prepared, with permission, from Dr. Chapman's manuscripts, and published with his approbation; by N. D. Benedict, M. D., &c. Philadelphia: Lea & Blanchard; 1846. 8vo.: pp. 258.

The mere circumstance that a work appears from the pen of the distinguished Professor Chapman, is, of itself, a sufficient recommendation to most of the members of the profession in this country; and even his sanction or approbation conveys a great influence, as emanating from an authority from which we tremble to differ. But in examining a book, the question will always arise, as to what useful end is intended to be attained by its publication; and when this question arises in reference to the present work, it will, in all probability, be difficult to obtain a satisfactory answer.

So great, however, is our respect for the talents and opinions of Professor Chapman, that we should feel guilty of a high degree of presumption in attempting to criticise any thing written or sanctioned by him; and as we have not been able to detect any thing in the book at all commensurate with our opinions of the learned professor's abilities, we will say no more about it, for fear of being regarded as assuming the same ungracious attitude as that in which that notable critic, Gil Blas, was placed, by his friendly comments on the archbishop's homily.

W. M. C.

V.—*An Essay on the Pathology and Treatment of Trismus Nascentium, or Lock-Jaw of Infants.* By J. Marion Sims, M. D., of Montgomery, Ala. Lea & Blanchard; 1846.

This is a republication of an article which appeared in the April number, 1846, of the *American Journal of the Medical Sciences.* The author has been led to present it to the profession, in a separate publication, from the value and importance which he attaches to the subject. Concerning the importance of the subject, we believe every practitioner in the South and South-West will agree with him.

The following extract will show how the author came by the peculiar views which he entertains of this disease.

* * * "My friend, Dr. Vickers, visited the little sufferer with me this morning. Many experiments were performed to demonstrate, before the Doctor, the reflex action of the excito-motor system, and its exceedingly delicate impressibility to the gentlest touch. To show the great rigidity of the frame, I caught hold of the feet, and raised the whole body without flexing the thighs on the pelvis. At last, I run my hand under the head, for the purpose of elevating the body in the same way, when I immediately detected a *remarkable irregularity* in the feeling of the bones. *It had lain, during the whole of its illness, exactly in one position all the time, the weight of the head resting wholly on the os occipitis.* Its pulse was now uncountable; respiration more frequent than I had ever seen it before under any circumstances; it was breathing 120 times in a minute, and looked as if it could not possibly live an hour. I raised it up, to examine the head more particularly, and set it on my knee, or rather leaned it against the knee, for the tonic rigidity of the muscles prevented the flexure of the thighs to a sufficient degree for the sitting posture. After holding it so for some ten or twelve minutes, what was my surprise to find a rapid amelioration of all its bad symptoms! True, the tonic rigidity remained the same, but the clonic spasms became less frequent and less intense; the whole expression was less disturbed; and the respiration fell, in this short space of time, from 120 down to 70 in a minute. I now felt convinced that *position* had a great deal to do in the production of this disease. On examining the head, I discovered that the fontanelles were open and very large, particularly the anterior; that the bones were loosely attached by their commissures; and that the *os occipitis was pushed in on the brain, being overlapped for a quarter of an inch or more along the whole course of the lambdoidal suture, by the edges of the ossa parietalia.* I had the child laid on its side, so as to take the weight of the body from the os occipitis. It died about sundown, on Friday, 25th July, having been sick about ninety-six hours.

"Through the kindness of Mr. Stickney, and with the consent of the mother, a post-mortem examination was made the next morning at 10 o'clock. I am indebted to my friend, Dr. Ames, for the following notes, made at the time.

" 'No emaciation; countenance tranquil; slight cadaveric discoloration of back.

" '*Head.*—Anterior fontanelle large, triangular; coronal suture open in its full extent; sagittal same, open to ossa nasi; parietal bones overlap the frontal slightly; *the occipital for the fourth of an inch along the whole of the lambdoidal commissure;* gentle pressure on the occipital bone projects the brain forwards, producing a considerable fullness of the anterior fontanelle.

" '*Brain.*—Superficial vessels full of black blood, *particularly posteriorly.* Interior of the brain natural in appearance.

" '*Spine.—Coagulum of blood occupying the spine in its whole length, enveloping perfectly the medulla spinalis; thicker as it approaches the brain. Spinal veins full of black blood.*

" 'Thoracic and abdominal viscera healthy; nothing unnatural about the appearance of the umbilicus.' "

The extract below will inform our readers of the view which the author takes of the pathology and etiology of this "insatiable baby-killer."

"Curling, in his admirable '*Treatise on Tetanus,*' tells us, that 'Dr. Goëlis, of Vienna, in the examination of children who have died of this disease in the Foundling Hospital of that city, found an appearance of *increased vascularity in the substance of and in the membranes enveloping the upper part of the spinal marrow;*" and that Dr. Thompson, of Philadelphia, had also *observed the same thing.*

"M. Billard (Stewart's edition, page 490), says, that he 'found, on dissection, *nothing more* (!) *than an effusion of a quantity of coagulated blood in the spine.* This blood was effused between the two laminæ of the tunica arachnoidea, and *filled the whole of the medullary canal from the medulla oblongata to the sacral region.*' And after finding this morbid appearance and no other, he gravely asks the question, 'Were the symptoms of tetanus to be ascribed to this hemorrhage of the spine?' What more did he want to account for the distressing symptoms and fatal termination of the disease than this 'hemorrhage of the spine?' Did ever cause and effect stand more intimately connected? Is not the presence of this effusion a sufficient explanation of all the phenomena of the disease? Are not the tetanic spasms the very result that we would reasonably and almost invariably calculate on finding in 'spinal hemorrhage?' I think so. And, as the real character of this affection has evaded the efforts of all pathological inquirers—Goëlis, Thompson and Billard being the only writers that I can find who agree on any single point relating to it,—and as my observations are wholly corroborative of theirs, I must here insist that the true seat of the disease is the spinal canal; that its *morbid anatomy consists, first in a congestion, and then in a rupture of the minute veins and capillaries of the medulla spinalis;* that this is the special and pathognomonic feature of the disease, while every other alleged cause and explanation of symptoms are but mere coincidences.

"The next question that naturally arises is, what produces this venous congestion and capillary rupture, this effusion of coagulated blood, this *spinal apoplexy,* if I might be allowed to apply a term significant of the abnormal appearance? *I answer, that a simple and ample explanation may be found in the anatomical peculiarities of the fœtal cranium, and spinal circulation, in connection with the imprudent and careless habit in mothers and nurses of allowing infants to remain too long in one position.* The imperfect ossification of the cranial bones is *essential* to the production of the disease, while the *position* of the child is its accidental or exciting cause."

The rest of the argument consists in showing, from the anatomy of the veins of the spinal marrow and cerebellum, how congestion, and consequently effusion of blood on the spinal cord, must or may ensue from continued pressure on the occiput.

We have read this paper with a great deal of interest, and we think the arguments of the author exceedingly plausible, but we cannot agree with him that compression of the occiput is the main or sole cause of *trismus nascentium.* If such were the case, how comes it that this disease is in many places endemic? How comes it that it is so rare in England, and so frequent in Scotland and Ireland? How explain, that although it "occurs very rarely, if at all, on the main-land of Iceland, it is eminently disastrous in Heimaey, one of a group of islands consisting entirely of lava, situated on the southern coast"?* Upon this hypothesis, how account for the singular facts mentioned before the Physico-Medical Society of New Orleans, by Drs. Axson and Lindsay, as published in this journal? Dr. Axson expressly stated, that "there was nothing peculiar in the location of the plantation" on which, he had been told, "only four or six children had been raised, all the others born on the place having died of trismus. On an adjoining place," he goes on to say, "distant about a mile and a half, and owned by a brother of the proprietor of the first place, this disease was rare, and many young and vigorous children were found. *The same midwife officiated on both places.* She was an old black woman, with all the requisite pretensions to such an office;" &c. Besides, the habits of negroes and the poorer classes of whites are, in this respect, pretty much the same all the world over. Thousands of new-born children are laid on their backs every day. In certain regions we find this disease prevailing, whilst others are exempt from it.

In our opinion, nothing whatever is known concerning the remote, or rather the efficient causes of tetanus. One man may have a limb wrenched off by machinery, and recover, without a single symptom of the disease;—the scratch of a pin may bring it on in another.

But though we think thus, we are thankful to the author for putting his views before the public; and we have reason to believe they will be of much practical value. Why we think so, will appear from what follows.

The writer of this notice was called, February 14th, 1845, to see the child of a gentleman residing in New Orleans. It was the third day from the birth of the child, and the father informed us that the labor had been a protracted one,—that the child, soon after birth, had been seized with convulsions, which had been continued at intervals of from one to three hours up to the moment of our visit. We were also told, that the physician who had been in attendance had retired, giving up the case as hopeless. The child's bowels had been evacuated, and it was a well-formed, stout infant.

Upon examination, we found the patient extremely drowsy, as if suffering from cerebral congestion: we found, also, the pupils considerably dilated. There were no tonic spasms—nothing, in short, characteristic of tetanus. During the examination, a convulsion came on, which

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* Curling on Tetanus.

lasted about ten minutes, and then ceased, leaving the child tranquil as before. The examination being resumed, we discovered the injury spoken of by our author — namely, a depression of the occipital bone under the parietals. The depression was very great, but considerably greater on the right than on the left side.

As warm baths, and, in short, every thing usually resorted to in such cases, had been employed in vain, it struck us that the child might be relieved by an operation. We called on Dr. Hunt, and requested him to see the case with us, telling him the particulars, and mentioning, that if he agreed with us, we would, with the consent of the parents, perform the operation. Dr. Hunt coincided with us in the propriety of the operation, and having obtained the parents' consent, the operation was performed. It consisted in cutting down on the right side, where the greatest depression was,—cutting through the soft union between the occipital and parietal bones with a common scalpel,—and then reducing the dislocation as much as possible, by means of the director found in the common pocket-case — using it as a lever.

Immediately after the operation, the child was seized with a violent convulsion; but it was the last one it ever had. Up to the present time (June 1st, 1846,) it has enjoyed uninterrupted good health, and is as fine and stout a child of its age as any in the city.

In justice to Dr. Sims, we must also mention, that, in consequence of the wound on the right side, the child was kept reclining on its left. According to his views, this position may have had great influence in the happy issue of the case. J. H.

VI.—*The Anatomical Remembrancer, or complete Pocket Anatomist; containing a concise description of the Bones, Ligaments, Muscles, and Viscera ; &c. &c.* From the second London edition, revised. New York: Samuel S. & Wood; 1845. 12mo.: pp. 245.

This tiny volume, though published last year, has just reached us, owing to a long voyage and other, sometimes unavoidable, delays. The remarks respecting it in the London Medical and Surgical Journal convey correct ideas of what the work is. "It contains but two hundred and fifty pages, and is really an anatomical *multum in parvo.*"

Part Third.

EXCERPTA.

1.—*Mülder on the Vital Force.* Extract from "*The Chemistry of Vegetable and Animal Physiology:* by Dr. G. J. Mülder, Professor of Chemistry in the University of Utrecht. Translated from the Dutch, by P. F. H. Fromberg, First Assistant in the Laboratory of the Agricultural Chemistry Association of Scotland. With an Introduction, by Professor J. F.W. Johnston, F. R. SS. L. & E. First authorized American edition, with Notes, by B. Silliman, Jr.

CHEMICAL AND ORGANIC FORCES.

In the doctrine of life, no position has been more strenuously maintained, than that a peculiar force exists by which organic substances are governed, by which they are arranged in certain specific modes, and are enabled to assist in sustaining the living system, and to create a chain of phenomena, which as a whole are called *phenomena of life*. This vital force has been described as one quite peculiar, of which not the slightest trace is to be found in inorganic nature. Organic and inorganic nature are often, indeed, contrasted. We hear of the animate and inanimate forces of nature; we used to shrink from observing any connection between them; and, in particular, it was thought that the endeavor to explain many phenomena of life by means of the so-called dead forces might detract from the doctrine of life.

In the true study of nature the principal aim ought to be, not only to make ourselves acquainted with the phenomena and laws which distinguish and regulate living and dead matter, but also to arrange those phenomena and laws, and exhibit them in their several relations. The more our knowledge of these two departments is extended, and the nearer the several parts of the great science of nature are seen to approximate, the more firmly must we embrace the idea, as necessarily conformable to truth, that the same forces govern alike the animate and inanimate kingdoms.

Those who proceed on the assumption that no such connection exists, will certainly not be able to trace it; but a search conducted with impartiality, will be rewarded with the discovery of whatever exists.

In the natural sciences, the words *matter* and *force* are continually recurring. We endeavor, by an effort of imagination, to separate the one idea from the other: yet we cannot conceive of matter without force, and scarcely of any force which does not react upon matter. We encounter many difficulties, while attempting to penetrate into the properties of matter. We are perplexed, first, with its being divisible either finitely or infinitely; secondly, with the great diversity of substances which exist; thirdly, with the great number of elementary bodies now known to chemists.

Moreover, the idea expressed by the word *atom* is by no means distinct, though the term must frequently occur in treating of natural philosophy.

In the natural sciences, we do not seek to go beyond the knowledge which is acquired by observation and by the comparison of visible objects, and thus we avoid that labyrinth in which many of the wise and learned, both in former and later times, have involved themselves, by clinging to abstract ideas borrowed from the visible world. We acknowledge material diversity because we observe it; we acknowledge a great number of elements, because we see them; we do not meddle with the term *atom*, but substitute for it, wherever we can, the more comprehensive and intelligible expression—*equivalent.*

In the natural sciences, *force* is used to signify an assumed cause of observed phenomena; we therefore do not observe forces, but suggest their existence to ourselves; and we do so in conformity with sound principle, for the phenomena constrain us to presume that such forces exist. No cautious inquirer into nature goes farther at the present day; we do not introduce forces, to which we assign properties; but we form the idea of some particular force, after the necessity for its existence is demonstrated by the observation of natural phenomena. The idea of force is therefore a concrete one, by which every speciality in the phenomena is embraced, and unity is given to the whole.

Such are the simple principles by which we are, in the present day, guided in our inquiries. The forces of nature, which we recognize, are in number and kind such as we learn from the phenomena they ought to be; and the error, so prevalent in former times, of attributing properties to forces, without previously proving from observation that those properties existed, is now carefully avoided.

Proceeding from this point of view, we propose to make some observations on the organic and inorganic forces of nature, sometimes called the living and dead forces. These observations will not add any thing to the amount of our scientific knowledge, but may serve to explain and establish the proposition that a connection exists where hitherto it has not been recognized.

It is scarcely necessary to observe, that the expression *dead force*, when used to denote the operating causes in inorganic nature, has no substantial value: still, it is understood to signify the force of dead nature, in opposition to that of living nature, where a particular series of phenomena appears; and this is the meaning in which we use the expression.

The chemistry of the present day, which is occupied especially with the doctrine of molecular forces, perceives peculiar causes, operating in the small particles of matter. Our inquiries may not inappropriately be commenced with the investigation of this subject, the study of which is the foundation of any knowledge we acquire in regard to organic forces; for every organism is composed of materials which are subject to the laws of those peculiar forces that belong to chemical substances.

While, however, we would endeavor to show, that in all organisms, living or not, the same molecular forces operate as efficient causes, we are not to be understood as holding that they are combined necessarily with consciousness, or the more elevated rational principle.

I will not venture to raise the veil, by which the action of the nerves, or the higher functions of the mind, have hitherto been shrouded from our observation. As man has an immaterial and immortal part, which is identical with his real being, and of which alone he will consist when the material frame by which he is bound to the earth, shall be dissolved; and as the inferior animals possess, in common with man, certain powers of perception, associated with certain appropriate organs, whose functions have no connection with consciousness; so do animals and plants perform in common a great many operations which are distinct from both of those now mentioned, or which at least have their origin in distinct causes.

It is only the latter class of which I speak, and to which I apply the general term of organic life. To that subject I shall restrict my remarks.

It is well known that, in animals, such operations are performed through the agency of the nerves, and in them, therefore, they are generally more complicated than in plants. The similarity of the operations themselves, however, intimates the existence of a connexion between the causes from which they respectively arise. But the nature of this connexion, as well as its strength, is and ever will be an enigma, as much as the action of the nerves itself. The peculiar nature of organic life—the difference between living nature and what is called dead nature—is however, not determined by the action of nerves. The properties which are common to animals and plants will alone be treated of; these properties being included in the general idea of functions of life. In the first place, we shall review in succession, the properties of the elements, which enter into the composition of all that exists in nature.

We shall be the first to admit the force of the objection, with which we may be met, that it is more easy to pull down than to build up. Science has not yet made such progress as to enable us, from a more elevated point of view, to take a comprehensive survey; but this will never afford a rational ground for adhering to incorrect propositions. The suggestions which follow are therefore to be regarded as thrown out only for consideration. They will require to be much more fully developed before they can pretend to form a complete system.

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ORGANIC FORCES.

A. *Connection between Organic and Molecular Forces.*

All vegetable and animal substances are composed of those bodies, which in chemistry are called elements, and which combine with each other in very different ways. The question now to be considered is, whether the organic forces, which operate in the organic kingdom, depend either in whole or in part on the molecular forces of the elements. This is indeed a question difficult of solution. We shall see how far science in its present state, enables us to reach a satisfactory result.

If we assume, that an organic whole is governed by a general force called vital force, then we ascribe to that whole something which is not learned from observation. We perceive an aggregation of phenomena, which we comprehend in a general idea, expressed by the term *life*, but that idea is a concrete. It consists of a multitude of parts. The function of every organ, though a function of life, has an individual existence, in some respects separate from the aggregation, in others not. The function of the liver is not dependent, and yet it is dependent, on that of the kidneys;—not dependent, because the liver has within itself the power of secreting the bile, with the requisite organs;—dependent, because any great disturbance of the function of the kidneys, influences, and may even prevent the secretion of the bile. The idea of health implies, as the principal condition, an undisturbed function of each organ and of the whole. The idea of life implies, as the principal condition, the exhibition of the chief phenomena, the subsistence of the chief actions, proper to this whole.

The entire organism, and consequently each organ, each part of an organ, consists of elementary substances, which not only are individually supplied with indestructible forces, but may possess these under very different modifications. Oxygen, hydrogen, carbon, nitrogen, iron, sulphur, phosphorus, and iodine, are the substances which by mutual combination produce organic bodies; but to these are added a great many other substances, which are seldom wanting in living organic bodies. A great many acids, bases, and salts, are there present, which are just as indispensable to the existence of organic substances, as the eight elements above mentioned. Albumen, for instance, is an albuminate of soda; casein is a combination of protein with sulphur and phosphate of lime;—in a word, the intermixture of substances in the organs, and so in the organism, is not a simple but a complex one.

All these elements and compounds are severally accompanied by forces of their own. Their materiality is by no means to be called their chief characteristic, but that by which matter is governed, *i. e.* its peculiar force. They all manifest themselves as adapted for mutual combination, and appear after such combination as new substances, of which the forces are again modified, and applied by the chemist to produce new combinations.

If we pass in review the substances present in the organic kingdom, we perceive an endless series of combinations from either two or three or four elements only. This is enough to show that there is an unlimited capacity for modification in the primary forces which operate in the elements. The influence of one element upon another is thus unlimited also. A slight difference in the state of an element is sufficient to give it the appearance of a new and entirely peculiar substance, as compared with the other elements. Let us take, for example, starch, gum, sugar, acetic acid, glucic acid, inuline. All these are composed of the same elements, taken in the same proportions. Thus they consist severally in equivalents of

	Carbon.	Hydrogen.	Oxygen.	Water.
Starch, -	12	9	9	+ HO
Gum, -	12	9	9	+ HO
Sugar, -	12	9	9	HO
Acetic acid,	$\frac{1}{3}$X12	9	9	
Glucic acid,	$\frac{2}{3}$X12	9	9	—1½HO
Inuline, -	2X12	9	9	+ 2HO

The carbon of one of these substances is no doubt equal to the carbon of any of the others, in so far as it exhibits the same properties if separated from its combination. But it is incorrect to suppose that the carbon, hydrogen, and oxygen in sugar, are identical with those in acetic acid; for there is a great difference between sugar and acetic acid, and we cannot attribute this difference to any thing but the dlfference of the forces by which the same substance is governed. Thus the carbon, hydrogen, or oxygen, is not in any two cases supplied with the same properties. They assume in each substance a peculiar form. The general idea, comprehending carbon, hydrogen, or oxygen, in sugar and acetic acid, must therefore be modified, because the forces peculiar to matter must necessarily be modified, as matter is itself unalterable.

This will appear clearly, if we consider the combinations of carbon with hydrogen. If we supposed the carbon and the hydrogen in C^5H^4, $C^{10}H^8$, $C^{15}H^{12}$, $C^{20}H^{16}$, to be always the same, we should be constrained to assume the identity of the substances, and any distinction would be impossible.* Among the elements we know a considerable number which, without entering into any combination, present an entirely different appearance in consequence of but a slight difference in the circumstances under which they are placed. For example, phosphorus becomes black when heated and then suddenly cooled; and by means of a red heat merely, silica is so modified, that the substance after and before the application of such heat, might be taken for two different substances if we looked to its properties only. The interesting experiments recently made by Berzelius as to the allotropic character of phosphorus, have opened a new path for scientific investigations. If the *simple* substances can assume the permanent appearance of unlike bodies without forming any combination, their compounds can do so much more. And such an assumption of

* The term *impossible* here used, appears to me to be too strong. It restricts more than is necessary the powers of the elementary bodies to form compounds possessed of different properties. If into the simple combination CH, one of the elements, say C, enters in two several allotropic states, C_a and C_b, we can readily understand

other characters must take place in all cases, in which no other mode remains of explaining the diversity of the compounds, than in the supposition of a real difference in the component elements themselves.

If we apply these principles to the known compounds, a field of boundless extent is opened to our view. What we call protein in animal chemistry, is a substance which we know is composed of $C^{40}H^{31}N^{5}O^{12}$, which is insoluble in water, alcohol, or ether, but soluble in alkalies, whence it can be precipitated by acids; and which produces ammonia by means of powerful bases, &c. But what meaning do we attach to all this? What conception do we form from these considerations of the substance itself? We have a mere idea of distinction in comparison with other substances which have another atomic composition, and are soluble in water, alcohol, or ether, which in other words, possess different properties. But as little ground have we for supposing, that protein is the same in the very different modifications to which it is subjected in the body of animals, as we have for saying that the carbon in sugar and in acetic acid is governed by the same forces. The carbon in these two substances cannot therefore be comprehended under the same idea.

It is said in physiology, that in the embryo of the egg there is nothing but a shapeless mass, which by decomposition produces always the same substance —that is, a compound of protein. From this embryo, however, is gradually evolved, during the growth of the egg, a series of germs of organs, which are

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that C_aH and C_bH would or might be different substances, the peculiar properties of which arose from the peculiar state in which the C existed in each. This is Mulder's view, generalized from the very interesting facts contained in Berzelius' paper upon the combinations of sulphur with phosphorus, to which he subsequently refers. The idea is very simple and very beautiful, and throws a new light upon the combinations of the organic kingdom.

But how do the allotropic states of the elementary bodies themselves arise? It is the effect of circumstances, which are special for each state, upon the forces of *cohesion* which reside within the similar molecules. Of course, we cannot describe the way in which different circumstances act upon the molecules so as to produce these different states. It may be, that under certain conditions the forces become stronger in a given direction, and cause the molecules to unite in that direction rather than in another. And that such an arrangement, by different sides or at different distances, does sometimes take place among the elementary molecules, is shown, I think, by the fact, that in their several allotropic states, carbon and sulphur crystalize in different forms.

Now, if a new and different arrangement of the molecules cause or accompany a new allotrophic state in the elementary body, may it not be so also in the compound body? If C_aH and C_bH be different substances, because in each the elementary *molecules* of C are in different state of arrangement, may not $C^5_aH^5$ and $C^5_bH^5$ be different respectively from C_aH and C_bH, because of (or in connection with) a new arrangement of the *equivalents* of which they severally consist?

We thus recognize two causes of diversity among isomeric and polymeric bodies.

1. The different allotropic states of the elementary bodies, these being probably caused by a change in the relative positions or distances of their molecules.

2. A new arrangement, in distance or position, among the *equivalents*, whether of simple or compound bodies, the allotropic states of the elements remaining the same.

3. Both of these causes may operate together. Some of the equivalents of C or H may be in one state, some in another, and they may arrange themselves in many different ways, so as, in a complex body like protein, to produce numberless diversities or modifications in appearance and properties, the ultimate chemical composition remaining the same.

In other words, we may have an *allotrope* of the elementary molecules, an allotrope of the compound equivalents, or a combination of the two.—J.

soon developed as entire organs, together composing the chicken. A general force is assumed—influencing that shapeless mass, the embryo, originally existing as a whole—and represented as the same with that which the complete animal possesses. Müller says: "The simple embryo, which consists of a granular and shapeless substance, is to be regarded as the potential whole of the future animal, supplied with the essential and specific force of the future animal itself."*

No idea can be less distinct than this. According to Müller, the forces which will afterwards govern the organs of the chicken, should be present before the existance of those organs themselves—before the production of an organism from the granular and shapeless substance of the embryo.

If we recur to what we have stated as to the idea of force, then we see that Müller has deviated far from the true method of physical investigation. In physiology, the existence of a similar general force governing the whole, is assumed in the fully formed organism. Respiration, the circulation of the blood, the function of the nerves, &c., are effected by one force, which is called *vital force.* This vital force causes respiration here, digestion there, the secretion of the saliva and of the pancreatic juice in other parts of the body. It maintains at once the substance of the bones, of the muscles, and of the brain. It is supposed that this same force is modified with reference to the different organs which it influences. This idea is also inconsistent with a sound method. What would remain of the primary idea of force, if we saw force here causing motion, there effecting a chemical alteration, elsewhere producing feelings or sensations? It seems to me, that in its ordinary signification, vital force expresses an idea as incorrect as if we supposed that one single force, differently modified, operated in a battle fought by thousands—a force which acted so as to fire cannons and muskets, cut with swords, transfix with lances, sound trumpets, and keep men and horses in constant agitation. The army appears as a substantial whole, and produces phenomena. The organism, composed of the most different organs, also appears as a substantial whole, and produces phenomena. If we assume for the latter a single force, differently modified as the organs vary—a single *vital force* by which the whole is animated—then, to be consistent, we should assume a *fighting force* in a battle. The existence of such a vital force has by some writers been maintained, by way of distinguishing animate from inanimate nature; for in a stone there is no appearance of a general force capable of assuming new powers depending on the nature of the organ. A prejudice in favor of living nature, as peculiarly directed and sustained by the Almighty hand, has caused every opinion which conceived of these forces as residing in the molecules themselves, to be looked upon as savoring of materialism. It was not borne in mind, that, in adhering to an intricate and obscure idea of vital force, we do not at all approach to an acquaintance with the manner in which the organic world is maintained by that Almighty Being, who with unlimited wisdom created and still sustains every animate and inanimate substance. Let us then proceed to inquire, what forces we must regard as existing in organic substances, and where we must commence in order to arrive at a sound conclusion.

Let us ascend from the simple to the complex. It has been ascertained from observed facts, especially from the application of them by Liebig, that some plants† will grow, when supplied with carbonic acid, ammonia, and water, pro-

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* So muss man die einfache, aus kœrnigem formlosen Stoff bestehende Keimscheibe, als das potentielle ganze des Spætern Thieres betrachten, begabt mit der wesentlichen und specifischen Kraft des Spætern Thieres. (Physiologie, I, p. 23.)

† By saying, that *some* plants *will* grow, it is not implied that plants in general do not extract soluble organic substances from the soil, viz. humate of ammonia, crenic acid, aprocrenic acid, &c, It has, however, been ascertained, that many plants can, produce organic compounds from carbonic acid, water, and ammonia.

vided the bases and acids (that is, the salts) be added to these, which are necessary to every kind of plant. Now, it must be supposed, either that the plants are nourished by carbonic acid, water and ammonia; that with new materials the plants receive new forces from those bodies; or that the plants can themselves *communicate* forces to the elements of carbonic acid, water, and ammonia. The idea of *communication of forces* is unsound; it is only what is substantial that we can communicate. Forces may be *excited*, they cannot be *communicated.*

This is fully explained by the phenomena produced by the magnet. A piece of steel possesses magnetic forces, though not magnetized. These forces are slumbering, that is, such an equilibrium exists between them, that they cannot be externally perceived. They nevertheless exist. They reside in the molecules of the iron. Three elementary substances are known, to which they are peculiar: iron, nickel, and cobalt. Every particle of these metals is possessed of the same forces, and these forces can produce that series of magnetic phenomena which science has observed. But these forces cannot be communicated to tin, lead, or silver. When we appear to communicate them to a piece of steel, we merely excite what previously was hidden in it—we separate what was united. In the same way, plants excite forces in the elements of carbonic acid, water, and ammonia, whenever these are absorbed, and their elements are combined in different modes, so as to form acids, bases, neutral compounds, resins, fats, volatile substances, and the like. If there be only excitation from the plants, then the forces originally existed in the elements of carbonic acid, water, and ammonia: that is, though slumbering they *are* present. Hence it results, that every transformation in plants is effected by the molecular forces of carbon, hydrogen, oxygen, and nitrogen,—the elements of carbonic acid, water, and ammonia,—the forces being excited in these elements by the plants themselves.

By the plants themselves? What does this mean? Does *the entire plant* excite slumbering forces in carbonic acid at the very moment the acid enters the plant? Or is such excitation produced by some one *part* of the plant? It is produced by the part of the plant, with which the carbonic acid is in contact, at the moment it becomes changed, producing new and indeed organic substances by the assistance of water, or of water and ammonia. Let us take starch as an example. It is not the entire plant which produces a grain of starch from carbonic acid and water, with separation of oxygen, but a small organ of the plant. By this organ a force is exercised, exciting forces which slumbered in the carbon, oxygen, and hydrogen, or rather *modifying* the forces existing in these, so that 12 equiv. of carbon (C^{12}) combine with 10 equiv. of hydrogen (H^{10}), and 10 of oxygen (O^{10}), and from 12 equiv. of carbonic acid ($12CO^2$) and 10 of water ($10HO$) starch is produced—24 of oxygen (O^{24}) passing off.* Any one who imagines that there is any thing else in action than a molecular force, than a chemical force, sees more than exists. Such a phenomenon is common, and it is one of a chemical nature, produced as new combinations are in the inorganic kingdom. It is only the circumstances which differ.

Are those small organs, however, necessary to the manifestation of such forces? and are those forces to be found only in plants? Certainly the different organs, which produce different substances in plants, exert peculiar forces; yet these are not entirely peculiar to those organs. Gum and sugar can be formed without the intervention of the plant, as well as in its interior; and the same is the case with the benzoic, the cinnamic, and the valerianic acids. The chemical forces, therefore, which proceed either from the minute organs of plants, when they form new compounds out of carbonic acid, water and am-

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* We do not mean that starch is formed directly from CO^2 and HO; we have merely taken an example from a familiar substance.

monia, or from these substances themselves, after they are combined in some manner, may be excited in a common chemical way also; and so far as this cannot be effected, an accidental, not a real difference is the cause. Every compound substance requires peculiar circumstances for its formation; that is, the forces in the elements of the compound must be excited by the influence of peculiar circumstances. Starch and cellulose have not as yet been produced, except in the living vegetable, and may never be so; but neither is sulphuric acid produced in plants from sulphurous acid and oxygen, nor hyper-manganic acid, nor chloride of iron, nor deutoxide of hydrogen, nor peroxide of potassium. The forces excited in the elements vary with the influence which certain agents—temperature, moisture, light, &c.—exert. By the aid of crucibles and retorts, therefore, compounds can be formed which differ from those produced by the organs of plants; while from carbonic acid and water plants can produce cellulose and oxygen, a result which cannot yet be attained by art.

Of what do these small organs, producing cellulose and starch in plants, consist? They have originally been produced in a similar way with the grains of starch itself, that is, from carbonic acid, water and ammonia, by the operation of organs previously existing. On their production they received peculiar forces, as the grain of starch does. The difference between the organ producing starch, and the grain of starch produced, is not that the former can alone excite force, and that the latter is passive;—both are active in their turn. If starch be put in contact with nitric acid or sulphuric acid, the acid is powerfully decomposed by the starch, which itself at the same time is altered. Though starch be formed from carbon, hydrogen, and oxygen, it still exhibits, under different circumstances, a great variety of chemical tendency in its elements, inducing the production of new compounds. Starch yields gum and sugar by means of diastase; sugar may be changed into carbonic acid and alcohol by means of yeast. Alcohol is changed into oxide of ethyl by means of acids, and into haloid compounds of ethyl by means of hydracids; when brought into contact with the air, and either an organic substance or spongy platinum, it produces acetic acid; with a more elevated temperature, it produces aldehyde; by means of chlorine, chloral; and lastly, by combustion in the air, carbonic acid and water. At this point the primary elements of starch reassume the same form in which they were originally presented to the plants, part of the carbonic acid, however, being separated during fermentation. By the combustion all the elements become fitted again for nourishing plants, and a small organ, similar to that which produced the grain of starch, may again produce starch from this carbonic acid and water.

To express briefly what we have now endeavored to explain—no excitation of the forces of carbonic acid and water could be effected in the plant by its organs, unless those forces already existed, any more than chloral could be produced without alcohol and chlorine.

To whatever organ of plants we direct our attention, all, without exception, are formed in the same manner as the grain of starch, and all have received peculiar molecular forces at the moment they were formed from chemical molecules. The notion that heterogeneous forces can excite each other, is opposed to the primary idea of force. The force of gravitation cannot excite the magnetic force. It is only homogeneous forces that can set each other in action. Every decomposition, every formation of new compounds, the products of molecular forces, can therefore be effected by molecular forces alone; in other words, the small organs, which form a new compound from substances supplied to them, and which disturb the existing chemical equilibrium, can do so only through chemical forces possessed by them—through the chemical tendency possessed by their elements. This is the source of every excitation of new forces giving rise to a new combination. Further, these exciting forces proceed not from masses, but from molecules; they are molecular forces, and have therefore nothing in common with what proceeds from the whole individual.

Starch is not formed by the plant, but by the molecules of the organ in which it is produced, because those molecules modify the chemical equilibrium.

This may be explained by an example. During the transformation of starch into gum by sulphuric acid, each molecule of the acid influences each molecule of starch, the latter thereby becoming gum ;—the mode in which the elements of starch are combined is changed by the sulphuric acid, the alteration proceeding from molecule to molecule. The production of gum from starch in the organs of plants is similarly effected.

Wherever forces are found in organic nature, there are substances which are all supplied with molecular chemical forces. Even those singular structures, the nerves, consist of the same elements as the ordinary substances of the organic kingdom. It is thus undeniable, that the molecular forces act a chief part in the organism, so far as a change of substances takes place therein ; and that no general, no vital force, should be assumed as the source of those molecular forces. Such a vital force is irreconcilable with the true principles of science, which require that nothing should be *assumed* as existing, but that every thing should be *sought for* in nature ; which teach us to ascend only from an unprejudiced consideration of the phenomena to their causes, and to assign those causes only as we deduce them from the observed phenomena.

B. *The Developement of a Germ.*

If we review the phenomena of life, caused by change of materials, we must go back to the original formation of organs—to the growth of an individual from a germ. We perceive no greater trace of the future oak in the acorn, than of the chicken in the embyro of the egg. Should we say that the acorn is governed by an oak-forming force, the embryo by a chicken-forming force ? Does there exist a *general* force, which governs in particular all the molecules of tannic acid, starch, cellulose, &c., in the acorn, and all the particles of protein in the embryo of the egg ? A *particular* or *peculiar* force is the active cause of *peculiar* effects ; a *general* force is the active cause of *general* effects. Nobody can form any other idea of the terms. Though it cannot be denied, that in the embryo the rudiments of the future organs of the chicken are not to be found ; yet we do find the materials from which the first rudiments of organs will be produced, that is, we find rudiments of rudiments. The forces which are inseparable from matter, their molecular forces, are present as well as the materials. If in these molecules there exists no capacity of becoming the germ of organs, and if in the germ of organs there exists no capacity of ultimately becoming organs, no chicken at all is produced. The capacity, this predisposition, must be present in the molecules, otherwise the heat necessary for hatching would be insufficient to produce germs of organs in the first place, and organs afterwards. This is the only reason why the embryo of the egg will not produce an oak, nor an acorn a chicken.

As the materials differ, so do their forces. Some who concur in this conclusion are of opinion, that the granular shapeless mass, which they suppose exists in a passive state, is acted upon by a power that can only be described as a chicken-forming force in the embryo. This is, according to Müller, "possessed of the real and specific force of the future animal."* This animal, however, does not as yet exist, and not even a single organ, nor the germ of a single organ ; and are we to suppose that in that shapeless mass the *peculiar forces* of the animal exist, though that animal itself does not yet exist ? I must acknowledge that I can scarcely imagine a gall-secreting force in the perfect liver ; and I believe that no human being can possibly conceive of a gall-secreting force in the embryo of the egg, which does not yet possess even the rudiment of a liver. This idea is also unphysical. In the science of nature, we

* "Begabt mit der wesentlichen und specifischen Kraft des spætern Thieres."

infer that forces exist whenever phenomena are observed; but if the non-existence of the organs, by which they are produced, render such phenomena impossible, no question can arise as to such forces. No forces peculiar to the future animal, therefore, can exist in the embryo.

Let us take an example from the inorganic kingdom. If we evaporate a solution of sulphate of soda in water, we get prisms. In what form must we suppose that the sulphate of soda exists in the solution? In that of minute prisms? By no means. In that of molecules, supplied with a prism-forming force? We must reject this supposition too. There exists only a force of molecular attraction in a determinate direction, the formation of the prism being *the last result.* Between the first attraction of the molecules in a determinate direction, and that last result, a great variety of states of the particles intervenes—a great many forces come into action, which are present, and excited *only when* the molecules, by the position they have already obtained, cause them so to be excited in and by each other. According to the doctrine of isomorphism, the arrangement of the molecules causes the formation of the prism; for every substance composed, for instance, of MO and RO^3, assumes forms, among which a crystallographic connection exists.* But as we have no reason for imagining a prism-forming force in each of these isomorphic substances, in sulphates, seleniates, &c., so have we none for holding that this primary force, dependent on the position of chemical molecules, is the only cause of all the phenomena, perceptible during the formation of prisms.

Let us apply this to the substances present in the embryo. Whoever perceives in them nothing but protein and certain salts, examines that shapeless mass in a very cursory manner. Our object should be to mark what we see evolved from it, namely, compounds, which, in chemical composition, differ, either slightly, or not at all, from protein; but which, nevertheless, do differ from it.

This shapeless mass begins to exhibit, here and there, very small points, which are arranged as particles. These are produced from the existing materials—their forces being excited by the increase of temperature. Without elevation of temperature, such new arrangement of particles cannot be effected; but at the same time, the molecules must possess a capability of being arranged, just as those of the sulphate of soda. The first crystals of this salt cannot be formed without the evaporation of the water, &c., and without a power in the molecules of attracting each other in a determinate direction. In the embryo, this arrangement of the particles increases more and more, so as to produce a greater complication. This does not result from the primary molecular forces, till after they are modified by those other forces, which the substances received on their first arrangement. That first arrangement of the particles in the embryo, as well as the subsequent, must, of course, differ from that of sulphate of soda, because protein differs from sulphate of soda. But neither is the protein, in each of its particles, *the same protein, in a chemical sense*, that is to say, yielding on an analysis, no other results than C^{40} H^{31} N^5 O^{12}; the protein, therefore, does not always possess the same primary *arranging forces.* A minute difference of condition, causes the deposition of sulphate of soda in very different forms, either with 10 equiv. of water of crystalization, or without any; the former are produced at a temperature below, the latter, at a temperature above, 91° Fahr. The molecules of the substances which occur in the organic kingdom; those of carbon, hydrogen, oxygen, and nitrogen, seem to possess an unlimited power of forming mutual combinations. The number of compounds which they form, indeed, is incalculably great. That power is *inherent* in these ele-

*MO denotes the protoxide of any metal M, or a combination of one equivalent of oxygen (O), with one of metal. $RO3$ represents an acid composed of one equivalent of any radical R—such as sulphur or selenium—with 3 equivalents of oxygen ($O3$). All compound sulphates, seleniates, &c., represented by ($MO+RO3$) in which M may be any one of several metals, and R any one of several radicals, crystalize in forms, which are either identical, or are closely related to each other.—J.

ments; neither in the animal, nor in the vegetable kingdom do they acquire it; it is only excited therein. The diversities of form which the elements of sulphate of soda may give rise to, are very limited; but, on the other hand, the elements of protein, by their unlimited power of chemical arrangement, can originate endless diversities of combination. If subjected to chemical examination, each particle of the embryo in the egg would appear to be protein; but by such examination we could not discover the peculiar condition which makes one particle differ from another. We can no more explain this, than we can the real difference between the transparent and the milky arsenious acid, between the yellow and the red biniodide of mercury. We perceive the differences, however, and therefore admit their existence; and so when we perceive them in reference to albumen, fibrin, casein, we are equally constrained to admit them.

Undoubtedly, the differences which exist between the particles of the same organic substances are not chemical, in the ordinary gross signification, but are of the nature of those which are connected with polymorphism. The chemist gives us but a rude result—the composition in a hundred parts, frequently not affording us any insight into either the real characters of substances, or into their real differences. Whenever such dissimilar particles come together, a compound must be produced, possessing peculiar forces, which, though dependent upon the molecular forces of the elements, are yet not determined by these alone. The new arrangement causes a modification of those primary forces. Whenever it takes place, they appear modified, and therefore indicate their presence by producing new effects. In sulphate of soda, the whole collected forces of its constituent molecules—those of sulphur, sodium and oxygen, are still existent; and upon these alone depend its quailities, composition, and crystalline form. Sulphate of soda cannot possess other qualities—cannot become other in property—than what results from its elements, and exclusively originates in these.

Thus, then, we suppose that the molecules of the substances in the embryo are arranged, in the first place, simply, and afterwards more complexly. Not a trace of any organ is as yet perceptible, however; nor of any force, therefore, by which these organs will be governed. By the new arrangement of the particles, the molecular forces are modified anew, and this process is continuous. Although the primary forces, once united with the materials, remain the source of every action, of every manifestation of phenomena, of every chemical and organic, that is, physical, combination; they must, nevertheless, produce different effects, as the combinations become more complex. Each existing particle is the germ of a subsequent one, which is more complex; and, while the temperature necessary for hatching keeps the primary forces always excited, there is originated in the new arrangement of the particles, and also in the forces proceeding from the groups recently formed, a modification of these primary forces, which is constantly on the increase.

The whole material of the embryo in the egg is gradually brought, in this manner, within the circle of action. Then the circle is still more extended, and in its action are comprehended the elements of the yolk, and also of the albumen. These are erroneously called the food of the newly formed chicken, or its rudiments. In these elements there are forces also conjoined with the materials—chemical forces, analogous to those which exist in the embryo, and contributing to the production of the whole. These forces differ from those found in the embryo, not in nature, but only in direction, or in the mode of manifestation.

Thus we do not at all suppose that the molecules of the heart in the complete chicken, existed in the germ as heart molecules, or the molecules of the intestinal canal, as intestinal molecules. A series of successive transformations proceeds in the embryo—the production of the heart, of the intestines, of the whole chicken, being the final result. Neither do we suppose, therefore, either a heart or intestine-forming force in the germ, but a gradual evolution of new series of forces, resulting from the same primary forces of the molecules of carbon, hydrogen, oxygen and nitrogen, and which are to be considered as so many exhibitions of new groups, produced in their turn, by the causes previously in operation.

In this way, therefore, neither does a gall-secreting force exist previously to the production of a liver, nor the force which effects the circulation of the blood, previously to the production of the heart and blood-vessels.

It is an impenetrable mystery how the particles of substances, both organic and inorganic, mutually combine in different modes; and after they have so combined, again form new compounds. This mystery, as regards organic substances, is not, however, greater than that which surrounds the inorganic. If any one fancies that it is more easy to conceive of the production of a crystal, than of a texture composed of fibres, globules and cells—in other words, any animal or vegetable organ—of the origin of a precipitate, than a primary fibre; of the state in which crystalized sugar exists in solution, than of the first rudiments of organs in the embryo—he greatly errs. To penetrate into either is not as yet permitted to man.

To express our idea in a few words:—the elements of the organic kingdom, carbon, hydrogen, oxygen, and nitrogen, are susceptible of endless modifications of their primary forces. For that reason they can form, with minute changes, a great diversity of products; and, by the operation of the same primary forces, they stand towards each other in entirely different relations from those assumed by all the other elements; so that they can produce a peculiar series of bodies, which are called organic substances.

C. *Equivocal Generation.*

Upon the principles which have been stated above, no room is left for the dispute as to *equivocal generation* and *epigenesis*. The doctrine of epigenesis has originated in the idea, that every living thing is produced from ova alone; because in these only can all the forces and germs of organs exist, which are found in the future plants or animals. The dictum of Harvey, *Omne vivum ex ovo*, has been strongly defended, and has still many supporters. On a sound view of organic nature, and of the essence of organic beings, this doctrine appears to be perfectly reconcilable with that of equivocal generation. If we consider an *ovum* as an organic molecule, or an organic body, made up of the four organic elements, combined in various groups, then the conclusion of Harvey is no doubt true. It is still true, if by an ovum we mean a molecule of a determinate kind; for this is also proved by observation. The mites of cheese are peculiar to cheese; and from certain parts of plants—fruits, for instance—peculiar fungi are produced also. It is a general rule, that from organic molecules of a given and determinate kind, only specific compounds—specific or peculiar forms, can be produced.

The term *ovum*, however, has been taken in a limited sense, to signify the germ of an individual, produced by peculiar organs only;—a germ in which every thing belonging to the future animal is concentrated. Such ova, we have seen, do not exist. There is a *development*, an ascension from the simple to the complex. According to the supporters of the epigenesis, an egg is such a germ as, in favorable circumstances, always produce the same species. And in reality, the supporters of equivocal generation do not entertain any different idea. In their opinion, there exist certain organic substances,—organic molecules,—which are capable of evolving and forming something new, whence individual plants or animals may also finally proceed. Why may not cheese be an agglomeration of ova, that is, of molecules, from which an individual being may be produced, as from the ovum of an insect? It cannot be denied, that the existence of the spermatic animalcules proves, that animals, or at least their germs, gradually growing, by merely floating in a fluid, may be secreted. What we know of the Acarus Scabiei, the Filaria aracunculus, the Echinococci, and a great many other entozoa, shows that they may be produced from ordinary organic molecules in the animal body, as every small organic globule of mucus, for instance, or of milk, or of pus, &c., is formed. As the germs of the spermatic animalcules are secreted animal germs, so may the molecules of casein also be the *ova* of mites, though these remain molecules of casein. The one idea is not excluded by the other;—an egg consists of the white and the yolk; both, however, constitute the germ of a chicken. In the very same manner the ergot of rye (Secale cornutum) is produced. From the top of this same fruit, under different circumstances, a different plant arises—a fungus instead of a grass.

The idea of an ovum, therefore, coincides exactly with that of an *organic molecule;* that is, of such a molecule as consists of elements which may exhibit themselves under a change of circumstances in infinite modifications, may form new combinations, attract other elements, incorporate them, unite into definite compounds, and thus separate from other bodies with which they were originally combined. Among species of this nature, formed from organic molecules, are the mites in cheese, and the mould upon rotting fruits,—resulting from the molecular forces of the elements, like the spermatic animalcules.

Finally, the ordinary ova of plants and animals consist of nothing but organic molecules, such as those of which all organic substances are composed. They are the products of organic bodies, and therefore differ neither in composition nor in character from the germs of those substances which are said to originate through equivocal generation. Nay, it is a peculiarity of organic molecules that they produce organic molecules. As regards this leading characteristic, ova and organic molecules resemble each other. Of this the vegetable kingdom exhibits an endless variety of striking examples. The stem of a tree produces not leaves, but branches; these branches produce not leaves, but petioles, and the leaves grow from the extremities of these. From the end of the foot-stalk, a flower grows, in the same manner as a stem from the seed deposited in the soil. No other part can produce a flower. And in the flower itself every separte portion derives its nutriment from the spot to which it adheres. The extreme cells found there, are the feeding cells of entirely peculiar parts, which no other kind of cells can produce.

Each cell, therefore, is, as it were, the ovum of peculiar parts of plants; as, for instance, the cells at the very extremity of the petiole are the ova of the evolving leaf; the cells at the very extremity of the foot-stalk are the ova of the beautiful shaped flower, with all its parts. This is proved in the clearest manner by grafting.

The idea of an ovum is thus reduced, in truth, to that of an organic molecule; and the dispute as to equivocal generation and epigenesis is at an end. In the same way, however, the general vital force is reduced to molecular forces. The effects of the vital force are effects produced by the cells, which are so very diversified, and which exhibit phenomena that vary with their own differences. We must in a single plant divide this one vital force into thousands,—into as many forces as there are series of cells, producing different substances. In other words, we must reduce them to what is effected by the cells themselves; and thus we find that the idea of vital force coincides with that of molecular force.

D. *Transference of Vital Force.*

The idea of transferring vital force is opposed to the idea of force. A slumbering force is awakened, a weak force is strengthened; but it is impossible to imagine the transfer of a force from one material mass to another. We must first refer to this point in pausing to consider the manner in which we must conceive of the transition, into a new organic whole, of life originating in another. If animals impart vital forces to their offspring, then such forces must, no doubt, be lost by themselves. We do not perceive, however, that this takes place; on the contrary, they retain their strength, sometime for many years, after having produced new beings, completely formed, of the same species. The tree which, for a great many years, produces fruit, and the seeds of which have, in great numbers, become full-grown trees around the parent stem, nevertheless lives as at first. A single poppy plant produces thousands of small seeds, each of which grows up to be a poppy plant equally perfect. Hence we are not entitled to admit the idea of transference of vital force, even where it is not opposed to the idea of force itfelf. With our own eyes we perceive the evolution of that force from its phenomena.

What part then bears the germ of that vital force which is afterwards to be developed? and at what point does the development commence? It is the poppy seed which bears the germ,—a small quantity of organic molecules, different in nature from all others,—some carbon, hydrogen, nitrogen and oxygen, combined in a certain manner into substances, which are peculiar in respect to their composition and arrangement. The peculiar quality, which distinguishes these substances from amorphous precipitates and crystals, they owe to their origin. Their elements were brought into a state of peculiar tendency by the forces residing in the organic molecules of the plant, whose influence had previously governed them. They do not possess more than they exhibit, and they exhibit nothing but a change of their chemical composition. The seed changes its starch into gum and sugar, and so chemical forces forthwith appear, which seem to be intimately connected with the development of new forms. Another phenomenon soon succeeds, namely, the absorption of substances from without. These bring something with them: they are not merely *substances*, but the residence also of peculiar active forces to which they owe their composition. Of these forces, some portions combine with those which are derived from the seed itself. And now, the previously existing molecule, and that which is formed from external matter, both live—the former through what it received from the parent plant, the latter through its having assumed the form of the other. This conformity arises from matter, and from the property which matter possesses, of obeying the action of forces, proceeding from the first molecule; in other words, the second molecule must necessarily be endowed with the capacity of becoming similar to the first, through the influence of the first. Thus, the second molecule did not receive its peculiarities, but these were excited in it: and so this molecule also lives as well as the first, and in its turn can take up new substances from without, and entirely assimilate them. So again, the third lives also, and thus is life gradually extended. The molecules, after being once arranged in a certain manner, give birth to new arrangements, to new forces, as we endeavored before to explain.

From what has been stated, it will sufficiently appear, that the oak, when losing its acorns, loses none of its vital force; that an animal loses none by generation; but that, in both cases, the generating substances leave only certain quantities of matter, endowed with the power, first of somewhat increasing, then of taking up other substances, and with these just so much life as *they* afterwards manifest. Organic substances, whether called germs of food, possess properties of a peculiar kind, existing in the four elements of which they are all constituted.

Part Fourth.

MEDICAL INTELLIGENCE.

FOREIGN.

1.—*On the supposed Binoxide of Protein.* By Baron Liebig.

According to Bouchardat, if moist fibrine be immersed in water acidulated with a two-thousandth part of hydrochloric acid, the fibrine speedily becomes gelatinous, and on continuing the action of the dilute hydrochloric acid, a solution is obtained, which is turbid. This turbidity appears to arise from a slight admixture of a fatty substance. That portion of the fibrine which is thus soluble in hydrochloric acid, Bouchardat designates by the term albuminose, the insoluble part by that of epidermose.

Mülder states that this experiment was repeated by Herrn Von Baumhauer. The solution of fibrine in hydrochloric acid Baumhauer precipitated by means of carbonate of ammonia, and treated the precipitate with alcohol. After drying, the precipitate yielded, on analysis,

	1.	2.
Carbon,	53.64	55.62
Hydrogen,	6.88	6.73
Nitrogen,	15.88	——
Oxygen,	23.64	——

Mr. Mülder concludes, from these experiments, that "the albuminose is a product of the oxidation of protein, which, no doubt, is formed by the action of the atmospheric oxygen upon the matter formed and dissolved by the hydrochloric acid. It is *binoxide* of protein$=C_{40}H_{62}N_{10}O_{14}$."

We have of late been so overwhelmed with descriptions of substances, which, although greatly differing in their properties, yet, according to their analyses, must be considered as oxides of protein, that I was very desirous of convincing myself, by experiment, of the existence of at least one of them.

I have found, that if we are to understand by the designation, *binoxide of protein*, a substance which contains no sulphur, the substance investigated by Baumhauer cannot belong to such a class of bodies as oxides of protein, since the whole amount of sulphur present in fibrine, exists unaltered in this new substance.

In the first place, I observed that the atmosphere and its oxygen have no share in the solution of fibrine in hydrochloric acid. If moist fibrine is allowed to become gelatinous in dilute hydrochloric acid, and the mixture copiously diluted with water, and immediately afterwards filtered, before the solution has taken place, the presence of lime and potass may be detected in the filtered fluid. If the fluid be exposed to the protracted action of a higher degree of heat, so as to effect the solution, and a portion of this heated to boiling, with an excess of potass, the subsequent neutralization with acetic acid gives rise to a copious evolution of sulphuretted hydrogen, and a drop of acetate of lead produces in it a black precipitate. Upon solution of fibrine in dilute hydrochloric acid of the above given strength, therefore, the sulphur remains combined with the other elements of the fibrine. If this solution of fibrine in hydrochloric acid (the albuminose of Bouchardat) is mixed with a solution of common salt, sulphate of soda, or nitrate of potass, the mixture coagulates, forming a caseous or albuminous mass, which may be washed with a solution of common salt, the fluid which runs off, containing common salt, contains no longer any perceptible traces of lime or phosphoric acid: these two substances are, however, contained in the precipitate. The fluid contains no oxygen compound of sulphur, and gives no re-action of sulphuret of potassium when boiled with potass ley. But, according to Mulder, the albuminose of Bouchardat is binoxide of protein; were it so, the precipitate ought to contain neither phosphate of lime nor sulphur; but on dissolving it in potass ley, and boiling, and adding, subsequently, salts of lead, a black precipitate of sulphuret of lead is produced, and the action of acid produces an evolution of sulphuretted hydrogen.

This substance is, therefore, not a binoxide of protein.—*London Lancet.*

2.—*M. Bricheteau on the Antagonism of Ague, and of Pulmonary Consumption.*

This question has been much discussed of late by French medical practitioners, as our readers are well aware. M. Bricheteau, physician to the "Hôpital Necker," analyzes the various communications that have appeared on the subject, including documents from different parts of Algeria, from Bourdeaux, Strasbourg, Lyons, the department of the Ain, Rochefort, Rome, &c.,—all localities in which intermittent fever is rife,—and appears to come to the conclusion that there cannot be said to be antagonism between the two diseases—that is, exclusion of the one by the other; although the circumstances which favor the development of intermittents may be, and in all probability are, unfavorable to the development of phthisis. M. Bricheteau thus concludes his remarks:

"Although, on examining the etiology of these diseases, we do not find incompatibility between the causes of phthisis and intermittent fevers, it is impossible not to recognise, either in the climate of marshy districts, or in the influence of marshy miasmata over the economy, conditions favorable to tubercular patients. Our knowledge of this fact is to be referred to the authors of the labours which we have enumerated. But instead of calling to our assistance some obscure antagonizing tendencies, would it not be possible to account for this kind of prophylaxy, by attributing it to the moist uniform heat which reigns in some marshy districts, and which, by favoring the development of fever, may impede that of pulmonary tuberculization. Does not this appear proved by what takes place at Strasbourg, where the climate being both damp and cold, the town is ravaged by intermittent fever and by phthisis; whereas the more southern departments of L'Ain, La Niévra, Le Var, &c., are decimated by intermittent fevers, but offer very few phthisical patients? We may also add, that it is impossible to deny that in all countries intermittent fevers preserve from other affections. The Dutch appear to be aware of this fact, as Boerhaave informs us, that they are in the habit of congratulating themselves on the return of their fevers. The same Boerhaave, along with Hoffmann, Lancisi, and Sydenham, thought that intermittent fevers freed us from various diseases, and even predisposed to longevity: 'Febres intermittentes, nisi malignæ, ad longevitatem disponunt, et depurant ab inveteratis malis.' Some recent writers think that typhus fever is rarely met with in countries ravaged by endemic intermittents."—*London Lancet.*

3.—*Remarks on Scrofulous Degeneration of the Kidneys.* By Professor SCHOENLEIN.

The symptoms in the early stage of the disease have often a great resemblance or analogy, in the mode of their development, to those which usually indicate the existence of tubercles in the lungs. The constitution exhibits the marks of a scrofulous temperament, and the patient—not unfrequently a boy or girl of from seven to ten years of age—is always more or less ailing, especially after exposure to cold or any irregularity of diet. Complaint is made of an uneasy feeling in the loins; and, when a more particular inquiry is made, it is discovered that the uneasiness is felt, not in the course of the spine, but rather on one side of it. There are frequent calls to pass the urine, especially when the weather is cold and chilly. This frequent desire is often greatest during the night (just as we observe to be the case with the cough in tubercles of the lungs), and the child may therefore be apt to wet his bed during sleep. The urine at first has been observed to be pale and clear, or partially opaque and troubled, from an admixture of mucus; this comes from the bladder; and it is well known that, in the early stage of pulmonary phthisis, the sputa are at first from the trachea. As the disease advances, a more decided pain is experienced in the renal region, and then the urine may exhibit marks of an admixture of blood with it; here, again, we have an analogy to the hæmoptysis so frequent in the second stage of pulmonary tubercle. If the patient be arrived at the age of puberty, there will perhaps be erections, and crampy pains in the scrotum at the same time. Hitherto, the renal affection may be amenable to medical treatment. With respect to the treatment, local bloodletting, the ioduret of potassium, or mercury (rubbed in upon the loins), the use of salt baths, and of a mild unirritating diet,—these are the principal means to be employed. When the disease is further advanced, we can only relieve symptoms.—*Med. Chir. Rev.*, from *Schoenlein's Clinical Lectures.*

4.—*State of the Urinary Organs in Scarlatina.* By M. SCHOENLEIN.

There is a peculiar phenomenon that occurs in the convalescent stage of this exanthem, which is interesting and worthy of notice in many points of view. That there is an exfoliation of the mucous membrane of the mouth and fauces, is known to every medical man; but few are aware that a similar process not unfrequently takes place along the whole course of the urinary organs. That such is the case, may be discovered by examining the urine with the microscope. If this be done, we shall often find a large number of epithelial scales, which, to the unassisted eye, look, in the mass, like a mucous sediment or opalescent muddiness. Schoenlein is of opinion that this exfoliation of the mucous membrane of the uropoietic organs, is the real cause that predisposes the patient to that form of dropsy which is so apt to occur after scarlatina, and in which the urine is well known frequently to contain a number of blood-globules, as well as a quantity of albumen. Such a condition of the urinary secretion may very reasonably be regarded as indicating a state of high irritation of the mucous surface along which it flows. It is, therefore, a very natural and obvious deduction, that a patient should never be pronounced quite convalescent by his physician until not only the cutaneous desquamation has entirely ceased, but the urinary secretion also has resumed its healthy condition in every respect. If this rule were more uniformly followed in practice, many of the most unpleasant *sequelæ* of scarlatina might unquestionably be avoided. The patient should be strictly guarded from cold, and the state of the urine be sedulously watched for several weeks after the decline of the eruption.—*Med. Chir. Review; notice of Schoenlein's Clinical Lectures.*

5.—*The Causes of Albuminuria Illustrated by Experiments.*

M. Fourcault, being of opinion that albuminuria was a morbid result of suppression of the cutaneous function, instituted an examination of the urine of those animals whose surface he varnished or coated. He found that when dogs so treated began to exhibit symptoms of suffering and difficulty of breathing, the urine first became albuminous; the albumen being often mixed with blood-globules. Very generally, when the animal succeeded in removing the substance with which it was coated, the albuminuria ceased, and the urinary salts re-appeared in large quantities. A shaved rabbit was coated with dextrine, and so inclosed in an apparatus that the urine could be collected unmixed with fæces. A considerable quantity of albumen appeared in the urine. In another rabbit so treated, the pericardium was found to contain an albuminous fluid. The urine of dogs thus coated, previously acid, became gradually less acid, then neutral; and when it contained a large quantity of albumen, a tendency to alkalinity. M. Fourcault flayed Guinea-pigs and rabbits alive, replacing the skin in its proper position, and he was astonished to find that they lived two or three times longer than if they had been encased in an impermeable coating. They maintained their natural temperature, and were lively and vigorous to within a few hours of their death! If, however, a layer of dextrine was laid over the flayed surface, albumen appeared. From these and other facts, M. Fourcault infers that the skin is solely an excreting organ. Its function is to throw off the free lactic acid and lactates already present in the blood. If this acid be retained, it is in excess, and, destroying the equilibrium of the organic affinities, precipitates albumen upon the urinary organs, when the soda of the urine renders it soluble. The cutaneous salts being also thrown back into the circulation, pass off by the kidneys, and render the urine alkaline.

According to the preceding hypothesis, the introduction of lactic acid into the circulation would be followed by albuminuria, and M. Fourcault details some experiments which, in his opinion, prove the fact decisively. These experiments do not, however, appear to be of more than questionable value. The same remark applies to the comparison of the phenomena of the cholera Asiatica with those of suppressed cutaneous excretion.—*Brit. and For. Med. Rev.*

6.—*Case of Excessive Secretion of the Ammonio-magnesian Phosphate by the Kidneys, with long-continued Vomiting.* By Golding Bird, A.M., M.D., F.R.S., Fellow of the Royal College of Physicians, and Assistant-Physician to Guy's Hospital. Royal Medical and Chirurgical Society, March 24, 1846: Dr. Chambers, F.R.S., K.C.H., President, in the Chair.

This case was brought forward by the author in confirmation of the value of irritability of the stomach as a diagnostic sign of calculous affection of the kidney. A lad, æt. 18, was admitted into Guy's Hospital, under Dr. Golding Bird's care, with all the symptoms of scirrhous pylorus, with the exception of non-existence of epigastric tumor. He had for four years been subject to constant irritability of stomach, and had never for 24 hours consecutively ceased from vomiting, never having retained a meal during the period alluded to, for more than an hour, often only for a few minutes. He was emaciated to a skeleton, and appeared actually sinking from exhaustion. There appeared (unless the vomiting were regarded as such) to be no demonstrable evidence of organic disease; the urine had a fœtid, fish-like odour, was copious, limpid, rarely whey-like, of moderate specific gravity, and deposited crystals of triple phosphates in abundance. No hippuric acid or other abnormal element. The urine was alkaline on secretion, and always contained a large excess of the crystals alluded to. Treatment directed specially to the gastric symptoms, having failed to relieve in able hands, Dr. G. Bird prescribed strychnine in doses of gr. 1-16. The result was the gradual diminution of the gastric irritability, the restoration of the acidity of the urine, and the disappearance of the phosphatic crystal. After a few weeks' continuance of the medicine the lad became convalescent, and actually fat. The author entered then into the question of the etiology of the disease, and expressed an opinion of its connection with functional derangement of the spinal and ganglionic nerves. In conclusion, he pressed upon the notice of the meeting the value of strychnine as an anti-emetic remedy.

Mr. Lloyd was of opinion, with Dr. Bird, that the secretion of phosphatic urine, in cases of disease of the spinal marrow, was by no means uncommon; indeed, it was a sign for which he generally looked in those cases, but this state of urine was not, in his experience, at all associated with loss of flesh, or with stomach disease, as noticed by Dr. Bird. He had frequently met with these cases, and he mentioned one in particular which had existed for several years, the chief symptom being pain in the lower part of the spinal region; the disease was considered to be dependent on repeated attacks of lumbago. The urine, however, eventually became phosphatic, and the true nature of the case revealed. He was in the habit, in all such cases, of examining the urine by the aid of the microscope, and had always found phosphatic crystals present. He was not aware whether Dr. Bird, in his paper, had alluded to any other morbid products which were found in cases of this description; but he, Mr. Lloyd, had generally detected much mucus, or a great number of pus corpuscles, in phosphatic urine generally. He mentioned one case in which the urine was loaded with phosphates, and contained a great number of epithelial cells and other morbid products. He had never met with a similar example. In this case, also, the stomach did not sympathize with the morbid condition of the spine; the patient was still under treatment, but progressing towards a state of convalescence.

Dr. Golding Bird could not help thinking that two very distinct classes of cases had been confounded by Mr. Lloyd, both of which were characterized by the presence of phosphates in the urine, but the circumstances of each class of cases were widely different. One of these classes of cases, occurring frequently to the surgeon, consisted of those in which a diseased secretion was poured out from the mucous membrane of the bladder, as the result of chronic inflammation of that organ, stricture, enlargement of the prostate, or where, after mechanical injury to the spine, a more or less complete paraplegic condition had been induced. In these cases, the alkaline and phosphatic state of the urine was produced from changes taking place in the fluid after it had reached the bladder. The urine in these cases was always ropy, and also frequently fœtid. If, in these cases, however, the bladder were carefully washed out, and the urine secreted during the next few minutes were examined, it would be usually found to be acid. The second class of cases were altogether of a different nature from these, which, from their dependence on mechanical lesion, were strictly surgical. In the second class, from some antecedent cause, very likely having relation to the function of the spinal nerves, a low form of inflammation or irritation was set up in the kidneys, and the consequence was, a secretion of alkaline urine, which urine contained an excess of phosphates. Dr. Bird believed that the mechanical irritation of the tubular structure of the kidneys by the phosphatic crystals, was the immediate exciting cause of the irritation of the stomach, or of the vomiting.

Dr. Taylor thought that the distinction to which Dr. Golding Bird had just referred was an important one,—viz., that between urine which was secreted in an alkaline condition, and urine which had become alkaline only in consequence of chemical changes taking place in it after its secretion. This distinction had been first drawn, or, at least, had been especially insisted on, by Rayer, who considered that urine, which is secreted alkaline, is an important symptom of simple chronic nephritis. Dr. Taylor had had the opportunity of satisfying himself of the fact that the urine is in some cases alkaline when secreted, having made the experiment, referred to by Dr. Bird, of washing out the bladder with water, and collecting the urine in a very short time afterwards. In more than one of these cases he had also had the opportunity of ascertaining, after death, that the kidneys were inflamed, and there was no other appearance to account for the alkaline urine. Whether this class of cases, however, is attended with more disorder of the stomach than other cases in which the kidneys are diseased, he thought was another question. Nausea and vomiting are well known to be common symptoms of various morbid conditions of the kidneys. In the cases of alkaline urine, observed by Dr. Taylor, the sickness had not been more marked than in other renal affections. The same thing seems to be implied, if it be not directly stated, in the work of Rayer. The great reason why Rayer insists so much upon the importance of alkaline urine as a symptom of nephritis, is this, that very often there is no other obvious sign of the disease. In such cases the inflammation runs a very insidious course. A man complains of some languor, and looks a little out of health, and he may or may not have some slight aching about the loins, and these may be the only symptoms of the disease except the alkaline urine.

Dr. Bird could not believe that the mere secretion of alkaline urine, when there was no irritation from a calculus, was sufficient to produce vomiting. He referred to Guy's Hospital Reports, which contained a paper by Dr. Barlow, as evidence in favour of this opinion.

7.—*Materia Medica and Pharmacy.*

Aconite. The most important contribution to our knowledge of the materia medica, that has appeared during the last six months, is undoubtedly Dr. Fleming's treatise on aconite.* Passing over the first two sections, which embrace the consideration of the history, botany and physical characters of the aconitum napellus; the influence of climate and culture on its properties; the respective activity of different parts of the plant; the influence of seasons on the activity of the roots and leaves; and the physiological action of the plant on vegetables and animals,—we arrive at the consideration of its physiological effects on man. The topical action is first considered. It acts as a direct sedative to the nerves of sensation, and, as might be expected, its action is most marked when applied to a surface abundantly supplied with nerves. The physiological action on man, in small or medicinal doses, is considered under the four following degrees of operation:

First degree of operation.—In the course of twenty minutes or half an hour after the exhibition of five minims of the tincture, a feeling of warmth in the stomach is usually experienced, which is occasionally accompanied by a slight nausea and oppression of the breathing. After the lapse of thirty or forty minutes, this sense of warmth is diffused throughout the body, and in a few minutes more is attended by numbness, tingling, and sense of distension of the lips and tongue. There is also tingling at the tips of the fingers, and a peculiar sensation is felt at the roots of the teeth. The feeling of warmth soon disappears, but the numbness and tingling of the lips and fingers continue for a period, varying from one to three hours. Slight muscular weakness is generally experienced, with indisposition for exertion, either mental or corporeal. In about half an hour more the pulse is found to be diminished in strength; and in another hour, both the pulse and the respiration have become less frequent. Thus a pulse which, in the normal state, beats seventy-two in the minute, will by that time have fallen to about sixty-four, and the respirations, supposing them to have been eighteen, to fifteen or sixteen.

Second degree of operation.—Should a dose of ten minims be given at first, or a dose of five minims be succeeded in two hours by another of equal amount, these symptoms supervene more rapidly, and with greater severity. The tingling extends along the arms, and the sensibility of the surface is more or less impaired. In an hour and a half the pulse will probably have fallen to about fifty-six beats in the minute, and become smaller and weaker than before,

* An inquiry into the Physiological and Medicinal Properties of the Aconitum Napellus. By Alexander Fleming, M. D., President of the Royal Medical Society of Edinburgh. London, 1845.

still maintaining, however, perfect regularity. The respirations will have diminished to about thirteen, presenting at the same time a slow, laboring character. Great muscular debility is now experienced; a giddiness, with confusion of sight, comes on when the erect posture is assumed. The individual sinks into a lethargic condition, evinces great disinclination to be disturbed, although he rarely falls asleep, and complains much of chilliness, particularly in the extremities, which are cold to the touch. These phenomena continue in their full intensity from three to five hours, when they gradually disappear, a sensation of languor, which lasts for several hours more, alone remaining. This is the utmost extent to which I would recommend the physiological effects of aconite to be carried, in order to obtain, with safety and success, its therapeutic action.

Third degree of operation.—On the administration of five minims more, two hours subsequent to the last dose, the sense of warmth, and the numbness and tingling, again spread rapidly over the body. The sensibility of the surface is still further diminished; lancinating pains in the joints are occasionally complained of; the headache, vertigo, and dimness of vision are aggravated; the countenance grows pale and anxious; the muscular feebleness increases; the voice becomes weak, and the individual is frequently impressed with the dread of approaching dissolution. Occasionally the pulse is reduced still further in strength and frequency, perhaps falling to 40, or even 36 beats per minute, but still maintaining its regularity. More frequently, however, it rises to 70 or 80, and becomes small, weak, and probably more or less irregular. The respiratory movements are also irregular, being either short and hurried, or deep and sighing. The surface is moist, and still farther reduced in temperature. Sickness may now come on; and, if formerly present, is much aggravated, and probably attended by vomiting. These symptoms do not entirely subside for two or three days.

Fourth degree of operation.—On the administration of a fourth dose of five minims, two hours after the third, the symptoms assume a more alarming character. The countenance becomes pale and sunken; froth issues from the mouth, and the prostration increases. Some thus affected have stated that they felt as if dying from excessive loss of blood. Consciousness usually remains, or there may be slight wandering delirium, as occurs also after profuse hemorrhage. The voice is whispering, or is altogether lost. The pulse becomes still smaller, weaker and more irregular, and the breathing more imperfect. The surface is colder than before, and is covered with a clammy sweat.

I have seen patients recover from this state under the administration of proper remedies. When the action of the drug is carried to a fatal extent, the individual becomes entirely blind, deaf and speechless. He either retains his consciousness to the last, or is affected with slight wandering delirium, the pupils are dilated, general muscular tremors, or even slight convulsions, supervene; the pulse becomes imperceptible, both at the wrist and heart; the temperature of the surface sinks still lower than before, and at length, after a few hurried gasps, death by syncope takes place.

Dr. Flemming then treats in detail the effects of aconite on the different systems of organs. We regret that we have merely space for his most important conclusions.

With respect to the action of aconite on the cerebro-spinal and muscular systems, he finds:

1. That it is calmative, anodyne, and antispasmodic.
2. That it is an advisable antiphlogistic in apoplexy, phrenitis, or any disease in which the circulation of the brain is excited.
3. That it is contra-indicated in headache arising from anemia or chlorosis, and whenever there is a torpid or paralytic condition of the muscular system.
4. Its properties suggest its employment in convulsive or spasmodic diseases.

The following are the practical inferences deducible from a consideration of the action of aconite on the circulation:

1. That it is a powerful antiphlogistic.
2. That it is calculated to be of great value in all cases when there is inordinate activity of the circulation.
3. That it is contra-indicated, when there is obvious mechanical impediment to the passage of the blood, particularly through the heart or lungs. It is requisite, therefore, in every case, to ascertain that no such obstruction exists before commencing its use.
4. That it is contra-indicated, whenever there is irritability of the circulation, with great diminution of power, such as occurs after severe hemorrhage.

The practical inferences respecting its action on the respiratory system are these:

1. Aconite will probably be found a highly advantageous antiphlogistic in pneumonia, pleuritis, &c.
2. It seems calculated to be serviceable in spasmodic asthma.
3. It is contra-indicated in difficulty of breathing, arising from any other cause than inflammation or spasm.
4. In cases of advanced bronchitis, with excess of secretion, it would prove highly injurious, by diminishing still further the power of expectoration.

After noticing the effect of aconite on the alimentary canal and secretory system, he proceeds to the consideration of the effects of the drug in large and poisonous doses. This section belongs more to medical jurisprudence than to materia medica. He concludes it with the observation, that "four grains of the alcoholic extract have proved fatal, and and two grains have produced the most alarming symptoms."

Section 4th, embracing the therapeutic action of aconite, abounds in matter of the highest interest, and deserves a most attentive perusal. We could not, in justice to the author, much abbreviate it, and we shall therefore conclude our notice with a few observations on the method of administering aconite. The preparations expresssly noticed by Dr. Flemming are:

a. Tinctura Aconiti. Take of root of A. Napellus, carefully dried and finely powdered, 16 ounces troy; rectified spirit, 16 fluid ounces; macerate for four days, then pack into percolator: add rectified spirit until 24 ounces of tincture are obtained. It is beautifully transparent, of the color of sherry, and the taste is slightly bitter.

b. Extractum Alcoholicum Aconiti.—This is prepared by distilling, at a low temperature, the spirit for the tincture, until the consistence of an extract has been obtained. The process should be completed in a vapor bath. The color is dark brown, or almost black; it has an agreeable smell, and a bitter taste. the dose is one-third of a grain thrice daily; commencing with one-sixth of a grain.

Of these two preparations, Dr. Flemming prefers the tincture, from its geater uniformity of action; the average dose is five minims three times daily, and to be increased, if requisite, by one minim each dose.

c. For external use, the following formula is recommended:

℞.—Aconitinæ . . gr. xvj.
Spirit. rect. . . ♏xvj. fere optime.
Deinde adde axungiæ . ℥ j, ut fiat unguentum.

If, after a few applications, the ointment loses its effect, the proportion of aconitina must be increased to three, four or even eight grains to the drachm.—Day's *Report* in *Ranking's Half-yearly Abstract*, vol. ii.

8.—*Turpentine in Large Doses in the Treatment of Purpura Hemorrhagica.* By Dr. J. Moore Neligan, (*Monthly Jour. of Med. Sci.*, Dec. 1845.)

Dr. Neligan, whilst acting as one of the Physicians of the city of Cork Dispensary, met with eight cases of purpura hemorrhagica of the worst form; the district in which they occurred being the poorest in the city, and those attacked with the disease were nearly all of broken-down constitutions, owing to over-work and insufficient nutriment. As they were of an asthenic character, he treated the first two cases which came under his care, on the tonic plan, without success; in the next case he had recourse to free purgation; but this case, which was not seen, however, until the disease was very far advanced, also terminated fatally. The fourth case, in which the individual was younger and of a more robust habit of body, terminated favorably under the free use of purgatives.

"From the result of these four cases I was led," Dr. N. states, "to place but little reliance on the use of barks and acids in the treatment of this disease, and to look more favorably to the employment of purgatives. I thought, however, that still more favorable results might be expected from the administration of oil of turpentine, which, while it acts as a powerful cathartic, also possesses the property of checking hemorrhage, depending on an atonic state of the smaller blood vessels, owing, probably, to its powers as a diffusible stimulant. In consequence of those views, I employed this remedy in the four cases that afterwards came under my care while in charge of the district and they all recovered. I prescribed the oil both in the form of draught and of enema; the usual dose for adults being from one ounce to an ounce and a-half, and for children from two drachms to half an ounce, generally in combination with castor oil, to render its cathartic action more certain.

"Since that time I have employed oil of turpentine in every case of purpura which has been under my care, and its use has been invariably attended with beneficial results."

Three cases are related by Dr. N. in illustration, one of which is the following:

William Flanagan, aged 50, a laborer, admitted into Jervis-street Hospital, July 1, 1845. The entire of the body and limbs is covered with small circular spots of various size and color; from half a line to a line in diameter, and varying in color from the florid red of arterial blood to a purplish-black hue. There are also several large, ecchymosed patches of a deep greenish-purple colour; those are situated chiefly on the right mamma, the elbows, the loins, and the backs of both legs. Firm pressure produces no effect on either the small or large spots. He complains very much of weakness, with pain in his back, which, together with a feeling of great lassitude, has, from the commencement of his illness, altogether prevented him from working. He is constantly coughing up a frothy serum, deeply tinged with blood; the gums also bled slightly, and he states that, previous to his admission into hospital, he passed bloody stools. The pulse beats about 60 in the minute, but is feeble and very compressible. The body is emaciated, and the countenance very expressive of anxiety.

In early life the patient was addicted to intemperance, nevertheless he enjoyed perfect health until the first attack of the present disease, which was about six months ago. Since that time he has been repeatedly attacked with the disease, but at no time so severely as at the present. He was in an hospital during the first seizure, where he was cured of it, but it reappeared in three months afterwards; he was again admitted into the same hospital, but having been discharged before the spots completely disappeared, they in a few days began to increase in size and number, and he has never been free from them since. The great size of the vibices, together with the bloody dejections and sputa, and the complete prostration both of mind and body, compelled him at length to seek admission into this hospital.

July 2d. Many new spots have made their appearance since yesterday, and the bowels have not been moved since his admission. ℞.—*Olei terebinthinæ ℥ iss; syrupi ℥ ii; aquæ menthæ piperitæ ℥ ii. Misce. Fiat haustus statim sumendus.*

3d. Was somewhat intoxicated yesterday after taking the draught, which vomited and purged him freely, the stools being slightly mixed with grumous blood. He feels much better to-day, and eats with an appetite, which he has not done for some time. The spots are darker colored than on admission, and some new ones have made their appearance, but the sputa are not so bloody.

4th. The large blotches are fading, and turning of a yellowish-green colour, while the small spots are disappearing; sputa still tinged with blood; bowels not moved yesterday. ℞ —. *Olei terebinthinæ* ℥ *iss; olei lini* ℥*i; decocti hordei* ℥*xvi. Fiat enema, et statim adhibeatur.*

5th. The patient is improved in every respect, with the exception of the sputa, which are more bloody; the bowels were affected only once by the enema; there is no appearance of blood in what he passed. ℞ ,—*Olei terebinthinæ* ℥ *i; syrupi* ℥ *ss; aquæ menthæ piperitæ* ℥*ii. Misce. Fiat haustus, statim sumendus.*

7th. Still improving; both large and small spots are gradually disappearing; bowels rather confined. The draught to be repeated and to have full diet.

9th. Feels quite well to-day; none of the small spots to be seen, and the larger blotches much diminished in size; has had no expectoration for the last two days; as the bowels were confined, he was ordered the common castor oil draught.

12th. Flanagan was discharged to-day quite cured, having been kept in hospital until all the stains disappeared from the skin.

9.—*Variola, varioloid diseases, and vaccination.*

Dr. L. Wagner* describes a very mild *epidemic of smallpox* in the district of Neufeld, near the Danube. The disease attacked 2509 out of a population of 70,000, who resided in a district containing twenty square miles. Of those who suffered from the disease, 102 only had been vaccinated, and but nine of these presented well-marked cicatrices. The disease was very mild even in those who had not been vaccinated, since the total mortality did not exceed 222 or 8·7 per cent. In all the vaccinated, the disease ran a modified course, and only one of them died. In many cases when vaccination was practised in consequence of smallpox having occurred in the house, variola appeared on the 2d or 3d day afterwards, while at the same time the vaccine vesicle ran a perfectly normal course. Dr. Woppisch† gives the particulars of an epidemic of smallpox at Zeitz, in 1841, which appear to him to support the opinion of the *identity of varioloid and variola.* The facts on which he founds his opinion, are that the first two cases which occurred, were cases of varioloid in two vaccinated children, the next in the same house was a case of variola in an unvaccinated child. At the commencement of the epidemic, the vaccinated suffered exclusively from varioloid, the unvaccinated from variola, but as the disease grew more prevalent, varioloid occurred likewise among the unvaccinated. Most of the cases of variola occurred in unvaccinated children under the age of 1 year, not a single vaccinated child under 7 years had true variola, and only 12 a very mild form of varioloid. Up to the age of 14 indeed, all the vaccinated children who were attacked, had a very mild varioloid; while persons between the ages of 20 and 40, although vaccinated in their infancy, had confluent varioloid closely resembling smallpox. From these facts, Dr. Wagner infers that the varioloid is smallpox mitigated by vaccination. This conclusion, however, is opposed to observations made apparently with equal care by Dr. Fisher‡ of Tambach, in the Duchy of Gotha, who observed an epidemic of *varioloid quite independent of smallpox*, but alternating with epidemic scarlatina. He found his opinion as to the non-affinity of the two diseases, on 1st, the shorter duration of the eruption, the fact that it appeared first on the extremities, and that it was always succeeded by desquamation of the skin. 2d. The absence in its course of any affection of the conjunctiva. 3d. The invariable occurrence of erythema before the eruption, and the fact that the red spots of the early eruption, had not the central hardness of

* Oesterr. Med. Jahrb. Nov. 1844. † Med. Zeitung, Feb. 28 and March 20, 1845.
‡ Casper's Wochenschr., Dec. 28, 1844.

variola. 4th. The absence of smallpox odour, or of the suppurative fever, and the desiccation of the pustules on the 6th, not on the 8th or 9th day after their appearance. 5th. The circumstance that the course of the attack was in no degree modified by previous vaccination. 6th. The very mild character of the epidemic. M. Legendre* has investigated the very difficult subject of the *simultaneous existence of variola and vaccinia*, of which he has observed 10 instances. His conclusions, which are founded on a comparison of 56 observations derived from different sources, are to the effect that vaccination almost always modifies the characters of variola, but that the performance of vaccination in a child previously exposed to the contagion of smallpox, seems to favor the appearance of that disease, though in children above 4 years of age it usually appears in a favorable and greatly modified form. That while vaccination performed during the incubation of smallpox, modifies the characters of that disease, the vaccine vesicle itself is usually modified in a degree directly proportionate to the shortness of the interval between the performance of vaccination, and the appearance of smallpox. When vaccination is performed after the appearance of variola, the vaccine vesicle sometimes runs its course, but does not modify the variola. The practical inferences which he deduces, is that in young and weakly children who have been exposed to the contagion of variola, the performance of vaccination only increases the danger, and is therefore to be avoided. Mr. Wylde† brings evidence of the *good results of vaccination in Ireland*, from the tables drawn up during the census of that kingdom, in 1841. He shows that, notwithstanding the vast increase of population of Dublin, the deaths from smallpox during the past 10 years, have scarcely amounted to half of the number who died from the same cause in an equal space of time during the middle of last century. He states further, that the superiority of vaccination over inoculation is shown by the fact that smallpox mortality is highest in those provinces in which inoculation is most practised, and vaccination least. The proportion borne by smallpox to all other epidemic diseases is—

Leinster . .	1 : 8-9	Ulster . . .	1 : 5-96	Dublin . .	1 : 13-79
Munster . .	1 : 6-6	Connaught .	1 : 5-35		

[This, however, is not of itself proof of the rarity of smallpox in Leinster or Dublin. It may result, and in Dublin it doubtless does, in part from the greater frequency of typhus and other epidemic diseases.]

The comparatively small *success of vaccination* in India, has given rise to an inquiry, the results of which are contained in the valuable Report of Dr. Duncan Stewart.‡ The chief causes of this want of success may be referred to the heads of—1st. Native prejudice. 2d. The propagation of a spurious disease, owing to the carelessness of native vaccinators. 3d. The influence of climate, which for about six months in the year renders the vaccine vesicles imperfect, and for three out of those six months so modifies the virus as usually to render vaccination altogether unsuccessful. 4th. The fact that this influence of climate varies much in different parts of India, coming into operation in some places as early as March, in others not till two or three months later; and the additional fact that a similar variation will take place at the same locality without any known cause. 5th. The existence of some constitutional peculiarity in the natives, which renders them indisposed to the reception of the vaccine virus, or at least interferes with the full development of the vesicles; and renders the protection afforded by vaccination imperfect, as is shown by the fact, that smallpox after vaccination occurs in a grave form more frequently in natives of India than in Europeans. In a paper read before the Medico-Chirurgical Society,§ Dr. Gregory mentions some facts which transpired during the

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* Arch. Gén. de Méd. Sept. 1844. † Edinb. Med. and Surg. Journal, April, 1815.

‡ Report on Smallpox in Calcutta and Vaccination in Bengal, 8vo; Calcutta, 1844.

§ On Jan. 28, 1845; reported in the Medical Gazette, Feb. 7, 1845.

smallpox epidemic of 1844, and which shows the preservative powers of vaccination in a rather questionable light. He states that the deaths from smallpox in London, were during its prevalence as numerous as 60 years ago, that half of the patients who were received into the Smallpox Hospital had distinct cicatrices of vaccination, and that 8 per cent. of them died. Since then at least 7 per cent of those who have smallpox after vaccination die, while the deaths from inoculated smallpox do not exceed 1 in 500; he is disposed to recommend inoculation as a test of a person's safety more satisfactory than revaccination. He would therefore wish for such a modification of the present government regulations as would allow of the performance of variolous inoculation of persons between the ages of 10 and 20, as a test of the success of their previous vaccination, and of the persistence of its protective power. M. Calosi* makes the assertion, unsubstantiated, however, by confirmatory documents, that of 38,137 inhabitants of Tuscany, vaccinated in infancy, whose ages varied from infancy to 34 years, many have been revaccinated without success, and none have contracted variola, though among them are several who have frequently been exposed to its contagion. He hence draws a conclusion adverse to the supposition that the protective power of vaccination becomes impaired by time, and consequently adverse to the practice of revaccination. The question of the degeneracy of the vaccine virus, of the decline of its protective power by the lapse of time and of the *utility of revaccination*, continues to engage much attention on the continent. Revaccination is still practised annually throughout the whole Prusssian army, with a tolerably uniform result, the disease being produced in about 50 per cent. of those who are vaccinated.† M. Villaret§ has published an account of a series of revaccinations carried on during four years in a regiment of dragoons. The following are the results which he obtained:

	Number vaccinated.	With success.	Unsuccessfully.
Had had smallpox,	273	183	90
Had been previously vaccinated, with success,	848	716	132
Had been previously vaccinated, without certain success,	121	94	30
Had not had smallpox, nor been vaccinated,	160	150	10
	1405	1143	262

Dr. Condie‡ has from various sources compiled tables which yield 133,042 successful revaccinations, and 53,654 spurious vesicles out of 346,583 revaccinations. It further appears that out of 220,818 persons who were revaccinated, 173,659 had perfect cicatrices, 32,418 imperfect, and the remainder had none at all. In 87,399 of these, revaccination was perfectly successful; and on subjecting 61,746 of those in whom it had failed to a second revaccination perfect vaccine vesicles were produced in 9,238. Almost all writers on the subject agree in advocating revaccination, and likewise coincide pretty nearly in the arguments they adduce in its favour. The late Dr. Forry,|| whose premature death is a loss to medical science, M. Schæffer,¶ Dr. Losetti,** and the candidates for the prize offered by the French Institute for the best essay "on vaccination and its influence on smallpox,"†† agree on this point. Their main arguments are derived from the fact, that while smallpox occurs but seldom after vaccination in children under ten or twelve years, its attacks are much more frequent after this period, and increase both in frequency and severity up to about the age of 35, when the constitution seems to acquire a comparative insusceptibility to the poison of variola. Hence they deduce the practical inference that a second vaccination should be performed at about the age of 15; and its repetition again at 25 has not been without its advocates. Of course,

* Bulletino dell Scienze Mediche, Giugno 1844.

† The results of the year 1813 are contained in Med. Zeitung, April 3, 1844; those of 1844, Ibid., April 9, 1845.

§ Gazette Médicale, Mars 2, 1844.

‡ Op. cit.

|| New York Journ. of Med. Sept. 1844.

¶ Med. Zeitung, March 27, 1844.

** Gaz. Méd. de Paris, 11 May, 1844.

†† Revue Médicale, Mars 1845.

there are many facts by which they support their opinion, as well as some objections which might be raised to their inferences, mention of which is prevented by the limits of this Report.

Dr. Fiard* inquires into alledged *degeneracy of vaccine virus*, by its transmission through many individuals. He is a believer in the reality of this occurrence; the first indication of which he thinks is presented by the diminution in the duration of the eruption as compared with that of an earlier date, and that a difference in the development of the vesicles on the 8th or 9th day is not observed till afterwards. He applies this hypothesis to the vaccine virus of 1836; the vesicle from which runs the same course with that produced by the virus of 1844 up to the 8th day. At the 9th day desiccation of the vesicles of the old vaccine commences, and is complete by the 13th or 14th day, while the new runs its course more slowly and its desiccation is not complete till the 16th or 17th day. He states that a similar difference was observed some years ago, between the vesicles resulting from the old Jennerian vaccine, and the then new virus of 1836. Dr. V. Fradeneck† has *discovered the original vaccinia* among some cows, in part of Carinthia. The vesicles which it produced differed in no respect from those which resulted from the old virus, a fact from which he draws inferences unfavorable to the alleged degeneracy of the vaccine matter.

Dr. Pluskal‖ gives an account of a series of experiments on *retrovaccinatoin*, which he carried on for several years on a great variety of animals. It appears that it was only in those animals in whom vaccinia occurs spontaneously that vaccination was followed by the appearance of characteristic vesicles, and that the experiment succeeded best in those animals which were most nearly allied to the ox tribe. He regards the results of retrovaccination when practiced carefully on the cow, as affording a good criterion of the goodness of the lymph, but does not believe in its utility as a means of regenerating a deteriorated virus.

Dr. A. F. Tassani§ relates a very singular history of the apparent *communication of syphilis* to several infants by *vaccinating* them from a child in whom syphilitic symptoms subsequently appeared, though no sign of any such disease existed at the time when vaccination was performed.

Dr. Osbrey‡ relates two cases of *gangrenous inflammation of the vaccine vesicle* coming on about the 12th day, and proving perilous, though not fatal to two children, one of whom was 18 months, the other 5 years old. In one of the cases in addition to the local gangrene, sloughing of the mucous membrane of the mouth occurred, attended with hemorrhage from it. Recovery was slow in both instances. No cause could be assigned for the occurrence, as the previous health of both children had been good. [Dr. Osbrey quotes Dr. Labatt as mentioning this accident, but is apparently unacquainted with other instances of the occurrence of erysipelas after vaccination. F. de Wuerst, in his dissertation De erysipelate Neonatorum post Vaccinationem, Dorpat, 1835, 8vo, mentions twenty cases, and gives references to several recorded by different writers. In none of the Wuerst's cases, however, did gangrene occur; the children dying as from ordinary erysipelas.]—*West's Report on the Progress of Midwifery.—British and Foreign Medical Review, October*, 1845.

10.—*Chemical relations of Asparagine.*

M. Piria announces some very interesting experiments on asparagine. He confirms his former results respecting the conversion of this substance into succinic acid. He has found, also, that it displaces acetic acid from its combination

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* Bull. de l'Acad. Roy. de Méd. Nov. 30, 1844.
† Oesterr. Med. Jahrb. Mai, 1844 ‖ Oesterr. Med. Wochenschr., Marz 1844.
§ Gaz. Med. di Milano, Ottobre, 14, 1843.
‡ Dublin Medical Journal, March 1844, p. 133.

with the oxyde of copper, when boiled with an aqueous solution of acetate of copper. It then forms a crystalline precipitate of ultramarine blue, which is composed of —— $C_4\ (H^7\ Cu)\ N^2\ O^3$
Asparagine being $C^4\ H^8\ N^2\ O^3$

By means of sulphureted hydrogen, the asparagine may be again obtained separate with all of its properties.

By contact with nitrous acid at the ordinary temperature, asparagine and aspartic acid are decomposed into nitrogen and *malic acid.* This would indicate that asparagine and aspartic acid are amides of malic acid.

The organic radicals as may be seen, have had their day; it becomes necessary to make some theory more conformable to truth.

—*Journal de Chimie et de Pharmacie, Jan.* 1, p. 55.

11.—*Iodine found in some species of Adiantum.*

Mr. Righini, in 1838, expressed the opinion, that iodine was not confined to algœas and other maritime plants, and added that circumstances had convinced him that this substance was a product resulting from alterations of certain vegetable organic substances. He does not mean by the word alteration, only the *eremacausis* of Liebig, but any decomposition of vegetable matter taking place under the influence of heat.

He has detected the presence of iodine in the fallen and decayed leaves in mountainous districts, and where eremacausis gives rise to the production of chlorides, iodides, sulphates, and carbonates, with earthy or alkaline bases.

Finally, he has lately, in using the methods of Henry Rose and Cantu, discovered iodine in the dried plant of *adiantum capillis veneris* and *asplenium trichomanes*, L., collected on the rocks, in the mountains of Comasco; either by submitting these plants to prolonged maceration; or by burning them, and submitting the ashes to analysis.—*Journal de Chem. Medicale, Dec.*, p. 645.

AMERICAN MEDICAL INTELLIGENCE.

NATIONAL MEDICAL CONVENTION.

New York, May 5, 1846.

At a meeting of the National Medical Convention, pursuant to the call of the New York State Medical Society, convened in the building of the Medical Department of the University of the city of New York.—On motion of Dr. Edward Delafield, of New York, Dr. Bell, of Philadelphia was appointed Chairman, and Dr. Buel, of New York, Secretary, to act until the Convention be duly organized.

Drs. Baxley, of Maryland, Davis, of New York, and Arnold, of Georgia, were appointed a committee to receive the credentials of Delegates.

The Committee, after having performed that duty, made the following report:—

The Committee appointed to examine the credentials of the Delegates to the National Medical Convention, recommended to be held by the New York State Medical Society, in the city of New York, report, that they have performed that duty.

The Committee have felt themselves embarrassed by conflicting resolutions passed by the New York Society in 1845 and 1846, viz.: in 1845, it was "*Resolved*, that the New York State Medical Society earnestly recommend a *National Convention of Delegates* from Medical Societies and Colleges in the whole *Union*, to convene in the city of New York, on the first Tuesday in May, in the year 1846, for the purpose of adopting some *concerted action* on the subject set forth in the foregoing preamble."

In February 1846, it was "*Resolved*, That the preamble and resolutions passed by this Society at its annual session, Feb. 6, 1845, did not contemplate the appointment of Delegates to the National Convention, by County or merely local Societies in those States where Delegates *are* appointed by a regularly organized *State* Society."

They have therefore thought proper to report the names of all gentlemen from Medical Societies, Colleges, and Institutions, of all the States who come properly accredited, in accordance with the original resolution, and leave it to the Convention to determine whether such shall constitute the National Medical Convention; or whether the interpretation of the Resolution of 1845, adopted by the N. Y. Society in 1846, which excludes delegates from local or voluntary societies from States sending delegates from regularly organized State Societies shall be strictly adhered to.

Respectfully submitted,
H. W. Baxley,
N. S. Davis,
Richard D. Arnold.

The following list of Delegates was then handed in:

FROM VERMONT.

Society of the Alumni of Castleton Medical College.—Drs. Simeon A. Cook, Joseph Perkins, Egbert Jamieson, Horace Green.

Vermont Medical College.—Dr. Alonzo Clark.

FROM NEW HAMPSHIRE.

Centre District N. H. Medical Society.—Drs. Charles P. Gage, Richard P. J. Tenney.

FROM MASSACHUSETTS.

Berkshire Medical Institute.—Dr. Alonzo Clark.

FROM CONNECTICUT.

State Medical Society.—Drs. V. M. Dow, Rufus Blakewood, William H. Cogswell, J. G. Beckwith, Eleazar Hunt, D. T. Brainard, Richard Warner.

Medical Institution of Yale College.—Drs. J. Ives, J. Knight.

FROM NEW YORK.

Medical Society of City and County of New York.—Drs. James R. Manley, Isaac Wood, John W. Francis, Benjamin Drake, Gilbert Smith, W. W. Minor, F. U. Johnston, James Stewart, Thomas Chalmers, H. D. Bulkley, W. P. Buel, J. R. Wood, John S. Heard, A. N. Gunn, B. R. Robson, John Watson, J. S. Ferguson, R. L. Morris, R. T. Underhill, S. P. White, J. R. Van Kleek, J. C. Cheesman, A. C. Post. G. Buck, O. S. Bartles.

N. Y. Medical and Surgical Society.—Drs. F. Campbell Stuart, J. A. Swett, Edward L. Beadle.

Bloomingdale Asylum.—Dr. Pliney Earle.

Kings Co. Medical Society.—Drs. Chauncey L. Mitchell, Bradley Parker, J. Sullivan Thorne, T. L. Mason.

Alumni Geneva Medical College.—Drs. Peter Wilson, H. M. Gray.

Medical Faculty of College of Physicians and Surgeons.—Drs. John B. Beck, Willard Parker.

Medical Society of State of New York.—Drs. John Stearns, Stephen Hasbrouck, Merrit H. Cash, S. M. Crawford, Joel A. Wing, Daniel Ayres, Darius Clark, Thos. W. Blatchford, Sumner Ely, John McCall, N. S. Davis, Augustus Willard, L. G. Tefft, Alexander McIntyre, Mal by Strong, Charles Winnie.

Madison Co. Medical Society.—Dr. D. E. Hurd.

New York Hospital.—Dr. J. H. Griscom.

University of the City of New York.—Drs. G. S. Pattison, G. S. Bedford.

Buffalo Medical Association.—Dr. Bryant Burwell.

Erie Co. Medical Society.—Dr. Austin Flint.

Albany Medical College.—Drs. J. McNaughton, A. March.

Genesee Medical Society.—Dr. John Coates.

Trustees of the College of Physicians and Surgeons.—Drs. A. H. Stevens, Thomas Cock, Edward Delafield.

Geneva Medical College.—Dr. Charles A. Lee.

FROM PENNSYLVANIA.

Philadelphia Medical Society.—Drs. John Bell, Henry Bond, Geo. W. Norris, Isaac Hays, Isaack Parrish, Joseph Warrington, Alfred Stillé, J. Rodman Paul, Francis West, Gouverneur Emerson, Caspar Morris, Meredith Clymer.

Medical Department of Pennsylvania College.—Drs. H. S. Patterson, W. A. Atlee.

FROM DELAWARE.

Medical Association of Wilmington.—Dr. Lewis P. Bush.

Medical Society of Delaware.—Drs. James W. Thompson, E. S. Richard, William W. Stewart, William Cummings, Gove Saulsbury, James Couper.

FROM MARYLAND.

Washington Medical College.—Drs. H. Baxley, Charles Bell Gibson.

FROM VIRGINIA.

Medical Society of Virginia—Drs. Robert W. Haxall, Samuel A. Patterson, Charles Mills, Frederick Marx, James Conway, J. Cullen.*

FROM GEORGIA.

Georgia Medical Society.—Dr. Richard D. Arnold.

FROM MISSISSIPPI.

Mississippi State Medical Society.—Drs. E. D. Fenner, of New Orleans, C. S. Magoun.

FROM INDIANA.

La Porte University.—Dr. Azariah B. Shipman.

FROM ILLINOIS.

Medical Department of Illinois College.—Drs. Edward Mead, William A. Cheatham.

FROM TENNESSEE.

Medical Society of Tennessee.—Dr. William A. Cheetham.

It was on motion *Resolved*, that the Committee be continued to receive the credentials of such Delegates as may hereafter arrive.

On motion of Dr. Arnold, it was *Resolved*, that all gentlemen who have presented credentials from any regularly organized Medical Society in this Union, be considered members of this Convention.

It was moved by Dr. Davis, and carried, that Dr. Theophilus C. Dunn, President of the Rhode Island State Medical Society be invited to take a seat as a member of this Convention.

It was moved by Dr. Underhill, and duly seconded, that all medical gentlemen in good standing, who may be present from States not otherwise represented, be admitted as delegates, which motion was carried.

Drs. E. C. Marsh, and Lyndon A. Smith, of New Jersey, were invited to take seats under the above resolution.

On motion, a Committee of one from each State represented in the Convention, was appointed to nominate officers for the Convention.

The following gentlemen were appointed:

From *New Hampshire*,	Dr. Gage.	From *Delaware*,	Dr. Bush.
" *Vermont*,	" Cook.	" *Maryland*,	" Baxley.
" *Massachusetts*,	" Clark.	" *Virginia*,	" Haxall.
" *Rhode Island*,	" Dunn.	" *Georgia*,	" Arnold.
" *Connecticut*,	" Knight.	" *Mississippi*,	" Fenner.
" *New York*,	" Stearnes.	" *Indiana*,	" Shipman.
" *New Jersey*,	" Marsh.	" *Illinois*,	" Mead.
" *Pennsylvania*,	" Bond.	" *Tennessee*,	" Cheatham.

* Dr. Cullen represented the Hampden Sydney College, (Medical Department,) Richmond.—*Ed. Bulletin.*

The Committee, after having retired, returned and made the following report:—

The Committee appointed to nominate suitable officers to preside over the Convention, report that they have unanimously agreed to propose the names of the following gentlemen:—

For President.—Dr. J. Knight, *of New Haven.*

For Vice-Presidents.—Dr. John Bell, *of Philadelphia;* Dr. Edward Delafield, *of New York City.*

For Secretaries.—Dr. Richard D. Arnold, *of Savannah;* Dr. Alfred Stillé, *of Philadelphia.*

The Report was adopted:—*Committee discharged.*

The Officers, with the exception of Dr. Delafield, took their seats.

Dr. Bedford, of the University of the city of New York, moved the following preamble and resolution, seconded by Dr. Pattison, also of the University of the city of New York:—

Whereas the call of the State Medical Society of New York, for a National Medical Convention to he held in the city of New York on the first Tuesday in May, has failed in a representation from one-half the United States, and from a majority of the Medical Colleges; and whereas, the State Medical Society has emphatically stated that there is no mode of accomplishing the object of the Convention, without concert of action on the part of the Medical Societies, Colleges and Institutions of *all* the States, therefore Resolved that this Convention adjourn *sine die.*

The Yeas and Nays were called on this, and were as follows:—

Yeas—Drs. Beckford and Pattison—2.

Nays—Drs. S. A. Cook, H. Green, C. P. Page, R. P. J. Tenney, A. Clark, W. H. Cogswell, J. G. Beckwith, R. Warner, D. T. Brainard, B. Burwell, A. Flint, J. McNaughton, A. March, J. Coates, F. C. Stewart, J. A. Swett, E. L. Beadle, C. A. Lee, J. Bell, H. Bond, G. W. Norris, I. Hays, J. Parrish, J. L. Warrington, A Stillé, J. R. Paul, F. West, G. Emerson, M. Clymer, W. A. Atlee, L. P. Bush, E. S. Richards, G. Saulsbury, J. Couper, H. W. Baxley, R. W. Haxall, S. A. Patteson, J. Cullen, R. D. Arnold, E. D Fenner, A. B. Shipman, E. Mead, W. A. Cheatham, P. Wilson, D. E. Hurd, S. Wood, G. Smith, H. D. Bulkley, W. P. Buel, J. K. Wood, J. S. Heard, A. N. Gunn, J. Watson, R. L. Morris, R. T. Underhill, S. P. White, J. C. Cheeseman, A. C. Post, G. Buck, O. S. Bartles, S. Hasbrouck, J. A. Wing, D. Ayres, N. S. Davis, A. Willard, P. Earle, T. Cock, C. L. Mitchell, J. S. Thorne, T. S. Dunn, E. J. Marsh, L. A. Smith, W. W. Stuart, and J. Knight.—74.

Dr. Clymer then offered the following resolution:—

Resolved, That a Committee be appointed to provide other accommodations for the sittings of this Convention, than in the University of New York.

Several amendments were offered to this resolution, but while they were pending, Dr. Haxall moved to lay the whole upon the table, which was carried,—Yeas 34, nays 31.

Dr. N. S. Davis moved the following, which was adopted:—

Resolved, That a Committee of nine be appointed to bring the subject of Medical Education before the Convention in the form of distinct propositions, suitable for discussion and action, and that it report at the next meeting.

The following gentlemen were then appointed:

Dr. N. S. Davis,	Dr. Watson,	Dr. Bush,
" Marsh,	" Brainard.	" Haxall,
" Hays,	" Stearnes,	" Bell.

On motion of Dr. R. Wood, of N. Y., the President was added to the Committee.

Dr. Buel offered the following, which was adopted:—

Resolved, That the Committee be instructed to receive and submit propositions on all subjects proper to be brought before this Convention.

On motion the Convention adjourned until 10 o'clock next morning.

Wednesday, May 6th.

The Convention met pursuant to adjournment. The roll was called, and 63 delegates answered to their names. The minutes of the last meeting were then read and confirmed. The Chairman of the Committee on Credentials announced the presence of Dr. C. A. Pope, delegate from the Medical Society of the State of Missouri, and Drs. Anderson, G. Dana, J. Spaulding, and J. A. Allen, Delegates from the Vermont Medical Society.

Dr. N. S. Davis, Chairman of the Committee, appointed at the last meeting to present the subject of Medical Education to the Convention in a form proper for discussion, and, generally, to prepare business for its action, reported that, owing to the short time allowed them, the Committee had not been able to agree in regard to the subject especially entrusted to them, but that they had unanimously determined to lay before the Convention certain resolutions which they believed adapted to fulfil the immediate objects contemplated by the Convention. Whereupon Dr. Hays, from the same Committee, submitted the following preamble and resolutions:

Whereas it has been shown by experience, that the Association of persons engaged in the same pursuit, facilitates the attainment of their common objects, therefore—

1st. *Resolved*, that it is expedient for the medical profession of the United States, to institute a *National Medical Association*, for the protection of their interests, for the maintenance of their honor and respectability, for the advancement of their knowledge, and for the extension of their usefulness.

2d. *Resolved*, that a Committee of seven be appointed to report a plan of organization for such an Association, at the meeting to be held in Philadelphia, on the first Wednesday in May, 1847.

3d. *Resolved*, that a Committee of seven be appointed to prepare and issue an address to the different regularly organized Medical Societies and chartered Medical Schools in the United States, setting forth the objects of the National Medical Association, and inviting them to send delegates to a Convention, to be held in Philadelphia, on the first Wednesday in May, 1847.

4th. *Resolved*, that it is desirable that a uniform and elevated standard of requirements for the degree of M. D., should be adopted by all the Medical Schools in the United States, and that a Committee of seven be appointed to report on this subject at the Meeting to be held in Philadelphia, on the first Wednesday in May, 1847.

5th. *Resolved*, that it is desirable that young men, before being received as students of medicine, should have acquired a suitable preliminary education, and that a Committee of seven be appointed to report on the standard of acquirements which should be exacted of such young men, and to report at the meeting to be held on the first Wednesday in May, 1847.

6th. *Resolved*, that it is expedient that the medical profession of the United States should be governed by the same code of Medical Ethics, and that a Committee of seven be appointed to report a Code for that purpose, at the meeting to be held at Philadelphia, on the first Wednesday in May, 1847.

Dr. J. S. Copes was announced to be a duly authorized delegate from the Medical Society of Mississippi; and Dr. G. H. White of the Hudson Lunatic Asylum was admitted to a seat in the Convention.

Dr. G. S. Pattison moved that the Report of the Committee on Business be adopted. Carried, *nem. diss.*

Dr. Stearns moved that the resolutions be considered *seriatim*. Carried.

The preamble and 1st resolution were then adopted *nem. diss.*

The 2d resolution being under discussion, Dr. S. Hasbrouck moved that the committee to be appointed under that resolution report on to-morrow morning. After some remarks by Drs. Haxall, Thompson, and Davis, the motion was. withdrawn.

Dr. Griscomb moved that the next Convention be held in September, 1847 After some remarks by delegates from the South, declaring that physicians of that region could not possibly be spared from their duties in the autumn, the motion was rejected.

The 2d resolution and then the 3d, were adopted, without further discussion, *nem. diss.*

Dr. F. Hasbrouck, as Superintendent of the N. Y. City Lunatic Asylum, was here admitted to a seat in the Convention.

The 4th resolution was then adopted, *nem. diss.*

The 5th resolution, after some observations from Drs. Manley, and H. S. Patterson, and the loss of an amendment proposed by the latter gentleman, was adopted *nem. diss.*

The 6th resolution was in like manner adopted, without debate; after which, on motion of Dr. Bush, the preamble and resolutions collectively were unanimously adopted.

The Convention then took a recess for half an hour, after empowering the President to fill the Committees called for by the resolutions just passed.

On reassembling, the Convention received, Dr. G. Sumner, a delegate from the Medical Society of Connecticut.

Dr. Clymer asked, and received, permission to correct an error personal to himself in the report of a morning newspaper (the N. Y. Herald), concerning the vote on Dr. Haxall's motion to lay on the table the resolution offered by Dr. Clymer for adjourning forthwith from the rooms of the Faculty of the University of N. Y. city. The newspaper in question had stated that the only serious opposition to laying this resolution on the table proceeded from the delegates of the College of Physicians and Surgeons, whereas the vote had really stood 34 in favor, and 31 against disposing of the resolution in this manner.

Dr. J. B. Beck, of the Faculty of the College of Physicians and Surgeons, stated that so far from any opposition to laying the resolution on the table having proceeded from the body which he represented, neither he nor his colleague, Dr. Parker, the only delegates from that body, were even so much as present during the discussion, or at the vote, on the resolution.

The Secretary asked permission to insert the correction in the minutes of the last meeting as confirmed; stating that the vote was upon the original record of the proceedings, but had been omitted in transcription. Leave was granted.

Dr. Sumner moved to reconsider the 2d resolution; the motion, after remarks by Drs. Baxley, Haxall, and Davis, was laid on the table.

Dr. Bartles introduced the following resolution:

Resolved, That the Union of the business of *Teaching*, and *Licensing* in the same hands, is wrong in principle, and liable to great abuse in practice. Instead of conferring the right to license on Medical Colleges, and State and County Medical Societies, it should be restricted to one Board in each State, composed in fair proportion of representatives from its Medical Colleges and the profession at large, and the pay for whose services as Examiners should in no degree depend on the number licensed by them.

Dr. Sumner moved to lay the resolution on the table. After remarks by Drs. F. C. Stewart and Clymer, the motion was withdrawn.

Dr. Parrish moved that the resolution be referred to the Committee under the 4th resolution, reported by the Committee on Business.

Dr. Baxley moved an amendment, which was accepted by Dr. Parrish, providing that all communications upon the subject of medical degrees be referred to the same Committee.

Dr. Manley remarked at length upon the subject of Medical Education. Dr. Baxley replied. Dr. Manley rejoined; and then moved to refer the resolution introduced by Dr. Bartles to a special Committee.

On motion, the subject was temporarily laid upon the table in order that the President might announce the Committees he had appointed.

They were as follows:

Under the 2d Resolution.—Drs. J. Watson, J. Stearns, F. C. Stuart, N. Y.;

A Stille, Philadelphia; N. S. Davis, Binghampton, N. Y.; Cogswell, N. Haven, Conn.; Fenner, New Orleans.

Under the 3d Resolution—Drs. E. Ives, Dow, and Sumner, of New Haven, Conn.; J. McNaughten, Albany, N. Y.; Blatchford, Troy, N. Y.; Burwell, Buffalo, N. Y.; and Baxley, Baltimore, Md.

Under the 4th Resolution—Drs. Haxall and Cullen, Richmond, Va.; S. A. Patteson, Manchester, Va.; G. W. Norris, Philadelphia; A. Flint, Buffalo, N. Y.; J. Perkins, Castleton, Vt.; and J. A. Wing, Albany, N. Y.

Under the 5th Resolution—Drs. J. Couper, Newcastle, Del.; L. P. Bush, and J. W. Thomson, Wilmington, Del.; E. Mead, Jacksonville, Ill.; A. March, Albany, N. Y.; J. Atlee, Philadelphia; and D. J. Brainard, New London, Conn.

Under the 6th Resolution—Drs. Bell, Hays, and Emerson, Philadelphia; Morris, Dover, Del.; J. C. Dunn, Newport, R. I.; Alonzo Clark, N. Y.; and R. D. Arnold, Savannah, Ga.

Dr. Haxall moved that the President be added, as Chairman to the Committee under the 3d resolution. Carried unanimously.

The consideration of Dr. Bartles' resolution, as amended by Dr. Manley, was then resumed, and it was ordered that when the vote upon it is taken, it be taken by yeas and nays.

Drs. S. Hasbrouck and Davis remarked at length upon the subject of the resolution. Observations were also made by Drs. Baxley, and Underhill.

Dr. Sumner moved to lay the resolution on the table. Lost by a vote of 34 to 40.

Dr. Parrish having withdrawn his amendment, the yeas and nays on Dr. Bartles' resolution, as amended by Dr. Manley, were called, when it was adopted by the following vote:—

Yeas.—Drs. Arnold, Ayres, Bartles, Beadle, Beck, Bulkley, Burwell, Bond, Buck Buel, Cheeseman, Cock, Coates, Copes, Cummins, Cook, Davis, Delafield, Ferguson, Drake, Fenner, Gage, Gray, Griscom, Gunn, F. Hasbrouck, S. Hasbrouck, Heard, McGoun, McNaughton, Mitchell, R. D. Morris, B. Parker, W. Parker, Paul, Parrish, Pope, Rickards, Saulsbury, F. C. Stewart, W. W. Stewart, L. A. Smith, Stille, Stearns, Swett, Thorne, Underhill, Van Kleek, Warner, Watson, Warrington, West, Willard, S. P. White, G. H. White, Wing, J. Wood, J. R. Wood—59.

Nays.—Drs. Baxley, Beckwith, Bedford, Blakeman, Brainard, Bush, Cheatham, A. Clark, Clymer, Couper, Cullen, Emerson, Flint, Gibson, Hays, Haxall, Lee, March, H. S. Patterson, G. S. Patterson, S. A. Patterson, Shipman, Sumner, Thompson.—24.

Dr. Griscom moved that a Committee of *five* be appointed to consider the *expediency*, and if expedient, the *mode*, of recommending and urging upon the several State governments the adoption of measures for a registration of the births, marriages, and deaths, of their several populations. Carried, *nem. diss.*

Dr. McGoun moved that the thanks of the Convention be presented to the officers of this Convention, for the prompt and efficient discharge of their duties. Carried, *nem. diss.*

Dr. Griscom introduced the following resolution:

Resolved, That Mr. Lemuel Shattuck, of Boston; Drs. Jarvis, of Dorchester, Mass; Emerson, of Philadelphia; T. R. Beck, of Albany; and C. A. Lee, of N. Y.; be a Committee to prepare a nomenclature of disease, adapted to the United States, having reference to a general registration of deaths; to report to a future Convention, which was adopted; and on motion, Dr. Griscom was added to the Committee as Chairman.

Dr. Swett of N. Y., offered the following, which after receiving the approbation of the delegates from New York, was adopted:

Resolved, That a publication of the proceedings of this Convention be made in pamphlet form; that ten thousand copies be printed; that the N. York City delegation assume the expense; and that it be referred to the first Standing Cummittee.

The President then announced the Committees appointed by him, under the resolutions of Dr. Bartles, and of Dr. Griscom, as follows:—

Under Dr. Bartles, in reference to the separation of teaching and licensing; Drs. McNaughton, Albany, N. Y.; J. R. Manley, N. Y.; J. W. Frances, N. Y.; Isaac Parrish, Philadelphia; R. Blakeman, Fairfield, Conn.; J. Cullen, Richmond, Va.; and Thomas Cock, N. Y.

Under that of Dr. Griscom, in reference to the subject of registration: Drs. J. H. Griscom, N. Y.; G. Emerson, Philadelphia; A. Clark, N. Y.; C. A. Lee, Geneva, N. Y.; and James Stuart, N. Y.

The following resolution, offered by Dr. Thomson, of Delaware, and seconded by Dr. Clymer, of Philadelphia, was unanimously adopted;

Resolved, that this Convention hereby tender to the Medical Faculty of the City of New York, and to the Trustees of the College of Physicians and Surgeons of the State of New York, its unanimous thanks for the courteous offer of their respective buildings for the sittings of this Convention.

Dr, Bell introduced the following resolution, which was adopted, *nem, diss*,

Resolved, that the Convention give its approval to the objects and labours of the Sydenham Society.

Dr. Cogswell offered the following resolution, which was unanimously adopted:

Resolved, that the thanks of this Convention be presented to the President, for the able, gentlemanly, and impartial manner in which he has discharged the duties of the Chair.

Upon motion of Dr. Hays, the Convention then adjourned, *sine die*.

—*Bulletin of Med. Science. Phil.*

2.—*Osteo-Sarcoma of the Lower Jaw.—Resection of the body of the bone.—Cure.* By J. Marion Simms, M. D., Montgomery, Ala.

The subject of this case was a negro man about 26 years of age. The disease involved the *body* of the bone, extending from the third molar tooth on the left to its fellow on the right side. From the left lateral incisor to the third molar on the right, the teeth had all been removed, and their places were occupied by a large granulated, fungo-fleshy looking mass, constantly discharging a fœtid sanious secretion. On the left the teeth were firm, but somewhat displaced, being pushed upwards, their crowns inclining slightly inwards. The protuberance on each side of the bicuspids was very elastic to the touch. The whole under-surface of the jaw was of a bony hardness, the right of the symphysis being larger than the left, and projecting a little lower.

The following account of the history of the case is from the master of the boy, R. R. Mosely, Esq.

"Some five years ago Sam had syphilis, and was some time under the influence of medicine before a cure could be effected. About a year after he got well, a rising commenced on the inside of the jaw, on the right side, resembling a gum boil; but it continued so long that I began to think it was the effect of the medicines he had taken to cure the disease. I got a doctor to look at it, who pronounced it a gum boil, and as such opened it, but it did not go away. Some considerable time afterwards, I got the doctor to examine it again. He found all his teeth on that side loose, entirely out of their sockets, and just sticking in the gums. The doctor then cut down to the jaw bone and found it diseased, and matter on it similar to brains. That was fifteen or eighteen months ago. Sam has been taking some kind of medicine for it ever since. This is a short and imperfect account of his case, but about the best I can recollect at present."

The tumour was never painful, but had put on such a frightful appearance, that it warned his master of the necessity of having something done for his relief. He accordingly sent him to one of the most distinguished surgeons of the whole country, who immediately took steps for the performance of an operation. The patient was seated: an incision about an inch long was made on the left side of the jaw, when he resisted the efforts of the surgeon, by springing suddenly from his seat, and refusing to submit to the cutting,—nor could any entreaty induce him to do so. He persisted so obstinately in his foolish determination, that the surgeon was compelled to send him home, trusting that time and a little reflection might bring him to a sense of his danger, and show him that his only safety consisted in the extirpation of the disease.

Soon after his return home, his master sent him to Montgomery, hoping that he might yet be induced to undergo an operation. I was not long in ascertaining that it would never be done *with his consent;* his only objection being, that "it would hurt too bad."

Having made up my mind to give him the only chance for his life, and having determined not to be foiled in the attempt, I contrived the following method of securing him:

Everything being ready, the operation was performed on Thursday, 15th May, 1845, at 11, A. M. The apparatus consisted of a barber's chair, on which was placed a plank about twelve inches wide and five feet long, the other end of it resting on a common bench or stool, of the same height of the chair. Persuading him to sit down on the chair, with his legs extended out on the plank, he was secured tightly to it by means of straps made of surcingle webbing, which were passed successively over the thighs, knees and ankles. A strap around the abdomen, or rather pelvis, fastened behind, and another across the upper part of the thorax and points of the shoulders, running downwards and backwards, held him so firmly that it was impossible for him to move his body forwards. Some bands made of the same substance, (surcingle webbing,) fitting accurately each wrist, (after the manner of "handcuffs,") were buckled together with a strong leather strap, and this made fast to the band that passed over his knees, thus keeping his arms extended. His elbows were pinioned to his sides by a strap buckling behind. His legs, body and hands being now immovable, it only remained to fix his head, which was done by a band passing around it, and having attached, at the occiput, a strong leather strap. By laying hold of this and pulling directly downwards in the course of the spine, his head was so far controlled that an assistant could hold it in any position that I wanted. He appeared to be very much alarmed. Dr. Baldwin counted his pulse, and found it varying from 122 to 128 beats in a minute.

Taking my position on his right, an incision was commenced on the left side, a little more than half an inch anterior to the angle of the jaw, and continued along the base of this bone to the symphysis. At this cut he made a most furious effort to get loose, which proved that I had not put myself to any *unnecessary* trouble in securing him.

The facial artery being secured, each end requiring a ligature, the incision was continued from the chin, along the right side of the jaw, to a point corresponding with its commencement on the left. The divided ends of the right facial artery, (like the left,) each required a ligature.

The upper flap was dissected rapidly from the tumour, and held up in the usual way by an assistant. The lower flap was in a like manner dissected off and turned down. This was somewhat tedious, in consequence of the thinness of the skin, and its close adherence to the diseased mass. The posterior fang of the second molar on the left (its crown being decayed,) was extracted, to make room for the saw. I attempted to cut the bone with a small, long, narrow saw, but made such slow progress that I laid it aside, and picked up a very strong pair of Liston's bone forceps, with which I was equally unsuccessful. I then resorted to the chain-saw, passing it around the bone in the manner usually directed, by which it was severed in a few seconds. Its application on the right side was quite as successful, dividing the maxillary just anterior to the third molar tooth. A strong double ligature was now passed through the frænum linguæ, to prevent the spasmodic retraction of the tongue, and the operation was completed by dissecting the lingual muscles from their attachments to the bone. The retraction of the tongue was pretty strong at the moment of separation; though easily controlled by the ligature, which proved the safety and utility of this precautionary measure. There was a good deal of hemorrhage from the nutrient vessels of the diseased part; but no ligature was needed.

The operation lasted forty minutes. From his constrained position, and loss of blood, the patient was quite exhausted. He was loosed from his fetters, laid on a bed, and took some brandy and water; which by the by, had been given to him occasionally during the operation. The wound was not adjusted till reaction had been fully established, and the oozing of blood entirely checked. The ligatures of the facial arteries were left hanging from their respective places. The ligature of the frænum, and those of the ranular arteries, were drawn through the opening at its central point; the wound was closed by some six or eight interrupted sutures, and a water dressing applied. He had taken sixty drops of laudanum previous to the operation, which did not appear to produce any effect until it was over, when he seemed almost narcotized, sleeping profoundly the whole afternoon and all night. Mr. Norris, one of my students, sat by his bedside the whole night, watching his tongue, and keeping the dressings constantly moistened with cold water.

The frænum linguæ ligature was cut loose and drawn out on the second day; but the dressing was not disturbed till the fourth, when I found the wound healed through its entire extent by the "first intention," except just at the points where the ligatures hung out. They came away in due time, and their points of exit at the chin and on the right side granulated directly; but on the left there remained a fungous growth sprouting up above the level of the surrounding skin, about the size of a pea, which did not get well till an exfoliation of bone was thrown off through this opening. On the right there was a like exfoliation, but it was discharged by an opening on the inside of the mouth.

For several days I observed, that when he would lie on one side, the large, flabby, *skinny* chin would gravitate to that side; and when he would lie on his back, its own weight, assisted by the inspiratory act, would cause it to *cave in*, as it had no support on the interior.

Sam left Montgomery on the 12th July, perfectly well. Previous to the operation, he was never known to laugh or even to speak to any of the other patients in the infirmary; but now, his mouth is almost always on a broad grin, and he is continually cracking jokes and playing pranks on his companions. I have rarely ever seen a patient exhibit more real heartfelt gratitude than he does.

His mastication is very good, having the third molar tooth left on each side; but the action of the pterygoid muscles has a tendency to draw the ends of the bones inwards, and thus mastication is performed, not with the crown, but rather with the outer edge of the tooth. This, I fear, will, by and by, cause them to become displaced, loose, and useless.

The operation was performed in the presence of a large number of medical gentlemen,* and I am under especial obligations to Drs. Boling, Baldwin, Blakey, Bellangee and Vickers, for their valuable aid.

A review of this case presents to my mind the following points of interest:

1st. It adds another to the long list of successful operations for this disease.

2d. It proves the practicability of the operation, whether the patient is willing or not.

3d. The chain-saw is to be preferred for the division of the bone, when it is of a *healthy hardness*. It *is a labor, time and pain-saving instrument.*

4th. There is safety in the frænum linguæ ligature.

5th. The water dressing is preferable to every other.

6th. If any apology were necessary for the length of time (forty minutes) taken in the performance of the operation, it might readily be found in the constrained posture of the patient, and consequently the increased urgency for rest, which, according to my experience, is all important in any capital operation.

If I had to do this operation again, I would not bring a single ligature through the wound, but would leave them long, bring them out at the angles of the

* Ten medical students, and fifteen doctors.

mouth, and fasten them to the cheeks with adhesive plaster, thus allowing the wound to heal up entirely by the first intention, and avoiding the deformity of a cicatrix from granulation.—*American Journal Medical Sciences.*

Remark.—It affords us pleasure to record this highly creditable achievement of a Southern surgeon. We see it has been incorporated in Mott and Velpeau's great work on Operative Surgery.—Editors.

3.—Spurious Medicines.—We have reason to believe, that a large proportion of the drugs with which practitioners are supplied, and which are dispensed by apothecaries, are deficient in purity, either from imperfect preparation or adulteration. The editor of the New York Journal, in the number for September last, announced his intention of giving an *exposé* of some startling facts, in his possession, on this subject. It has not, however, as yet appeared; but we trust he has not relinquished his intention. The profession, generally, are doubtless not aware of the extent to which this imposition is carried on, and it is highly important that the facts should be known both by the profession and the public. The responsibility lies both with druggists and physicians. The former may not be competent to determine the purity of the articles which they vend; and the latter seldom take pains to do so. We refer now particularly to those practitioners who dispense medicines to their patients, as is generally the case except in large cities. We are sorry to say, that physicians not only omit the duty of close examination of the quality of the medicines they use, but sometimes obtain those of an inferior quality, knowingly, because they can be purchased at a cheaper rate. We have been told by druggists, that they are obliged to keep articles of an inferior quality to suit this class of customers. This abominable practice cannot be reprehended in too severe terms. What ideas can such persons have of the practice of medicine, and their obligations to their patients? For the honor of the profession, we trust such instances are not numerous; but we are compelled to believe that habitual inattention to the selection of medicines is quite common.

The importance of precise and uniform properties in medicinal substances is too obvious to require illustration. Established principles of therapeutics, and the practical precepts of every member of the profession, as a matter of course, are based upon them. In proportion to their variableness, scientific observations and individual experience, involve incongruities and errors. Doubtless, it is to this source that much of the discrepancy of facts appertaining to different medical records and opinions is to be attributed. The uncertainty which attaches to this point always imposes the necessity of a conditional acknowledgment of deductions from the trial of remedies, if adequate testimony of the purity of the medicines be not at the same time given. These considerations, the force of which is sufficiently apparent, assuredly make the subject one of great moment. Our contemporary of the New York Journal is entitled to much credit for broaching it, and if he will prosecute his intention as announced, he will render a most valuable service to the profession, to science and to humanity.

The practical question comes up in connection with these remarks, how can the evils referred to be remedied and effectually prevented? The observance of greater caution on the part of conscientious apothecaries and physicians will do much; but this is not all, as it seems to us, that the subject demands. The truth is, it is a subject in which legislation ought to interfere, and protect the safety and interest of the public, by establishing properly qualified inspectors of medicines, and imposing high penalties for vending or dispensing drugs which have not passed the ordeal of a thorough examination. The public see the propriety of making necessary articles of food, and even some luxuries, subject to inspection before sale; but concerning medicines, with regard to which most persons can exercise no discrimination whatever, the necessity of some legal precautions has never been recognised. Are not the dangers to which the public is exposed from inert and adulterated drugs quite as great as from poor flour, pork, fish or tobacco? It is not probable, however, that an effort to induce legislators to act in this matter, for the public weal, would be successful. We should, perhaps, be accused of some sinister motives—some scheme of monopoly. It remains, then, to inquire if the Profession cannot do something without the aid of legislation, to correct the evils which now exist. It appears to us, that if this matter was taken up by the national convention, or by the permanent association which, it is to be hoped, will grow out of this convention, commissioners might be appointed in our larger cities, under whose supervision medicines should pass, and it should be recommended to the profession generally to repudiate all drugs which had not received their sanction. This plan would promote the interest both of practitioners and respectable druggists, and both would, we conceive, readily fall in with it. The detailed provisions necessary to carry this improvement into effect, we will not now consider. Perhaps, there are difficulties in the way of its successful operation which do not occur to us. We give the idea as it has suggested itself, without having bestowed upon this branch of the subject lengthened reflection. Possibly legislation might be more amenable to the public welfare than we anticipate, but here a difficulty would arise in want of co-operation or uniformity in different States. Since none can doubt of the great importance of the object, whatever may be the way in which it can be most readily and effectually obtained, we trust some good may follow the effort to direct the attention of the profession toward it.

—*Buffalo Medical Journal.*

Remarks.—We agree with the Editor of this journal, that the subject of spurious and adulterated medicines is one of vital importance, and demands the most serious attention of physicians. This, we believe, is especially the case in the Southern country, which, by its remoteness from the chief markets, the cupidity of apothecaries, and the negligence of physicians, necessarily suffers most from the use of spurious and adulterated medicines.—Eds.

4.—*Case of Lodgment and Retention of a Foreign Body in a Bronchus for a space of sixty years, with its final expulsion.* (Communicated by Rodman Bartlet, Student of Medicine, Salisbury, Conn.)

The following case being somewhat unique in its character, may, perhaps, be deemed of sufficient interest to find a place in your valuable Journal. It occurred in the practice of my preceptor, Luther Ticknor, M. D., who is ready to vouch for the facts here stated.

Richard Moore, of Salisbury, Conn., now aged sixty-three years, inherited a good constitution; but when three years old, swallowed accidentally a piece of bone, which nearly suffocated him at the time, and which, from that day to the present, has produced a series of phenomena, which could only be accounted for, on the supposition that the bone was lodged in some portion of the bronchial tree. For a long time he was harrassed night and day with a cough, and severe dyspnœa, attended with a rattling, or sibilant ronchus, fœtid breath, more or less pain deep in the right side, about opposite the fifth or sixth rib, and midway between the sternum and dorsal vertebræ. These symptoms continued to harass him until nearly the age of puberty, when other symptoms appeared, of a still more alarming nature. At this period, the cough began to be accompanied with expectoration of purulent matter, tinged with blood; attended also with marasmus or atrophy, and other symptoms of confirmed pulmonary disease. At the age of nineteen, he was suddenly attacked with hæmoptysis, which continued, at irregular intervals, for a series of years; during which time, his general health and strength were greatly impaired, so that he was incapable of much exertion, being, indeed, confined to the house for more than eight years. At about the age of twenty-eight, his general health began to mend; although still troubled with cough. For the next fifteen or twenty years he was able to perform some labor, though not without considerable inconvenience from the symptoms above mentioned. In January, 1844, for the first time since the third year of his age, the cough suddenly left him, and did not return for several months, when it again began suddenly to harass him, as before. On the 8th of October, 1845, he experienced a remarkably uneasy sensation, of a pricking nature, deep in the right side, which excited violent coughing, and after one or two severe paroxysms, he experienced a sensation, as if something had ruptured, or given way. This was instantly followed by the passage of something into the trachea, producing suffocation, which was forcibly ejected upon the floor, succeeded by the expectoration of purulent matter, streaked with blood. On examination, the foreign body turned out to be a *bone*, nearly three-fourths of an inch in breadth, and one-twelfth in thickness, oblong, and somewhat triangular in form, smooth and convex upon one surface, the other covered with sulci and protuberances. It appeared that the bone which he had originally swallowed, from the traditionary accounts preserved in the family, was a splinter of a rib from which he was sucking the meat, and it seemed highly probable, on examining the piece ejected, that this, also, was originally a portion of the same bone, as the spongy lamellæ were still visible on one side, while the smooth convex surface of the other was equally manifest. Now, then, are we not authorized in believing that this bone was lodged in one of the rami of the right bronchus for this long period of sixty years, producing the phenomena which I have briefly described; including partial ulceration, whence proceeded the blood, and purulent matter, &c.? Your opinion on this point will be gratifying to one, at least, of your numerous friends and readers.

Salisbury, Conn. Nov. 15th, 1845.

[There are numerous cases on record where foreign bodies have been lodged in one of the bronchial tubes for several years, but we know of no instance where a body of the size of that above described, has remained for so long a period in a bronchus, without causing fatal disease; and doubt whether a parallel case can be produced.—Ed.]—*N. Y. Journal of Medicine.*

5.—*A case of Small Pox occurring five days after vaccination.—Progress of the vaccine disease arrested for ten days.*

Dr. Alfred C. Post related the following case:

Doctor C., who had previously resided in a small village in Connecticut, removed with his family to New York in August, 1845. He had three children, the eldest of whom was eleven years of age, the second seven years, and the third an infant at the breast. The eldest child had been successfully vaccinated in infancy: the others had never been vaccinated. During the month of September, the two younger children were vaccinated: the vaccination of the infant was entirely unsuccessful: that of the next child was followed by the occurrence of a vesicle, which the father thought to be genuine, but which left behind it a small ulcer, which remained unheeded three months.

On Friday, December 5th, the infant was taken sick with fever: on Tuesday, December 9th, a variolous eruption made its appearance, and soon became confluent over almost the whole body. On Tuesday, December 16th, the child died.

On Tuesday, December 9th, the day on which the eruption appeared upon the infant, the second child, a girl seven years of age, was again vaccinated. The vaccine disease pursued its regular course until Sunday, December 14th, when the patient was attacked with fever and delirium, which continued until Wednesday, December 17th, when the variolous eruption made its appearance upon the backs of the hands. The eruption subsequently appeared upon the chest, face, and other parts of the body. It was confluent upon the backs of the hands and feet, coherent upon the extremities generally, and discrete upon the face and trunk. The delirium gradually subsided as the eruption came out, but a great degree of constitutional disturbance continued until the decline of the eruption, so much so that I did not regard the patient as being out of danger, until the process of desiccation had made considerable progress. The subsequent convalescence was very slow, several weeks having elapsed before she was able to walk. The variolous eruption was regular in its course, except that the pustules on their maturity were flatter than they usually are. The desiccation of the pustules commenced on Friday, December 26th, and was considerably advanced on the following days: on Sunday, 28th, it was nearly complete, except upon the feet. On Tuesday, December 16th, a week after vaccination, the vaccine vesicle was normal in appearance, with the exception that there was a slight trace of an areola. During the subsequent week, it underwent no change, except that there was a little increase of size and fullness, and it became opaque, as the variolous eruption approached its maturity. It was distinguished from the variolous pustules by its large size, and more regular form: several small variolous pustules coalesced with it at its circumference. On Thursday, December 25th, there was a slight incrustation at the central depression: there was very little increase of this on Friday and Saturday, so that there was less tendency to desiccation in the vaccine pustule than in the surrounding variolous pustules. On Sunday, December 28th, when the desiccation of the variolous pustules was almost complete, the vaccine pustule was found to have broken, and to have left behind it a superficial ulcer.

The above case presents several distinct points of interest. The most remarkable is the manner in which the progress of the vaccine disease was retarded by the occurrence of small-pox. The patient had been vaccinated five days at the time of the invasion of the variolous fever, and eight days when the eruption of small-pox made its appearance. The eruptive fever did not appear to affect the condition of the vaccine vesicle, except, perhaps, in preventing the formation of a distinct areola. But from the time that the small-pox eruption made its appearance, the vaccine disease remained nearly stationary, and did not go through its usual changes. It, however, increased a little in size, and the fluid contained in the vesicle became opaque, but it retained the distinct pustular character without desiccation until Saturday, December 27th, eighteen days after the vaccination, and ten days after the commencement of the variolous eruption.

The second point of interest, is the failure of the vaccine disease to exert any material influence on the progress of the small-pox. It is probable that the vaccination which had been performed in September was spurious, and that it had no modifying influence on the subsequent variolous affection. The last vaccination, although it appeared to be genuine, had not reached a sufficiently advanced stage to exert any manifest influence in mitigating the severity of the small-pox. It may perhaps have retarded the occurrence of the eruption, which commenced twenty-four hours later than it usually does. But considering the favorable age of the patient, the attack of small-pox was much more severe than in the average of cases, and for nearly a fortnight, serious apprehensions were entertained as to the result.

Another interesting feature in the case, is the fact that the eruption was later in its appearance upon the face, than upon the upper extremities, and that it remained more discrete upon the head and trunk than upon the upper and lower extremities. After the eruption appeared upon the face, I directed a mask to be worn, composed of linen spread with mercurial ointment, with holes cut for eyes, nose and mouth. This had the effect of arresting the development of the eruption on the parts which it covered, so that it remained throughout in the papular stage. This fact was verified by Dr. Bulkley, who saw the case about the time the eruption had reached its maturity. I have on other occasions pursued a similar practice, and with satisfactory results.

New York Jour. of Medicine.

6.—*The Illinois and Indiana Medical and Surgical Journal.*—This is a new title adopted by one of our old neighbours, the Illinois Medical and Surgical Journal, hitherto edited by J. V. Z. Blaney, M. D. In this change D. Brainard, M. D., Wm. B. Herrick, M. D., and John Evans, M. D., have become the associates of the former editor; and, notwithstanding the reputable character that the old Journal bore, we predict that the arrangement will prove a most advantageous and satisfactory one. The reason of the change of title is explained in the following extract from the editorial announcement:

As a number of our subscribers, and anticipated contributors are residents of the State of Indiana, in which there has, as yet, no medical journal been issued, and the services of Dr. John Evans having been secured as co-editor, we have altered the title of our journal, to express the better its more extended sphere, and will hereafter issue it simultaneously at Chicago, Illinois, and Indianapolis, Indiana. Our labor, also, will be further divided by the association of *four* co-editors, as seen by our title page. By this division of labor we hope to improve the Journal in spirit and practical value.—J. V. Z. B.

7.—*The Credit System in Medical Schools.*

[The following remarks on this subject, from the *Western Lancet*, (May, 1846), are wholly just, and we rejoice to learn that the Transylvania School has determined to abolish the credit system, and unite in the hope that the schools will follow the example.]

We are fully satisfied that an influence extremely detrimental to the profession has grown out of the custom of granting credit in medical schools. The object in adopting this course is, to secure a large class, perhaps for the purpose of out-numbering a rival school; and the consequence is, that all who present themselves, wholly irrespective of merit or qualifications, are duly enrolled as medical students, and their names go out to the world, swelling the catalogue of some highly flourishing school. The success of such pupils operates as an incentive to others to engage in the study, seeing that it is so cheap; and many a one who either has no occupation, or who may be too indolent to follow a mechanical pursuit, forthwith doffs his humbler business, and unites in swelling the Æsculapian throng. In this way the profession becomes thronged with practitioners of every grade, and all parties, even those favored with the gratuity are ultimately injured. It is true some meritorious men will be found unable to pay; but there can be no doubt, that the indiscriminate admission of pupils into medical schools is not only an act of injustice to those who do pay, but also tends directly to degrade the profession. We are gratified, therefore, to state, that Transylvania Medical School has determined, by formal resolution, to abolish, *entirely*, the credit system: and these resolutions will be strictly adhered to. We hope all other schools will manifest a similar regard for the interests of the profession, and follow an example so worthy of imitation.

Med. News and Library.

In this hope we fully concur. The course adopted by some schools in this country is not only undignified, but highly injurious to the profession; and must ultimately lead to their own disgrace and downfall. In order to swell their numbers, for of numbers is their only boast, they will take any who offer. The great misfortune, too, is that pecuniary deficiency is often not the most serious one that stands in the way of these aspirants, in their attempts to obtain admission into schools where less importance is attached to numbers. We have known ignorant mechanics, who could with difficulty write their own names, to turn steam doctors; and, after spending their hard-earned pittance, in endeavoring to carry out the tricks of fraud and ignorance, they determined to obtain a backer in some medical school. This they may do, "without money, and without price," and we may add, without labor, study, or any other qualification: it is enough that they have honored the learned professors with the light of their countenances. They are even offered advantages over the better class of students; for they have only to apply to the Dean, who registers them as Mr. ———, of ———, Practitioner; and, after four months of attendance, he goes forth, without fail, to come out under new colors. In this way our country is now being flooded with men, whose only title to respectability of any kind is in the parchment issued by schools which are certainly jeopardizing their own claims to the respect of the profession generally.—Eds.

NEW ORLEANS, JULY 1, 1846.

HEALTH OF THE CITY.

The unusual wetness of the season, up to the present time, has afforded a wide field for the usual calamitous forebodings in which the *old inhabitants* annually luxuriate. Each spring has its harvest of predictions: if the preceding winter has been unusually cold, a terrible epidemic of yellow fever invariably follows: if the winter has been warm, another set raises the cry that the cold has not been sufficient to kill the seeds of the preceding year's epidemic, and we shall soon have it again. If the spring is wet, the swamps are filled with water, which forms stagnant fens to evaporate, as soon as the heats of summer commence, spreading miasmata and pestilence around; if dry, a great quantity of mud, and decaying matter is exposed through the summer, from which arise putrid exhalations, and all manner of intolerable things. Whether warm, or cold, thunder, or no thunder, we are sure of the same train of woes; but above all the harbingers of approaching disasters, flies and mosquitoes, are regarded by our sages as the most infallible.

Now, we are most anxious to guard against the imputation of credulity; but, at the same time, candor compels us to admit that, as yet, unfortunately, these *calamitous predictions* sometimes prove true. We hope the time will come when these prophecies will no longer meet with even an occasional fulfilment.

The unusual quantity of rain that has fallen here in the last three or four months, has had less influence upon the health of the city than might reasonably be expected, from the prolonged prevalence of humid weather at this season. The only indication of any influence exerted by this weather, is in the increased number of cases of dysentery, and other derangements of the bowels. Some of the cases of diarrhœa assume a serious character, and are accompanied with a state of congestion, and prostration; sometimes with severe cramps. These cases, coupled with the circumstance that the Asiatic cholera is reported to have again reached Russia, in its western progress, create an impression that a state of predisposition exists which is regarded as the forerunner of an epidemic of that dreaded pestilence. We cannot admit the truth of this view. The cholera may, indeed, visit us again; but the present prevalence of complaints of this nature have nothing to do with it; but must be attributed to the long prevalence of a humid state of the air, which by diminishing cutaneous transpiration, augments the activity of the internal exhalent surfaces.

Intermittent and remittent fevers have not prevailed to so great an extent as usual this season, up to date. There have been some cases of a typhoid nature, with catarrhal symptoms, which yield readily to treatment. These cases are probably due to the same cause that predisposed to the bowel affection.

In other respects the health of the city is much the same as it usually is at this season; the same prevalence of measles, with a few cases of scarlet fever, all of a mild type. We have enjoyed an unusual exemption from small-pox, while it has prevailed to so great an extent in some of the other American cities.

HEALTH OF THE COUNTRY.

Over a great portion of the South and South-west there seems to have been an unusual prevalence of rainy and humid weather, which, in the spring is almost sure to be accompanied by a preponderance of bowel affections. This predisposition has been remarked by physicians whom we have met here from many parts of this and the adjoining States. With this exception, the sanatory condition of the country seems to be unusually good. We subjoin the following extracts from our correspondents:

NASHVILLE, June 2d, 1846.

Messrs. Editors:—Since my last letter, there has nothing unusual occurred in the range of my observation, in our profession, except some individual cases. Our community has not suffered from sickness to any extent, until within the last ten days, when diarrhœa made its appearance, and is even now prevailing to considerable extent. The cases present nothing unusual, and are generally mild. This is with us, commonly, a healthy season of the year: a middle ground between the spring and summer diseases.

HUNTSVILLE, April 23d, 1846.

"The country and town have been more healthy than usual. We have had fewer cases of pneumonia during the winter and spring, than for many years. Rheumatism has been more prevalent than I recollect to have seen it. I have treated several cases successfully, with large doses of quinine; 15 grs. morning and evening, with occasional doses of calomel and opium. The quinine appeared to control the disease, by its action on the nervous system, very effectually."

Cambridge, Ala., June 2d, 1846.

April and May are the healthiest months we have in Alabama. We have had very few diseases in the country this spring. A few cases of scarlatina, of a mild character, have fallen under my observation. Measles, with typhoid pneumonia following the desquamation, has prevailed upon one plantation in my neighborhood. There were out of fifty cases, six or seven with well marked typhoid fever. One death resulted from neglect, and want of proper treatment.

I have met one case of trismus nascentium, at the sixth day since Dr. Simms' paper was published in the American Journal of Medical Sciences. I found the child upon its back, "*with the occiput shoved in,*" spasm occurring upon the slightest noise or touch. It was in articulo. I left it dying. I called too late in the afternoon to make a post mortem. I am persuaded that Dr. S. has taken a correct view of the pathology of this disease. I have never seen a case recover; and I have never seen a white child suffer with the disease. This fact is strongly corroborative of Dr. S.'s views. It is a very common disease with the negroes.

Trinity, La., May 15th, 1846.

"My locality for observing the diseases of the "swamps" is one of the best: including the neighborhoods of Tensas, Ouachita, Little and Black Rivers, situated thirty miles distant, little south of west from Natchez.

The season, up to the present period, has been uncommonly healthy; very remarkable exemption from intermittents, of ague and fever, so peculiar to the low lands.

During the months of February and March, we had some few cases of pneumonia and pleuro-pneumonia—very few cases of pleuritis. During the last month, we had much infantile gastro-intestinal disorders, accompanied with convulsions, etc., among children.

The weather has been very changeable—much cold and moisture. I have resided here since 1840, and do not recollect so healthy a spring; although we have so much rain, wind, and cold mornings and evenings.

In surgery I have had some little practice. On the 30th of January, I performed the double flap operation, for disarticulation of the humerus at the shoulder-joint, successfully. Injury occasioned by gun-shot wound. May 12th, reduced a dislocation of the hip-joint—subject, young negro belonging to the plantation of Mr. Rice Ballard. There was something strange in the displacement. The head of the bone was thrown upward and inward—the foot *everted*—the limb was shortened fully two and a quarter inches. By the assistance of three stout men, it was returned into the acetabulum, with a very loud snap. I then applied the deal-board recommended by Liston, for fractures. The displacement had existed for upwards of a month. Our rivers are high, and the swamps full."

La Barita, (Mexico,) June 10th, 1846.

Gentlemen:—It may not be uninteresting to advise you of the nature and type of diseases which attack the volunteers from our gallant State. At Point Isabel, where we encamped four or five days, the soldiers suffered severely with bowel affections; chiefly serous diarrhœa, some assumed a dysenteric form. This was evidently due to the bad water, which, to my taste, (for upon this alone I had to rely,) contained a large per cent of glaubers salts and common muriate of soda. The ordinary astringents combined with anodynes sufficed to check the discharges, yet the cause continuing in full force, they speedily returned with more obstinacy than before. During our stay at the Point, a stiff breeze from the Gulf, swept over the encampment; often strong enough to lift from the earth a small fine sand which filled the eyes and excited considerable irritation in that organ; besides a quantity of it became mixed with our food and drink; and this doubtless added to the irritating effects of the water. At the end of the 4th day, we removed our quarters to the Island Brasos Santiago where more serious difficulties and obstinate affections awaited us. It may be premised that this Island is nothing but a barren sand bank, covered with a sparse growth of tough sea-grass, peculiar to such situations. By excavating the sand to the depth of three or four feet, and sinking small barrels in the sand, an abundance of water as clear as crystall rose and filled the barrels; and this supplied two regiments. But in addition to a slight brackishness, it possessed a taste and a smell, which, to me, were peculiarly nauseous. When put in vessels and allowed to stand for a few hours, it seemed to enter into putrefaction, and emitted a very disagreeable odor. Such was the water we were compelled to use for six or eight days. It is hardly necessary to say that it affected almost every man. The diarrhœa which had afflicted us at the *Point*, suddenly assumed dysenteric symptoms, in many cases attended with griping, tormenting tenesmus, and frequent bloody discharges, which were difficult to control in some cases—very little fever, or excitement of the circulation accompanied these symptoms; and considering their severity and the intense heat of the sun, moderated however by the sea-breeze, was not a little singular. All who escaped the dysentery, as well as those who suffered from it, complained of the *black* appearance of their evacuations. This fact was the subject of common observation. During our stay on Brasos Island, we were visited by a thunder storm which prostrated our tents, and the rain which followed it, drenched us to the skin. After this, I found the sick, sicker; and those who had remained healthy up to that time, now began to complain. A peculiar symptom—a constant uneasiness and pain, before and after the evacuation, in the rectum, attended one half of the cases. It was that kind of sensation, caused by the presence of a rough foreign body in the rectum. We speak on this point from personal experience. This symptom was so severe, that a partial prolapsus of the rectum took place in some cases. The *treatment* adopted, was such as is used in these cases, and it succeeded. We began with the mildest laxatives in some cases, and followed it up with small doses of cal. et opii, vel, morph. and quinine when the subject was much reduced. The latter combination answered the purpose most admirably. In some instances, we began with opiates, without premising the use of laxatives. The diet was regulated when this was posible. All recovered. Since we reached this place, the obstinate cases have yielded. In haste, yours. A. H.

St. Mary's, 16th June, 1846.

Messrs. Editors:—Since our spring diseases subsided we have had but little sickness. I have seen but few cases of fever, and those were rather of a mild remittent than of an intermittent character, which latter is our usual form of fever at this season. But our sickly season has not arrived yet. In a month or two "nous verrons."

Woodville, Miss., 15th June, 1846.

Messrs. Editors:—There is no sickness at this time. The spring has been unusually wet and rainy. The crops are much damaged by the continued heavy showers; fears are entertained that the products of our farms will be quite short.

I have no data as to the winds or temperature.

HOSPITAL REPORT.

NEW ORLEANS CHARITY HOSPITAL.

Monthly Report for April and May.

MAIN BUILDING.

April—Admitted: Males, 409; Females, 86.
Discharged: Males, 397; Females, 72.
Died: Males, 29; Females, 5.
Remaining on the 1st of May, 328

May—Admitted: Males, 433; Females, 97.
Discharged: Males, 372; Females, 96.
Died: Males, 47; Females, 14.
Remaining on the 1st of June, 327.

INSANE DEPARTMENT.

April—Admitted: Males 33; Females, 19.
Discharged: Males, 27; Females, 7.
Died: Males, 1; Females, 1.
Remaining on the 1st of May, 82.

May—Admitted: Males, 27; Females, 15.
Discharged: Males, 30; Females, 13.
Died: Males, 1; Females, 1.
Remaining on the 1st of June, 78.

We have no special reports from either the Surgical or Medical Wards.—Eds.

ABSTRACT OF A METEOROLOGICAL JOURNAL FOR 1846.

BY D. T. LILLIE, AT THE CITY OF NEW ORLEANS.

Latitude, 29 deg. 57 min.; Longitude, 9 deg. 07 min. west of Greenwich.

WEEKLY. — 1846.	THERMOMETER.			BAROMETER.			COURSE OF WIND.	FORCE OF WIND, Ratio 1 to 10.	Rainy Days.	Quantity of Rain. — *Inches.*
	Max.	Min.	Range.	Max.	Min.	Range.				
April - - 25	82.7	64.	18.7	30.23	29.90	.33	S.	2½	5	2.930
May - - 2	83.2	64.	17.2	29.98	29.83	.15	S.	4¼	2	1.350
" - 9	88.5	67.	21.5	30.06	29.80	.26	S.	2¾	2	3.740
" - 16	80.5	61.	19.5	30.16	29.90	.26	N.W.	2	2	2.755
" - 23	88.5	67.5	21.0	30.18	30.04	.14	S.W.	3	1	0.380
" - 30	85.5	72.	13.5	30.18	29.88	.30	S.	3	3	2.785
June - - 6	88.2	66.5	21.7	30.09	29.97	.12	S.E.	3½	6	3.940
" - 13	82.5	66.	16.5	30.07	29.91	.16	N.W.	2¾	3	1.057
" - 20	90.	75.5	14.5	30.10	30.01	.09	S.W.	3	4	1.987

REMARKS.—The Thermometer used for these observations is not attached to the Barometer, but is a self-registering one, and is placed in a fair exposure. Regular hours of observation, 8 A.M., 2 P.M. and 8 P.M.

The Barometer is located at an elevation of 19 feet above the level of the ocean, and is suspended clear of the wall of the building.

The Rain Guage is graduated to the thousandth part of an inch, and the receiver is elevated 40 feet from the ground.

We had heavy showers of rain, on each of the 12 consecutive days preceeding the 10th of June.

ERRATUM.

Page 79, line 13 from bottom, for *factious* read *factitious.*

THE

NEW ORLEANS

MEDICAL AND SURGICAL JOURNAL,

DEVOTED TO MEDICINE

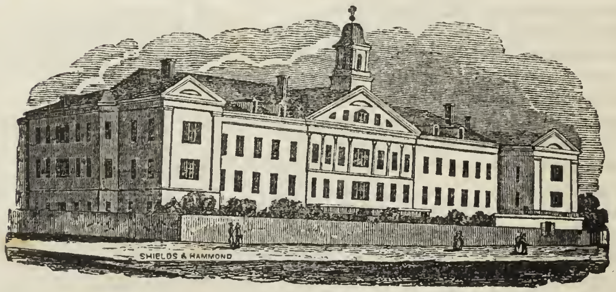
AND

THE COLLATERAL SCIENCES.

EDITED BY

W. M. CARPENTER, M. D.
E. D. FENNER, M. D.
J. HARRISON, M. D.
A. HESTER, M. D.

"Summum bonum Medicinæ, sanitas."—GALEN.



NEW-ORLEANS CHARITY HOSPITAL.

SEPTEMBER, 1846.

NEW-ORLEANS.
PUBLISHED BY S. WOODALL, 49, CAMP STREET.
1846.

TO READERS AND CORRESPONDENTS.

Our esteemed cotemporaries will please excuse our omission to notice in this number their varied and interesting contents. It will not be so again.

Communications have been received from Drs. Lopez, of Mobile, Nailor, of Miss., and E. Hughes, of Trinity, La.

In our next number, Dr. Wedderburn will give a new view of the position of the Epigastric Artery, and its agency in determining the position of the internal inguinal ring.

We have been compelled to postpone several reviews and bibliographical notices to our next number.

The following pamphlets have been received, viz:

1. *Some observations on the Ethnography and Archæology of the American Aborigines.* By Samuel George Morton, M. D.

2. *Experimental Researches on the Post-mortem contractility of the muscles, with observations on the Reflex Theory.* By Bennet Dowler, M. D.

3. *A Practical Treatise on the art of preserving the Teeth, &c., &c.* By A. L. Plough, Dental Surgeon.

We have had access to the latest Foreign Medical Journals.

The following American Journals have been received regularly in exchange:

American Journal of Medical Sciences. (July.)
American Journal of Science and Arts. (July.)
Boston Medical and Surgical Journal. (July and August.)
Western Lancet and Medical Library. (July and August.)
Western Journal of Medicine and Surgery. (July and August.)
Buffalo Medical Journal and Monthly Review. (July and August.)
St. Louis Medical and Surgical Journal. (July and August.)
Missouri Medical and Surgical Journal. (July and August.)
Illinois and Indiana Medical and Surgical Journal. (July and August.)
New York Journal of Medicine. (July.)
New York Medical and Surgical Reporter. (July and August.)
Medical Examiner. (July and August.)
Bulletin of Medical Science. (July and August.)
Medical News and Library. (July and August.)
Southern Journal of Medical and Pharmacy. (July.)
Southern Medical and Surgical Journal. (July and August.)
British American Journal of Med. and Physical Science. (July and August.)

☞ The *Bulletin of Medical Science* for August, which has just reached us, contains some bitter complaints against us, for expressions uttered in the Review Part of our Journal for May last. We have neither time nor room to notice the Editor's remarks at present.

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THE NEW ORLEANS

MEDICAL AND SURGICAL JOURNAL.

SEPTEMBER, 1846.

Part First.

ORIGINAL COMMUNICATIONS.

I.—*Remarks on Gastrotomy.* By John P. Ford, of Nashville, Tenn. (Read before the Tennessee State Medical Society, and ordered to be published.)

I do not so much expect to present new ideas on this subject as to collect, and perchance arrange those which are scattered through the pages of surgical writers. The importance of the operation will commend itself to the mind of every medical man, on account of the difficulty which often meets us in our efforts at diagnosis as well as the dangerous tendency of those affections which we may think require its adoption, and the capital character of the operation itself. There are many affections which have, by common consent, been denominated *opprobria medicorum ;* and if any efforts on our part can tend to reduce the number of them or in any way lessen the certainty of their character, they will be well applied. The consciousness of having mitigated human suffering and prolonged human life, is the fullest and most grateful reward incident to the practice of our profession.

The operation of gastrotomy for the delivery of the fœtus from the mother's womb, when from any cause the effect cannot be produced through the natural passage, is, according to the generally received opinion, of ancient origin. That it is often successful is abundantly attested by the numerously recorded cases scattered along the history of surgery, and particularly in our medical periodicals of late years. Dr. Churchill has collected the number of cæsarean operations, and finds 409 cases in which 228 mothers were saved.

Another class of diseases often calls for this operation, much more commonly than the first mentioned, and on account of the nature of the parts implicated, is much more successfully treated—for while in the cæsarian section, not only the walls of the abdomen are penetrated by the knife, but the no less important organ (the uterus also); in hernia our incision only reaches the cavity of the abdomen, and not ordinarily afflicting injury on other important viscera. Yet in some instances, even here, the intestinal tube does not escape the surgeon's cut, and success stands ready to crown his efforts.

In this, as well as the former class of affections, the operation is only mentioned to show, that gastrotomy may be performed in numberless instances, without being necessarily fatal. And we may not reasonably say, that the operation for strangulated hernia, is not in the full sense gastrotomy. To all practical intents, it is the same with that operation properly called gastrotomy.

In the one as well as the other, the incision is made through the skin, the cellular substance, the muscles, and peritoneum down to the intestines. The best authorities as well as our own observations, prove that the operation for strangulated hernia, if properly performed, and performed sufficiently early, is one of comparatively little danger.

It is an opinion supported by ancient and somewhat modern authority in surgery, that in all operations involving the parieties of the abdomen, peritonial inflammation was more to be dreaded than any other consequence. But it has been left to recent investigation to establish the fact, that such fears have been by our surgical fathers greatly overrated. Travers, many years ago, and lately Dr. Gross, have proved by their experiments, that if the wounds of the intestines were properly attended to, inflammation of the peritoneum was not regarded as of very serious importance—if escape of fœcal matter was prevented, peritonitis rarely supervened to a dangerous extent—while it cannot be denied, that wounds of any important organs of our system are accompanied by danger to the sufferer; the idea I would advance is, that peritoneal inflammation does not now stand pre-eminently and necessarily fatal in wounds of the abdomen, whether inflicted extensively by accident or made more neatly and sparingly by the surgeon's knife.

The diseases to which I would call the attention of the Society, as justifying and even sometimes loudly demanding the operation of gastrotomy for their relief, belong to that class which may be denominated obstructions of the intestinal tube from mechanical causes. All along the track of medical record, for a long time back, may be found here and there an isolated instance of daring surgery, which has tended to relieve the patients in some cases or to establish in my estimation, the practicability and justifiableness of such an operation, in later days; especially since, by a careful collection of facts, we may better understand the value of the operation and its modified application to the various affections which we may be called to treat.

Invagination of the bowels is an affection of exceeding interest to the medical profession. However much medical science has advanced towards perfection in early days, to the present time, on many points, this disease, then as now, has been looked on generally in the light of a fatal one. Dr. Bigelow says, "internal strangulation, we have reason to believe, is a fatal disease, except in rare instances, in which a spontaneous restoration of the parts, under favorable circumstances, may have taken place." Dr. Dunglison says, in speaking of intussusception, the only hope we can have is, that the invaginated portion may be thrown off, and a cure thus obtained; although such a result, it must be admitted, is extremely rare. Heberden says, in such cases physicians should try to disarm death of some of its terrors; and if they cannot make him quit his prey, they may still prevail to have life taken away in the most merciful manner. Alluding to the administration of opium as the only means of soothing the pangs of death, Parr expresses the same opinion; and in fact, it is the tone of almost all writers on the subject, that the disease is necessarily fatal unless purgatives relieve. There are some exceptions to this remark, as I shall show presently.

It is a matter of interest in the present inquiry to ascertain, if possible, the condition of the parts involving such serious consequences as we see in this affection. The symptoms of invagination are pain in the bowels, costiveness without fever and without tenderness, at first; often an elongated tumour may be felt in some part of the abdomen; as the case advances there appear disturbance of the circulation, enlargement of the abdomen, and soon come on those alarming symptoms, such as small rapid pulse, distress of countenance, difficulty of respiration, vomiting of stercoraceous matter, cold tremulous extremities, cool skin bathed in clammy sweat, which becoming more violent, loudly proclaim to the physician that death is at hand. From the discovery, that gangrene has often affected the bowels in intussusception, it was readily supposed that that was the cause of death; and were the fact of its presence universally established, it would certainly tend greatly to lessen the chance of successful treatment by an operation. But it can be shown, I think, that such is not always the case; and when that condition of the bowels does exist, it is only incidental. The autopsy of Mr. "Legare, late Attorney-General, showed the abdomen greatly distended, sigmoid flexure of the large intestine in such a state of distension that its external circumference was in one place 15 inches. It had a dusky green color as if from commencing gangrene, but there seemed to be no softening nor diminution of its natural polish."

Lawrence in his work on Rupture says, "above the obstructed part the bowel is found after death inflamed and greatly distended—from the contracted part downward the bowel is smaller than usual, and not inflamed." Mr. Stephens, in a work on Hernia, published in London, in 1829, gives the history of a case in which, on post-mortem examination, the bowel was found doubled on itself, so as to obstruct peristaltic action and the passage of its contents; yet there was no stricture nor inflammation of the bowel or peritoneum.

Sir Charles Bell says, "the symptoms depend on the obstruction to the descent of the contents of the bowels, and not on the state of the intestines of the sac," (speaking of Hernia), and says, "it is shown by dissection, that distension and the consequent excitement of the muscular coat produce those very symptoms which attend strangulated hernia; consequently all the symptoms will be rendered milder, and the life prolonged, by the ease with which the stomach ejects its surcharge. By the inverted action, and vomiting of stercoraceous matter, the distended canal is in a certain measure relieved.

It is within the recollection of several members of this Society, that such was pre-eminently the case with a patient, attended by my colleague, Dr. Winston, Mr. Craig's negro girl, of this neighborhood—it was a case of obstinate obstruction, in which all the symptoms advanced to the last stage of distress and danger—stercoraceous matter was vomited freely, and at every discharge, which was exceedingly copious, the patient expressed herself greatly relieved. No doubt, the free evacuation of the bowels above the obstructed point was one of the chief means of her ultimate recovery.

The details of the case I have not at hand, nor would they be of unusual interest, except to illustrate this point. A case occurred in the practice of my friend, Dr. R. C. K. Martin, which was shown on dissection to be a complete obstruction, by the passage of a knuckle of the ilium through another bend of the same bowel. There was great distension and no gangrene—and of such character, may it be seen, is the tsstimony of all who have made post-mortem examinations in this disease; not in every case of course, I would be understood, but in enough to show, that although gangrene may exist, it is not a necessary concomitant of diseases of obstruction, which even end in death. It can be easily conceived, that that amount of obstruction might exist which would allow the circulation of the blood to some extent, and still prevent the passage of the contents of the bowels—enough circulation going on in the parts to prevent gangrene; but still enough of pressure and distension capable of bringing about those symptoms of which we speak, and death itself.

When an obstruction exists in any portion of the intestinal tube, which prevents the passage of its contents beyond that point, and it is of such a character as not to cut off the circulation of the obstructing point entirely, the consequence is, the accumulation of matter above, distension and a corresponding pressure of the vessels of the neighborhood—this distension and pressure produce the strange, perhaps unaccountable lesion of the nervous system, as evinced by the rapid pulse, the gastric disturbance, and in a later stage all the urgent symptoms of collapse and speedy death; it may require a few days or as many hours to produce these effects. If the obstruction is entire and sudden from the beginning, these nervous symptoms supervene suddenly, as in the case of strangulation of the bowel, in hernia; whether produced suddenly or gradually, the condition of the nervous system will be the same, sooner or later. Now, if the circulation be cut off by the intensity of the strangulation, gangrene necessarily supervenes, though not as a certain connexion with the cause of death. To show that distension may produce those symptoms which will end in death, you will revert to the case of Craig's girl mentioned above. Again, in the case reported at the last annual meeting of this Society, by Dr. Manlove, the same train of facts was clearly exhibited. It will be recollected, that the boy was suffering under those symptoms which denote with unerring certainty, the approach of dissolution; and when the distention was relieved by the incision made in the bowel and by the discharge of its contents, they gave way as by magic.

Petit is said to have punctured several times the sacs of strangulated hernia, and thereby relieved the distension caused by the superincumbent mass, and the patients were cured (although in one case there was evidently gangrene) without artificial anus. A charlatan, not understanding the nature of strangulated hernia and supposing them to be abscesses, is said by Velpeau to have gained a brilliant reputation by puncturing them, and effecting a cure in many cases. The intestines being freed from distension by the puncture, soon recovered from the symptoms of strangulation.

Mr. Velpeau alluded, in the Academy of Paris, to a case of tympanitis, when a variety of means had been resorted to without success, in which he plunged a trochar into the intestines, and gave vent to a large quantity of gas through a canula. In the course of five days he made four punctures; the man recovered perfectly. These cases are mentioned only to show the great influence of distension as a prominent cause of the distressing symptoms.

It will be, perhaps, always a source of difficulty to determine with absolute certainty, the existence of a mechanical obstruction in the bowels. But in regard to diagnosis in almost all important and serious diseases, we may not expect to be without difficulty.

Velpeau says, if the affection attacks suddenly upon a strain or violence, if the patient thought he perceived a tearing, accompanied with crepitation and pain, propagated from a given point to the rest of the abdomen—if from this moment vomiting, first of mucous and alimentary substances and then of stercoraceous matter continues, while alvine evacuations have been impossible, and the usual signs of evident peritonitis are absent, it would be very difficult not to admit the existence of internal strangulation. The distinctive symptom to which Dr. Watson, of the Royal College of Physicians, attached more value than any other was, that the intestines rumble and roll, and propel their contents downward to the same spot and no further. But it will be of necessity left to the judgment of each practitioner to determine the value of all the symptoms which may be present in the case, and which may point his mind to the conviction of the existence of strangulation—his practice must be modified by circumstances impossible for me to enumerate in this paper. I would, that I could here point out that correct diagnosis which would lead us aright in all cases.

Should we still continue to view these cases as beyond the reach of remedial means? Shall we stand silently by and witness those results without an effort to relieve them? who does not regard a case of invagination or tortion of the bowels as necessarily fatal? It is so taught in the books—it is so practised by physicians.

Called to attend a case presenting the symptoms generally described above, in which all the remedies have been used which may be suggested to the mind, and still seeing the slow but certain approach of death, what should be done? We cannot doubt, but that this condition is brought about by a temporary injury to the bowels, nor can we doubt the fact, that if by any means we could remove the injury of the bowels, the symptoms would disappear as by a charm. Gastrotomy is the resort which I would recommend, an incision made into the cavity of the abdomen, would almost certainly reveal the point of mischief, and the course of conduct should be directed by circumstances. If, on reaching the intestines, it is found that a portion of the bowel is invaginated, a relief of that condition will probably be easily effected, when it will be seen that nature will again pursue her usual tract and health will be restored. To this result, the greatest impediment will be the wound in the walls of the abdomen; and in the present state of medical science, I presume, no one will contend that such a wound, inflicted by a surgeon's knife, must be fatal.

Samuel Cooper, speaking of hernia, says there seems to be ample cause to believe, that the generality of fatal events consequent to the operation, are attributable to the disease and not to the attempt to relieve it. Mr. Potts' opinion was, that the operation when performed in a proper manner and in due time, does not prove the cause of death oftener perhaps than one in fifty cases.

Again, should it, on reaching the bowels, be found that there exists torsion or a passage of a portion of the bowel through the mesentery, the course is plain, the position should be corrected.

But suppose it is found, that there is neither invagination nor tortion, but a stricture of the bowels, producing permanent obstruction or impacted fœces or other substances forming an insuperable barrier to the passage of matter from above, and this distension exists to which I have attributed so much mischief; what is the plan to be adopted? I should puncture the bowel as near and above the point of obstruction as convenient, and relieve the distension by the escape of the distending matter, taking care to avoid, by the usual means recommended by the best surgeons, the escape of fœcal matter into the cavity of the peritoneum, thereby forming a temporary artificial anus, if the symptoms would justify me in attempting the operation of gastrotomy.

I would believe, that unless relief were speedily afforded, death would ensue. And, although, an artificial anus, permanent or temporary, may be justly considered as a great evil, yet it must be viewed as a less one than death. By puncturing the bowel, under these circumstances, we would gain time for the future administration of remedies suited to the character of the obstruction. With these views in the performance of the operation, I would hold myself authorized by the best dictates of humanity and science.

In performance of this operation, there are many obstacles to success which will need to be met by the ingenuity and firmness of the operator; the opposing coats of the invaginated bowels sometimes become agglutinated by the deposition of lymph—if the connection be slight, but little tractile force will be required to disengage the bowel, or a careful dissection may become necessary. The same line of conduct should guide us in the event of finding the bowel in a gangrenous condition. It does not comport with the character of this paper to elucidate the plan to be pursued in minutiæ—that is sufficiently laid down by our excellent late surgical writers, as how the sphacelated portion is to be cut off and the sound ends united; by what sutures, whether the glovers' or interrupted suture, whether cylindrical substances should be used to sew the two ends of the intestines on or not—these are not the subjects; they are to be determined by the taste of each one who reads the views of those who have written on the subject. My object has been to bring to the consideration of the Society, what I consider to be leading points in the subject, hoping to attract their attention to a condition of things seemingly to be too much neglected.

I will not tire the patience of the Society by the enumeration of the cases of gastrotomy which are scattered (rather sparingly I acknowledge) through the surgical books and journals. There are, however, many to be seen, and they may serve us as beacon lights to guide our professional conduct in similar cases.

In this paper I have endeavored to show, that distension above the point of obstruction being one of the prominent causes of danger, the success of the operation may be calculated on with more certainty than if a gangrenous condition of the bowels were always present. That invagination, torsion, and even permanent obstruction of the bowel may be relieved by the operation of gastrotomy—the last mentioned, perhaps, imperfectly; the two first perfectly, with a restoration to health. That the operation, under proper circumstances, need not be necessarily fatal oftener than many other capital operations, undertaken by the most prudent surgeons, as every day occurrences.

I am aware I have trodden on somewhat forbidden ground. I think that good authority may be quoted to prove, that under any circumstances, the operation of gastrotomy is an unjustifiable operation; that it has been said to have been wisely discountenanced by almost all therapeutists, and condemned as the procedure of unenlightened quackery, and wholly inadmissible. Notwithstanding these things, let us go on and diligently add one mite of information to another until we shall have collected such an amount as we shall be able to draw and be served from in time of need, in professional conduct. No subject is at once cleared of its difficulties; and may we not hope that the period of investigation is now dawning, when not only this, but others, equally intricate and important, may be so far divested of their darkness and their doubts, that we may not err therein.

The most I can hope for in such a paper as this, with views so crudely and hastily thrown together, is to incite inquiry and experiment among the members of the Society, by which this subject may be brought to light in its various bearings on some of the direst ills to which the human system is liable.

II.—*A case of Congestive Fever, its history, symptoms, and treatment.* By A. L. C. MAGRUDER, M.D., of Vicksburg, Miss.

The case before us, presents a striking illustration of the importance, in the treatment of diseases, of varying the remedies to suit the different symptoms presenting themselves, of treating no disease by name, and of discarding everything like a routine practice. In this case, the remedies which were suitable and proper in one stage of the disease, if persisted in when symptoms of a different kind presented themselves, would have inevitably sacrificed the patient.

Upon a proper discrimination of symptoms, and from them a knowledge of the true pathology of the disease, will the success of the physician depend; and he who treats disease according to the dictum of any theorist, regardless of the idiosyncrasy of constitution and the peculiarities of each case, will often be placed in a dilemma, from which he will find it difficult to extricate either himself or his patient. Without farther remarks, I report the case, which I hope may not be found void of interest to some of your readers, who may meet with cases of a similar description.

Monday night, 9 o'clock, June 15, 1846.—I was called to visit John Lane, aet. about 16 years, of good constitution and habits; in consultation with Dr. B. T. Hicks, of this place, a very experienced physician, from whom I learned the following to have been the previous history and treatment of the case:

On Friday, the patient had bathed imprudently, exposed to the hot sun. On Saturday, complained of general *malaise*, aching of the bones, and the usual premonitory symptoms of an attack of fever.

Sunday morning, complained of chilly sensations. On Sunday evening, Dr. Hicks being in attendance on another patient, was requested to see John, the family believing him to be only slightly indisposed. The Doctor found him in a drowsy, semi-comatose condition, with some fever; his case very evidently assuming symptoms of an alarming character. The following prescription was given—

℞ Pilulæ Hydrarg.
Extract Colocynth, *aa* gr. v.
M. and divide into 3 pills. S. Give at one dose.

Monday morning, very slight operation from the medicine; the liver inactive; a cathartic enema was ordered, which produced but little effect—the patient without fever. The following prescription was ordered.

℞ Pilulæ Hydrarg.
Quinine Sulphatis *aa* gr. xij. M.
Make 6 pills. S. Give one pill every two hours.

About 11 o'clock, the patient felt chilly sensations passing down the spinal region;—cold hands and feet; these symptoms continuing for a short time, were followed by the most violent reaction. Dr. Hicks was summoned immediately to his bedside—found him perfectly unconscious of everything around; hot burning skin, the temporal and carotid arteries pulsating with great vehemence, and every evidence of cerebral congestion; the patient restless and unmanageable, beating the bed with his hands, and raising his feet and legs with great violence. He was cupped upon the back of the neck and upper part of the sternum, and ice applied constantly over the frontal region, to diminish the flow of blood to the head; and obviate, if possible, the lesions which would be likely to occur in the brain, by compression. On Monday evening, a large blister was applied over the epigastric region, and one over the nape of the neck, and allowed to remain until their full rubefacient effect was produced, which was accomplished in about two hours; his feet had been immersed in a hot mustard bath, which produced a temporary perspiration. At this period of the disease I saw the patient, the temperature of the skin variable—pulse 160 in a minute; intellectual faculties entirely suspended—could not be aroused sufficiently to notice or answer a question; restless, groaning, tossing his head from side to side; and his extremities, particularly the feet and legs moving up and down continually, and the powers of life evidently giving away very rapidly. The case appeared to be a hopeless one. What was to be done?

Everything now depended upon the course of treatment to be adopted, if anything could be accomplished by treatment; for the probability was, that effusion, or some organic lesion of the brain had already taken place. The question now was, shall we use stimulants, when the brain has been suffering so much from congestion, and remedies of so opposite a character have recently been used to subdue that congestion?

We at once determined upon a vigorous course of stimulants and rubefacients as the only sheet-anchor of our hopes. The first step taken was to have the patient enveloped in sinapisms of mustard; next gave at one dose—camphor, ammoniæ carbonas *aa* gr. v.; sacch. albæ, pulv. accac. gum. *aa* q. s.; aquæ f ℥ j. M.; whilst the following prescription was being prepared:

℞ Ætheris sulphuric rectificati f ʒ iij.
Pulveris camphoræ gr. xxjv.
Pulveris accac. gum.
Sacch. albæ *aa* ʒ ij.
Aqua f ℥ jv. M. S. Give a tablespoonful every hour.

Towards the latter part of the night, the patient appeared to be sinking so rapidly, that the stimulant was given every half hour, which had the effect to diminish the frequency and restore to some extent the fulness of pulse and heat of the skin.

Tuesday morning, 8 o'clock—found the patient still insensible, but improved in many of the symptoms—pulse reduced to 120 in a minute, with more volume; warmth of the skin not entirely restored, pupils of the eye very much dilated. Continued the camphor and ether mixture of half the strength of the first every two hours; and the intermediate hour, the following prescription to be given—

℞ Pilulæ hydrarg.
Quiniæ sulphatis *aa* gr. xij. M.

Make 6 pills—S. Give one pill every two hours. At the same time ordered an enema of starch, half a pint, olei terebinthinæ f ʒ j., merely for its revulsive effect.

Tuesday evening, 4 o'clock. Visited the patient; symptoms improved, pulse reduced to about 100 in a minute—pupils not so much dilated—a bilious operation had been procured; intellectual faculties partially restored; would notice and answer questions. The blue mass appearing to produce nausea, the following solution was substituted for the combination of quinine and blue mass.

℞ Quiniæ sulphatis gr. xxjv.
Acidi sulphurici aromatici *gtt* L.
Aquæ destillatæ f ʒ vj. M. S.

Give a tablespoonful every two hours.

The ether mixture continued every two hours as before, alternately with the solution of quinine. Under this treatment the patient continued to improve.

Wednesday morning, 8 o'clock. Saw the patient—pulse reduced to about 70 in a minute—soft and moderately full—pupils a little dilated; perfectly rational—would answer questions propounded, and ask for lemonade, ice water, or whatever he desired. He complained of a slight head-ache. The quinine and ether mixtures were continued as before, and an enema of half a pint of starch, with olei terebinthinæ f ʒ j., ordered to be given. The patient complained of being hungry—drank a few spoonsful of gruel, and sucked an orange during the day.

Wednesday evening, 6 o'clock. Found him in most respects very much improved—pupils not so much dilated—intellectual faculties restored—a bilious operation had been procured—the tongue, which at first was clean, retained a bluish appearance, which had been produced by the blue mass ; pulse about 70 in a minute, with a quickness of pulsation and a slight febrile excitement. The patient had slept several hours during the day. The quinine solution was continued as before, every two hours, and the ether mixture reduced to half the strength of the previous one, given on the alternate hour. The febrile excitement, which was barely perceptible in the evening, continued to increase until about 12 o'clock at night ; the patient became very restless, groaning and beating the bed, in the greatest anguish ; when it was ascertained that the patient had lost the powers of speech and deglutition, but retained his intellectual faculties perfectly. He attempted to make signs with his fingers ; when a slate and pencil being handed him, he wrote very distinctly : How long will it take the doctors to cure me, so that I can speak ? It was found impossible for him to swallow, in consequence of which the medicines were suspended.

Thursday morning, 8 o'clock. Visited the patient, found him laboring under all the symptoms of incipient apoplexy—stertorous breathing, slow, full, cerebral pulse, vessels of the head turgid—powers of speech and deglutition entirely suspended—making a moaning noise, and every few minutes raising forcibly up in the bed, and then falling back again—pupils of the eye very much dilated. His temples were immediately shaved and a cup applied to each of them, by which three or four ounces of blood were extracted. Both sides of the head were then extensively shaved and two large blisters applied, covering the whole of the temporal and parietal regions, extending as far back as the occipital, and forward so as to cover a small part of the frontal region on each side. The blisters were allowed to remain until they drew well, and the following enema ordered to be given and repeated every three hours—half a pint of gruel with two drachms of the oil of turpentine. In a few hours the remedies had a very happy effect, the speech and deglutition restored—the pulse became soft and the pupils not so much dilated. In the evening, the patient complaining of pain in the bowels, an enema of starch, with a few drops tinct. opii. was administered, which soon relieved the pain.

Thursday evening, 5 o'clock. Symptoms improved—powers of speech and deglutition restored—intellectual faculties perfect—pulse about 70 in a minute and soft, temperature of the skin uniform. The patient was restless and could not sleep ; to subdue these unpleasant symptoms, the following prescription was given—

℞ Extract hyosciami gr. xij.
Pulveris glycyrrh. q. s. M.

Make 6 pills. S. Give one pill every two hours.

This had the effect to quiet the restlessness, and procured for the patient a good night's rest.

Friday morning, 8 o'clock. Found him very much improved—pulse soft and regular, about the healthy standard. He was a little restless ;

the hyosciamus was ordered to be continued in the same dose as before, occasionally until the restlessness was quieted. Towards the middle of the day the hands and feet became cold—the nervous agitation greatly increased—*subsultus tendinum* and the powers of speech and deglutition again partially suspended. The following enema was prescribed to be repeated every four hours—quiniæ sulphatis gr. v., tinct. opii. *gtt* xx., starch f ℥ jv. M. Under this treatment the unpleasant symptoms soon disappeared ; gruel was allowed for diet.

Saturday morning. Patient improving—pulse regular and soft, anxious for food—gruel and chicken water allowed in small quantities, and the quinine injections continued.

Sunday morning, 21st. Clear of fever—pulse natural, soft and regular—appetite good—symytoms all improved. Cold infusion of cinchona was prescribed to be given in the dose of a wine-glass full three or four times a day. The patient continued to improve, and was speedily restored to health.

This case presents several anomalous and very interesting features ; not only that the patient should have been arrested several times as it were from the very grasp of death, when hope itself seemed almost to have fled ; but that in the different stages and phases which the disease assumed, remedies of a diametrically opposite character had to be used and varied, to accomplish the object in question. In the first place, to relieve the congestion of the brain, cupping and cold applications were used ; as soon as this was accomplished, the vital energies of nature were found to be sinking and giving away so rapidly, that stimulants of the most powerful and diffusible kind had to be resorted to, and pushed to their utmost extent to sustain the sinking energies of life, until the powers of nature could rally and a reaction be brought about. No sooner was this object accomplished, than nature seemed determined to run into the opposite extreme, and again we were compelled to resort to depletory means, to subdue and relieve the cerebral compression. It appeared as if life were sporting between two extremes.

The case was interesting from other causes, as proving conclusively and irresistibly, in different stages of the disease, the power of certain portions of the brain to preside over distinct functions, and exhibit entirely different physiological actions.

In one stage of the disease the cerebral lobes being involved, the intellectual faculties were entirely suspended, whilst all other functions remained perfect ; in a different stage of the disease, the matter was reversed ; the medulla oblongata being involved, that portion of the brain from which originated the hypoglossal or ninth pair of nerves, distributed to the organs of articulation and deglutition ; those functions were suspended, whilst the intellect remained unimpaired.

In conclusion, I would remark, that in the sinking stages of congestive fever, I have prefered to depend upon the liberal use of French brandy, or a combination of camphor and carbonate of ammonia, as being diffusible stimulants of a more permanent kind, and with success ; but in this instance, from the peculiarities of the case, it was deemed best to trust to a combination of camphor and sulphuric ether ; and with the success of the remedies and the result of the case, we have had every reason to be pleased.

III.—*Remarks on Dr. A. W. Ely's "Examination of the Riddellian Philosophy."* Published in the July number of the New Orleans Medical and Surgical Journal. By J. L. Riddell, M.D.

1. Dr. Ely has done me the honor, to publish a courteous and well written article, fourteen pages long, purporting to contest the tenability of certain views, advanced by me in the March number of this Journal, respecting the Constitution of Matter. In acknowledging my obligations to him, for the notice he has seen proper to take of my memoir, I will avail myself of the fit occasion to subjoin some remarks, upon certain matters wherein I conceive he has misapprehended my meaning; or put forth in opposition, doctrines and arguments which I believe to be erroneous.

2. His main object appears to be, to protest against the sacrilege of an attempt to subvert, what he miscalls the Newtonian Philosophy. I have incontrovertibly shown, that the phenomena of gravity cannot arise from any occult, inherent quality of matter. By so doing, I have provoked him to allege, that I have "no very great veneration for the theory of the immortal Newton." Dr. Ely is not alone in the error of misnaming the absurd hypothesis of inherent gravitation, the Newtonian theory. The truth is, Newton himself entertained no such idea. Witness his own words: "That gravity should be innate, inherent, and essential to matter, so that one body may act on another at a distance, through a vacuum, without the mediation of anything else, by and through which their action and force may be conveyed from one to another, is to me so great an absurdity, that I believe no man, who in philosophical matters has a competent faculty of thinking, can ever fall into." (Vide, Harper's edition of Mill's Logic, p. 461.)

3. From my language in ¶ 45, upon the absurdity and inconceivableness of inherent gravitation, Dr. E. makes a sweeping and unwarranted inference; for he says (E ¶ 3.), "the position assumed by Professor Riddell is, that whatever is inconceivable, is an absurdity." Now, the true position virtually assumed by me, is equivalent to this: that if we admit certain attributes as predicable of a subject, it is absurd to predicate other attributes of the same subject, which cannot co-exist with the first. In other words, that it is absurd to affirm and deny the same attribute of the same subject. I affirm that matter is inherently inert, and possesses no primeval attributes, but extension and passive mobility; (Vide, E ¶ 8 iv. vi.), the truth of which, Dr. E. says (E ¶ 13.) "all admit." The propositions, side by side, read thus:

A. Matter possesses no primeval attributes but extension, inertness, and passive mobility.	B. Matter possesses the primeval attribute, inherent gravitation.

Now, since inherent gravitation is not equivalent to, nor deducible from inertness, extension or passive mobility, it is clear that if proposition A be true, which Dr. E. has virtually admitted, (E ¶ 8, iv. vi.), proposition B must be absurd; false and inconceivable. It is certainly inconceivable, that two contradictory propositions can both be true. To affirm them true is an absurdity.

4. There are many phenomena in nature, whose specific modes of

production, our limited knowledge has not yet enabled us satisfactorily to imagine or conceive. The mechanisms productive of such phenomena, though not yet truly conceived by the human mind, on account of its limited range of perception, and limited stock of relevant truths, cannot be said to be in their nature inconceivable. Unimaginable at the present time, to us they may be; but inconceivable if clearly developed and set forth, they cannot be; unless we admit that truths can be inconsistent with each other; or that a thing can exist and not exist at the same time.

5. The hypothesis of inherent gravitation, can have no foothold in a mind conscious of the true relation of cause and effect. Unfortunately, this subject is one, upon which philosophers and metaphysicians have generally entertained the most vague and erroneous notions. Thomas Brown, in his monograph on the relation of cause and effect, undoubtedly comes nearest the truth. Yet he affirms, that invariable sequence in point of time is the only connection subsisting between them. This, I admit, is all that mere observation can reveal to us. But in my opinion, the relevant meaning of the page of nature, if rightly interpreted, is something more than is implied by invariable sequence. The relation of cause and effect is that of necessary and invariable sequence, in accordance with the law of physical continuity, as exclusively subservient to the conservation of momentum, which is indestructible. The widely prevalent, yet erroneous idea, that an effect has no physical connection with its cause, has been the prolific source of absurd hypotheses in philosophy.

6. Like many other instances that might be cited, occurring with those who follow great names, my critic has carried the views of Brown, to an extent never contemplated by Brown himself. What he says on the subject of cause and effect, will be sufficient exemplification: "What we call cause is but an antecedent of which the consequent is called the effect. The former is but the sign, and not the cause of the latter." (E ¶ 15). By virtue of what reason or authority, can Dr. Ely assert that a cause is *not* a cause. The supposition is inadmissible, that any rational man would seriously contend that a steamboat is not a steamboat. I am therefore, in courtesy, bound to suppose, that Dr. E. really means, that no such thing as a cause exists. He would, hence, deny that there is continuity in the progress of nature, and affirm that each and every phenomenon is a disconnected, mystery-shrouded, independent event, and therefore a miracle. With the book of nature legibly before me, I can say for myself, that it is impossible to entertain a belief so unnatural. He who has imbued his mind with such doctrines, must have set out with the selfish and erroneous assumption, that nature's truths depend for their existence, upon the ability of the human mind to observe and appreciate them.

7. Dr. E. cannot brook the idea of the existence of a material interplanetary medium. He complains that I do not dwell "upon this all important subject at all." "Let him take up this subject," he proceeds, "and if he can establish the existence of a resisting medium, he will then have some foundation for his theory," (E ¶ 11). "In regard to the luminiferous medium, we are to bear in mind, that although it

affords a very satisfactory explanation of the phenomena of light, its existence is not proved." (E ¶ 12).

How any rational man, accustomed to reflect on philosophical matters, can for a moment doubt the existence of some kind of medium or substance, which is the instrument of conveying to us the light of the heavenly bodies, I am unable to comprehend. As well might he doubt the ordinary evidence of sound as indicating the existence of the atmosphere; or of the waves of the ocean, as indicating water. I confess, I have scarce patience to reason with the full grown man, who contends that an absolute vacuum is maintained between the heavenly bodies. Light travels at a known rate, a determinate velocity. In an absolute vacuum there would be nothing to move, hence there could be no velocity. Every sane thinker must then admit the existence of a luminiferous interplanetary medium; a something occupying space, and therefore material.

8. I regret my critic has not thought proper to take up, comment upon, and disprove if possible, the specific topics and demonstrations which make up my memoir. I also regret that he has in many instances misconceived, and in his own language mistated my opinions. The latter he might easily have avoided, by making literal quotations, and designating the same by double commas, as is customary. This he has done in two unimportant instances only, embracing together, some nine lines.

9. In short, I do not think Dr. Ely has, in this instance, written with his accustomed ability, which may in part be attributable to the haste in which his article evidently was composed. This I infer, from many small instances of vagueness, and even implied contradiction with which it abounds. As for instance, in denying the existence of a ponderefacient medium, (E ¶ 10) he says, "of the existence of such a medium there is no satisfactory evidence whatever." In his first paragraph he cites La Place as having approved the Newtonian theory as he calls it, meaning the theory of inherent gravitation; while in his tenth paragraph he says, "the calculations of La Place would make the tenuity of the ponderefacient medium, fifty millions of times greater than that of the luminiferous medium."

10. Dr. E. admits, that matter is inherently and necessarily possessed of no qualities, unless its extension, mobility and inertness be called qualities. (E ¶ 13). While in another place (E ¶ 26) he sets about contradicting his own admissions, by contending that all matter is inherently possessed of weight; because, he says, "we can conceive of no matter destitute of weight." He first forms a pretty fair conception of such attenuated matter as necessarily could not manifest to us the precise phenomenon which we call weight; and then absurdly denies the conception he has formed.

11. It is evident from his remarks in E ¶ 26, that Dr. E. has no clear idea of what he means by weight. Matter is so constituted and aggregated as to manifest as circumstances determine, two universal tendencies: expansion and contraction. 1. The translatory motion individually possessed by the integrant molecules of any substance, or medium, tends by centrifugal force and mutual mediate collision, to make the body occupy more space; hence repulsion and expansion: 2. The currents of momentum by collision from the atoms of far more

attenuated media, co-existent and circumambient, being from each other mutually intercepted by the grosser molecules, causes them to tend towards each other; and hence attraction and diminution of bulk.

The integers or atoms of every grade of matter (except in the extremes of the series which cannot be assigned), must be more or less under the dominion of these opposing tendencies of attraction and repulsion. The manifestation of the former, (impulsive attraction), between spheroids and masses of molecular matter, such as come under our cognizance, produces the phenomenon which we call weight. Between remote terms of matter, remote in point of magnitude, the mutual manifestation of this tendency, must be incomparably less, somewhat in the ratio of the terms themselves. Hence we cannot expect to find precisely what we call weight in the luminiferous, and much less in the ponderefacient matter. Mass, which in one sense only, we are entitled to regard as the equivalent of weight, all matter must necessarily possess, independent of attraction and repulsion. In further elucidation of this subject, I will refer to paragraphs 47 and 120 of my Memoir.

12. Dr. E. contends ardently, that all physical action is action at a distance; never by contact; in support of which he says, (E ¶ 18) "when motion is transmitted through a body, there is no proof that the spaces between the particles are diminished." By this expression, Dr. E. evinces that it has been some years since he gave special attention to these matters. That there really is a diminution of distance between the particles, in the transmission of momentum through a body, he cannot fail to be convinced, if he will recur to what Newton, La Place and Brewster have written upon the propagation of sound. (Vide, Art. Pneumatics, Ed. Ency., and also Newton's Principia.)

13. One of Dr. E.'s principal misapprehensions of my views is this: he seems to think that I either propose to show (which I do not) how motion originates, (E ¶ 14) or that it is incumbent on me to do so. I do not pretend to inquire about the first beginning of motion, but merely its conservation; and therefore beg to refer him to ¶ 56, for my views thereon.

Some few other topics of minor importance, contained in my memoir, Dr. E. has honored with a passing notice of disapproval. A satisfactory reply to all the counter arguments which he advances, can be gathered from the memoir itself; and I am satisfied, if he had taken time to give it an attentive and unprejudiced perusal, his criticisms would have been very much modified or abridged.

IV.—*Strychnine.* By Benj. R. Hogan, M. D., late U. S. A., Cambridge, Dallas Co., Ala.

CHOREA.

Case 1. In October, 1845, I was called upon to treat an interesting young lady, M. J., aged 17, of bilious, nervous temperament, who had been attacked three weeks before with Chorea.

Her general health had been good, and catamenia regular. Had menstruated once, since the seizure, without any aggravation of her symptoms.

The exciting cause may have been a quantity of *Underwood*'s pickles, eaten at a late hour, the night before the attack. At breakfast the next morning, she could not command the muscles of the right arm. In a few days those of the right lower extremity were involved. Aphonia, difficulty of deglutition, succeeded, and all the voluntary muscles were similarly affected. She had slept but little during her illness,—when she did sleep, the paroxysms were suspended. She would awake almost frantic, and had to be restrained constantly to keep her from injuring herself against surrounding objects. She had taken blue pill, magnes. æt. Rhei, morphine and antispasmodics, and been blistered upon the spine, without any relief or benefit.

There was no spinal irritation nor tenderness over the spinal ganglia of the sympathetic; the tongue was too large, slightly furred, presenting a congested appearance, with an excess of saliva.

I treated her with emeto-cathartics (calomel and ipecac.), carb. iron precip., nit. arg. etc., with the electro-magnetic shocks, for five or ten minutes every night, for ten days, without any relief, except that her nights were more tranquilly passed, and her sleep more refreshing.

I now put her upon Strychnine, 1-20th gr. of the alkaloid, *ter in die.* There was an evident amendment within three days, and she was perfectly restored to health in one month. The larynx was the last organ to recover fully its power, and the voice its full, rich tone. I did not think it advisable to gradually increase the dose of the Strychnine—its specific influence was slightly developed, several times during the treatment, and the remedy intermitted for one or two periods of administration.

Case 2. Dec. 23, 1845. Mrs. R——, bilious lymphatic organisation, aged 26, now six months advanced in her fourth pregnancy. Has had Chorea, involving the muscles of right upper and lower extremity, for six weeks. Her general health has been delicate, though there is a general fullness and plumpness of the muscles. Attributes this disease to violent mental emotion, from being almost an eye-witness to the murder of her sister and the suicide of the murderer, which took place in her own house a few days before the attack. The twitching commenced in her right hand, has gradually become more constant, and extended to the right foot two weeks ago. Is now incessant. There is no apparent derangement in the organs or functions of nutrition, and the condition of my patient forbidding more active remedies, I order:

℞ Strychnine, 1-20th of a grain three times a day, to be increased to $\frac{1}{8}$ of a grain *ter in die*, within ten days, should it not produce its specific effect.

Jan. 4, 1846. Mrs. R. has taken 24 pills, each 1-20th grain of Strychnine, since the 23d ult., with no untoward symptom, and thinks she is improving.

Direct.: Pill. Strychnine, 48, in each 1-20th gr. To be given as last directed.

Jan. 18. Has several times felt some giddiness and involuntary twitchings of the face; has taken all the pills and is nearly well.

I now send her: ℞ Strychnine, 3 gr. div. in p. No. XXVI. To commence with one three times a day, and increase one pill each day, until she feels their peculiar influence.

These completely relieved her, and at the full time she was delivered of a fine healthy male child.

I have used Strychnine, with much advantage, as a tonic in the convalescence from intermittent and congestive fevers. No relapse of the latter has followed its use.

In *subsultus tendinum*, from extreme exhaustion, in an anœmic patient, with diarrhœa, I found it a most valuable combination with the *persesqui-nitratis ferri.*

In the atonic dyspepsia of the habitual drinker who wishes to become a *teatotaller*, it has a comforting and permanent value.

The experiments of Magendie and Andral prove that Strychnine is rapidly absorbed, and enters the circulation; those of Vernière and Segàlas, that the blood is changed by its admixture, and we are admonished to be cautious in continuing its use too long, as we are told that from its influence upon the blood, in changing its constituents in some unknown manner, it frequently reacts upon the brain and nervous system, causing convulsions, spasms, paralysis and death!

These grave consequences admonish us not to continue it too long. I look upon it as a potent and valuable agent, and one whose use as a therapeutic will be greatly extended as its valuable tonic properties are appreciated.

V.—*A Case of Malignant Tubercle.* By J. B. WILKINSON, M. D., of Louisiana.

On the morning of the 6th March I was called to see Mrs.——, affected with cold, accompanied with feverish symptoms, pain in the side, and headache. A small pustule had formed on the right side of the upper lip, about the size of a No. 4 shot—this gave her some pain, and had a hard base. I thought it the bite of a spider; but her friends as well as herself supposed it to be merely a fever blister, and considered it of no importance. I prescribed a dose of salts, and a mustard plaster to be applied to the side. Calling again in the evening, found that the salts had acted slightly; pain in the side disappeared; tumour on the lip had increased in size, felt hard and was painful. Applied a hop fomentation to the tumour, ordered a hot foot bath for the headache which was quite severe, also gave 10 grains calomel.

On my visit next morning was told that Mrs.—— had passed a bad night; had suffered from headache, with accession of pain in the side and fever. Mustard plaster had been re-applied to the side in the night, with relief to the pain. Examined the side with the ear; found vesicular respiration perfect; no abnormal sound; no pain upon pressure between the ribs; tumour on the lip increased in size and painful; tumefaction extended to the *meatus auditorius externus;* had a hard, lobulated feeling; hip poultice had given some relief, but in consequence of becoming loosened had been taken off; tumour presented an ugly livid hue; headache continued; pulse was *hard* and frequent. I thought now of bleeding the patient; but knowing her constitution to be feeble, her general health indifferent, that she was of a very nervous temperament, and subject to attacks of great nervous depression, I substituted for it the use of the tart. emet. solution during the day, and directed that a flaxseed poultice should be applied to the tumour. The calomel and salts previously taken had acted well during the night. For diet, ordered chicken soup.

I called again at 1 o'clock, and found the patient with pale face; skin natural; pulse not so hard, increased in frequency; tongue perfectly natural; expression of eyes languid; complained of great debility; feet at times cool; indisposed to mental or physical exertion; tumefaction extending under the throat.

I felt now much anxiety for the patient—symptoms seemed to indicate the action of a sedative poison. Arrested the exhibition of the tart. emet. solution; ordered as much chicken soup to be given as would be taken. Called again at 6, P. M. Found Mrs.—— worse; tumefaction increased; respiration embarrassed; skin cool; pulse weak and frequent; somewhat stupid; moaned a good deal in sleep, which was very interrupted; said she was perfectly free from pain, but evidently suffered, as indicated by moaning; required to be addressed in loud tone to attract her attention; seemed at times delirious. Commenced giving ammonia and wine which were repeated at frequent intervals during the night, but without any amelioration of her condition; on the contrary, the patient grew gradually worse, until 9 A. M. of the 8th (about 48 hours from the time I first saw her) when she expired. At the time of death the swelling had extended unto the left side of the face, and appeared of the same color. The foregoing, no doubt, was a case of "Malignant Tubercle" which, although rare, occasionally occurs in this country, and is considered identical in nature with the *Charbon disease*, so fatal among cattle. The only known cure to the farmers is immediate cauterization of the part, or its extirpation—and had it been practised at once in this case, the result might have proved different.

VI.—*Ligature of the external Iliac Artery, with a report of the treatment of other surgical cases in the Charity Hospital, during the months of February and March.* By A. J. Wedderburn, M.D., Professor of Anatomy, in the Medical College of Louisiana.

Henry Watkins, laborer, aged 22 years, born in the State of Ohio, entered the Charity Hospital, on the evening of the 23d of March, after having lost about a quart of blood from a large ulcer in the groin, which extended as far upon the abdomen as the point over the internal inguinal ring. The bleeding was arrested, but it occurred again betwen 3 and 4 o'clock the next morning; and was again controlled by the application of cold lotions with slight compression.

I saw the patient on the morning of the 24th, for the first time, about

9 o'clock, and upon examination discovered the existence of an aneurism of the femoral artery, which had burst the evening before, and from which the hemorrhage had been arrested by the formation of a clot within the sac. The pulsation could be easily detected over a space of about 2½ inches in diameter; and the point from which the bleeding had occurred exhibited clearly the walls of the sac, exposed so as to present a conical shaped elevation, with a base of about ¾ of an inch, situated immediately over the femoral vein.

The only history of the case I could obtain from the patient was, that about three months previously, he had suffered with a large bubo, which opened spontaneously, and then followed a large sloughing ulcer, which entirely healed. After which a swelling commenced again, followed by ulceration; but he could not say that he had noticed the pulsations in the tumor. The only conclusions, then, that I could arrive at from his statement, were, that the coats of the artery must have become impaired from the existence of the bubo; in the first instance giving rise to the aneurism, and after the cure of the original ulcer, the cicatrix was dissolved by the gradual enlargement of the aneurismal sac.

After consulting with Drs. Harrison, Wedderstrandt, McCormick, Wilson and Farquharson, I determined to ligature the external iliac artery, at 4 o'clock in the afternoon. When we arrived at the Hospital at the above mentioned hour, I learned that since my visit in the morning, about a pint or more of blood had been lost, and the hemorrhage arrested by pressure. The patient by this time, from the repeated bleedings, had lost at least half a gallon of blood, leaving him in a very debilitated condition.

Operation.—In consequence of the extent of the ulcer, the incision was commenced about an inch within its limits, at a point above the internal inguinal ring, and carried in a slightly curved direction, as high as, and within ¾ of an inch of the anterior superior spinous process of the ilium. The first incision exposed the tendon of the external oblique, which was divided upon a director; a small opening having been first made into its lowest exposed part. I endeavored then to introduce my finger under the arched border of the internal oblique and transversalis muscles, but the tissues at this point were so much transformed, that I was compelled to cut carefully through the muscles with the scalpel. After exposing the facia transversalis, in which an opening had been made large enough to admit the point of a finger, in dividing the muscles, I introduced two fingers beneath the facia, for the purpose of guarding a probe pointed bistoury, with which it was divided. The peritoneum, then brought into view, was turned aside until the sheath of the vessels, with the nerve lying upon it, was clearly exposed. The sheath was opened with a pair of forceps, the nerve drawn well to the outer side, and an aneurismal needle, armed with a ligature, passed behind the artery from within in an outward direction. Compression then having been made on the artery, and the pulsation in the tumor entirely arrested, the ligature was tied about an inch above Poupart's ligament. In about half an hour after the application of the ligature, the edges of the wound were drawn together by four interrupted sutures, and adhesive strips applied.

In consequence of the constant occurrence of erysipelas on operations in the wards, lint saturated with a solution of the sulph. of quinine, 10 grs. to the oz. was ordered to be constantly applied over the incision; and sulph. quinine grs. x., opium grs. ij., administered in consequence of a pain in the limb, complained of at night.

March 25th.—Slept tolerably well during last night—no pain in the limb—temperature of the leg good, sensation somewhat impaired; pulse regular but threadlike. Ordered, Vallet's ferri carb. grs. iij., sulph. quinine grs. ij., every two hours. Evening—ordered, pulv. Doveri, grs. xv.

26th.—Rested well—pulse 104; treatment same as yesterday.

27th.—Improving, pulse 106—slight discharge of healthy pus from wound; removed a coagulum about the size of a hen's egg from the aneurismal sac. There remains still a partially organized clot with attachment, which prevents its removal without force. Slight tenderness over the abdomen;—treatment same.

28th.—Pulse 108 and regular—slept well last night, tongue looks well, spirits good, ulcer and wound secreting healthy pus; tenderness of the abdomen has subsided. Had a small evacuation from the bowels, produced by an enema of cold water, introduced by a hydrostatic injecting tube. Dressing to be applied morning and evening.

29th.—Slight febrile action last evening; administered ¼ gr. acet. morphiæ. Has had an evacuation from the bowels this morning, produced by an injection of cold water. Pulse 104, increasing in fullness. Ulcer and wound look well; discharge diminished, appetite good. Diet, milk and mush, and chicken water;—wine. Sensibility of the limb returning.

30th.—Morning, pulse 104, temperature and sensation of the limb entirely restored. About six o'clock in the evening, a profuse hemorrhage occurred from the aneurismal sac, which was arrested after the loss of about 2 lbs. of blood, by the application of cold lotions and moderate compression. Ordered, syrup morphiæ ℥ ss.

31st.—Morning visit, rested badly—complains of pain in the limb, foot and leg cold; sensibility in foot impaired. Pulse 110; continue quinine and iron. Ordered the limb to be enveloped in flannel.

Evening.—Pulse 118. Ordered Dover's powder 10 grs., to be taken at 9 o'clock.

April 1st, Morning.—Rested badly last night—pulse 116. Iron and quinine continued.

Evening.—No improvement; pulse 118. Ordered sulph. quinine and Dover's powder, 10 grs. each, at bed time.

2d, Morning.—Pulse 160; hemorrhage occurred twice last night from the sac; lost a large quantity of blood. Continue the quinine and iron.

Evening.—Pulse 132. Ordered sulph. quinine, grs. xv., to be taken at once, (6 o'clock) and Dover's powder, grs. xv., at bed time if necessary.

3d, Morning.—Rested badly, limb has continued to be cold since second hemorrhage, and the lower part of leg and foot present a gangrenous appearance. Pulse 140. Ordered carb. ammoniæ, grs. x., every two hours.

Evening.—No improvement. Dover's, gr. xv., at bed time, and continue carb. ammoniæ.

4th.—The gangrene has extended above the knee—pulse 160. Discontinue the carb. ammoniæ, and administer the sulph. quinine grs. xv., tinct. opii. *gtts.* xxx., in an ounce of water, and ordered the same at noon and night.

5th.—From the time of the last hemorrhage, the patient gradually declined, and died at 4 o'clock, this morning.

Post-mortem examination.—On opening the abdomen, it was ascertained that the peritoneum had undergone no change from its normal condition. Adhesion had already formed between the peritoneal sac and the iliac facia, so that considerable force was required to expose the iliac vessels. The ligature was found as mentioned in the description of the operation, about an inch above Poupart's ligament. Although it had not yet come away, well organized clots had formed in the artery,—the one above the ligature extending as far up as the internal iliac, and for the distance of an inch above the ligature; so firmly connected with the internal coat of the artery, as to have effectually prevented the occurrence of secondary hemorrhage. The clot below the ligature was not so large, but well organized. The aneurismal sac was so situated, and of such dimensions as to occupy a space of more than two inches, and involve the profunda—the external pudics, and the internal and external circumflex arteries; all of which were found to be constricted at their points of communication with the sac.

This large number of arteries communicating with the sac, account sufficiently well for the profuse bleedings which occurred after the operation; it being very probable that the communicating arteries had commenced adapting themselves to the condition of things previous to the rupture of the aneurism.

The question of most interest to the surgeon in this case is—would it not be proper in all such cases, after tyeing the artery above, to open the sac freely, remove the clot, and fill the cavity with lint, with a view of obliterating the vessels communicating with it, by the process of granulation? The circumstances connected with this case, I am induced to believe point to such a course of treatment.

* * * * * *

Since the report of the January number of this Journal, concerning the treatment of indolent ulcers, in my ward, in the Charity Hospital, with the sulph. quinine, I have continued its topical application, not only to indolent ulcers, but have used it with marked success in other varieties.

I have noticed the following facts, that when applied to the surface of an inflamed ulcer, the redness surrounding the part has been subdued in less than 24 hours—and the same holds good concerning the pain in an irritable ulcer, whilst almost invariably in the same space of time the surface is made to present an healthy, granulating appearance, secreting healthy pus. When this remedy is applied to a small indolent ulcer, its effect is not so prompt as when applied to a large ulcer of the same variety. Now this would lead us to suppose that the remedy is absorbed; but from experiments, I have ascertained that the statements made by M. Martin Solan, in the *Bulletin de Thérapeutique,* and republished in the March number of this Journal, are entirely correct; viz: that the sulph. of quinine when applied to the surface is not absorbed; i. e., if its not being detected in the urine is evidence of this fact. I have always been able to detect it when given internally in small quantities, when I have failed at the same time in testing the urine of those when it had been applied to the surface. In a case of phlegmonous erysipelas of the thigh of a woman in the Hospital, extending over a surface of 8 or 10 inches in length, and occupying the entire breadth of the external part of the thigh; with an opening in the centre about an inch in diameter; under the inflamed surface the cellular tissue was so completely destroyed, that a probe could be passed freely in any direction. Into this large sinus an ounce of water, holding in solution 10 grs. of the sulph. of quinine was placed, three times during the day, making in all 30 grs. The woman was placed in such a position, that the escape of the solution was entirely prevented. The next day, the pain and redness of the surface had almost entirely disappeared; and it was clear that the three ounces of the sólution had permeated the tissues. The urine voided before the administration of this remedy, and that passed during the administration, were tested with all the tests for quinine, and the effect produced upon both was the same, no precipitate being afforded in either case. This case recovered in a very short time, no other remedy having been used during the treatment.

Although I could mention a large number of cases exhibiting the superiority of this over any other remedy as a topical application to ulcers, I will only mention one case which I had under treatment.

A. B. Was admitted into the Hospital, with an extensive syphilitic ulcer extending from the upper part of the sacrum to the anus, exposing a large portion of the ligaments covering the sacrum—this case had been under treatment for more than ten days—various applications had been made without the slightest effect, when I determined to resort to the topical application of the sulphate of quinine. I ordered the part to be washed with a solution of tannin, 8 grs. to the ounce of water, and lint to be constantly applied, saturated with a solution of quinine, 10 grs. to the ounce. The constitutional symptoms under which she was suffering, with the excessive pain of the part, was much relieved in about 24 hours, and in three days the entire surface was covered with healthy granulations. The quinine was applied about 20 days; the ulcer improved since its first application daily, and entirely cured in a few days, as the granulations soon became even with the surface of the skin.

* * * * * *

The operations since my last report have been, one amputation of the breast; one of the middle finger, at the second phalangeal joint; one of the middle finger in the continuity of the metacarpal bone, by the oval method; one of the little toe, at the metacarpo-phalangeal articulation. Two operations for phimosis, one for fistula in ano; two for strabismus, and one operation for hydrocele, by Ricord's method.

The case of hydrocele mentioned above, was one from which a quart of water was evacuated after the introduction of six double ligatures, at distances of about ten lines apart. The patient was placed on his back. The testicle occupying the back part of the tumour was grasped by the left hand, and a long needle, armed with a double ligature, was passed through the tumor at its upper part; and at the distance of about ten lines below, the same was passed through the tumour again, and so on, until six double ligatures were introduced; when a small trocar was passed into the dependant part of the sac. After evacuating the contents of the sac, a quill was placed upon either side, between the ends of the double ligatures, which were separately tied. The ligatures remained about 30 hours; and although the testicle, after the removal of the fluid was found to be very much enlarged, the patient was discharged in about three weeks from the Hospital, perfectly cured of the hydrocele, and the testicle very little above the normal size. The treatment after the operation was only the administration of about 35 grs. of the iodide of potassium daily, which was ordered in consequence of the enlargement of the testicle. The advantages of this operation, which I consider one of the greatest improvements in modern surgery are, that the inflammation which is necessary in effecting adhesion between the walls of the sack, is entirely under the control of the surgeon, it being in his power to remove the ligatures at any time, when such removal may seem necessary in his judgment, and again when this double quill suture is properly applied, the walls of the sac throughout its whole extent are brought in contact, and by the adhesive process it is in a few days obliterated.

VII.—*Report of a case of Trismus Nascentium. By* Wm. B. Lindsay, M.D., of New Orleans.

Trismus Nascentium is at this time attracting much professional attention, and it is to be desired, that the spirit of inquiry will be continued until the nature and treatment of it is better understood. It is not to be expected, that any practitioner within himself will be able to point out a method of cure that will suit the majority of cases, but if the experience of many be combined, much may be done to lessen its terrible ravages. In treating this disease I have followed the directions of popular writers, and have often regretted my inability to relieve the little sufferers.

The object of this paper is to give a plain statement of the facts, in a case which came under my care a few days ago. If I shall succeed in imparting one item of information in relation to this devastating malady, I shall think I have done some service.

June 30. Requested by a friend residing on Magazine, near Edward street, in this city, to visit at his house a little negro child, who had been sick several days. Whilst there, learned that it had been sick three days, was just two weeks old, was still under the management of the midwife, that it had been healthy and fat up to the time of its attack; never had been unwell before that; the parents were young and stout servants; that it had refused the breast ever since its illness; that the cord had come away only the day before! that the umbilicus was tumid, and not disposed to heal up. The mother said he had fits—mouth closed and could not open it; the attempt to pass my little finger between its gums brought on clonic spasms and violent screams; from having been fat three days before, it was now emaciated; face looked old and wrinkled, thumb stiffened across the palms of the hands, as mentioned by Dr. Eberle in a late paper. The midwife had given it a small quantity of magnesia, a few hours before I saw it; bowels moved, discharges described as yellowish, and like curdled milk; they were afterwards black—there was some fever—slept but little, if any—as to its position, the mother said it was sometimes laid on one side and sometimes on the other. Was satisfied that it was trismus, and the family knew it had "lock-jaw" before I was sent for. Notwithstanding the many interesting papers that have appeared upon this subject, I am inclined to believe that there is but little known about it. Experimenting physicians are often justly dreaded by the public; but in a case of this kind, there is much to be set down in extenuating such a course. I instituted the following treatment, entirely new to me at least: I directed the scorched cotton, charcoal powders, &c., to be discontinued; and applied to the umbilicus in their stead, an unguent of ℞ sulph. quinine grs. iij., sulph. morphine gr. ss., axung'e ʒ j vel. ij. Did not attempt to administer any thing, *per orem*, on account of difficult deglutition.

July 1. Spasms not frequent, masseters rigid; face remarkably wrinkled, appeared to be extremely sore, as it screamed upon being moved. As there was still some fever, directed tepid baths, every three hours during the day. Alvine dejections not copious, but dark. Directed the mother to draw some breast milk and feed it. Slept a little or was quiet.

July 2. Spasms ceased—sleeps better—mouth open—feeds freely—but would not suck; umbilicus improving, discontinue quinine and morphine; dressed umbilicus with castor oil alone. Directed a few teaspoonsful of cold flaxseed tea to be given to it as a drink.

July 3. Sleeps well, cord healing, feeds freely, does not suck yet; no fever, looks lively. Continue cataplasms and ol recini dressing to umbilicus.

July 4. Still improving in every respect; discontinue regular visits.

July 6. Improving fast; umbilicus healing up finely, sucks freely, sleeps well.

July 7. Same report as yesterday.

Reflections.—Some years ago, a relation of mine, and an independent planter, of Claiborne county, State of Mississippi, was bitten on the ankle by a rattle snake. Annually this leg swelled, and was on some occasions the cause of serious apprehension. On one of these periodical attacks I was sent for, found an ulcer as large as half a dollar (if I recollect correctly) on the calf of the leg; applied a plaster of quinine and laudanum, and gave him ten grain doses of the sulph. quinine every morning; put him on high diet, no difficult matter in his case as he usually lived in that style, and he was well in a few days; the result of this case induced me to try it in the case of trismus, although there does not seem to be any resemblance betwixt them. In those cases when the umbilicus has healed up; a small blister could be made and then the quinine be used. Did this case get well in spite of treatment? I never knew a case to recover before. Did the magnesia cure it? Aperients have not usually cured the disease. Could the morphine have been the principal agent? Opiates have signally failed to relieve it.—Shall we credit the quinine?

VIII.—*Researches, Historical, Topographical, and Critical, on Yellow Fever.* By Bennet Dowler, M.D., of New Orleans.

[The following fragment, with its title,* is copied from the pages of an unpublished volume. A few facts, not properly belonging to the times and places with which they here stand connected, have been added, and, certain controverted opinions, incidental to the same, are rather indicated, than proved, for the plain reason, that the evidence is too voluminous to be admissible on the present occasion. A spirit of free inquiry is necessary in science. Many gentlemen who adorn the profession of medicine with talents of the highest order, dissent from some of the views here expressed; at the same time it is hoped, that they will find nothing in the tone or expressions of this paper, devoid of deference and respect, though the history of nearly all discussions relating to contagion, have been marked by a personal animosity which has proved decidedly contagious, and, sometimes fatal. Doctors Bennet and Williams, of Jamaica, ended their disputes on the contagion of yellow fever, by a duel, in which both were killed.]

Many circumstances have contributed to throw obscurity over the medical history of Louisiana. The rise, progress, and, even the present condition of its population, present all the elements of a transition state—a state, however, which will soon be replaced by all that stability, or rather progression towards social, political, and scientific perfectibility, once deemed visionary, but now actualized to a great extent as characteristic of the Anglo-Saxon race. The Nomadic form of society has few or no records; its barbarous monotony presents nothing worthy of history. In the transition state, the mind is active, but its direction is irregular; events abound, but their continuity is broken. The present is everything; the past and future nothing.—Transferred from France to Spain, and from Spain to France again, and from the latter to the United States, Louisiana has been thereby a great loser in early, and authentic memoirs, chronicles, and annals, which have been either buried in the archives of foreign cabinets, or published in foreign languages so long ago, as to be now out of print. Besides, the population was a heterogenous compound of Frenchmen, Spaniards, Englishmen, Germans, Italians, and Anglo-Americans, speaking as many tongues, devoted almost exclusively to the pursuit of wealth, and but little inclined to enter upon original laborious, and prolonged investigations, which held out neither a hope of compensation, nor those laurels of honorable distinction which are never withheld in the highest state of civilization,—a state which develops the loftiest traits of intellect, the treasures of science, and the beauties of art in the highest degrees.

Physicians, with few exceptions, like others, sought to make fortunes in expectation of returning to their fatherland. Divided by language,

* Hence the reader will see, that this *title*, if not a misnomer, is at least too extensive in its import, for the present paper. One reason for not changing it is this—I hope that some of my *confrères* will kindly furnish me some facts or references to books or papers upon the subjects which it designates, however old and obsolete. I am already under obligations to several gentlemen for the use of books.

by nativity, education, and medical doctrines, and by professional rivalries, they lived isolatedly with respect to each other—an evil which has unfortunately descended to our own generation. The advancement of medical knowledge in a new and peculiar climate where the maxims of different latitudes and places had little, or but a limited application, required, not only will, energy, disinterestedness, unity of purpose, and a noble emulation in science, but a vast accumulation of facts, with labor upon labor. This work required time, study; the comparison of era with era, of place with place, of fact with fact, of deduction with deduction. The last century was not very favorable to the untrammeled operations of the mind. Theory was mightier than observation and experiment. Isolated cases were often preferred to numerical analyses and facts *raisonêe*, and nosological systems to pathological anatomy, rigid faith to rational skepticism, a golden headed cane to physical diagnosis, an air of mystery to a chemical examination and interminable labor.

Formerly, medical services, in New Orleans, were compensated with a liberality never, perhaps, equalled in any other city. This golden age was poor in literature. Physicians grew gray in the most extensive practice, and left the theatre of their fame, for the "undiscovered country," without bequeathing any scientific information for the benefit of posterity. Perhaps they believed that as posterity had done nothing for them, they owed to it nothing. Some of these venerable fathers of medicine, descended to the tomb but a few years since. The late Dr. K., who practiced until the last two or three days of his long life, left no account of his experience, though, he is said to have treated no less than twenty thousand yellow fever patients alone.

Thus the knowledge acquired by the resident physician during the longest life, perished with him, or was carried, in some instances, to his native land, and published in periodicals or books of temporary duration and limited circulation, soon to be forgotten.

In the absence of scientific annals and monographs illustrative of our early medical history, I have resorted to such broken files of newspapers, miscellaneous journals, medical periodicals, and books as were within my reach, with the view of giving to certain important facts, now scattered over the surface of yellow fever literature, some degree of consistency. I do not aim at giving a history, or even an outline of the entire subject implied by the title of this monograph, but simply such salient points with which I happen to be most familiar, possessing something of a suggestive character, and which may be comprised in the narrowest limits.

If I shall fail in the execution of my task, (which is altogether of secondary importance to my main design,) due allowance must be made for the imperfection of my materials. New Orleans has not a public Library! Indeed, I have avoided the reproducing of many facts, simply because they are well authenticated and generally known; for example, Dr. Rush's histories of Yellow Fever in Philadelphia, not to mention other similar works.

If authorities are not always mentioned, the reason is obvious. A multitude of useless references with the name of the author, the title, the volume, the page, the date, the edition and the like, would occupy almost as much space as these historical notices themselves. An event spread over many pages, when divested of all local and temporary circumstances incidental to its era, may often be expressed by a single sentence or figure, without impairing its intrinsic value. In many instances, impartial justice demands special acknowledgements. These are never intentionally withheld. The building up naked pyramids of bibliography, without any compensating advantage, is, sometimes as ostentatious, as it is useless. Such literature is the personification of dullness, requiring nothing but a large library, and the patience necessary to copy title pages, and, is but little more pleasing to the eye than a company of grinning skeletons at an evening party.

The origin of yellow fever, though, comparatively modern, cannot be fixed very exactly. Some authors, especially the advocates of contagion and the importability of this disease, have erred in dating its origin so late as the last decennial period of the seventeenth century, the epoch when as they erroneously affirm, it was first introduced into the West Indies from the Kingdom of Siam. In the earliest records on the subject, the symptoms are generally either not mentioned, or they are imperfectly described, and a name only is substituted. Different nations and often the same nation, adopted different names; such as the putrid, bilious, malignant, ardent, Bulam, Barbadoes, infectious, pestilential fever; the pestilence; the bilious plague; the American plague; infectious distemper; causus; mal de Siam; Fièvre de Boulam; Fièvre de Matlot; febris ardens biliosa; vomito prieto; vomito negro; on many of the tombstones in New Orleans, *Fléau*, instead of *Fièvre jaune*, is inscribed as the cause of death: finally, in English books, yellow pestilence, black vomit, bilious remittent, and bilious yellow fever, especially the latter, came into general use, though latterly, the epithet bilious has been omitted. This confusion in nomenclature renders it difficult not only to identify the disease in old books, but to trace its origin, as the name is generally given for the description of symptoms.

1618—1647.

Origin of Yellow Fever in America—Indians—Barbadoes.

1618. Mr. Webster, who has collated the various authorities in relation to the origin of yellow fever in America, shows in his work on Pestilence, that when the whites arrived in New England, in 1620, some of the Indian tribes had been reduced from 30,000 to 300, two years previously. The survivors asserted, that the sick bled from the nose, and turned yellow like a garment of that color which they pointed out as an illustration. Their statement as to the great mortality which the malady caused, was fully confirmed by the number of recent, unburied skeletons, strewed about their towns.

This author, at a later period, after more thorough investigation, reiterates the statement, "that a pestilential yellow fever prevailed among the nations of New England, about two years before the settlement made by the English, is a fact as well attested as any historical fact on record."*

The settlement of Virginia preceded that of New England. Some of the colonists in the former, before those of the latter arrived, had witnessed the almost entire destruction of many Indian towns by an epi-

* Med. Rep. v. 7.

demic,* which, though vaguely called the plague, was probably the yellow fever. No diagnostic criteria have been transmitted to our times, by which this conjecture can be satisfactorily decided. With respect to the New England epidemic alluded to, the case is different; three of the most salient characteristics of yellow fever, viz., great fatality, yellowness of the skin, and hemorrhages, are enumerated distinctly. There is nothing improbable, in admitting this statement, fatal as it may be to the doctrine of imported contagion. The Indians, even the Southern Indians are, as I know from personal observation, sometimes the victims of yellow fever, when recently from rural situations. It is almost certain that the native Indians of the South, constantly resident in large towns, would be little, if at all, liable to this disease, while the same description of persons, in northern towns, would like the whites of the north, enjoy no such protection. The localities which develop yellow fever, are urban, not rural. Hunting and war, the elements of savage life, are but little favorable to a dense town population. Hence, savages seldom suffer from this disease. It cannot, however, be denied that very small towns are liable to suffer.

The infrequency of yellow fever among the savages of America, cannot be inferred from the silence of authors on the subject, with but few exceptions, for nearly two centuries. The difficulty of getting information from them in relation to their maladies, will, in a great measure, furnish a sufficient explanation. A few facts concerning this race may not be improper in this place, though, perhaps constituting a chronological digression.

In 1746, the Mohegan tribe was wasted by this malady, which began with pain in the head and back, followed by fever, and on the third or fourth day, with intense yellowness of the skin, black vomiting, and bleeding from the nose and mouth. This fever commenced in August, and ended with the approach of cold weather. Albany, in New York, was at the same time the theatre of a similar visitation, as was the Seneca nation of Indians the year before.† If the circumstances of savage life were equally favorable to the production of yellow fever, as those of civilized life, still, however, the difficulty of obtaining from hostile savages any authentic information in relation to it, will explain one of the causes which have clouded its early history among the nations of North and South America, together with those of the Islands washed by the Carribian Sea and the Gulf of Mexico.

Dr. Cartwright in his account of the epidemic yellow fever of Natchez, in the year 1823, says, "Five of the aboriginal inhabitants, belonging to the Choctaw tribe, came into the city during the prevalence of the epidemic, and afterwards encamped two miles from the city; four took the disease, three men and one squaw. They were most barbarously burnt;" [by themselves, with a curative view.] "The squaw was covered with large ulcers, produced by fire, from the pubis to the chin, and was writhing and groaning by the side of her grave, which the well Indian had dug to put her into, as he had prognosticated her death; but the men bore their pain in sullen silence and with savage fortitude—disdaining to disgrace themselves, as men and warriors, by imitating the

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* Webster. † Webster.

groans of the squaw, but applied to their own skins the lighted spunk, nor seemed to feel its corroding fire."

Columbus, in giving a new world to the whites, gave to them, indirectly, a new malady; though some learned doctors of our day translate certain expressions of Hippocrates, so as to make him describe yellow fever. A glance at the principal medical events which preceded the black vomit era, would show how carefully ancient authors had enumerated the most striking epidemics, and how improbable it is, that they would have wholly omitted to notice this disease, so remarkable in its external physiognomy, and so fatal in its progress. It is now nearly three thousand years since the first temple arose to the honor of Æsculapius. Four or five centuries subsequently, he was worshipped in Rome, where epidemics became very frequent and fatal. Brutus (514 B.C.) sent an ambassador to Delphos, on account of the plague, then prevalent. Epidemics prevailed in the years 451, 432, and in 396 B.C.; the latter gave rise to the ceremonies called the *Lectisternium* or funeral banquet to the gods. A ceremony frequently repeated on epidemic occasions, as in 345 B.C., for the fourth, and in 323 B.C., for the fifth time. A terrible plague, which continued for three years, began 293 B.C.; in consequence of this, ten ambassadors were sent from Rome to Epidauris. They brought back the god Æsculapius, under the figure of a serpent; which, however, afforded no protection; for 21 years after, the plague appeared again in the city. In the year 263 B.C., an epidemic wasted the Roman army, which was again ravaged, from the same cause, at the siege of Syracuse, 212 B.C. Seven years after, the city was again desolated; and a fifth *Lectisternium* was celebrated. The plague in 131 B.C., extended over many countries.

Greece, as well as Rome, was ravaged by epidemics: Homer opens his great poem by alluding to one that destroyed dogs, mules and men; another, 430 B.C., was most destructive at Athens. Thucydides, himself a personal sufferer from it, has given a minute description of its symptoms. Hippocrates was so successful in his treatment of it, that he was presented with a massive crown of gold, and the highest of public honors. He describes the epidemic as a physician; while Lucretius did the same as a poet. Athens suffered again in a similar manner, five years after. Near the commencement of the Christian era, Celsus, and in the next century, Galen, gave to the world their learned and voluminous works on medicine. In the sixth century, a plague was recorded as being universal. Smallpox (A.D. 565) was noticed as epidemic in France. In the tenth century, the learned work in the Arabian tongue, by Rhazes, appeared, on smallpox, and many other prevalent diseases. Avicenna followed, in the same language. Before the middle of the thirteenth century, medical schools had been organized at Montpelier and at Damascus; and the Parisian College of Surgery soon after. Scurvy and plica were described about this time.

In the fourteenth century, several systems, manuals and pandects of medicine appeared, together with the description of several new diseases; among the latter, St. Vitus' Dance, which was epidemic on the Rhine. The whooping cough prevailed several times in France and elsewhere, during the next century; accounts were given of the sweating-sickness, and the venereal. In the sixteenth century, inoculation, blenorrhagia, and syphilis, were described; while accounts were given of the sweating-sickness, scurvy, pleurisy, phrenitis, whooping cough, the Hungarian disease, the cereal convulsion, and the petechial fever, all in the epidemic form, and some new. This brief summary reaches nearly to the epoch of yellow fever in New England and the West Indies, and affords strong presumptive proof, that this disease could not have prevailed, without having attracted some attention from so many observing writers, for the long period of thirty centuries. The appearance and extension of yellow fever in Southern Europe, where its mortality has been unparalleled, took place at a period earlier than is generally supposed. Boisseau, a recent elementary writer on febrile diseases, designates the era of the general diffusion of yellow fever as late as 1684,—a period not sufficiently remote from our times, though anterior to its pretended importation from Siam.

According to Mr. Webster, the true yellow fever has been known in the British Islands, in the West Indies, from their first settlement.—Cromwell's forces were reduced by it, when Jamaica was taken in 1655; this, however, is a date too recent for its origin. Sir G. Blane, a contagionist, fixes the great epidemic era of yellow fever nearly half a century anterior to its alledged Asian importation. He enumerates as one of the most remarkable of yellow fever epidemics on record, that which prevailed in 1647, in Barbadoes*,—an Island, concerning which, a few general remarks are deemed, on several accounts, very important in a topographical and medical point of view.

1647. Barbadoes, (thirteen degrees north of the equator,) is twenty-one miles long, and one third less in breadth, consisting of a soft, calcarious rock, arising from the bosom of the sea, by gentle acclivities, to the centre, where the elevation is nearly two thousand feet. It is covered with a vegetable mould. Lind, in his work on climates, says, that this Island is "free from trees, underwood, and *Marshes.*" Wells afford good water, though there are no water-courses. The mercury ranges from 76 deg. to 82 deg., rarely to 88 deg. Here stagnant air, one of the alledged causes of yellow fever, has no existence, as the sea breezes and trade winds "operate unspent," and with great force, augmenting, sometimes to powerful hurricanes, which latter are often more destructive than yellow fever itself. In 1674, two hundred persons were killed by a hurricane; in 1780, by the official account, 4,326, and in 1831 about 3,000 more.

Lind quotes at some length from the latin treatise of a native of Barbadoes, Dr. Bruce, who wrote on yellow fever, and who says: "Febris putrida, apud Nostrates dicta flava, quoniam plerumque, sub finem morbi, cutis flavedine suffunditur, ab Hispanis vomino preto," [*vomito prieto?*] "et a Gallis Maladie de Siam vari nominatur. * * * In omni anni tempestate, sese offert hic morbus," etc.

Barbadoes was settled by the English in 1605, and was soon put in a high state of cultivation. In less than a century, it contained about 150,000 inhabitants, more than 500 to the square mile—a density exceeding that of nearly all other countries. In 1786, the population had declined to less than 80,000—a result, perhaps, more attributable to the

* Med. Logic.

exhaustion of the soil, than to the ravages of yellow fever: for this Island like most places to which yellow fever shows a marked geographical predilection, is, paradoxical as it may seem, extremely healthful, being little, if at all subject to other forms of epidemic fever. Lind, repeatedly calls it the most "pleasant and healthful" of the West Indian Islands. Towards the close of the eighteenth century, an English traveller, Sir W. Young, thus describes it: "The Island is dotted with houses as thickly as on the declivities in the neighborhood of London or Bristol, but with no woods; two or three straggling cocoas near each dwelling, were all the trees to be seen."

For two centuries, this Island has been the hot-bed of yellow fever, which Dr. Ferguson, Inspector general of hospitals in the West Indies, declares to be of the very worst form.* If the logic of contagionists is inconclusive, that of miasmatists is evidently erroneous. No one can deny that temperature, humidity, and the like, greatly influence human health. It does not hence follow that the swamps of Louisiana emit any mystical miasm, (any more than so much rose water,) which is the cause of the yellow fever: indeed the most swampy portions of the State are freest from this disease. When swamps cannot be found, as at Barbadoes, Vera Cruz, Havana, not to name scores of towns in Spain, including that dry mountain of rock, Gibraltar, still the advocates of this doctrine would have us to believe, in opposition to the evidence of our senses, that as the disease is present so is its assumed cause, marsh exhalation. This extensive subject can only be glanced at in this place. Bancroft† attempts to account for the great empidemic of 1647, not by the agency of swamps, but by stating, that, as Barbadoes had been then settled but little more then than twenty years, and "that so little of it was at that time cleared and cultivated, that *dry* weather, assisted by great heat, was best suited to the production of noxious miasma; contrary to what has been the case at Barbadoes since it attained its highest state of cultivation many years ago."—He totally forgets, that as the cultivation increased, so did the yellow fever for two centuries.

It is worthy of observation, that intermittents are not found in this Island. Dr. Gilpin, principal medical officer, at Gibraltar, never knew of but one intermittent in that great fortress of yellow fever; yet, miasmatists persist in referring these two maladies to the same cause.—This, however, is a subject which will be investigated elsewhere, and is merely alluded to here, in connection with the topography of the island under consideration.

Sufficient number of authorities exist, to prove that a great yellow fever epidemic prevailed in this island, in the year 1647, already mentioned. These authors having been contemporaneous with each other, with the events which they have recorded, agreeing in their relation of the same, and having been without any interested motive to deceive themselves or others, their testimony, so far as it goes, is entitled to the utmost credit. Their history of symptoms is necessarily imperfect, representing in this respect, the science of medicine at that dark period. Dr. R. Vines, a planter and physician, describes this epidemic, at Barbadoes, as "a plague, very infectious, first attacking the ablest men, of

* Med. Chir. Trans. † On Yel. Fev.

the greatest bodily ability." Ligon, whose history of Barbadoes appeared ten years after the epidemic, arrived in that Island early in September. He says, that before the close of that month, the living were hardly able to bury the dead.* He gives a very remarkable trait of yellow fever, which two centuries have confirmed, and one which is, in the histories of febrile maladies, very peculiar; namely, that "*for one woman that died, there were ten men.*" The mortality was five or six thousand in this small island and infant colony!

Father Du Tertre, who lived in the West Indies, in 1635, wrote an account of an epidemic, which prevailed in St. Christopher, before that already mentioned in Barbadoes, and which he called, "*la peste*," and described as being accompanied with headache, constant vomiting, and death in three days. It destroyed, in a few months, one-third of the entire population of that island.

The assumption, that yellow fever is the product of marsh poison, is not only unsupported by its topographical affinities, but is irreconcileable to its modern appearance.

Sterne has somewhere asserted, that our earth is the vilest and dirtiest, and, therefore, the most miasmatic, it may be supposed of all the planets, being made wholly out of the refuse clippings of the rest. The geological account of its primary condition, is not very flattering in a sanitory point of view; since, upon the theory of marsh exhalation, yellow fever must have been as old as the creation; and withal, infinitely more common and fatal in ancient than in modern times. The land as it arose from the water, boggy, fenny and marshy, shot forth gigantic ferns, and rank herbage of many kinds, which, judging from their fossil remains, now constituting the immense coal fields of our globe, were more like our present trees, in luxuriance, than the aquatic plants of our era. Mountains upheaved by volcanic action, by enclosing vast basins, into which alluvial matter, was constantly descending, for countless ages, formed vast swampy areas, from which a concentrated malaria, equaling that which probably followed the Noahic flood, must have caused yellow fever epidemics, sufficient to have destroyed, at least, the males of the Caucasian race, or a great proportion of them, though many white females, and nearly all the negroes, might have escaped then, as now.

To say nothing of the valley of the Mississippi, its delta alone, from Cape Girardeau to its mouth forms, according to Mr. Forshey, 31,200 square miles, having a mean width of 418 miles, and a length of 600; the depth of the alluvion having a mean of 50 feet, required 13,684 years for its formation, by deposits from the river.† During this long period it must have been excessively swampy. The condition in which De Soto found it, in 1539, was of course worse than at present;‡ yet,

* Bancroft and Webster. † Newspapers, 1846.

‡ So little has the Louisiana coast, along the Gulf, changed, since 1685, when La Salle committed the fatal mistake of landing—not at the mouth of the Mississippi, as he intended—but west of it, that Mr. Darby has been able to identify the exact place of his debarkation, between the Vermillion and the Mermentau rivers, from the minute description, at that period, of this shore, with its extremely uniform, low, shining banks of sand; all of which, he declares, he took some pains to ascertain.—*Descrip. La.*, 1817.

the yellow fever did not appear in New Orleans until a period very recent, when compared with the Northern States and the West Indies, which had been previously desolated by many epidemics, for nearly a century and a half before it visited swampy Louisiana. The valley of the Mississippi, not the little fens of Old England, nor the mill-ponds of New England, nor the Pontine marshes, or rather the Pontine pasture lands of the Eternal City, offers the best and the most extensive field for testing every possible malarial question which the most imaginative miasmatist has ever been able to conceive or propound since 1717, when Lancisi published his *De Noxiis Paludum Effluviis*. In the entire valley, an area of nearly the third of a million of square miles, since its discovery, the aggregate mortality from yellow fever to the present time, has not equalled the half of that during a single year in Old Spain, where it has amounted by the official reports to one hundred and twenty thousand, in places free from swamps, and even among mountains.

Omitting the palpable anachronism of the alledged Asian importation, there seems to be as little plausibility as probability, in seeking the cause of a malady in a country where the malady itself is unknown. It may be patriotic, however, to defend our country from imputed contagion, right or wrong, and to throw the blame of yellow fever upon our antipodes, though, without better evidence than has yet been adduced, this process is far from being altogether scientific. Charge the Siamese with originating the contagion,—they will charge the Japanese, or some other people. Thus, contagion, like the wandering Jew, will be driven "from Indus to the Pole," and from the pole to the burning Zone. March! March! will be the universal cry. It is affirmed poetically, no doubt, that the great pendulum of the clock of Eternity in its vibrations utters but two words EVER! NEVER! So it is with this endless question of contagion—*Ever* assuming, *Never* proving its conclusions. Doomed to march! march! it crosses the Atlantic, doubles the Cape of Good Hope, traverses the Indian Ocean, passes through the Straits of Malacca—a distance of twenty thousand miles, to get a product, which a crowd of facts show to be indigenous to certain places of the western hemisphere, or to certain local climates, acting during the hot season, and for only a few years, on unassimilated constitutions not long resident in cities or villages.

The cause, which in one locality, produces gôitre; in another, elephantiasis; in a third, Plica Polonica; in a fourth, the yaws; in a fifth, leprosy; in the sixth, yellow fever—is quite unknown, though, many pretended explanations have been given. As some places produce wheat, or bananas; live oaks, or cypresses; hanging moss, or palmettos; mosquitoes, or alligators,—so may yellow fever be produced, without our being able to show the cause of the one or the other class of effects.

That the essential cause of yellow fever will ever be discovered, or, being discovered, will be controlled, or prevented by human art is altogether improbable. Its mysterious cycles, culminate, decline, and reappear. Charleston, desolated at the close of the seventeenth century, was exempt in the first quarter, but a sufferer in the second quarter of the eighteenth—nearly half a century of exemption followed again—(a period much longer than that which now cheers the cities of New York, Philadelphia, Boston, and Bâltimore, with hope that yellow fever has taken its leave of them forever.) But the last decennial period of the past century, and the first of the present, relumed the flames of the epidemic in Charleston, where they had smouldered so long, and in which they still continue to break out almost annually. Charleston suffered nearly a century in advance of New Orleans, and is still as great a sufferer as the latter.

It is to be lamented that the topographical ameliorations which have been pushed forward with a celerity characteristic of New Orleans, have not diminished or modified the yellow fever of the place, though, strange to say, this opinion is so unwelcome to all, and so humiliating to theorists, that it is constantly repudiated, while "the long funerals which blackened" all the streets no less than five different seasons from 1837 to 1843, inclusive, are still but too fresh in recollections of a hundred thousand people. As gloomy prognostications are as useless as unwarrantable, let us hope that New Orleans has entered upon a non-epidemic cycle, not only for a half a century, as once happened to Charleston, but forever. The yellow fever prophets and prophetesses, have not, hitherto, been able to read the Sibylline leaves of its etiology, so that epidemics can be certainly known until after their occurrence.

The rejection of our error never justifies the adoption of another, unless it be on the principle of Dean Swift, that all happiness consists in being *well* deceived, or on that of Fontenelle which makes philosophy itself consist in much curiosity and very bad eyes. To explain the cause of yellow fever seems to be regarded, not as supererogation, but as a paramount duty enjoined in the medical decalogue. It is scarcely reckoned a sin against logic, to resort to almost any etiological *non-sequitur*, for this purpose. The cause is assumed; when the effect appears without the cause, either nothing is said of the absence, or some other cause, condition or circumstance is assumed as a sufficient substitute for the truant; when the assumed cause exists in the greatest concentration, without the presence of any effect whatever, some assumed contingency is supposed to counteract its power: as a *ruse de guerre*, a mere incident, will, however, always answer, and can always be found:—dry, or wet; hilly, or marshy; rocks, or mud; vegetable, or animal matter; stagnant air, or storms; heat, or cold; infected ships, or a "pair of trowsers;" immigrants, or insects; gases, or graveyards; absolute, or conditional contagion and malaria, both foreign and domestic. This sort of ratiocination was not, altogether unknown to Joe Miller:—"The Frenchman, who observed that an Englishman recovered from a fever after eating a red herring, administered one to the first of his fellow-countrymen whom he found laboring under that disease, and having found that it killed him, noted in his tablet that a red herring cures an Englishman of a fever, but kills a Frenchman."—"Rotten coffee" is found in Philadelphia; three barrels of spoiled mackarel, sour-crout, sour porter, rotten corn, two thousand pounds of bad bacon, with the heads and entrails of some catfish, are found in a certain town in the State of Mississippi; and a "trash wharf," in the city of Augusta, Georgia, etc.—these are gravely substituted for contagion, for "a dirty pair of trowsers," (from Martinico,) in which, according to the most "potent" authorities of a New England city, a most deadly epidemic was imported. Have not "dead fish," "trash," "dirty trowsers," and the like, abounded since the foundation of the world, in all climates and places? Does not the law of continuity in the cause, ["trash,"] require a corresponding continuity in the effect, [yellow fever?] Were yellow fever producible by a few pounds of "rotten coffee," would not the incendiary, when sated with conflagrations, amuse himself, by way of variety, by kindling up an occasional epidemic, especially, if he were himself acclimated, and wished to profit by the commercial speculations, always incidental to such an event, whether his stock in trade consisted of cotton, sugar, red herrings, drugs, or coffins.

In denying, or rather doubting that yellow fever is produced by any emanation from the sick, or from marshes, nothing more is intended than that such emanation is unsupported by any evidence worthy of belief.—Were a subterranean, a solar, a lunar, or a stellar theory substituted, it might be true, or not; the *onus probandi* belongs to the proposer of such doctrines.

An alledged cause ought to be invariably followed by its effects: for example, if the doctrine of imported contagion be adopted, then the town of the Balize, at the mouth of the Mississippi, is the most exposed point on the globe, as the vessels supposed to be infected, are often detained there, for pilotage, and towage, and from getting fast on the bar; adopt miasma, as the cause, and yellow fever ought to be eternal, as this is, of all towns, the most exposed to marsh exhalation, and yet, yellow fever is unknown to the residents.

The *morale* of contagion and miasma is very dissimilar: the one is anti-social, repulsive, dooming its victims to cheerless insulation—to withering neglect. It cries—stand off!—perish in ships!—perish in lazarettos!—perish in hovels!—"let the dead bury the dead!" The other, like the good Samaritan, fearing nothing from contagion, offers not only personal attendance, but sympathy in the hour of need. Besides, the utilitarianism of this latter doctrine stands forth, actualized in the improving, draining, cleansing and embellishing of both town and country localities. Yet in the sciences, the utility of an error cannot plead its justification. Sciolism may declaim against the pulling down of existing systems. It seems to abhor a vacuum. But the massive columns of truth will never arise until the foundation shall be clear.

It is hoped that this paper is not imbued with such a fault finding spirit, as to justify the question which Göthe puts in the mouth of a character in his Faust:—"*Seems nothing ever right to you on earth?*"

IX.—*Observations upon Congestive Fever.* By T. A. Cooke, M.D., of Opelousas, Louisiana.

The disease called congestive fever, according to the reports of physicians, prevails more or less extensively every year in the South and

South-Western States; but from frequent conversations with most respectable practitioners, from different regions of the country, the prevalence of this endemic appears to me not so extensive as public belief credits. It is very true, that in the Middle-Western States particularly, some sections of the country are occasionally visited with the disease, quite in the form of an epidemic; but as a general rule, according to my experience, which has been confirmed by others who have enjoyed a better opportunity than myself for observation, it does not bear to our common autumnal bilious fevers, a ratio greater than 2 per cent. For the period of eleven years, during which I have practised medicine in Opelousas and the adjacent country, it has not been my lot to observe, on an average, more than about 12 cases a year; and I have been somewhat astonished to hear of the frequency with which this disease is met with, by physicians residing in sections of the country no more favorable to its production than that in which I live.

I now proceed to describe the affection as it has presented itself in my practice. Its probable causes, its pathology and treatment will be considered. I do not propose to write a regular systematic essay on the subject, and therefore shall be excused for the absence of order and method in my remarks.

That condition of the human body, to which the term congestive fever, *peste froide*, cold plague, is commonly applied, comes on under different circumstances, sufficiently well marked in their character, to justify distinct divisions of the disease.

The form of the disease generally noticed, may be termed intermittent. The patient experiences a chill or ague, similar to the chill of common fever. The reaction which ensues may or may not be strong; and soon terminates in perspiration. On the next day, or more generally on the second day, another chill; sometimes more severe, sometimes milder than the first, comes on; the pulse will be found small and threadlike; occasionally much nausea exists, and generally speaking there is constipation of the bowels. From this chill, very frequently in this form of congestive fever, there is no reaction—never the full condition constituting fever. The hands and feet continue cold, presently this coldness invades the whole length of the extremities—a cold clammy sweat ensues, and with the exception of the epigastric region and along the neck, the whole body is of cadaverous coldness to the touch. I wish it to be observed that the coldness of the extremities and sometimes of the whole periphery of the body, occurs frequently with a dry skin, that is without any manifest perspiration whatever. Such cases, though rare, I have met with. The perspiration is not the cause of the coldness, but may, when it occurs, as it most always does, when the part perspiring is exposed to the air, increase the sensation of coldness to the touch. As the congestive stage progresses, numerous other distressing symptoms come on, as restlessness, increasing to the extent of the most distressing jactitation. Nausea, more or less constant, an inability in some cases to retain anything on the stomach; so great, that sometimes calomel will be vomited after it has been changed by the action of the fluids into a dark oxidated matter. The matter vomited at first, may contain some bile mixed with injesta and the secretions of the stomach; but frequently consists of what the patient has taken. The bowels may be constipated in this kind of an attack of congestive fever ; but I have generally found them laxative. The discharges, whether spontaneous or brought about by medicine, vary in their nature ; sometimes of an earthy character and whitish appearance, in other cases of a greenish black color ; and again, of a muco-sanguinolent hue. I have never seen vomiting of blood, but frequently excessive hemorrhages from the bowels. There is never, that I am aware of, any noticeable swelling of the abdomen from wind or gas, nor that deep depression of its parietes often seen in the low typhoid grades of autumnal fever. The thirst is generally intense, but not invariably so—being sometimes so slight as to elicit no notice or complaint. Biliary secretions in a vast majority of the cases is arrested ; and its re-appearance, as far as the subsequent evacuations would seem to show, has been simultaneous with returning warmth and pulse.

It frequently happens, that in the beginning, a dose of emetic is prescribed, or an active drastic purgative, which is followed by that condition of the system to which we give the name of congestion. To the same symptoms described above, is frequently added in the place of those above mentioned, rice water evacuations. Then the *tout ensemble* of symptoms are such as are characteristic of the malignant Asiatic cholera. The cause of the disease seems to have increased in fearful energy, or the medicine has developed its fearful effects to a terrible degree ; all the extremities, and sometimes the whole body is of marble coldness, with more or less perspiration ; the patient sighs often, without any difficulty of breathing, for the air penetrates freely the cells of the lungs, the pulse often ceases to beat, the tongue appears dark, and to the touch as cold as the skin, the hands and feet become shrivelled, as if they had been steeped in a warm astringent liquid ; the alæ nasi contract, the expression of the face is haggard, the lamp of life appears to flicker, death seems ready to encircle the patient in its very grasp. Appalling as is this state of things, it is nevertheless far from being desperate ; for patients do and will recover under such circumstances, which apparently preclude all hopes of recovery. One of the favorable circumstances in this affection, is the rapidity with which this collapse has taken place, when it has come on suddenly ; whether or not propelled or caused by medicine ;—the good effect of stimuli is more apparent than when the state of congestion has been gradually stealing on. This fact is, I believe, one in which all my medical associates concur. Though frequently witnessed, it nevertheless suffers many exceptions. It has certainly appeared to me a prominent fact. Another form of this disease appears to be, or to grow out of, a remittent affection. There is from the beginning little or no chill—perhaps the feet and hands will be found cool—and there is seen nothing of the sort afterwards. The fever, with which the disease is ushered in, is not intense ; it remits and exacerbates for a day or two, the pulse being sometimes small and frequent ,or full, but readily yielding to pressure. The evacuations are of a sero-sanguineous character ; bile being absent. The patient is very much alarmed ; he complains of great inward heat ; is thirsty, and very much incommoded by the frequency of his alvine discharges. At lentgh he complains of weakness ; his extremities grow cold ; mustard plasters and stimulant frictions avail nothing in re-establishing the heat.

The coldness continues, his pulse becomes thready, and there the whole coterie of the symptoms of congestive fever sets in.

I remember well, one case of pure congestion, which was never preceded by increased heat of skin or febrile pulse. Such cases, I believe are not uncommon. There is here no reaction ; the feeling of lassitude continues until the patient takes to his bed. The circulation grows weaker and weaker ; the temperature of the body diminishes. The extremities grow cold ; common purgative medicine administered, either proves impotent, or hypercatharthis will ensue. In the case alluded to, the patient was restless in the extreme, constantly changing from the recumbent to the erect position, and walking about until fatigue would compel him to rest in his bed. His bowels were excessively constipated. Mustard plasters, blisters and mild mercurial purges were administered without any noticeable improvement ; at length, broken doses of salts and magnesia were given, with apparently the happiest effect—his bowels were freely opened, but he was still cold, and all the time complaining of excessive heat. He would suffer no covering over him. Without my permission, he determined on taking the cold bath, to relieve him from his imaginary burning heat. No re-action resulted—on the contrary, the blood seemed to concentrate itself still more within—so far at least, as the coldness of his body growing constantly greater, and the apparent paralysis of all external capillary action would indicate. Presently he commenced to experience difficulty of respiration, retaining all the time his mind in a state of perfect integrity. This difficulty in breathing continued to increase ; in the course of a few hours, he had to be propped up in bed. Then succeeded a wild incoherent talking, manifesting the implication of the brain—finally stupor and coma ensued, and he speedily died.

The progress of the case described is no fanciful sketch, I have witnessed similar conditions of things, in very many cases ; and in none is the impotency of art more forcibly demonstrated—certainly none is more calculated to excite the most painful sensibilities of our nature than the procrastinated agony of death which we are called to witness on such occurrences, without a weapon to arrest the ravages of our universal enemy. In these cases we mark the progress of the disease with our eyes. We are enabled, by including their organs whose functions are healthily performed, to localize the congestion—and then to count every step it takes in its tardy march—seizing first upon this organ and then upon another, until life is finally extinguished.

The duration of the period of congestion varies from a few hours to several days. In the intermittent form, reaction will often be effected in a few hours,—the patient will be again seized with a chill, symptoms of congestion will return but with less menacing intensity than in the first instance, and convalescence be rapid. But in these cases without the aid of appropriate medicines a second paroxysm of congestion is very apt to prove fatal and even in the course of a few hours. In 1835, a patient of the name of Wright, a carpenter by trade, of a sanguinious temperament and robust health, but much addicted to whiskey, was treated jointly by my friend Doctor Robert Smith, of this Parish, and myself. When called to him, he presented all the symptoms of the congestive fever. He complained of great thirst, his passages were of an inky hue and very small, and were so represented to have been from the beginning. He was occasionally delirious, as cold as ice and almost pulseless. Several stout men had to keep watch over him,—for even in his senses he could not control his extreme restlessness, and would frequently jump up, and after some exertion his strength failing him, he would fall to the floor. This man continued in this condition for six days, during the whole of which period there was no perceptible reaction. His appearance was ghastly; his voice hoarse and gravelike. In the evening, for several days, without any increase in the strength of the pulse or return of warmth, he was subject to the delirium. Sleep seemed to have departed from his eyes, at length, about the seventh day, dating from the commencement of the congestive stage, there was a manifest amendment; and convalescence was soon confirmed.

In 1842, I was called early one morning to see a young girl, belonging to one of our most respectable families. The night before the last, she had had a fever, but the day succeeding she did not appear to the family so very sick as to require medical aid. The night preceding my visit she had a return of the fever—was very restless, and could not sleep. On my arrival, I found her breathing frequently interrupted with deep drawn sighs—an uncontrollable restlessness—extremities cold—pulse rather small, soft and yielding—eyes filmy—no pain of a severe nature, but a general feeling of prostration and sickness; stomach very irritable—frequent dijections resembling rice water, with flakes of mucus. The symptoms continued rapidly to increase until she presented the most ghastly picture of a living being I ever witnessed. Twenty hours after my first visit, she presented the identical appearance of one in the last stage of malignant cholera. The dependent parts of the body were livid from an apparent stagnation of the blood. This very unfavorable symptom existed to an extreme degree. Twelve hours more elapsed before any change occurred; which fortunately was an improvement, and in some two or three days she was completely convalescent. I have mentioned these cases as examples of the severity of the disease and of its possible duration. As a general rule, the physician should be inspired with increased hopes by the continuance of the disease, provided no new organs become attacked. It has been my lot to be called to cases of this disease, after the attending physician, unacquainted with the miraculous recoveries, which sometimes occur, had condemned his patient, and ceased to administer any remedial agents; and to our great gratification, the recuperative energies of nature have been aroused; the almost stagnant blood has been put in motion; and confirmed convalescence in a brief space of time, effected. The period of the year, which appears amongst us most favorable to the production of the disease, may be comprised in the months of July and August. Certainly according to my observation it is more frequently met with in the beginning of the sickly season, than subsequently; during all autumn, and sometimes after frost, it is met with.

No subject in medicine is involved in greater obscurity than the Etiology of diseases. In the production of autumnal fevers the effluvia from vegetable mattter, called miasm, is considered the most potent of all causes. Chemical analysis has been incompetent to detect its nature. Air collected on the border of the Pontine marshes, in the infected atmosphere of a room crowded with patients, and on the tops of mountains, furnish the chemist with the same results. The conditions in our country, favorable to the production of the poison, are as little known as the nature of the poison itself. The circumstances, which reason would suggest as most favorable to its production, are found by observation to exert no influence in its propagation, and very frequently would appear to militate against it, instead of in its favor. We have seen our country enjoying one year extreme good health, notwithstanding the long continuance of the most intense heat, superabundance of rain, and easterly winds;—another year under similar circumstances, it has been ravaged by disease;—other years, when anticipating good health in consequence of a moderate temperature, a uniform season, and moderate rains, without prevailing East winds, we have also had to sustain the most extensive visitation of disease. No one in this section of the connty, resting on experience or observation, can designate any infallible circumstances as productive, or promotive of sickness. To say that autumnal fevers are produced by marsh miasm, is an assertion sustained only by the facts that these sections of country, exposed to the humidity and exhalations of marshes, are subject to such fevers, and that other sections differently situated are not subject to such fevers. May not there be causes which tend only to disturb the normal constitution of the atmosphere, besides miasmatic emanations, which would produce the same results attributed to such emanations. The subject appears to me, as already stated, buried in obscurity. There are more things in nature, than is dreamt of in our philosophy. Be the cause what it may, it is presumed by some to effect the nervous system; by others, the sanguineous system is thought to be the first attacked. One thing is evident, and I have never failed to verify it; and that is, that in the beginning of our fall fevers, when blood is drawn before any fever has declared itself, it is in a vast majority of cases, found to be of a much darker hue, than is its color in health. Its power of coagulation is impaired, a dark soft clot, with a little serum being the result. A few weeks ago, while attending in this neighborhood, Mr. P. R.——a gentleman occasionally subject to very severe inflammatory attacks, I observed his wife looking badly; on enquiry she expressed herself unwell, for some two or three days, chiefly with the head-ache, an affection to which she was very subject, and requested me to bleed her. The blood was drawn from a large orifice, flowed in full uninterrupted stream into a soup-plate—it was really black in color—in the course of half an hour, formed a soft grumous mass with little or no serum. I observed to her friends that she would fall sick inevitably; especially as her attendance on her sick husband prevented her from taking efficient medicines, for the purpose of stimulating the liver, and freely, evacuating the primæ viæ. Accordingly she fell sick, and was confined to her bed for a week. On the same occasion, blood drawn from Mr. R., suffering with symptoms indicative of inflammation, was much brighter in color than common venous blood, and separated into a hard reddish coagulum, and a considerable quantity of serum. This circumstance is mentioned simply as corroborated of what Andral has fully established, viz: that in the phegmasiæ there is an excess; in the pyrexiæ a diminution of the blood fibrine. No doubt on my mind exists of cases of the latter class, running a somewhat protracted course, and finally terminating fatally, not only without owing its origin to inflammation, but without even the supervention of that condition. In general our diseases result from a poison existing in an impalpable form in the atmosphere; or from the predominance of one of the elements of air, affecting or modifying the nervous system, or rather, in my opinion, sanguification; or from the constant vicissitudes of temperature, and the ever varying conditions of moisture, preceded by noticeable indispositions—by appreciable shades of a departure from health increasing in intensity, until the vis medicatrix naturæ is aroused, and fever, not only the consequence of the morbific causes, but one of nature's remedies, perhaps more important than any we possess, is established. It is not denied that the cause may be concentrated and active, so as to produce immediate indisposition and very speedily fever. But in the large majority of cases, want of appetite, nausea in the morning, sensations of uneasiness, or of pain in the head—constipation or diarrhœa—light colored evacuation—irregular action of the skin, and more or less derangement in quantity, or nature of most of, if not of all, the secretions,—symptoms which more or less precede the paroxysm of the fever,—certainly show conclusively the slow operation of the cause, whatever it may be. Such likewise, as far as I have observed, is particularly the case with the congestive fevers of Western Louisiana. All the symptoms previously to the congestive stage, and also we presume the causes of this disease, and of remittent, continued, and intermittent bilious fevers, are the same. There may, I believe, be cases of this disease in which the symptoms, preceding the paroxysm of excitement, so gradually occur, that no one can predict that in a few hours there would ensue the state of congestion.

What is the nature of congestive fever. All who have observed the disease and examined the blood, agree that it presents in this affection as great a departure from its healthy appearance as in any other disease;—that there is a similarity in its morbid character, in this and other malignant affections of the South in the fall of the year. I cannot doubt that the primary cause, whatever it may be, does exert a most malign influence on the organization of this fluid; that its alteration is the first link in the extended chain of phenomena, which we witness; and that the nervous system particularly, and all the other organs, are secondarily implicated. In all instances, I believe, previously to the breaking out of the disease, the blood from the first moment of the slightest change in the feelings for the worst, will be found darker, and imperfectly coagulating. For the regular and healthy performance of the functions of the organs, a blood is required, free from the presence of morbid elements, or from the existence in excess of one or more of its constituents. The symptoms of congestive fever lead most strongly to the inference that its cause exerts upon the heart, arteries, and nervous system, debilitating influences. In the congestive stage, there is an apparent prostration of all the energies of this system. This condition of things may endure for hours, for days, nay for a week—suddenly there is a change for the better, and the miracle of a resurrection is attested. The almost entire surface of the body is deathly cold—the extremities are shrivelled—except on the surface of the body, and in portions of the alimentary canal, scarcely is there an effusion or secretion; the artery may be punctured at the wrist, and blood will exude only by drops,—add to this the preservation of the mental faculties, and the capacity of full inspiration, and I will ask what has become of the blood which some hours or days ago coursed in a full and healthy current through every organ of the individual, of a large and robust frame, and sanguineous temperament. It seems to me that there can be but one answer—and that is, that under these circumstances, the three or four gallons of blood which ceases to circulate, has settled in the organs of the abdomen. In the liver and its appendages, in the mesentery and bowels, and the other abdominal organs, the blood must have accumulated or been congested. It is to be presumed, that post-mortem examinations would throw much satisfactory light on this disease. Certainly, but few if any autopsies have ever been made; and if made, they are not referred to by writers on the subject. I have, on more than one occasion sought to make them, but been refused permission by the unenlightened friends of the dead. Last summer, 12 months, I witnessed a partial examination of an individual, said to have died of this fever. A very intelligent gentleman, who watched the progress of the case, stated it to me pretty much as follows: The deceased had been complaining for some days—at length he was seized with a paroxysm of fever, which did not last long, but the patient still complained very much, and was very cold. His attending physician, I have reason to believe, was not at all conversant with the disease, and contented himself with an expectant mode of treatment. About 24 hours after the first paroxysm, some more fever or rather increased restlessness occurred, but he soon became very cold, and continued to live in this state, without delirium or dyspnœa, for about 12 hours. It was in his opinion, a pure case of the cold plague, and I have no doubt of the fact myself. I place full reliance in the opinion of the individual in question. I was invited to witness the post-mortem examination which, the weather being very warm, was commenced six hours after death. Not having witnessed the course of the disease, and having not a minute to spare, I did not wait longer than to examine the chief organs of the abdomen; and this even very superficially. The subject was a large robust body, of the bilious temperament. That which attracted our attention, was the presence of blood, as if injected into every organ of the abdomen. The liver was of a blackish red appearance, and when cut into without pressure, the blood poured from its substance. The peritoneal membrane appeared thoroughly injected—and so, also, particularly the mesentery and coats of the bowels. To the eye, it seemed that all the blood in the body must have been accumulated within the abdomen. After a few incisions, there was such an excessive amount of liquid black blood, in the abdomen, as to render further examination of its organs exceedingly difficult and disagreeable. Another physician and myself were both satisfied that the individual had died of congestion of the blood in the organs of the abdomen.

Congestive is unfortunately a term too loosely applied to the fevers of our country. Some physicians assert, that all the cases are of this order, whether there exists sthenia or asthenia—high continued excitement or marked debility. I can well conceive, that the disease may have been preceded by a continued fever of a day or two duration—that the forces of life on the cessation of fever sink, and that temporary or fatal congestions or accumulation of blood may centre in certain organs. In all the cases of this fever which I have seen previously to the collapse stage, there has been something in the general appearance of the patient, in his peculiar restlessness, his weak pulse, or one easily depressed, and in the coldness of the extremities, or of the fingers, toes, nose and ears, which pointed to debility.

The disease is not unlike all others in presenting different grades of intensity. All the symptoms in its severe form are appalling, and without a knowledge of the extraordinary recoveries which so frequently occur, would almost deter us from resorting to remedies for relief. The continued absence of bile, spasms, arrest of urinary secretions, and particularly the involvement of the organs of more than one cavity, are most unfavorable symptoms. The head and lungs being free from congestion, I have never known a case in which recovery was not rapid and certain so soon as the liver acted freely.

The phenomenon the most curious, which we observe in this malady, is the excessive coldness of the skin. It has been observed, that this peculiar condition has been seen unaccompanied by a degiee of perspiration sensible to the touch. That this coldness is not dependent on the evaporation of sweat is proved not only by the fact above stated, which has been over and over observed; but also by the fact, that however you cover the cold body, and apply rubifacients, and prevent evaporation, you will not succeed in restoring the warmth; by the fact that this coldness always precedes the passive transpiration which ensues; and also by the singular circumstance of the coldness of the breath, and frequently deathlike coldness of the tongue.

It is evident that one of the most remarkable phenomena of the disease, is the abolition, to an immense extent, of the function of calorification, dependent, undoubtedly, upon imperfect respiration, debility of the heart, and a complete paralysis of the nutritive or molecular actions between the solids and fluids. It is this latter circumstance alone, which affords to my mind an explanation of the astonishing fact, of the possibility of the system being involved in the cold embrace of this terrible disease for so long a period as 6 days, and then recover its healthy condition. The state of congestion is sometimes almost instantaneous in its attack. The whole system—some portions of which are scarcely supplied, some overcharged with blood—is thrown into a state of absolute torpor. The organs which execute the functions of waste and supply are paralysed. No destructive movements are going on—the small portion of arterialized blood is attracted to the brain. The patient remains in a horrible condition, vacillating between life and death, until at length, the never dying tendency of the natural energies to a restoration of health preponderates over the enemy—and health is restored.

In order to avoid misapprehension, the term congestive should be applied only to those cases which contra-distinguished from inflammation, present from the beginning, evidences of vital depression, and when fully declared, an entire absence of everything like inflammation,—the symptoms which mark the pure congestive or algid fever, a disease termed by many the cold plague. Its duration, the speedy restoration to health, under circumstances, which would, in many instances, throw the idea of inflammation, whether as cause or effect, entirely out of the question—the fatal effects of sanguineous depletion—the good effects of cordials and stimulants, all announce in this disease a condition of things diametrically opposite to inflammation. The pure congestive fever belongs, in my opinion, to the pyrexiæ, of presumed miasmatic origin; or in other words, I believe, the causes of common bilious fevers and this disease the same, that they act only with different degrees of intensity, or are followed by different effects, according to individual susceptibilities or idiosyncrasies.

The endless varieties of these fevers seem to me inexplicable, except on the last supposition, to which must be added, as a most important modifying circumstance, the incidental additions of local inflammations.

The derangement of the nervous system in all the pyrexiæ is too manifest to escape the attention of the most superficial observer, and whether it be as some believe a primary, or as others, in my opinion more correctly, consider a secondary link in the chain of morbid events, constituting the disease, it deservedly merits the utmost attention, not only as an ingredient of the disease itself, but as involving organs, perhaps, of co-equal influence with the blood itself, over the functions of each and every other organ.

In congestive fever, the supervention of an inflammation in some organ or organs may ensue, but under its overwhelming influence, it is announced by none of its usual symptoms; and if manifested, would rarely modify the treatment that would be adopted. The fact, of the occurrence of this disease late in the fall or winter season, is no satisfactory evidence of its originating from causes differing in their nature from those which produce the common fevers; for the latter are frequently met with in this warm climate, at all seasons.

Whites and blacks, old and young, are subject to this disease. I have not often seen it in subjects who had passed their 40th year. With regard to its mortality, age seems to exercise but little influence. I am not disposed to consider intemperance as a predisposing cause of the disease; nor do I believe it more fatal in individuals addicted to much excess than amongst the sober.

The existence of fever appears to me an evidence of the absence of passive congestion. In those cases of the genuine congestive fever, in which much marked febrile excitement preceded the stage of collapse, the excitement incompatible with congestion, has been but an impotent effect of nature to prevent the condition which has ensued. I have already stated, that according to my observation, during this very period of excitement, there will, on an attentive examination, be found most always evidences of tendency to the congestive stage. The exceptions, to the existence of such evidences, I believe are rare.

In fully developed cases of this disease, in all cases of true congestive fever, certain departments of the nervous system are particularly deeply affected, and being a diffused or general tissue, the integrity of which is necessary to the healthy operations of all the functions, though not the first, it becomes a constitutional element of the disease; perhaps, more serious than any other. Intellect, respiration, locomotion and sensation are rarely, if ever so much affected, as would previously be inferred would be the case, from a consideration of the appalling condition of the system. It is often astonishing, the activity—the intensity, with which these faculties will continue to act; often even when these organs seem almost deprived of arterialized blood. It is the entire paralysis of the nutritive actions which point to the ganglionic system of nerves, as that department of these organs most profoundly smitten in this disease.

In the common acceptation of that term, congestion, it will be seen from what has preceded, that it is, at least, in my opinion, in many instances misapplied. An individual is seized, for example, with any of the fevers termed malignant, as the typhus or scarlatina, in the progress of which there will ensue local deragements, such as engorgement of the lungs, of some portions of the brain, or of some other organ; indicated by the particular symptoms of the case. To the other symptoms of the disease this local affection is superadded; and it is a common thing, perhaps proper, for physicians to speak of such complication as congestive, while in all other respects, there is little or no modification of the symptoms. The skin, for example, may continue equally hot, the pulse may even augment in force, and yet the disease is termed typhus or scarlatina, with pulmonary or cerebral congestion. In what is considered, in this section of the country, pure congestive fever, we behold an assemblage of symptoms, a picture of disease, than which nothing in the form of a morbid condition contrasts more forcibly with the specimens of malignant affections above pressented. I might here cite an infinite number of diseases presenting even a highly excited state of the cutaneous capillary circulation, which are described as accompanied with local congestion.

The term malignant is applied generally to fevers of a low grade or type, and is intended to designate a peculiar tendency to putrescence or gangrene. It seems to me wrong to apply the same word to conditions of the system, very dissimilar in their nature, and as in our congestive fevers, I believe there is no tendency whatever to gangrene, and no resemblance whatever between its symptoms and those of typhus or scarlatina, in the parts congested. I think the term malignant should not be applied to it unless the term congestive, employed in the South and West, can be so so restricted as to indicate solely a condition of things opposite to that of inflammation, by a greater or less diminution of arterial action, accompanied with a more or less excessive accumulation of blood in the parenchyma of certain organs and their vessels; it would be much better in my opinion to employ the term algid, notwithstanding it involves a contradiction in its application to the word fever.

It is very difficult to arrive, at once, at a knowledge of the actual mortality in this disease. In giving an opinion, I am forced to draw my conclusion from the result of my own practice; and after due examination and reflection, I would place the mortality at the rate of 15 per cent. It must be observed, that I include all cases, whether occuring in town or its immediate vicinity, or at a distance varying from 5 to 30 miles. The present year, I embrace a case in the calculation, in which death occurred in not less than ten minutes after my arrival. In placing the mortality at 15 per cent., it must be understood, that it does not relate to one, but to an average estimate of the disease for ten years. This present year, remarkable for healthiness throughout Western Louisiana, I have had in my own practice 12 cases, besides the fatal one mentioned; there was one other of the same kind. It happens but seldom, that the physician is called to cases of this disease before strongly marked symptoms of congestion have set in. Generally assuming an intermittent form, the first paroxysm, and sometimes the second being slight, accompanied frequently by less agitation—pain in the head—back or limbs, than is experienced in the common ague and fever; the patient or his friends are not alarmed until, without fever, the general *malaise* has increased, with irritable stomach—low weak pulse—obstinate constipation; or if active medicine has been given, profuse purging of a highly morbid nature. At any stage of this disease would blood-letting be proper? When we recollect, that in pure autumnal fevers, great debility follows the period of excitement, that cordials and stimuli are frequently in their first stages necessary, even when none of the symptoms of positive congestion exist; it seems to me plain, that the existence of any signs of congestion would counter-indicate blood-letting. I have already stated, that in my opinion, the condition of the congested organs in this disease presents none of the phenomena of inflammation, and that it resulted—first, from vitiated blood; and secondly, from the loss of nervous energy, the consequence of the derangement of the humours. Congestion being threatened, the abstraction of blood is calculated to hasten and establish it more firmly. In the year 1836, I was called to see a working man, Mr. Spence, aged about 30, of a sanguino-bilious temperament, of very industrious habits, and addicted to his daily potations of whiskey. He had had a slight paroxysm of fever, while engaged at work, as plasterer, some ten miles from home. He was brought home after the second paroxysm, in a state of great debility. On the day after his return home, he was taken with a most violent chill; I was sent for, and found him with a full pulse, but soft, complaining of severe pain in the head—great prœcordial oppression, moderately warm skin. I drew from the arm about 18 ozs. of blood, with I thought a happy effect; ordered a mustard pediluvium and a mild purge. Perspiration came on and I returned home. Two hours had not elapsed before I was sent for in great haste, and on my arrival found him as cold as a dead body—but breathing well, and perfectly in his senses. I applied over his abdomen dry and wet cups, had him frictioned with mustard, pepper and hot turpentine; then applied mustard plasters, and a large blister over the stomach and liver. Gave him brandy toddy, pepperine, camphor, quinine and calomel; tried cold water injections, in a word,—the congestion continued to increase—he fell into an irretrievable collapse, and in about 14 hours after I bled him, he was dead. I have been always satisfied, that the loss of blood, in this case, hastened, if it did not actually produce the congestion.

In an article on the irritative or inflammatory form of yellow fever, I have lauded very highly the practice of copious blood letting in its treatment. Between such a disease and the bilious or congestive fevers, I believe no similarity exists—and the treatment in the one is confessedly different from that of the other.

Whoever, though, has attentively examined the symptoms of congestive fever, must very frequently have seen evident signs of irritation or inflammation in some portions of the gastro-intestinal mucous surfaces. But over such inflammation, blood-letting, perhaps, has less influence than over that of any other organ; though there may exist in this disease, in the beginning, considerable excitation, the fruitless effect of the vis medicatrix naturæ; yet the prime element of the disease is debility of the heart and arteries, and diminished nervous power. If, therefore, any symptoms of approaching congestion existed, even with unequivocal evidences of inflammation, which in the forming stage of the affection is very rarely seen, I would not resort to general blood-letting; for there is little hope, thereby, of inducing a diversion of the fluids from the organs threatened, or effecting favorably the process of sanguification, and of promoting the excretion of the morbid elements of the blood, The congestion will most certainly be hastened.

Pain in the head and along the spinal column, or nausea, with great heat in the epigastric region being present, I would use wet and dry cups to these regions. The next step, the exhibition of a purgative, is, perhaps, the most important, and at the same time one of the most delicate, uncertain and responsible acts. I believe it to be important as the only efficient means of evacuating the primæ viæ of morbid matter, which almost invariably accumulates there, and is at the same time dangerous, as liable to arouse or increase gastric duodenal and intestinal inflammation; and thus to arrest the discharge or excretions of effete and noxious elements of the blood, into this great outlet for the impurities of the system. Constipation almost always exists—and whether or not attended with irritation, it will increase with the accumulation of morbid secretions. Thence purgatives are necessary—and I prefer to begin with a dose of rhubarb 15 grs., calomel 15, ipecac. 3; following it up two or three hours afterward, with manna, senna, some rhubarb, etc., to which I add some carbonate of soda and nit. potass. This last medicine I would repeat every two hours, until the bowels were freely opened. If the stomach is irritable and will not retain this medicine, it is advisable to stop it at once—to employ a combination of anodyne, alkaline and saline mixture, to be repeated at short intervals, until the stomach is completely settled. For this purpose I have long used paregoric with carb. soda, and spirits of nitre and epsom salts. Emollient and purgative enemata should be given and repeated. In order to prevent a future paroxysm, mustard plasters should be applied early to the extremities, removing them every hour or two to fresh parts, and letting them remain only to redden the skin. Quinine should also be resorted to in large doses, in combination with acet. ammoniæ and a few drops of laudanum, and given in enema, so soon as a motion has been obtained by the purgative injection. Give the quinine by enema. If the bowels have been opened by medicine, give the quinine injections with the purgative. If the stomach is settled tranquil, it would be advisable to give by the mouth quinine also, in combination with the above mentioned saline mixtures.

Cases will occur of excessive constipation, which will appear to increase in proportion as purgatives are given—the stomach is very irritable and will retain hardly any thing; calomel itself appears to exert a most unfavorable influence; the patient is restless in the extreme; such cases frequently seen in children, from 5 to 10 years of age, and very generally accompanied with oppression of the brain; the scanty passages from the bowels contain no fæces—but are of a serous or mucous appearance—the skin is cold and the pulse thready—sometimes imperceptible. In these cases it is difficult to prescribe. Irritation of the gastro-duodenal surfaces and of the intestines to a greater or less extent. Congestion of the portal system—the nervous system highly excited or depressed, present indications very difficult of attainment. Dry cups may be prescribed, but when employed, seem to exert but little influence. The alkaline and saline mixture, alternated with small doses of calomel and ipecac., 2 grs. of one, with one of the other; blisters over the back of the head—over the stomach and liver, and injections of quinine, etc., with acet. of ammoniæ—mustard plasters frequently repeated to the extremities—frictions, with dry salt and pepper, immersing the body, up to the middle, in a bath rendered strongly irritant by the addition of salt and mustard—occasionally a teaspoonful of brandy julep. Under such a course, more may be expected than under any other I have ever seen tried. The continued employment of these remedies hold out the strongest hopes for the recovery of the patient. But, of course, no particular rule with regard to the periods of employing the remedies can be laid down. Let us content ourselves in these embarrassing cases with the slowest progress in the improvement of our patient; and beware, in our solicitude, of jeopardizing life, by the use of doubtful or heroics remedies.

The principles of treatment to be pursued in the intermittent form of this disease before congestion is evident, are more manifest than in its other forms. Quinine in full doses is the great remedy,—but efficient purging with manna and rhubarb, and epsom salts—preceded by rhubarb and calomel, is also necessary. If the bowels have been well evacuated, quinine may be given by the mouth in combination with the sp. eth. nit. and carb sodœ: if not it should be administered in doses of 20 grains by enema, and repeated, provided it is retained for half an hour every three hours, and after a shorter interval, provided it is retained for a shorter period.

The treatment to be pursued after the congestive stage is fully formed, and the patient is threatened with complete collapse, no matter how the disease may have commenced, with chill and fever of an intermittent, or remittent form, or not, appears to be very nearly the same; but always modified by the prominent symptoms which may accompany those of congestions. In these cases, in which the elementary canal—the brain—and the lungs present no marked symptoms of disease, it seems to me fair to infer that the organs of the abdomen external to the intestines are the chief seats of congestion. The great indication then is to equalize the circulation, by the means of stimuli and purgatives. The latter should be applied externally with perseverance, in the form of mustard plasters, stimulent embrocations, baths and frictions. A large blister should be placed over the liver and stomach; stimulating injections, brandy-toddy, a pill of calomel and comp. ext. of colocynth or rhubarb, should be employed until reaction and free evacuations ensue, and after reaction has taken place, it will be prudent to continue the stimulant and laxative remedies, according to the necessity which may appear to exist; substituting mild medicines in the place of calomel.

But we may expect to encounter many symptoms of an urgent nature, which require immediate attention, as for example incessant nausea and vomiting, rice water, or sanguineous evacuations, delirium, or stupor, unceasing restlessness, most painful spasms, great diminution or arrest of urinary secretion, or sometimes hurried and difficult breathing.

In the case of copious rice water evacuations, in addition to the external stimulant, laudanum, or paregoric must be added to the injections of quinine, and pills of calomel, quinine, camphor and capsicum, with brandy julep, or toddy, should be given at short intervals. Pepper or ginger tea, or brandy julep, will either frequently allay the irritability of the stomach, but if they should fail, it is needless to vex the stomach with purgatives, and we should content ourselves with simply the exhibition of an anodyne combined with some alkali, as syrup of morphine, with carb. of sodæ, or the latter with charcoal. So soon as nausea has ceased, the bowels will relax of themselves, or be acted on by the mildest purgatives ; such as a weak infusion of rhubarb, and manna, and magnesia, or a pill of blue mass, and rhubarb. With regard to hemorrhages from the bowels, unless excessive, I would not recommend by the mouth or anus, any of the astringent preparations. If they should become necessary, I believe a decoction of peruvian bark, with acet. plumb. would as likely be beneficial as any thing else. I never witnessed but one severe case of this kind, in which I attributed the hemorrhage to repeated large doses of calomel, notwithstanding the fact, that this remedy is highly recommended in small doses to stop bleeding from the bowels.

Where cases of delirium or stupor, and restlessness exist, the pouring of cold water over the head seems to have a happy influence. It would be well in these cases to shave the head in the beginning, and so soon as tranquility was restored, to apply a large blister on the back of the head and neck. In cases of painful spasms, the common laudanum and ammoniæ liniment should be rubbed in freely. The arrest of urinary secretion is a most dangerous symptom—it marks a severe if not a fatal case. Nitrous drinks and emollient applications over the region of the bladder should be resorted to, to relieve the distressing symptoms.

A combination of the non-purgative, saline and alkaline remedies freely diluted, appear to me to act well in all our summer diseases. They should be given in the form of drinks, frequently repeated, and in small doses ; and by the addition of epsom or glauber salts, in doses of from 5 to 10 grains, the bowels will be kept regular, and frequently be moved, when other active remedies seem but to aggravate nausea and constipation. In large doses, saline purgatives act very differently. They produce watery evacuation, and appear to favor the recurrence of the chill or fever.

Calomel, even at the present day, is regarded as the most efficient remedy in this disease, by many old practitioners ; administered in heroic doses, after a vain use of other remedies ; it often appears to exert a most salutary effect. I have seen more than one patient recover, and rapidly too, after abandoning all other remedies, and using calomel in repeated doses, of a hundred grains ; and in these cases salivation is a rare result.

In 1837, I invited a disciple of Professor Cooke to see a case of congestive fever, in the confirmed stage of collapse. I confess, that I had abandoned the man as past all remedies. The case, we both thought hopeless; but at his suggestion, calomel in 100 gr. doses were given every hour. Four doses were given (he was allowed claret wine ad libitum) when he commenced to rally, and was well in a few days.

X.—*Remarks on the disease, popularly known as the " Black Tongue."* By P. E. H. Lovelace, M.D., of Mississippi.

The above mentioned disease derives its popular name from the frequent occurrence of lividity of the tongue, though blackness of that organ was not invariably present even in those who died with it. It was about the middle of March, 1844, that it made its appearance in the city of Vicksburg. It was preceded and accompanied by great atmospherical vicissitudes, and storms, rains and floods. The months of December and January, were very wet. February unusually warm and dry, and March, and April were remarkable for an extremely low range of temperature. Soon after it broke out in Vicksburg, it appeared in various parts of the counties of Warren, Madison, Hinds, Claiborne, &c., spreading apparently from Vicksburg as a centre, and raging in some localities with much greater malignity than in others. No age or sex seemed to be exempt from an attack. It occurred in the young as well as the old, and in the robust as well as the feeble. My partner, Dr. D. B. Crawford, and myself, saw forty-two cases, simple and malignant, six of which terminated fatally. The greater number of these cases occurred on the plantation of Mrs. Stevens, situated about the middle of a ridge of highland, near fifteen miles wide, lying between the Mississippi, and Bigblack rivers, and about ten miles from Mrs. Robinson's plantation, when it prevailed with such fatal violence as described by Dr. Puckett, in the second number of the New Orleans Medical Journal. Six of the above cases were children, between six months and ten years of age, in all of whom the disease was well developed, and in one of whom, an infant at the breast, it proved fatal.

Its extension seemed to be by contagion. Many facts, several of which came under my immediate observation, would justify such a conclusion. A Mr. C., of Vicksburg, was attacked with the disease, and removed to the house of Mr. S., on Wednesday, where he was closely attended by Mrs. S. and her mother, Mrs. M. S., who happened to be there that day on a visit. On the day following, Mrs. S., who had several days before visited a lady afflicted with the disease, was violently attacked. On Sunday, four days after exposure, Mrs. M. S., who, in the meantime, had returned to her residence on a plantation, at a distance of about twelve miles from town, was seized with the disease in its most genuine form. At about the eighth day of her illness, one of her attendants, a servant woman, who had remained constantly by the bedside of the patient, was attacked. Her place was immediately supplied by another, who likewise soon became infected.— Thence it was extended to the " Quarter," which was distant about a mile from the residence, and which contained about forty persons, all of whom, with but two or three exceptions, suffered in the course of two or three months, an attack of the disease. Several instances of its propagation in a similar manner might be mentioned, if it were deemed necessary, in proof of its communicability. It must be admitted, however, that it is not highly infectious, and that casual intercourse is rarely attended with danger, unless the system of the person thus exposed be rendered peculiarly susceptible from some predisposing circumstances. In general, according to my observation, for the reception of the disease, exposure for several hours in a close room with the sick, where the miasm exists in a concentrated form, is necessary; an evidence of which is found in the immunity, with but few exceptions, of medical attendants and transient visitors.

Two cases that occurred, one in Warren, and the other in Claiborne County, would favor the opinion, that the disease is also communicable by inoculation. In one case, the disease gained admission into the system through an abraded surface on the head, in the other, through a puncture in the end of the finger, inflicted with a pair of scissors, whilst opening some erysipelatous blisters upon the body of a patient, ill of the disease. This latter case terminated fatally.

The disease occurred under a variety of prominent modifications, which would admit of a number of divisions and subdivisions, according to the nature of the attending fever, and the violence and peculiarity of the local affections; but as it is not my intention to write a systematic article on the subject, I will merely mention that its division into *simple* and *malignant* will, I think, be sufficient for all practical purposes.

It generally commenced with the ordinary symptoms of Catarrh, such as lassitude, depression of spirits, pains in the stomach, loins and lower extremities, fulness of the vessels of the face, redness of the eyes, harsh respiration, dryness of the fauces, cough, hoarseness, painful deglutition, and stiffness of the muscles of the neck and jaws. Towards evening most of these symptoms were aggravated, particularly the hoarseness, cough, stiffness of the muscles of the jaws, and difficulty of deglutition. In some cases, the cough from the first, was very severe and stridulous, resembling very much that of Croup. The skin dry and rough, and the pulse usually small, frequent, and irregular. The tongue, in the early stages of the disease, was coated with a white fur, through which appeared, scattered over the surface, small scarlet-colored points, resembling very much the characters used in music, called "*staccato*;" but becoming in severe, and protracted cases, successively red, and granulated, and furred, brown fissured. The mucous membrane of the mouth, and fauces always presented signs of inflammation—exhibiting an injected, red, mottled, or ulcerated appearance.

As the disease advanced, a variety of local affections manifested themselves. In some cases, the glands of the neck were so much swollen as to interfere with deglutition to that degree, that any attempt to swallow fluid, caused their rejection by the nose,—sometimes an erysipelatous inflammation would attack the face, restricted in some cases to the eyelids, the cheeks, or the nose, in others quickly involving all these parts in a tumefaction so great, that vision was completely destroyed, and every feature obliterated. Imagine the appearance of a body that has been dead for two or three days in the heat of summer, with face swollen until the very tips of the eyelids are hardly visible, covered here with vesicles of serum, there with putrescent matter and scabs, and a bloody fluid oozing from the mouth and nose—and you will have a tolerably corr ct idea of a case that came under my observation. In this case there were absesses about the eyelids, cheeks, behind the ears, and upon the back of one of the hands; otitis, cephalalgia, and great prostration of the vital energies. After a protracted illness of several weeks, during which the powers of life sank to the lowest point of depression from which a favorable termination could possibly have been expected, a gradual restoration to health took place, and the patient entirely recovered with the exceptions of impaired vision from corneal ulceration of the eyes. In a case very similar to this, in nearly every particular, incurable amaurosis occurred, with accretion of the lids to the globe of the eye, and in others, of which I heard, total destruction of the eyeball.

The inflammation appeared not to be limited to any particular spot, but attacked various parts of the body, accordingly as it was influenced by the existence of accidental local predispositions. In some instances it fell upon the groins and armpits, producing buboes; frequently upon the extremities, particularly the lower, ending sometimes in resolution, often in suppuration, and sometimes, as I have been told, in mortification and sloughing. Sometimes the brain, the lungs, or the digestive organs were implicated, in which cases, the symptoms characteristic of inflammation of the part affected were added to those already mentioned. In two of the cases which we lost, there was violent inflammation of the stomach, unfortunately induced by over doses of Tartar Emetic given by an overseer through mistake for calomel.

At times the disease was very insidious in its approach, the mildness of the attack bearing no relation to the subsequent violence and danger. In one case which terminated fatally, there were no symptoms preceding the delirium and coma, which could have excited a reasonable apprehension of danger. In addition to the usual initiating phenomena, which were very slight, there were pain and swelling of the knee, very much resembling acute articular rheumatism, which soon extended to the groin, when suddenly the most alarming constitutional symptoms ensued, accompanied with an exceedingly quick and fluttering pulse, and an extreme tenderness to the slightest touch of the entire surface of the body. A few hours before death, erysipelatous blisters made their appearance upon the back and shoulders. This extreme sensibility or soreness of the skin, and weak, fluttering pulse occurred in another case, which also proved fatal. When first called to this patient, I was informed that she had been attacked four days previously with chill and fever, and had been delivered the evening before of a child, in a putrid condition. I found the abdomen tense and sore, the pulse irregular and fluttering, the skin rough and sensible to the slightest touch—even the pressure necessary in an examination of the pulse gave pain. Shortly afterwards delirium and coma supervened, from which she never recovered.

Convalescence was generally slow and tedious, and there was a great tendency to relapse from exposure to cold and wet, excesses in eating, or local injuries. There was a renewal of the disease in a patient, (a woman who had suffered an attack about two months previously,) in consequence of a wound near the joint of one of her fingers, received from a thorn whilst working in a garden, near some plum-trees. Soon after the accident, the parts about the axilla became the seat of violent inflammation, which rapidly extended from the shoulder down the body, half way to the crest of the ilium, attended with a great deal of pain and swelling, nausea, vomiting, headache and fever. A large tumour, five or six inches in diameter, and as hard to the touch as if the skin had been stretched over a block of wood, now appeared to the right, and a little below the right mammary gland, from which point, the inflammation of the phlyctenoid variety, gradually extended, spreading in its progress, to the back of the neck and to the loins, again contracting and passing under the left arm, until it completely encircled the chest. In about four weeks, the tumour suppurated, was opened, and discharged an immense quantity of matter. From this time an amendment took place and the patient ultimately recovered.

The accompanying fever, which, in the simple variety was somewhat inflammatory, often manifested a tendency, no matter what the habit or temperament of the subject, to assume from the first a malignant or typhoid character—perhaps from a contamination of the blood by absorption of the products of inflammation—perhaps from the operation of the contagions virus directly on the nervous system.

With regard to the anatomical changes that may have taken place, I know nothing, as no necroscopical examinations were made.

The origin of the disease in this part of the country is involved in mystery. Whether it was introduced into Vicksburg from abroad, or originated there spontaneously, is not known. Possibly it may depend, as is generally believed, upon some peculiarity in the constitution of the atmosphere, but in what that peculiarity consists, no one, perhaps for want of philosophical instruments, or tests sufficiently delicate, has, as yet, been able to elucidate. It may owe its origin to some change in the electrical or magnetic condition of the air, causing an accumulation, or deficiency of those fluids in the animal economy:—or it may depend upon myriads of "minute substances, endowed with vegetable, or animal life, and developed in unusual abundance, under specific states of the atmosphere, in which they float, and by which they are carried hither and thither," coming in contact with the skin, and mucous membranes, and irritating those surfaces in a manner analogous to cantharides, mustard and other acrid substances, which frequently excite an erysipelatous inflammation of the skin.

But whatever be its origin, it is quite certain that when once engendered, it is communicable from person to person, and that vicissitudes in the temperature, and hygrometrical states of the atmosphere, favor the operation of the morbific agent, as it was observed to spread more rapidly, during the cold and variable weather of spring than in the summer. But assuming the disease to be contagious, may not its greater prevalence or activity at such season, be, in part, accounted for by the fact, that people are more crowded together, and houses less ventilated in cold weather than in summer?

The cause did not appear to be diffused through the atmosphere generally, but on the contrary to be limited to particular neighborhoods, and in some instances even restricted to particular plantations. In some localities, the miasm remained sufficiently concentrated to produce the disease in persons arriving from uninfected districts, long after its influence had apparently subsided. This fact was but too well exemplified on the plantation of Mr. Robinson, above alluded to. Upon that plantation the disease was revived in February last, among some negroes that were transferred there in January, from a plantation in Claiborne County, by a gentleman into whose possession the property came after its vacation by Mr. Robinson in December. If my information be correct, the overseer and several negroes died of it, and the gentleman himself narrowly escaped.

Bloodletting, which is generally considered as the most prompt and efficient of all our remedial means, and is indeed, regarded by some as a *sine qua non* in the treatment of inflammatory affections, could not be employed in this disease without considerable risk. The evidence against it far outweighs that in its favor. It was resorted to in many cases, I have been informed, rarely producing any good effects, but often suddenly precipitating the patients into a state of collapse, from which the most potent stimulants, both internally, and externally applied, were not sufficient to arouse them. We practised it in but one case, that of a young and robust female, and without any decided advantage. The progress of the disease was arrested, under the conjoined influence of that and other remedies, and we thought the patient cured, but in the course of two or three weeks it returned upon her again, and was successfully treated without further depletion. If we take into consideration the adynamic character of the disease, and its tendency to prostration, and to death by *asthenia* and *coma*, we can readily conceive why venesection should be, if not inadmissible, at least a doubtful remedy.

We generally commenced the treatment by the exhibition of an emetic, followed in a few hours by a tolerably brisk cathartic. If, after the operation of the emetic, there was still much febrile excitement, nauseating doses of a combination of the wines of Ipecac. and antimony, or Lobelia, with sweet spirits of nitre and paregoric, according to circumstances, were given in conjunction with warm drinks of an infusion of balm, sage, &c. This course rarely failed to excite the cutaneous exhalents and to improve the condition of the skin; and then quinine was administered in one or two grain doses, every two or three hours. In urgent cases the quinine was sometimes alternated with the nauseants in one grain doses, every hour or two, with the happiest effects.

When the fauces and glands of the neck were much inflamed, gargles of nitrate of silver, (from 2 to 4 grs. to an oz. of water,) with rubefacients to the neck, such as a mixture of equal parts of spirits of camphor, and turpentine, or sinapisms, followed by warm emolient poultices, were found to be the best applications of all the various substances which we used.

During the whole course of the disease the bowels were kept open by means of gentle aperients, such as seidlitz powders, rochelle, or epsom salts, and laxative enemata. By these means diligently employed, the train of morbid actions were frequently interrupted, and a further progress of the malady completely arrested.

In the malignant variety of the disease in which there was a tendency to universal collapse, we had recourse to stimulants, and tonics.—Carbonate of ammonia, camphor and opium—wine and bark—elixir of vitriol—turpentine enemata—and mustard, or capsicum pediluvia, were the remedies chiefly depended upon.

The most generally useful local applications were thought to be a strong solution of nitrate of silver, and poultices of slippery elm bark. As a lotion for the eyes, when they were involved in the inflammation, we used a solution of borax, or mucilage of slippery elm.

Blisters, which are recommended by the highest authority in erysipelas, were found to be extremely dangerous in this affection, from their tendency to induce gangrene. In two instances, of which I heard, gangrene of the blistered parts immediately followed their application.

XI.—*Brief Notes on a Medical Tour in the United States.* Communicated in Editorial Correspondence to this Journal, by Erasmus D. Fenner, M.D., one of the Editors.

MEDICAL AFFAIRS AT THE NORTH.

New York, May, 1846.

Messrs. Editors—Since it is my good fortune to be able, at this time, to make an excursion to our Northern cities, I avail myself of the opportunity to offer you a few observations on *medical affairs*, in this region. Nothing is better calculated to engender kind feeling and laudable emulation in the ranks of a profession, than an occasional personal intercourse and exchange of civilities. He that remains perpetually at home must necessarily move in a contracted sphere. It is true, that the facilities of literary correspondence enjoyed at this day, go far to keep us informed of every thing that is passing throughout the civilized world, and to place us in gratifying communion with those engaged in similar pursuits in distant parts ; yet this is not equal to personal observation, and the free and cordial interchange of views and feelings face to face. The warm grasp of the hospitable hand, the beaming of the countenance, and the free and unreserved conversation, are all requisite to make us understand each other fully. In offering the crude observations which are to follow, I do not feel that I am addressing you exclusively, but through you the great body of our Southern readers. I would let them know, as far as I have been able to ascertain, in what they have occasion to congratulate themselves, and in what they are excelled by their brethren at the North. I will commence my observations with a brief notice of the late—

National Medical Convention.—This body, consisting of 83 representatives of medical schools and societies, in fifteen different States, assembled on Tuesday, the 5th inst., and adjourned on the 6th. Having had the unexpected honour to receive, on the eve of my departure from home, a commission authorising and requesting me to aid in representing "The Mississippi State Medical Society," you will perceive my name recorded in the proceedings as a member from that State—indeed, I was the only one at the opening of the convention; though, two others, Drs. Copes and Magoun, came in afterwards. I subsequently received a similar commission from the Medico-Chirurgical Society of Louisiana, but it came to hand too late for the purpose designed.

The representatives were chiefly from New York, and the neighboring States, very few coming from the South and West. Although the call for a national medical convention, emanating from the New York State Medical Society, had received the approbation and encouragement of the medical journals throughout the country, yet the impression seems to have gotten abroad that it would not be responded to generally; in other words that it would be a *failure;* and therefore, many of the most influential colleges and societies did not send delegates. After arriving here, I discovered that an erroneous impression had prevailed abroad in regard to the origin of this convention. It was generally supposed to have been set on foot by the *medical faculties of this city*; and its grand object, the assembling of the profession here, for the purpose of displaying the superior advantages of New York, for giving medical instruction. This appeared to excite the jealousy of the rival schools, and they, therefore, determined to have nothing to do with it. You may, therefore, imagine my surprise, when, as soon as the convention was organized, a delegate from the New York university, offered a preamble and set of resolutions, declaring the attempt to assemble a National Medical Convention to be a *complete failure*, and moving its *immediate dissolution*. This motion gave rise to considerable excitement, and met with such a signal defeat, that it was evident to me, that the call of the convention had not originated in *that quarter*, but in quite an *opposite one.* Professors Bedford and Pattison expressed a willingness to accede to any action that would be calculated to elevate the general standing of the profession, but did not approve of so small a delegation as was here present, legislating for the entire profession throughout the country. From what I have since learned, these gentlemen were particularly anxious to guard against the undue influence of the New York State and County Medical Societies, with one of which this convention originated, and which had sent upwards of forty delegates to the present meeting. Happily, the convention, at once, assumed a *national character*, independent of all local or sectional influences, and there can now be no doubt that the enterprize is fairly set on foot. From an interchange of views among the members, there was an evident desire to establish a National Institution, to be composed of delegates from all the chartered Medical Colleges and Societies in the country, to assemble annually at the different large cities, and to be so conducted as to promote the best interests of the profession. The late convention made a *fair commencement* of this great work; and that was as much as could be expected to be done at this time. Committees were appointed to report at the next meeting, which is to be held in Philadelphia, on the first Wednesday in May, 1847, on the following subjects, viz:—1st, On a plan of organization;—2d, On a uniform and elevated standard of requirements for the degree of M.D.;—3d, On the standard of acquirement which should be exacted of young men commencing the study of medicine ;—4th, On a code of medical ethics ;—5th, On the propriety of placing the power of conferring degrees in other hands than those who give instruction. A committee was also appointed " to prepare and issue an address to the different regularly organized Societies and chartered Medical Schools in the United States, setting forth the objects of the National Medical Association ; and inviting them to send delegates to a convention to be held in Philadelphia, on the first Wednesday in May, 1847."

You thus perceive that these committees are entrusted with the consideration of important matters, and they will doubtless devote to them the most mature deliberation. It was understood amongst the members that this association is to be divested of all sectional or party character, and the voting on all important subjects is to be by States or societies, and *not by individuals*, which would give too much influence to the immediate neighbourhood of the place where the convention happens to sit. The members separated from each other with kind feelings, and much enthusiasm was displayed in regard to the great objects contemplated. It is to be hoped that this enthusiasm will be spread abroad throughout the Union, and that every medical institution in the country, will be represented in the next convention. The questions which are attracting most attention at this day evidently relate to *the proper standard of medical education necessary to gain regular admission into the ranks of the profession,*—and *the separation of the power to confer the degree of M. D., from the hands of those who give instruction.* These questions will doubtless elicit an able discussion between the professors of the different schools, and the mass of the profession, throughout the country. A collision of interests is involved, and an animated, if not an angry contest may be expected. The schools contend that none are so well qualified *to examine*, as those who are constantly engaged in *teaching*, and that it would be impossible to establish a disinterested and impartial Board of Examiners. Their opponents contend that the medical schools, already so numerous in the country, yet, annually increasing, are ruining the profession, by cheapening education, and lowering the standard of requirements in proportion, by which thousands of young men are induced to study medicine, who might succeed much better at some other vocation, and have the degree conferred upon them, before they are half qualified to practise. The *reformation* that is demanded in regard to medical education, seems to be environed with difficulties, and I do not now see how it is to be effected. The deliberations of the next convention, will doubtless throw some light upon the subject.

By reference to the copy of the proceedings which I send you, you will perceive some of the most respectable names belonging to the profession in this region. The members of the convention were invited in a body, to spend the evening of the 6th, with Dr. F. Campbell Stewart; and there we had a most delightful *reunion*. Dr. Stewart is already favourably known to the profession, by his writings, and appears to be an accomplished gentleman. We were invited to spend the evening of the 7th, with Dr. Edward Delafield, one of the oldest and most respectable physicians of the city. The assembly here was not so large, because many of the members of the convention had returned home.—

Those who remained, however, had the pleasure of meeting a number of the physicians of New York City, and spent a very pleasant evening. I will here close my remarks upon the national convention, and turn to such other objects of professional interest, as have fallen under my observation, since my arrival in New York.

The New York Hospital. This is a beautiful establishment on Broadway, situated on an eminence some distance from the street, with a handsome lawn, shaded with large trees in front. Dr. J. Kearney Rodgers, the principal visiting surgeon, invited the medical convention to call and see the hospital on the afternoon of the 7th May, but the weather was so very bad, that but few were able to attend.—Through the polite attention of Dr. J. Watson, also an attending surgeon, I was allowed the opportunity to go through the Institution, during the morning. It consists of three handsome stone buildings, for the accommodation of patients; and the necessary out-houses. One building is appropriated to United States Seamen, and answers the purpose of our Marine Hospital. There are about 30 wards, capable of containing about 350 beds. Number of patients in the hospital at this time, about 250. Every thing appeared to be remarkably clean and neat about the establishment. There are six attending surgeons, and four attending physicians, elected annually, but usually remain during their pleasure. They are not all in service at the same time. By the last annual report, I learn that the number of patients who received the benefits of this institution, in 1845, was 2970—of which there died 216. There is a very handsome pathological cabinet, containing many valuable preparations and specimens, and a library containing more than 5,000 volumes. I understand that valuable clinical lectures are delivered at this institution, by the attending physicians and surgeons. Among the pathological specimens, I was gratified to find a preparation of Dr. Rodgers' case of subclavian aneurism, recently published. Dr. Rodgers' operation is here considered one of the most brilliant achievements of modern surgery, notwithstanding the unfortunate result of the case. The dead house of this hospital is every way commendable.

Bellevue. This is an immense establishment, combining the varied requirements of a prison, an alms-house for paupers, a general hospital, a lunatic asylum, and an orphan asylum. The lunatic and orphan asylums, are on Blackwell's Island, across East river; I did not visit them. I walked through the wards of the establishment on this side. There are usually two or three thousand inmates in this institution—generally about 500 invalids on this side of the river. The whole concern is under the medical charge of Dr. Fenelon Hasbrouk, who receives a salary, and has two paid and six unpaid assistants. These are all graduates, and the latter pay for the privilege of living in the institution. I was surprised to learn that this immense field for medical observation is closed against lecturers and students. I was indebted to Dr. Watson for the oportunity of seeing this institution, and he then carried me to another hard-by, called

The Colored Home. This is a small hospital devoted to the infirmities of colored people, and under the care of Dr. Fitch. We had the good fortune to meet with the Doctor near the hospital, and he politely showed us through the wards. There are generally about 200 inmates, of which, from 30 to 60 are usually on the sick-list. Dr. Fitch informed me that about *two-thirds* of the deaths in this institution are from consumption. He thinks the colored people are vastly more subject to phthisis, in this climate, than the whites. Characteristic of their disposition, we found the inmates hovering round heated stoves, with doors and windows closed, notwithstanding the day was pleasant.

The New York Dispensary. This is a very extensive institution, founded by certain benevolent citizens of New York, in 1790. We learn from the last annual report, that 24,440 patients were attended by the physicians of the dispensary, and furnished with requisite medicines, during the year 1845. The amount of relief afforded to this large number of sick, was dispensed with an expenditure of $3,268. The number of annual subscribers to the institution is only ninety-eight. The officers of the institution are 13 trustees, 1 resident physician, 6 consulting, 3 district, 10 attending, and 4 assistant physicians, 1 apothecary, and 1 assistant apothecary. The city is divided into three districts, where consultations are held, and the hours of attendance are from 9 to 2 o'clock, every day, except Sunday. Each physician has his proper hour and class of disease assigned him. Through the kindness of Dr. H. F. Quackenbos, I had an opportunity to visit one of these departments, and to see how the business was conducted. There were some thirty or forty applicants for medical aid, consisting of men, women, and children, and presenting quite a variety of complaints. They were very carefully and kindly examined, and each given a written prescription, to be taken to the apothecary. In the course of the year, there were nearly 5,000 patients attended at their residences. Among the large number of medical attendants connected with this institution, only the resident physician gets any pay. Too much praise cannot be bestowed upon this noble institution, to which our profession contributes so largely, and which administers relief to so many afflicted poor. It presents to the medical student one of the finest fields of observation imaginable.

Cliniques of Professors Parker and Mott. Besides the dispensaries above mentioned, Prof. Willard Parker delivers a Surgical Clinique, every Monday, at the College of Physicians and Surgeons; and Prof. Valentine Mott, at the University Medical College, every Saturday. A large number of invalids assemble at these places, and receive the gratuitous advice and services of these benevolent and scientific gentlemen.

Professor Parker's Clinique.—I had the pleasure of attending Dr. Parker, on Monday, the 10th May, in the amphitheatre of the college. There were some sixty or seventy medical students present. The patients were brought in one by one; the Professor examined each case carefully and minutely; entered fully into the reasons upon which he based his diagnosis; pronounced it, and then prescribed the course of treatment to be pursued. Prof. Parker is a fluent lecturer, appears to have a clear and sound judgment, and explains everything relating to the case before him, in a manner which I should think would be very satisfactory to the student. In the course of two or three hours, he examined and lectured on about fifteen cases, of various kinds, the most interesting of which were two of chronic inflammation of the ankle, from sprain; one of cancer of the mamma; one of injury of the eye, and one of coxalgia; on all of which he made judicious practical remarks.

Professor Mott's Clinique.—This gentleman's fame is so extensive, associated as it is with so many brilliant achievements in surgery, that I had considerable curiosity to hear him. This I had the pleasure of doing on Monday, the 15th May, in the amphitheatre of the University College. A large number of students were in attendance. The patients were introduced in the manner above described, and the same process of examination and lecture followed. The distinguished Professor appeared to be in a very amiable humor, and interspersed his practical observations with many facetious remarks. He is very free in his discourse, both with his patients and the class, and displays a great deal of candour and benevolence. He tells his patients he will prescribe for them with as much care as if they were the wealthiest of the community, but that he will hold out no illusive hopes. In several of the cases presented before him, the diagnosis was very obscure, and he did not attempt to conceal the difficulties. Indeed, he called special attention to them, as being most valuable and instructive. Of this character, were one case of swelling within the abdomen, one of disease in the nose, and one of tumor on the head of an infant. There was one case of some derangement of the hip-joint, of long standing, in a man who had recently been under the treatment of *the natural bone-setter*, Dr. Sweet. This is a member of a notorious family of quacks, about New York, who claim the power of reducing all dislocations, as a *natural gift.* The patient gave us an amusing illustration ot Dr. Sweet's manipulations. The doctor told him that he put everything right, but the patient pronounced him an *impostor.* Dr. Mott said, that he had long known this celebrated family, and mentioned one or two instances of their imposture. Dr. M. prescribed for about fifteen cases, and occupied about three hours and a half. A number of cases had to leave without advice.

The name of Mott has been so long distinguished in the annals of American Surgery, that I expected to see a much older looking man. He appears to be quite vigorous and robust, and I should think is good for at least ten years more of active servie.

Medical Colleges of New York.—I examined the buildings of both these institutions, and found their lecture rooms spacious and comfortable; their museums, both of natural history and pathological anatomy, extensive and varied; their libraries well filled, and their dissecting rooms very commodious. Subjects for dissection are all brought from the cemeteries, and cost ten dollars. I became acquainted with a number of the professors in each school, and found them courteous and hospitable.

Medical Clubs.—Through the kindness of Dr. F. Campbell Stewart I was introduced to one of the Medical Clubs, and attended their sitting at the house of Dr. J. A. Swett. Here I had the pleasure of meeting twelve or fifteen physicians of the city, some of whose names are familiar to the profession, from their writings. One of the number acted as moderator, and called upon each of the others in succession, for anything he had to communicate. Several of them reported verbally such cases of interest as had occurred in their practice since the last meeting, which gave rise to familiar conversation, that was kept up for two or three hours. We were then invited to partake of some refreshments in an adjoining room; and the company separated about 11 o'clock at night.

I cannot too highly commend this social intercourse among the members of the profession. It serves not only to promote harmony and good feeling; but to stimulate them to close and correct observation. I was informed, that nearly all the articles written by the members of this Society, for the Medical Journals, were first read and discussed at these *reunions.* Their meetings are held at each others' houses, in rotation, every week or two. You will, doubtless, agree with me, that something of this kind is very much wanted in our city. I must here close my hasty observations on the medical affairs of New York. There are several other important and interesting institutions about this city, which I was unable to visit; but as I expect to send you some notes from Boston, Philadelphia, and other places, I must beware of trespassing on the patience of our readers.

Yours, truly, E. D. F.

Boston, May 15, 1846.

Messrs. Editors—I arrived at this place from New York, *via* Albany, on the morning of the 13th, and having ridden all night in the cars, felt a good deal fatigued. One or two hours sleep, however, restored my energies; and I sallied forth, introductories in hand, in search of doctors.

I first called on our brother Editor, Dr. Smith, of the Boston Medical and Surgical Journal. He gave me a hearty welcome; and notwithstanding the multiplicity of his engagements as editor, port physician, and general practitioner, found time to lay me under everlasting obligation by his kind civilities. I had the pleasure of making the acquaintance of the two Doctors Bigelow, Dr. John C. Warren, Drs. Jackson, Parkman, Townsend, and some others, who were all very courteous; but my stay in Boston has been too limited to allow me to see much of them. The elder Dr. Bigelow, you are aware, is the Professor of Materia Medica, in the Medical College at this place. He, and his son, Dr. Henry J. Bigelow, with one or two associates, have a private class of twelve or fifteen students, with whom they follow an excellent plan of instruction, consisting of a course of reading, lectures and examinations. Dr. J. B. S. Jackson is one of the visiting physicians to the Massachusetts General Hospital. He has devoted much attention to the study of pathological anatomy, and showed me his private cabinet, containing a large collection of beautiful specimens.

Dr. John C. Warren, who is generally known as one of the brightest ornaments of American surgery, is a very dignified and venerable old gentleman, remarkable for his firmness and composure. He has long been the most eminent surgeon of New England; and although now getting in "the sear and yellow leaf," he still cultivates his favourite branch with commendable zeal. He is a visiting surgeon of the above mentioned hospital, and is punctual in his attendance, every Tuesday and Friday. I was not introduced to his son, Dr. J. Mason Warren, who is likewise somewhat distinguished as a surgeon, and has performed some very creditable operations.

The Massachusetts General Hospital is the only medical institution in Boston, that my brief stay would allow me to visit. This is a most perfect and beautiful hospital, whose only fault consists in its being *unnecessarily fine* in its equipment. There is a centre building with two wings, of granite—an admirable piece of architecture, and combining every imaginable comfort and convenience. It is warmed in winter by a large furnace under the basement, which sends heated air through every apartment, by means of conduits ingeniously contrived. Water for all purposes is also conducted through the establishment in a similar manner. The beds, especially in one of the new wings just completed, appear to be fully as neat and comfortable as those of the Tremont or Astor Hotels. In short, everything about this establishment is in more magnificent style than I ever expected to see about an hospital. It is by no means exclusively a charity hospital; but is designed also as a resort for all such as are either absent from their homes, or who cannot be well attended to at home. The house is divided into *free* and *pay* wards, and is capable of containing about 150 beds. To show the extent of its endowments, we learn from the last annual report of the Board of Trustees, that "the present amount of the property of the institution, is $238,369 91, exclusive of the grounds and buildings." We suppose that this large sum has been accumulated from individual contributions. One individual recently bequeathed to the institution, *forty thousand dollars.*

There were admitted into this hospital, during the year 1845, 453 patients—of which 188 paid board, and 265 were entirely free.

There were discharged, 400; of which 20 are marked, "*not treated*," 2 "*eloped*," 37 "*not relieved*," and 6 "*unfit.*" The whole number of deaths was 54.

The price of board varies from $3. to $10. a week; and the amount received from paying patients during the year, was $3,710 79; considerably more than one-third of the whole amount of board charged to all the patients during the year. The average number of patients was 56; of which the average number *paying*, was 22; and *free*, 34.

The receipts for the year somewhat exceeded the expenditures. There are six physicians and six surgeons; two of each are in regular attendance for a term of four months. They receive no compensation for their services.

Thus, you perceive that this institution, the only one of the kind about Boston, although richly endowed and elegantly managed, presents but a limited field for medical observation, in comparison with our large Charity Hospital, which receives annually about 6000 patients, exclusive of lunatics.

I accompanied Dr. J. C. Warren on one of his regular visits to the hospital, and saw some interesting cases; among which were 1 of lupus; 1 of prolapsus ani, in which Dr. W. had applied the ligature; 1 of dislocated shoulder; 2 of ununited fracture of the arm; and 1 of necrosis of the tibia, following a compound comminuted fracture.

Dr. Warren displayed considerable skill in reducing the dislocation. The pullies and copious depletion were resorted to, and the old gentleman being unwilling to make much exertion on account of some injury to his chest, received not long since, stood calmly by, whilst a medical friend attempted the reduction. Tbis gentlemen thought he had accomplished it, and was leaving the patient, when Dr. Warren stepped up, took hold of the arm, and by a skilful manœuvre, replaced the head of the humerus into its socket, in a very striking manner.

In one of the cases of ununited fracture, Dr. Warren had inserted the seton, sometime previous, and the union was now becoming firm. He contemplates inserting the seton in the other, at his next visit. He removed with his fingers a thin shell of bone in the case of Necrosis.

The McLean Asylum for the Insane, situated in the suburbs of the city, some distance from this, is a branch of this institution. It is said to be one of the best asylums in the country, and I regret that I was not able to visit it. It is under the superintendence of Dr. Luther M. Bell, and contains an average number of 150 patients. I regret also, not having been able to visit the Naval Hospital, and the Asylums for the Blind, and the Deaf and Dumb. My time here was chiefly occupied in visiting the public buildings, the libraries, the museums, Harvard University, Laurel Hill Cemetery, and Bunker Hill Monument. I would gladly have spent a longer time in this interesting, and hospitable city, but wishing to be back at New York, on Saturday, to hear Prof. Mott's weekly Clinique, I had to hasten my departure. I was pleased to discover that the medical profession occupies an elevated position in the city, which has been termed the "Athens of America." The leading physicians are wealthy and influential members of society, and from the days of the Revolution, when one of the noblest martyrs of Freedom went from its ranks, the profession has continued to hold a high and honorable stand among the avocations of men. Do not suppose, however, that the Hydra *Quackery* is deterred from showing its hideous front, in this refined, and enlightened city; on the contrary, it boldly proclaims its arrogant pretensions, and lures many a hapless victim to ruin and death. If I am not mistaken, Boston was the cradle of Thompsonianism; and I am informed that in New England, Homœopathy, Hydropathy, and every species of Empyricism, still find numerous supporters. What a commentary does this form upon the intelligence, and liberality of this enlightened community! Thousands upon thousands, the accumulations of long lives of toil and self-denial, have been bequeathed by individuals, for the purpose of endowing benevolent institutions, and they are truly the pride and glory of this christian land; wherever these are to be found, and of whatsoever character, they require the superintending care and arduous labours of the scientific physician, and receive them for the most part, gratuitously; yet, the community, not here alone, but throughout the Union, unmindful of the large amount of gratuitous service, rendered by physicians to the indigent and afflicted, and also unmindful of the *wear and tear* of both body and mind, inseparable from the study and practice of the profession, seem to be led astray by every flaming newspaper advertisement, and bestow their money freely upon charlatans, and the most ignorant pretenders, whilst many a worthy and respectable physician is lingering in poverty and neglect. But this is a subject that would give rise to more reflections than would be fitting for this occasion, and I therefore defer it for some future lucubration. I will here close my observations on the medical affairs of Boston, and have only to regret that they are so crude and imperfect.

Yours, truly, E. D. F.

Philadelphia, May 23d, 1849.

Messrs. Editors:—I arrived at this famous and favourite seat of medical science, on the 7th inst., and enjoyed the kind civilities of its able and distinguished physicians for nearly a week. Gladly would I have remained much longer, to partake of their generous hospitality, and to see more of their numerous benevolent and scientic institutions, but as there are yet before me on my way home, a number of cities having medical colleges, and hospitals, of which I contemplate offering you some brief notes, and as New Orleans, since my departure has become the scene of so much excitement, from the war with Mexico, I begin to feel an urgent desire to get back to the South.

Philadelphia is the most ancient seat of medical learning in this country; and her first professors, educated in the best schools of Europe, and possessed of emminent talents and indomitable energy, were *just the men* to lay a broad and solid foundation for the true principles of medical science in Young America. The names of Rush, Physic, Wistar, and Dorsey, will be venerated while medicine shall be ranked among the *honorable pursuits* of man; and it will not be termed *adulation* in one who knows them only by reputation, to say, that those who now occupy their stations, some of whom are ripe in years, and verging toward the tomb, have nobly trodden in the footsteps of their *illustrious predecessors.* The accomplished *Alumni* of this ancient school, are scattered throughout this broad land. To meet the wants of a population that has increased with unparalleled rapidity, they have established medical colleges, far and near, from the immediate *shadow of its wings* to the distant recesses of *the far West;* still *old Pennsylvania* stands pre-eminent, and derives reflected glory from her numerous offspring. I walked through her spacious lecture rooms, so recently crowded with eager listeners, and mused on the many interesting associations, connected with her now silent rostrums and vacant benches. Here are annually proclaimed the results of experience, of extensive research, and profound meditation, and here too, attachments and friendships are formed, and *scenes* witnessed, which mark an interesting epoch in the lives of hundreds of physicians.

I find I have taken up the Medical School of Pennsylvania University sooner than I had intended in the course of my letter, but as strict attention to order is not to be expected in a familiar letter, I will go on at once, with what I have to say on this topic, and then proceed to other matters.

As I remarked, the lecture rooms in this college are spacious; and are furnished with the same kind of plain hard benches, that try the stamina of the *nates and spinous processes* of medical students in other similar institutions. A little more comfort might be easily added to these halls, but I am told it is altogether forbidden by the known *destructive propensities of young Americans.* The dissecting rooms are large and commodious, and contain a great number of tables. The museums, as well of Natural History, as of Anatomy and Pathology, contain a large collection of interesting specimens. Of the professors, I had the pleasure of meeting with Drs. Jackson, Wood, Hodge, and Horner; Drs. Chapman and Gibson were absent from the city. Prof. Wood did me the kindness to show me his botanical garden at his residence, which contains a variety of rare medicinal plants. They have been collected at considerable expense, but being in the heart of the city, they are necessarily too much crowded for a handsome display. I had some long conversations with Prof. Wood, concerning the principal remedies used in Southern practice, and am under many obligations to him for the polite attentions. I saw so little of the other gentlemen named, that I will only mention the *favorable impression* produced by a brief interview. They made special inquiry about the late medical convention, and I took occasion to remind them that their declining to attend, had kept away a great many representatives. If I am not mistaken, they are now more favorably disposed towards the undertaking, and I am induced to hope they will cheerfully co-operate in the proceedings of the next meeting.

If the Professors of this, and the Jefferson School, should authorize a declaration to this effect, it will doubtless go far to insure the success of the enteprize. And why should they not? They occupy the very front rank amongst the medical institutions of the country, and are certainly as well prepared as any others, to maintain as high a standard of qualification as the great body of the profession would desire. These prominent and influential schools should take the lead in medical reform; all others ambitious of honorable standing will co-operate; they will be sustained by the profession at large; and the schools which do not come fully up to the standard, will at once be assigned an inferior position in medical society.

The Jefferson Medical College. Of the Professors in this institution, I had the pleasure of being introduced to Drs. Dunglison, J. K. Mitchell, Pancoast, Mutter, and Huston. They are all gentlemen of high professional standing, and most kind and courteous in their attention to strangers. Their college building is now being remodeled and enlarged, and as every thing about it is in confusion, I had but an imperfect glance at a small portion of their fine museum. The professors of this comparatively new school, by their ability and untiring energy, have raised it to the first rank among American medical colleges. I believe their class last season was the largest in the Union. It was most gratifying to me to discover, that notwithstanding that spirited rivalry, which has existed between this and the old school for years past, the respective professors, with but one or two exceptions, maintain between each other the most kind and friendly relations. Here is an example, which, for the benefit of the profession, it were well to see imitated throughout our country. I have as much reason to hope that this school will unite in the next medical convention, as the one just mentioned.

I made the acquaintance of some of the professors in both of the other medical colleges of this city. They are men of talent, and only require time to enable them to build up most respectable institutions.—Philadelphia is certainly well supplied with medical schools—some may think *too many*, but I am not satisfied that it is the case in a place possessing the advantages of this, provided the standard of education be uniform, *and sufficiently elevated.* I cannot but think there are serious obstacles and disadvantages necessarily attendant upon very large classes; but I will not dwell upon the subject in this place.

I must be permitted to express my grateful acknowledgements for the marked kindness and polite attention shown me by our editorial brethern, and by the attending physicians, and surgeons, of the different hospitals, which I visited. They did every thing in their power to facilitate my inquiries, and to extend my acquaintance among the members of the faculty, a brief intercourse with whom served to satisfy me in regard to the well-grounded and elevated standing, which our profession has always maintained in this enlightened city.

Pennsylvania Hospital. This venerable institution was established in 1752. Since its foundation it has entertained something upwards of 43,000 patients, a little over one half of which have been "maintained and treated at the expense of the institution." It now receives about 1,000 patients annually, three fourths of whom are poor, and one fourth pay. The average number in the house is about 100. I visited this hospital twice; once I passed only through the surgical wards, with Dr. G. W. Norris; and the second time, through the whole house with Drs. Norris, Pepper, and Hodge. Dr. Norris is an accomplished surgeon, and a kind hearted, and most amiable gentleman. I could but admire the gentleness with which he addressed and handled his patients. He showed me a number of fractures, in which his dressings were very simple and appropriate. In a very bad compound fracture of the leg, which was suppurating freely, he enveloped the limb in dry wheat bran, and then enclosed it within a light fracture-box—thus the limb was kept cool, maggots kept away, and the broken ends of the bones held in apposition. I saw Dr. Norris perform the old operation of injecting port wine into the *tunica vaginalis* for hydrocele. It was the second time on this man. Dr. N., observed that he frequently found this affection quite obstinate.

At a subsequent visit, I went through the medical wards with the visiting physician, Dr. W. Pepper. Dr. P., was very careful in his examinations, and made some clinical remarks to about 20 students, who were in attendance. About the same number of students attended Dr. Norris' visits.

Dr. Hodge, conducted me into the Lying-in department, to witness a case of *double hare-lip*, in an infant recently born. It was a horrible deformity. Not more than forty infants are born in this department during the year. Every thing about the Pennsylvania Hospital is very plain, but sufficiently comfortable. I had expected to see a much larger establishment. The grounds are well shaded with large trees, and highly improved. This is now the chief, if not the only hospital for clinical instruction in Philadelphia. How greatly inferior to the New Orleans Charity Hospital, in point of number of patients!

Blockley Alms-House. This extensive establishment is located across the Schuylkill river, some two or three miles from the centre of the city. The institution is devoted to the following purposes, viz: 1st, a hospital far all diseases, except small-pox; 2nd, a lunatic asylum for destitute persons; 3d, a retreat for paupers; 4th, an asylum for destitute children. It is supported at a vast expense derived chiefly from a poor tax. The average number of inmates is nearly 1600; the most of these, however, remain a long time; the admissions and discharges per annum being only a little upwards of 3,000 each. I was informed by Dr. Henry S. Patterson, the attending physician, that the number of sick in the hospital, during the month of April last, was 328, which he thinks is about the average number for the year. This you will recollect is about 100 short of the average number in the New Orleans Charity Hospital. I learn from the "Annual Report of the Board of Guardians for the Relief and Employment of the Poor, in Philadelphia, &c," for 1845, that upwards of 3,000 out-door patients were attended by the physicians of the Board, and furnished with medicines. The average number of lunatics in the Blockley Alms-House is about 243; and of children, about 89. A large farm is attached to the establishment, the proceeds from which, and from work done in the house, form a considerable source of revenue. I walked through the extensive wards, under the polite guidance of Dr. Norris and two of the resident students, and found every thing in good order. The lunatics were generally of the lowest class, and for the most part in a hopeless stage.— The children appeared to be well provided for, but some of them were very pale and infirm.

Dr. Patterson was absent on our arrival, but came in before we left, and was very polite in his attentions. He is the Medical Superintendant, and is assisted by eight young graduates, who are elected for a term of two years. They are required to pay an *admission fee* of seventy-five dollars, which is refunded if they stay out the term; otherwise it is forfeited. These young gentlemen have to pay two hundred dollars a year for their board, in the hospital. Here again, we perceive a great superiority in the facilities afforded medical students in our hospital, where *ten are admitted, board and lodged, free of expense.*

On account of some difficulty which occurred last year, I believe, the professors and students of the Medical Colleges are not permitted to attend this hospital. From its distance, it must have been always attended with great inconvenience.

The Pennsylvania Hospital for the Insane, situated about four miles from the city, is, perhaps, not excelled by any institution of the kind in America. We gave, in a late number of our Journal, so minute a review of the last annual report from this hospital, that I shall now pass over its interesting statistics. I must be permitted, however, to express my admiration of its beautiful location, its commodious buildings, and its judicious management under the superintendence of Dr. Kirkbride. Dr. K. had just returned from a Convention of Physicians of Lunatic Asylums, recently held in the city of Washington. I found him to be a dignified and courteous gentleman; and I should think, every way qualified for the station he now occupies. Of this, however, his annual reports give the most satisfactory evidence. He showed us through the entire establishment, and I must confess, that I never saw anything superior to the neatness and order that everywhere met the eye. It was a fair and pleasant evening, and the utmost stillness prevailed. A few individuals might be seen quietly sauntering about the walks, playing a game of cards, or busily engaged in the work-shop; but, without being informed, I should not have suspected them to be lunatics. I met three ladies at the gate, returning from a promenade, one of whom I was told was a lunatic, and the others her attendants; but they all looked equally sane to me. The grounds around the hospital are extensive and highly improved. They are enclosed with a high brick wall, but this is run along the undulations of the ground, so as to be almost entirely out of sight from the house ; and the whole establishment looked far more like some delightful summer retreat for spending leisure hours, than an asylum for lunatics. The hospital can entertain, suitably, about 180 patients, and a large number of insane are here cured every year. It contains the best class of patients ; some of whom, being incurable, have been in the hospital for thirty or forty years ; indeed, no one after seeing this institution, could feel the least repugnance at sending here a friend or relative, who has the misfortune to be insane. The annual reports from this institution are exceedingly interesting, and should be extensively circulated, as insanity seems to be rapidly increasing in our land.

Wills's Hospital for the Blind and Lame.—This is a very neat little institution, founded for benevolent purposes. Dr. Fox is the visiting surgeon at this time ; he kindly showed me the patients, numbering about thirty, laboring under different varieties of ophthalmia. Dr. Isaac Hays and Dr. Bell are also visiting surgeons to this institution. The former is considered an able occulist, and the latter has recently published an interesting work on the eye, which was noticed in our Journal.

Academy of Natural Science.—Drs. Norris and La Roche did me the kindness to accompany me to this institution, where I had the pleasure of being introduced to Dr. Samuel George Morton, a gentleman who you know has attained eminent distinction in a new and interesting department of science, termed *ethnography.* I found Dr. M. an agreeable and courteous gentleman. He showed us his large collection of human skulls, containing specimens of every race, some well preserved *mummies*, and a fine collection of Natural History. The Academy is a handsome building, having ample space for the display of specimens, and a neat lecture room. I regretted not being able to see more of Dr. Morton, a man who has done his country honor, by his profound scientific researches.

Among other interesting places in Philadelphia, I visited the rooms of the American Philosophical Society—the House of Refuge for Juvenile offenders, and the penitentiary; but as these do not come strictly within the scope of *medical affairs*, I will not trouble you with an account of them.

Medical Clubs.—Dr. John Bell did me the kindness to take me to a meeting of his club, at the house of Dr. Henry Bond. There we met Prof. Wood, Dr. Jackson of Northumberland, Dr. Coates, and some others.

This club is composed of some ten or a dozen physicians, who meet together every week, at each others houses alternately, for the purpose of spending a social evening. Its object is the promotion of kind and friendly feeling among the members, rather than the discussion of medical subjects ; the latter indeed being rarely indulged. It is distinctly understood that the entertainer is to put himself to *as little trouble and expense as possible.* Accordingly, only a cup of tea, with other light refreshments are expected. If it should be inconvenient for a member to entertain in his turn, he apprizes the next in rotation, who informs the members where they are to meet. We spent a delightful evening with Dr. Bond, who is an experienced and most respectable practitioner. I was invited to meet another club at the house of Dr. Stewardson, but had to deny myself the pleasure, as I left the city the same evening. I was informed the organization of this club is similar to the one just mentioned. These economical clubs have gradually supplanted the ancient *Wistar Parties*, which I understand have been nearly abandoned on account of the extravagance they fell into. Here are to be seen united in the most friendly relationship, the professors of the rival medical schools, and the respectable practitioners of the city, and I can imagine nothing better calculated to foster a generous *esprit du corps.* Why do we not have something of the kind in New Orleans? And not only there, but in every city and village throughout the country? There is scarcely a respectable village in the South, which does not contain at least half a dozen clever physicians, either within its precincts, or its immediate vicinity; and no one can doubt the happy influences that would arise from their assembling once a week at each others houses, or offices, in a friendly and social manner. It is only necessary for some worthy member of the profession at each place, to make a move in the matter, and let *rigid economy* be the fundamental principle of the associations. I am convinced that the general practice of our profession is *too private and retired*—temptations to do wrong, and opportunities for concealment are so frequent, that to resist them always, is more than can be expected of poor frail human nature. Let every thing be brought to the light of day, and a thousand little meannesses that now skulk in darkness and degrade the profession, will be abandoned forever.

I will here close my observations on the medical affairs of Philadelphia. I am aware that they are very imperfect, and that I have not seen half that is worthy of being seen and noted in this delightful city. I regret particularly that my hurried visit did not allow me a further enjoyment of the society of her distinguished and hospitable physicians. But I fear I am again trespassing on your patience, and will conclude with the declaration, that the general bearing and conduct of the medical faculty of Philadelphia, is a pattern for the profession throughout America.

Yours, respectfully, E. D. F.

Baltimore, May 25th, 1846.

Messrs. Editors:—I arrived in this city on the morning of the 23d inst., and having been politely called on by Professors Charles Bell Gibson, and H. W. Baxley, of the Washington Medical College, whose acquaintance I had made at New York, I sat out at once in search of colleges, hospitals, and every thing of interest relating to our profession in Baltimore. Dr. Gibson first took me to the Penitentiary, of which he is the attending physician. He there prescribed for some 20 patients in hospital, and 8 or 10 going about. He showed me a horrible cancer of the penis, extending upwards to the pubis and groins. Several of the cases of acute disease seem to have arisen from fatigue and exhaustion. The convicts here work hard about 14 hours a day. The principal employments are weaving cloth and making shoes.

We then went to the *Medical College of Washington University;* which is a neat building situated on a beautiful eminence, but I should think rather remote from the business part of the city. There are two lecture rooms, and neat dissecting rooms, besides several wards appropriated to invalids, and some neat rooms for the accommodation of students. There were eight or ten patients, presenting nothing of extraordinary interest. This institution is yet in its infancy, but promises to become one of the most respectable in the country.

We next went to the *Medical College of the Maryland University*, but could not gain admittance on account of the absence of the Janitor. Near this is the *Baltimore Infirmary*, a neat little hospital exclusively under the direction of the medical faculty of this University, two of whom are in daily attendance in the medical and surgical departments. There are five large public wards, and as many private; the whole capable of about 100 beds. The terms of admission into the *public* wards are three dollars per week; in the *private*, four dollars in summer, and five in the winter. Charity patients, and small-pox, are alike excluded from this hospital. One ward is appropriated to United States Marines. The average number of patients is about *fifty*, most of whom are slaves. During the winter, clinical lectures are delivered twice a week, by the professors of surgery, and the theory, and practice. Here I again met with the benevolent *Sisters of Charity* so familiar to us in New Orleans; they have charge of the domestic management of the house, and every where may be seen evidences of their customary neatness and cares.

Alms-House of Baltimore City and County.—Dr. Baxley, who is one of the trustees, was kind enough to take me to this magnificent institution, situated about three miles in the country. There I found most beautiful and extensive buildings, attached to a farm of several hundred acres. This establishment is devoted to the purposes of a retreat for paupers—an asylum for orphans and poor lunatics, and a hospital. There are usually about 550 inmates, who, like those of the Blockley Alms House at Philadelphia, generally remain a long time.—The annual admissions are about 1500, and the discharges about the same. We learn from the last annual report, that there were 1802 medical and surgical cases treated in the Baltimore Alms-House during the year 1845, of which 204 died, and 261 remained on the sick-list, at the end of the year. There were 28 children born in the Lying-in department. The total number of surgical operations was 31. Here is certainly a very respectable field for medical observation. There are two attending physicians. Eight medical students are permitted to remain in the institution, who each pay three hundred dollars per annum, which covers all necessary expenses. It is a beautiful place to study medicine. I did not have the pleasure of meeting with either of the attending physicians.

Baltimore seems to be eclipsed at present, in point of medical classes, by several of her sister cities, yet she is entitled to the honor of having furnished them with some of their most distinguished professors. I regret very much not having been able to see more of the physicians of Baltimore, as I am informed that the profession occupies a high standing there, and that some of the members do as large and lucrative a practice as is done any where in the United States. I shall long remember the civilities of Professors Baxley and Gibson.

I will here close my observations on the *medical affairs of the North.* Indeed, Baltimore claims alliance with the South, and we certainly should feel proud of the connection, but writing as I now do for the *extreme South,* I have thought proper to associate her with the three other large Northern Cities. As I expect on my way home to visit the medical colleges and hospitals of Washington City, Richmond, Charleston, and Augusta, I will perhaps give you some observaions on medical affairs of the South, at some future time.

Yours, respectfully, E. D. F.

Concluding Remarks.—The foregoing letters were written whilst I was away, and forwarded for publication in the last number of our Journal, but they did not arrive in time. Although they have lost something of their novelty by the delay, it is still hoped they may contain matters of interest to many of our readers, and are therefore given, now, for what they are worth. I had hoped, on my journey back to New Orleans, to spend at least one or two days at each of the Southern cities, that have medical colleges, for the purpose of examining their institutions and making the acquaintance of the professors ; but, in the sequel, I found it out of my power ; and had, therefore, to give them only a cursory examination. I stopped at Washington, Richmond, Charleston and Augusta, only long enough to take a hasty glance at their college buildings, libraries, museums and hospitals ; and to have a short conversation with one or two of the professors, at each place. I found the buildings everywhere very fine, and admirably adapted to their purposes—the lecture rooms spacious and comfortable—the libraries and museums not large, but select, and annually increasing—their hospitals very convenient, but rather deficient in patients. I am indebted to Professors Johnston of Washington, Cullen and Maupin of Richmond, Frost of Charleston, and Paul F. Eve, of Augusta, for their kind civilities.

I spent a day very pleasantly with our intelligent medical bretheren in Mobile. I know of no city the size of this, that possesses an abler medical faculty; and they show the best evidence of a proper regard for the true interests of the profession, by shunning, as they do, the rage for establishing medical colleges, that seems to pervade the whole country. They know that a fair compensation is not to be obtained for *properly qualifying* a small class of young men for the degree of Doctor of Medicine—they could not reasonably expect to get a large one, amidst the multitude of schools—and they, therefore, commendably devote their whole time to the study and practice of their profession.

Few men, at this day, possess the necessary qualifications for attaining distinction as teachers of medicine ; and but few places command the requisite facilities. Medicine is a science of observation, so far as it is a science at all ; and cannot be taught, either by lectures or books. Disease must be *seen*, ere it can be known, and it is all folly for students to pay professors large sums for giving them brief and imperfect abstracts from popular authors, whose works might be read with at least equal, if not greater advantage at home. We hold it to be utterly impossible for an able physician to impart by words his skill and discrimination in the treatment of disease, in all its protean forms. It can only be done, and then with difficulty, at the bedside of the patient. Hence the great value of clinical instruction, and its indispensible necessity to the student. He may read books and hear lectures till doomsday, but it will only qualify him to *guess* at disease. Actual observation is indispensibly necessary, and this he must attain either at school or in the field of practice, before he can have any confidence in his own judgment. But the time is at hand, when the whole subject of medical education must be thoroughly investigated ; and the most beneficial results may be expected to follow. I shall not attempt to draw a parallel between the comparative advantages of the Northern and Southern Schools of Medicines ; they all require considerable reformation in their courses of instruction, and their requirements for the *degree.* Young men, calculated to make good physicians, are annually turned out from every college, and they will do themselves and their profession credit if they have the opportunity ; whereas, the most distinguished teachers in the world will fail to make able physicians of all young men who take a fancy to the profession. This is impossible, from the very nature of the *materiel.* It is undoubtedly true that the very best order of students, young men of the best preliminary education, generally go the Northern schools at this day, and they do so on account of the ancient and high reputation of those schools. I am inclined to think that this is the only satisfactory way to account for the apparent superiority of their graduates ; but the truth is, the standard of requirements should be *elevated* and *uniform* throughout the country, and none should be admitted to the *degree* unless their qualifications come fully up to it. But this is not the proper place for the discussion of this subject, and I will close my remarks by observing that whilst the physicians of the Northern cities are perhaps generally better informed in regard to the science and literature of the profession, I would be very unwilling to admit that they command or exercise greater skill in the treatment of diseases.

E. D. F.

New Orleans, July, 1846.

Part Second.

REVIEWS AND NOTICES OF NEW WORKS.

I.—*Lecture on Animal Magnetism.* Delivered (by request) before the *Mobile Franklin Society.* By J. C. Nott, M.D. Charleston: Burgess & James, 1846.

[In the course of lectures delivered last winter before the Mobile Franklin Society, it may be necessary to state, the choice of subject was, in all cases, left to the lecturer.

The lecture on Animal Magnetism was published in the "Southern Journal of Medicine and Pharmacy," and afterwards in pamphlet form.]

There is a class of books so completely beyond the pale of critical examination, that they safely bid defiance to it;—occupying that place which is said to intervene between the *sublime* and the *ridiculous* they are secure from either. To this class, we are of opinion fairly belongs the "*Lecture on Animal Magnetism*;" and we notice it more from a desire of exhibiting to the reader one of the medical curiosities of the day, than the hope, much as we desire it, that his opinion of our profession will be elevated by either the *tone*, the style, or *logic*, of the "Lecture."

The lecture commences with an anecdote of "a worthy old gentleman," of the Doctor's acquaintance, who could not believe in the earth's diurnal rotation, because "his mill pond was still full of water every morning;" the laws of gravity (continues the Doctor) to him, like those of mesmerism to many now, were all folly and falsehood."

With this *reasonable* and *philosophic* analogy, the Doctor proceeds to express his regret, that "mesmerism instead of falling where it belongs, into the hands of the medical profession, which alone is capable of investigating it, has unfortunately, from Mesmer to the present day, been confined to ignorant and dishonest charlatans.

We had heretofore thought, and the reader is probably of the same opinion, that mesmerism *had* received a due degree of attention from the profession; but whether it has, or has not, we do not consider it necessary to argue the point just now. A large portion of the lecture is devoted to the proceedings of the French academy, in relation to this subject, during the different periods animal magnetism came before it, and from a full examination of which the Doctor arrives at the following conclusion:

"Now I assert, and can prove, that if the *commissions* of the academy have done any thing towards deciding the question, the weight of their authority is largely in *favor* (the italics are the doctor's) of mesmerism."

Much argument, and copious extracts from the report of the commission of 1825, follows, by which the doctor shows, at least to his own satisfaction, the truth of his position.

Of the first commission appointed by the academy, he speaks as follows:

"The first commission appointed by the Academy was in 1784, in the time of Mesmer, when animal magnetism was presented in a *very different shape from what it has now assumed*, and we are told that the commission of which Dr. Franklin was a member, condemned mesmerism. It is true that that commission did decide against it, but *admitted the reality of many of the phenomena* which were attributed to the influence of the imagination. Dr. Franklin was ill at the time, and did not witness the experiments."

Now, if men who lecture and write on animal magnetism, were compelled to deal in matters of fact, we fear the doctor in the above extract would be found guilty of "*suppressio veri.*" He asserts that Dr. Franklin was ill at the time and did not witness the experiments!—This too from a writer who professes to be *au fait* with the proceedings of the academy and French medical literature!

(The reader will easily conceive the effect, Franklin's name is calculated to have on a mixed audience listening to the marvels of mesmerism.) If, however, Dr. Nott is by any possibility amenable to ordinary criticism, the following quotation will place him in no very enviable position.

"Some of the most interesting of these experiments (of the commission) were performed at Passy, at the residence of Dr. Franklin, who could not be present at Paris, at the public exhibition. Here M. Deslon, (who succeeded in having the commission appointed,) tried his art in vain upon the obdurate American, as well as upon the members of his family, who, notwithstanding that some of them were ladies in delicate health, were found quite insensible to the whole ceremonial of animal magnetism. Neither of the other commissioners could perceive any effect in his own person. One of the experiments made at Passy, is worthy of a particular recital. It consisted in the magnetizing of a tree in Dr. Franklin's garden. M. Deslon affirmed that if this was done by himself, and a youth introduced who should be purposely selected as an individual susceptible of the magnetic influence, the result would be manifest on his approaching the particular tree."

A boy, aged twelve years, was chosen by M. Deslon, who insisted on the necessity of his presence and co-operation. Care, however, was taken to prevent collusion. The boy was made to approach four trees successively, without knowing which was the magnetized one, having his eyes covered with a bandage, and to embrace each tree for two minutes, according to the previous arrangement with M. Deslon. That gentleman stood in the garden, and kept his cane pointed at the mesmerized tree, in order to maintain its magnetism. Under the first tree not magnetized, at the end of a minute, the boy perspired in great drops, coughed, expectorated, felt a slight pain in his head; he was then twenty-seven feet distant from the magnetized tree. Under the second tree, he felt stupor and the same pain in his head. Under the third tree, these symptoms were greatly increased; he believed himself to be approaching the magnetized tree; he was, however, then at the distance of not less than thirty-eight feet from it. Under the fourth tree not magnetized, at the distance of twenty-four feet from the magnetized tree, the young man fell into a crisis. He lost all consciousness, was carried to a neighbouring grass plat, when M. Deslon soon reanimated him. The operator accounted for this untoward phenomenon, by saying, "that the trees had probably become spontaneously magnetic." "But," rejoined the commissioners, "if trees are in the dangerous habit of assuming this state of their own accord, a susceptible person walking in a garden must incur the continual risk of falling into a crisis."* So much for Dr. Franklin and (we ask pardon for the conjunction) Dr. Nott.

We will now endeavor to lay before the reader, the author's views and opinions of Animal Magnetism; and this, we assure the reader, owing to the discursive, ill arranged (*rudis indigestaque moles*) manner in which the subject is discussed, is no easy task.

In order to fully appreciate the value of the following extracts, it is necessary to state, that although the author professes to be a "tyro" in the investigation, he admits that his "experiments have been continued for three years!"

"There is however, truth, and useful truth in mesmerism, and there is no man of well-balanced mind who will take the trouble to inform himself and make experiments in a proper manner who will not come to this conclusion."

* * * * * *

I have not been able to produce on my subjects *clairvoyance* and some other of the more remarkable phenomena, and it would be presumption in me to form an opinion on these *facts*, which men of high intellectual and moral standing assert to be true; but, that by certain *manipulations*, a peculiar sleep, catalepsy, various degrees of insensibility to pain, the relief of many neuralgic pains, and contraction or relaxation of muscles at pleasure, may be produced in a considerable number of individuals, there can be no doubt. I must, however, in candor acknowledge, that the testimony is so multiplied, and of such respectable character, to establish the point, that I am forced to believe, that persons, in a mesmeric sleep, do sometimes see objects with closed eyes, as we have every reason to believe natural sleep walkers do.

The following, then, are the facts which have presented themselves in my experiments. In a great many persons, particularly those of *robust* appearance and health, no appreciable effect has been produced; in others, heaviness or drowsiness is felt without sleep; in more than fifty persons of *temperaments more or less nervous*, besides muscular twitching, &c., sleep has been induced in from one to ten minutes, but varying greatly in intensity and duration,—in the majority of cases this sleep comes on in less than five minutes, and cannot therefore be attributed to fatigue; in some, the sleep will continue only ten, fifteen or twenty minutes, and then pass off spontaneously, or the patient is aroused by a word or a touch; in others the sleep will be profound for many hours, but they may be easily awakened at any moment by reversed passes.— * * * I have again and again taken not only young ladies † and gentlemen

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* *Vide*—Dr. Pritchard on Insanity, Encyclopœdia of Practical Medicine. Article—*Somnambulism.*

† Teeth have been extracted from nervous young ladies, mesmerized by the doctor. This fact can be attested by Dr. Parmly, one of *our first dentists*."—(Page 27.)

of the first circle in Mobile, but have taken children of 10 or 12 years old, and negroes who have never witnessed or heard of mesmeric experiments, and produced the mesmeric phenomena—they have been put to sleep in from two to five minutes—cataleptic state of the muscles was produced—insensibility to pain—contraction or relaxation of a limb according to the direction of the passes, and the subject then waked up by reversed passes without touching.—Now in all this I can see no possible ground for attributing these effects to the imagination.

Influence of the Will.—I have never in my own experiments, seen any influence of my *will* on a subject. I haved tried it repeatedly, but without effect.

Manipulations.—Being seated facing each other, I take hold of the patient's hands and request him to look steadily in one of my eyes. I look steadily at him in the same way, and fix my *will* by saying mentally *sleep, sleep, sleep.*—This is continued for a few minutes, and if the subject is susceptible, I soon perceive twitching of the muscles, a change in the expression of the eye, and a disposition in the lids to droop. When this is observed, I disengage one of my hands and lay it on the top of the head, and continue my gaze for a few seconds longer, when the eyes close. I next, to deepen the impression, make passes (in contact) down the sides of the head and face, and over the arms to the ends of the fingers. The whole process does not occupy more than from two or three to ten minutes; and in those of very susceptible temperament who have been mesmerized a few times, a few passes in the front of the face without touching, or simply standing off several feet, and looking the patient for a minute in the eye, will produce the full effect. Whether the phenomena be the off-spring of the imagination or of some subtle agent, is perhaps immaterial."

One of the chief arguments used in favor of the probability of animal magnetism, is derived from the phenomena exhibited by electricity, galvanism, and magnetism, in the various forms of *matter*. This point is discussed in the lecture with much plausibility. We make the following extract:

"One of the most interesting and curious experiments is that of Dr. Gibbes, showing that the human body is magnetic. The fact bears strongly upon our subject, and I will give the experiment in the doctor's own language:

"I procured," says he, "a long delicate magnetic needle, made a strong effort as if throwing off something from the fingers, and brought them carefully to the needle, avoiding to procure vibratian of the air, and to my satisfaction, *I found my right hand repel the North Pole.* I repeated the experiment and found it to *attract the South Pole;* proving North Polarity in that hand. I now tried the left hand and found it to exhibit the opposite polarity, &c."

We would be delighted to witness the highly gifted Gibbes perform this truly wonderful experiment. Making "a strong effort as if throwing off something from the fingers;" bringing them "carefully to the needle," and all this without causing vibration of the air! Of the wonderful, most wonderful is animal magnetism! Dr. Gibbes knew a very respectable surveyor, whose body was magnetic; Dr. Nott cannot wear a watch, because he is similarly endowed. *

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* It is strange that Dr. Nott who displays deep research on the subject of magnetic attraction, should have omitted the description of an island long since discovered floating in the atmosphere. For the advantage of future lecturers, we subjoin a brief description of it. "By means of this load-stone, the island is made to rise and fall, and move from one place to another. For with respect to that part of the earth over which the monarch presides, the stone is en-

The author borrows some curious illustrations in support of his views from professional authority, *par example :* "No two human beings probably were ever created alike—they differ in external appearance—they differ in their intellectual and moral characters—they are differently affected by morbific causes, and *each has his peculiar nervous system,*" (the italics are the Doctor's.) Again. "Inocculate a dozen patients, with the virus of small-pox, and the phenomena which result will be as *opposite* as those exhibited in the same number of mesmeric subjects."

Indeed! We were under the delusion, and we believe it is still taught by professors of anatomy, that there is a *fixed* uniformity in the distribution of the nerves of the human body;—that although there is *occasional* variation in other structures, the blood vessels, for instance, such is never the case in the nervous system. So with the virus of small-pox, it has been held that, however variable its effects seemed in different persons, it was but a difference in degree; the effects of the virus being always the same; the same precursive fever, followed by the same peculiar eruption, &c.

The lecturer, we presume, viewed these thing at the time with a mesmeric eye; this seems exceedingly probable, for we are told in another place, that in mesmeric subjects "the nervous fluid seems often to be playing through the system with all the irregularity of an *aurora borealis!*"

But the crowning triumph of the "lecture," in our humble opinion, is to be found in that portion which disposes of the objections brought against mesmerism by the "*small craft*" and *soi distant* philosophers of the profession.

Here the Doctor so completely transcends all former logical efforts, that ordinary minds are struck aghast at the brilliancy of the new philosophy. In justice to the author, we give the extract entire:

"Here is the great argument which has satisfied the mind of many sensible and reflecting persons. How, it is triumphantly asked, can any man pretend to reconcile such glaring and palpable contradictions? I have often been amused at the self-satisfaction with which this argument is used, and the air of commisseration with which some of our *soi disant* philosophers have looked at me, when I had the obstinacy still to believe.

Nature, say these philosophers, works by fixed laws; there must be uniformity between cause and effect.* But this adage, like many others which are received as true without examination, is liable to so many exceptions, that it scarcely deserves to be considered a *general rule.*"

The Doctor proceeds to demolish the received physical law of *cause* and *effect* in the following manner:

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dued at one of its sides with an attractive power, and at the other with a repulsive. Upon placing the magnet erect with its attractive end towards the earth, the island descends; but when the repelling extremities point downwards, the island mounts directly upwards. When the position of the stone is oblique, the motion of the island is so too: for, in this magnet, the forces always act in lines parallel to its direction."—(*Voyage to Laputa:* GULLIVER'S TRAVELS.)

* That there is uniformity in the *relation* of cause to effect, we can easily understand: How there must be uniformity *between* cause and effect, passes our comprehension.

"If, for example, you take a small slender piece of steel, and draw over it from end to end a piece of loadstone several times, *always in the same direction*, it becomes magnetic, and exhibits polarity—if you take two pieces of steel, neither of which is magnetic, and rub them together for a time in one direction, the same effect is produced, but in a less degree—if you rub backwards and forwards, in neither case will the same effect be produced—if you heat a piece of iron and place it at the angle of the dip of the compass, and give it a single blow with a hammer, it becomes magnetic—place a bar of iron in a certain position and leave it so for a length of time, and it becomes magnetic without doing any thing else to it—heating to a red heat a piece of hard iron and cooling it suddenly, renders it magnetic, while on the other hand, heating a magnet destroys its power—passing an electric current through a bar of steel at a particular angle, will render it magnetic."

Truly, well balanced minds are not necessarily profound ones ; and should the *small craft* and *soi disant* philosophers of the profession affect to regard the above extract as proving the law (of *cause* and *effect*) rather than the contrary—the author of the lecture must ascribe it solely to their unpardonable ignorance of Animal Magnetism.

J. McN.

II.—*Fevers: Their Diagnosis, Pathology and Treatment.* Prepared and edited, with large additions, from the essays on Fever, in Tweedie's Library of Practical Medicine. By Meredith Clymer, M.D., Professor of the Principles and Practice of Medicine, in Franklin Medical College of Philadelphia, &c., &c. Philadelphia : Lea & Blanchard ; 1846 : 1 vol., pp. 604.

When a labourer in any department of science offers the results of his observation to the public, in the shape of a book, his work is a fair object of criticism and analysis, and it is apt to obtain due notice from the periodical press ; but here is a book made up of other books and writings already in the hands of the Medical Profession, and of course the American editor can justly lay claim to no higher merit than as a judicious compiler ; nor can he expect his work to be minutely analysed or reviewed in the journals of the day. In his brief preface he says, the object of the present volume is to supply a deficiency in medical literature occasioned by "the want of a distinct treatise on Fevers, embodying the received doctrines of their pathology and treatment." Now it would certainly be very convenient for reference, to have combined in a single volume all the important doctrines that have been advanced on any subject ; but we conceive this to be a task difficult to perform well, and we doubt not it is often viewed by authors as a very *ungracious one*. When a writer has devoted months and even years to the laborious investigation of an intricate subject, he may then find it a difficult task to communicate his conclusions, or all he wishes to convey, in a clear and intelligible manner. What then must be his concern when he sees the *compiler* take up his work, make a few brief extracts, attempt to represent in a *paragraph*, what he could not explain satisfactorily in *fifty pages ;* or perhaps *misrepresent* him altogether, and then stitch him up in company with some half a dozen other mutilated authors, and palm them all off on the market, in real wholesale style! Without intending to be by any means particularly censorious, let us take the work before us, which claims to give "the received doctrines" of about fifty authors on the important subject of Fevers; and let us suppose it possible to assemble all these authors, living and dead, in *solemn conclave*, and that Dr. Clymer and his collaborators of Tweedie's Library were summoned before them, to answer all their charges and complaints for the manner in which they are represented in this book. Think you not it would be an *august tribunal*; and that the *defendants* might well appear before it with *fear and trembling?* It is possible that the quoted authors might confess their obligations for the honour confered upon them, and for the justice and fairness with which they had been represented; but on the other hand, in as much as Dr. Clymer himself, who gave the finishing stroke to this work, openly avows (at p. 54,) that Dr. Christison has misrepresented M. Louis' views in regard to the proximate cause of continued fever, it is but reasonable to presume that many similar complaints would be entered up against the Doctor and his worthy associates. These reflections have been suggested to us by the great rage for *book-making* that seems to prevail at the present day. The press is annually teeming with original productions of greater or less merit, but these are hardly allowed time to be diffused amongst the reading community, before a new set of literary labourers, anxious to facilitate progress on the road to knowledge, take them in hand, pick and cull them over, winnow out the *wheat* from the *chaff*, and attempt to give us, in a new and condensed form, what they conceive to be *received doctrines*. This is a service plainly enough called for in regard to the *Periodical Medical Literature of the day*, and Braithwaite & Ranking annually supply interesting volumes by selecting from the mass of contributions to the Journals such articles as mark the progress of medical science. The authors of our "Dictionaries" and "Libraries of Practical Medicine" undertake the business on a more extended scale, but we cannot say they do it in an unexceptionable manner. We must insist that an able author, to be *fully* understood, must be *fully* read. But a great many books contain so little valuable matter in proportion to their bulk, that they are hardly worth the time and trouble necessary to examine them.

The foregoing general observations are not designed to prejudice the reader against the work now before us, for although not altogether free from objections, we do not hesitate to pronounce it a valuable collection of useful knowledge, and well adapted as a book of reference to the practitioner. The essays on Fever in Tweedie's "Library of Practical Medicine" were contributed by Drs. Christison, Shapter, Burrows and Gregory, all men of high repute in the profession. In their scope they "embrace the whole class of Idiopathic Fevers—continued, periodical, eruptive and puerperal." Dr. Clymer has made additions "amounting to about one half of the volume, chiefly with reference to the fevers of this country." His labours show him to be a man of extensive research, and of an acute and discriminating mind. The work consists of *thirteen chapters*, embracing the following topics, viz: *General Doctrines of Fever*, *Continued Fever*, *Typhoid Fever*, *Plague*, *Yellow Fever*, *Intermittent Fever*, *Remittent Fever*, *Infantile Gastric Remittent Fever*, *Hectic*

Fever, Small Pox, Measles, Scarlet Fever, and Puerperal Fevers. Under these different heads may be found a vast amount of well-digested and well-arranged matter. Within the extended limitsh ere admitted, Fever is unquestionably the *favourite method* used by the Arch-Destroyer for cutting the thread of human life. It is *the grand enemy* with which the Physician has to contend, and causes more deaths, perhaps, than all other diseases combined. Dr. Christison in his definition (p. 19, this work,) says, "the character of Fever is *mutable*, presenting ever-varying forms and varieties." He follows this remark with a quotation from perhaps the best general treatise on Fever that ever was written :

"Fever," 'says Southwood Smith,' is a genus consisting of several species, and each species presents many varieties. The external characters of the varieties, and the internal states upon which they depend, are so opposite, that no two diseases in any two parts of the catalogue of nosology present a more diversified appearance, or require a more varied treatment, than may be the case with two different types of fever. *The fever of one country is not the same as the fever of any other country; in the same country, the fever of one season is not the same as the fever of any other season; and even the fever of the same season is not the same in any two individuals.* Many of the circumstances which constitute these varieties in the fevers of different seasons and of individual persons, are slight and trivial; but some of them are of the greatest possible importance, and those diversities, especially, which distinguish the fevers of different climates, are intimately connected with the causes, whatever they may be, which render the disease mild or severe, and consequently comparatively innoxous or fearfully mortal."

We have taken the liberty to italicise a passage in this interesting quotation, because we have been *taken to account* by some of our brethren at the North, for asserting the *peculiarities* of the fevers and some other diseases that prevail in our own region. We shall always be happy in being able to support any position we may assume, by such authority as is here offered ; but the truth is, this proposition is so *reasonable*, and so *universally admitted* by all physicians who have come into this climate to practice, after pursuing the profession in others, that we can but express our astonishment at any exception having been taken to it by our enlightened brethren at the North. But more of this anon.

The "*nature of fever*" is very ably discussed in this work, and an excellent review given of the "*general doctrines*," that have been advanced at different periods. In regard to the *classification of fevers*, the simplest arrangement is adopted, consisting of

1. *Continued Fevers ;* 2 *Periodical Fevers ;* 3 *Exanthematous or Eruptive Fevers.*

The chapter on *Continued Fever* (by Dr. Christison) is probably the ablest in this work. We extract from it (p. 91) the following interesting remarks in relation to one of the most *curious and fatal* symptoms observed in the fevers of the South, and particularly frequent in Yellow Fever. We allude to *suppression of urine* :

" One of the most interesting facts connected with the pathology, was the discovery of urea in the blood, and serous fluid of the ventricles of the brain, in some of the patients affected with cerebral derangement. The suspicion of this morbid condition was suggested to Dr. Henderson, partly by the occurrence of convulsions in several cases in which there was no jaundice, and partly by a case in which symptoms of oppression and confusion of mind, accompanied with diminution of urine, was relieved by the occurrence of diuresis after the exhibition of diuretics. In two cases which subsequently occurred, exhibiting indications of cerebral oppression, the state of the urine was attended to and the blood analyzed. In both cases the symptoms came on after the critical sweat; and the urine was somewhat diminished in quantity, although not materially. In one case, the patient, after oppression, stupor, and a repetition of convulsive fits, died, and three drachms of serum from the ventricles of the brain, with some clots of blood from the head, were examined. Crystals of nitrate of urea were obtained in moderate abundance from the serum of the brain, and a very considerable quantity from the blood. In the other case, after somnolence, confusion and languor, blood drawn from the arm yielded crystals of urea in small quantity. The character of the symptoms which succeed *suppression of urine*, and the explanation afforded of such cases by the detection of urea in the blood, have been long known to the profession. The possibility that a similar condition of the blood might be found to occur in other affections, and might afford the true explanation of sudden death in diseases not expected to terminate thus, was suggested, we believe, by Dr. Christison. But we have here, for the first time, met with the realization of this conjecture, in the proof afforded by the observations of Dr. Henderson, that such an event may prove the immediate cause of death in cases of *fever;* and that, too, even in cases where the obvious cause which might give rise to the presence of urea in the blood—namely, the *suppression* of urine—was absent.

The facts elicited in these observations led to the further investigation of the subject in Dr. Henderson's wards, by Mr. M. W. Taylor; and in other two cases of fever—one of the epidemic, and one of typhus, both exhibiting the development of cerebral symptoms, with *diminution* of the urinary secretion—a circumstance otherwise indicating a favourable prognosis—urea was detected in the blood. "The existence of urea in the blood," says Mr. Taylor, (*Scottish Med. Gaz.*, p. 231,) in other cases has been inferred from the occurrence of those symptoms of disorders of the nervous centres, which we know to be the consequence of its undue accumulation in the circulation. These phenomena have been observed in those cases in which, from some cause or other, the daily discharge of urine has undergone material diminution. This appears to take place chiefly at that critical period of the fever marked by copious sweating, at which the febrile symptoms begin to subside, or during the apyretic intervals between the attacks. Professor Henderson was the first who drew the attention of the profession to the fact of the occasional presence of urea in the blood, at this stage of the fever, under the above-mentioned circumstances." The inferences deduced by Dr. Henderson from these observations are of great practical importance, and may prove to be so in relation to the treatment of other fevers besides the one under consideration."*

Dr. Christison does not believe in the practicability of arresting continued fever in its *incipient stage*, by any heroic treatment; though he only mentions among the *bold remedies* used for this purpose, *Emetics and Bloodletting;* the latter after the fever has been fully formed. (p. 203). Dr. Clymer says the same thing in his interesting chapter on *Typhoid Fever:* "There are no means by which an attack of Typhoid Fever can be cut short, or its duration even materially abridged. All treatment must be auxiliary, and should be directed towards the shelter, from serious harm, of the essential functions." (p. 256). They both seem to be totally unacquainted with the powers of the *sulphate of quinine*, given in *large doses*, with this view. We would respectfully refer Dr. Clymer to the papers of Professor Thomas D. Mitchell, of Transylvania University, and Dr. Charles McCormick, Surgeon U. S. A.,

* [British and Foreign Medical Review, vol. xvii. p. 190.]

published in this Journal. Our own experience does not authorise us to speak on the subject, though we are inclined to think the remedy well worthy the attention of the profession. To accomplish this purpose, less than 20 *grains* ought hardly to be thought of as a dose.

Dr. Shapter's essay on Yellow Fever contains a great deal of very interesting matter, but we have no idea that it would qualify even Dr. Clymer to come to the South and treat the disease successfully *at once.* There is one point in the pathology of this disease, too often overlooked or slighted by writers, but which we think is of *vital importance.* We allude to *the general suppression of biliary secretion* in all bad and fatal cases. We are happy to find the following quotations in regard to it, in this work: Dr. Gilkrest says of the liver, at p. 339: "Scarcely any trace of bile is to be found throughout its pores." Louis found the bile in the gall-bladder scanty, thick, and of a dark green colour. (p. 341.) Dr. Ashbel Smith says, p. 340: "In all cases there appeared to be a suspension of the biliary secretion; no bile could be squeezed from the substance of the liver." "Dr. Wurdemann observes: 'If I were asked what was the most prominent pathognomoinc symptom in Yellow Fever, one that most distinguished it from those cases of remittent bilious fever complicated with gastritis, so prevalent during epidemics of the former, I would answer, the total cessation, or the much diminished and vitiated secretion of bile. For although it is sometimes ushered in by bilious vomiting, the bile thus ejected has been mechanically forced from the gall-bladder, where it had been collected previous to the attack. It is very certain that the restoration of the function of the liver is the most favourable symptom in the course of the disease." (p. 349).

These remarks from different authors are strongly corroborated by Dr. Cooke of Opelousas, La., in a communication to the July number of this Journal, and they correspond entirely with our own limited observations. The fact of the general suspension of biliary secretion in bad cases of Yellow Fever, and the good effects of its restoration or continuance, would suggest to many physicians a resort to *mercurials*, the remedies *par excellence* for disordered livers, to answer the indication; but the evidence in regard to their efficacy, is of a most contradictory character—some condemning them *in toto*, whilst others extol them to the skies. We must think that this diversity of opinion arises from the *peculiarities* of epidemics, dependent on climate, season, locality, &c. Dr. Cooke found mercurials detrimental in the epidemics of Opelousas, "however used, whether in small or large doses, repeated or not." While Drs. Stone and Kilpatrick of Woodville, Miss., in the epidemic of 1844 at that place, found them of inestimable value. We think they are used with a *sparing hand* by the physicians of New Orleans and Mobile, at this day. Yet the pathological fact established by the quotations above, point out an important indication to be fulfilled in the treatment of Yellow Fever. If *calomel and blue pill* will not answer the purpose, it must be done by other remedies. *The liver must continue to act, or the patient will be in great danger of death.*

In regard to the general pathology of Yellow Fever, we are inclined to concur with those authors, Stevens, Imray, Nott, &c., who trace the chief evidences of diseased action to *alterations of the blood.* But here again we are met with the following forcible remarks of the great French pathologist, quoted in this work, p. 576: "Andral has truly and sagaciously remarked, that no line of demarkation can properly be drawn between the blood and the solids, and that physiologically speaking, it is impossible to conceive that one of these two parts of the same whole could be modified without the other being so likewise. There is no longer any meaning, he observes, in the disputes between the Solidists and the Humoralists; the system appears to constitute but one great whole, indivisible in the state of health, as well as in that of disease. The division is a distinction of small importance, and one that is not always just, since it ceases to exist in the intimate structure of the organs in which all the grand vital phenomena take place, and in which also occur all the changes that constitute the morbid state."

Dr. Clymer makes only the following brief allusion to the novel practice of giving large doses of sulphate of quinine in Yellow Fever:

"It has been the practice, within a few years, in some of the Southern and South-Western States, to give very large doses of the sulphate of quinine from the commencement of the disease. A scruple to a drachm was often administered at a single dose. What success has attended this plan of treatment, the writer has been unable to ascertain with any degree of certainty."

If our recollection serves us, the results were given with the announcement of the new treatment. They were certainly very fairly stated by our colleague Professor Harrison, in the November number 1845 of this Journal; but we presume they are not credited at the North. The facts are not very *numerous*, but certainly very *remarkable*, and well worthy of being mentioned in a book like this. But perhaps Dr. C. does not consider as "*received*" the "*doctrine*" that prompted the use of quinine in this way. For ourselves, we are convinced that the profession at large have *much yet to learn* in regard to the remedial powers of *large doses* of the sulphate of quinine, in the *incipient stages of both fevers and inflammations*. The observations hitherto made require farther confirmation, but they are already sufficient to authorise us, with many others, to anticipate from them *a resolution in Therapeutics*.

The chapter on Remittent Fever, we consider to be the most objectionable in the work. Dr. Clymer has added notices of *American observations*, but confined them chiefly to our Northern writers, and relating chiefly to the *pathology* of the disease. Of our Southern writers he only quotes Drs. Dickson, Boling and Wharton; of whom Professor Dickson is decidedly his favourite. He seems to have had before him the late admirable essay of Dr. Boling, (published in the April number of the American Journal of Medical Sciences,) but valued it only for its graphic description of symptoms. He says of it: "This is an excellent paper on the symptoms of Remittent Fever." Dr. Boling is not mentioned in *a single instance* under the head of *Treatment*. This is very extraordinary; for if Dr. Boling's paper possesses any uncommon merit, (and we look upon it as one of the most valuable that ever appeared in this country,) it certainly consists in the *novel*, *simple and efficient* plan of treatment which he recommends, and which he has *fairly tested by experience*. These views do not belong *exclusively* to Dr. Boling, nor are they entirely *new* to the practitioners of this region; but they are expressed in good style, and ought to be published throughout the world. We are greatly mistaken if the author of such an essay would appreciate *very highly* the compliment of being referred to *merely as giving an excellent account of the symptoms of a disease.* If the paper was worthy of being noticed at all, the *peculiar views* set forth in it ought surely to have been mentioned.

In regard to the general plan of treatment in Remittent Fever laid down in this work, we should think it would find but few advocates in this region. Dr. Shapter says (p. 444) "it is similar in its essential details to that pursued in ordinary continued fever"—and Dr. Clymer, (same page,)" the simple expectant plan is the one which has been generally of late recommended by those who have had much experience in the disorder."

We must here close our remarks upon this work, extended far beyond our original purpose. We consider this complimentary to the book however, and would certainly recommend it to all practitioners who have not access to good libraries. The succeeding chapters on the Eruptive and Puerperal Fevers are from able pens, and interspersed with valuable notes from the American Editor. E. D. F.

III.—*An Introduction to Entomology, or Elements of the Natural History of Insects, &c., with plates.* By Wm. Kirby, M.A., F.R.S., and L.S., &c., and Wm. Spencer, Esq., F. R. S. and L. S. From the 6th London edition, corrected and enlarged. Philadelphia : Lea & Blanchard ; 1846 : 8vo., p. 600.

This work, the first edition which was published in 1815, has gone through six editions, the present of which brings the subject up to the state of science at the present time.

Our authors introduce their subject with a kind of apology, which, as the work was intended for popular instruction, was perhaps not altogether inappropriate, as but few, excepting men of science. can appreciate the importance of studies of this kind. Thanks to the advances of modern science, no such apologies are requisite to our readers, who are aware of important results obtained by the accurate examination of the lower orders of animals, and of the light thus shed on the science of Physiology, and other branches of physical science.

The work embodies a vast fund of curious and interesting information, and will richly repay the reader for the trouble of a careful perusal. Many of the chapters may be transferred with propriety to our pages, but would occupy more space than can be given to a mere bibliographical notice. That portion which treats of the diseases and injuries which are produced by insects, is peculiarly interesting. Many of the facts mentioned are new to us. We are informed that some insects possess the faculty of giving powerful electric shocks like those of the gymnotus and silurus. "I can assure you upon good authority, that *reduvius serratus*, commonly known in the West Indies by the name of the *wheel-bug*, can, like them, communicate an electric shock to the person whose flesh it touches. The late Major Davies, of the Royal Artillery, well known as a most accurate observer of nature, and an indefatigable collector of her treasures, as well as a most admirable painter of them, once informed me, that when abroad, having taken up this animal and placed it upon his hand, it gave him a considerable shock, as if from an electric jar, which he felt as high as the shoulders."

In the portion of the work devoted to the luminosity of insects, we find some interesting facts recorded respecting this curious property.

"With regard to the immediate source of the luminous properties of insects, Mr. Macartney ascertained that in the common glow-worm, and in *Elater noctilucus* and *ignitus*, the light proceeds from masses of a substance not generally differing, except in its yellow color, from the interstitial substance (*corps grassieux*) of the rest of the body, closely applied underneath those transparent parts of the insects' skin which afford the light. In the glow-worm, besides the last-mentioned substance, which, when the season for giving light is passed, is absorbed, and replaced by the common interstitial substance, he observed on the inner side of the last abdominal segment two minute oval sacs formed of an elastic spirally-wound fibre similar to that of the trachæ, containing a soft yellow substance of a closer texture than that which lines the adjoining region, and affording a more permanent and brilliant light. This light he found to be less under the control of the insect than that from the adjoining luminous substance, which it has the power voluntarily extinguishing, not by retracting it under a membrane, as Carradori imagined, but by some instructable change dependent upon its will; and when the latter substance was extracted from living glow-worms it afforded no light, while the two sacs in like circumstances shone uninterruptedly for several hours. Mr. Macartney conceives, from the radiated structure of the interstitial substance surround the oval yellow masses immediately under the transparent spots in the thorax of *Elater noctilucus*, and the subtransparency of the adjoining crust, that the interstitial substance in this situation has also the property of shining—a supposition which, adverting to the luminous patch under its elytra, and the fact that the incisures between the abdominal segments shine when stretched, may probably be extended to the *whole* of the interstitial substance of its body. *—

* The following interesting facts, in addition to those of Mr. Macartney, have been observed by M. Morren, Professor of Botany in the University of Liege. The corneous transparent cap (*calotte*,) which covers the sac enclosing the luminous matter in each luminous point of the penultimate abdominial segment of *Lampyrus notiluca*, presents on its exterior surface a network of hexagonal facets, convex above and concave below, constituting an apparatus absolutely similar to that invented by Fresnet for increasing the diffusion of light, and when this exterior portion of the cap is removed, the luminous matter loses a great portion of its lustre, which mainly depends on this curious and beautiful contrivance for augmenting it. The central facets are larger and more regular than those of the margins, and each facet has in the middle a corneous hair bent backwards, which hairs M. Morren conceives are intended to prevent the adhesion of dust. The luminous masses contained in the two sacs are intersected in every part with a vast multitude of trachean ramifications, which compose also their common envelop, the whole proceeding from a largetrachea, which issues from a spiracle situated immediately at the side of the luminous mass, with which it communicates by a small round lateral orifice near the margin of this last; thus fully confirming the opinion of those physiologists who conceive that the luminous power under consideration is essentially connected with the act of respiration. In fact, M. Morren found that when the spiracle next to the luminous material is closed, the light is immediately extinguished, and re-appears when it is opened. If the luminous sac be removed with its accompanying trachea, it continues to shine; but if this trachea be taken away or compressed so as to hinder the access of air, the sac becomes obscure. This fact explains how, in the insects of the genus *Lampyris*, as well as those of *Elater* (*Pyrophorus*,) the light is not constant, but becomes more feeble at intervals, and why it is increased

What peculiar organization contributes to the production of light in the hollow projection of *Fulgora laternaria*, the hollow antenæ of *Pausus spherocerus*, and under the whole integument of *Geophilus electricus*, Mr. Macartney was unable to ascertain. Respecting this last he remarks, what I have myself observed that there is an apparent effusion of a luminous fluid on its surface, that may be received upon the hand, which exhibits a phosphoric light for a few seconds afterwards; and that it will not shine unless it have been previously exposed for a short time to the solar light.*

With respect to the remote cause of the luminous property of insects, philosophers are considerably divided in opinion. The disciples of modern chemistry have in general, with Dr. Darwin, referred it to the slow combustion of some combination of phosphorus secreted from their fluids by an appropriate organization, and entering into combination with the oxygen supplied in respiration. This opinion is very plausibly built upon the ascertained existence of phosphoric acid as an animal secretion; the great resemblance between the light of phosphorus in slow combustion and animal light; the remarkably large spiracula in glow-worms, and the decided connexion of their light with respiration; and upon the statement, that the light of the glow-worm is rendered more brilliant by the application of heat and oxygen gas, and is extinguished by cold and by hydrogen and carbonic acid gases. From these last facts, Spallanzani was led to regard the luminous matter as a compound of hydrogen and carbureted hydrogen gas. Carradori having found that the luminous portion of the belly of the Italian glow-worm (*Pygolampis Italica*) shone in vacuo, in oil, in water, and when under other circumstances where the presence of oxygen gas was precluded, with Brugnatelli, ascribed the property in question to the imbition of light separated from the food or air taken into the body, and afterwards secreted in a sensible form.† Mr. Macartney having ascertained by experiment that the light of a glow-worm is not diminished by immersion in water, or increased by the application of heat; that the substance affording it, though poetically employed for lighting the fairies' tapers,‡ is incapable of inflammation if applied to the flame of a candle or red-hot iron; and when separated from the body exhibits no sensible heat on the thermometer's being applied to it—rejects the proceeding hypotheses as unsatisfactory, but without substituting any other explanation; suggesting, however, that the facts he observed are more favorable to the supposition of light being a quality of matter than a substance.§ Lastly, Dr. Todd finding that the luminous substance of *Lampyris* continues to shine when detached, sometimes for a longer and at others a shorter period, but never exceeding twenty minutes, and that under

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during the flight or other energetic movement of the insect, and diminished when it is in repose. It is, in fact, always in proportion to the energy of the respiration of the insect, which, having the power of opening or closing its spiracles at will, can thus also increase or diminish its light at pleasure, though whenever it respires it cannot prevent it from shining. Some differences excepted, the luminous apparatus *Lampyris splendidula* is similar to that of *L. noctiluca* above described; and it is probable that a similar organization exists in the genus *Pyrophorus*.

* *Phil. Trans.* 1810, p. 281. M. Macartney's statement on this point is not very clear. He probably means that the insect will not shine in a dark place in the *day time*, unless previously exposed to the solar light: for it is often seen to shine at night when it could have had no *recent* exposure to the sun.

† *Annal. di Chimica*, xiii. 1797. *Phil. Mag.* ii. 80.

‡ "And for night-tapers crop their waxen thighs,
And light them at the fiery glow-worms' eyes."

§ Some experiments made by my friend the Rev. R. Sheppard on the glow-worm are worthy of being recorded.—One of the receptacles being extracted with a penknife, continued luminous; but on being immersed in camphorated spirit of wine, became immediately extinct. The animal, with one of its receptacles uninjured, being plunged into the same spirit, became apparently lifeless in less than a minute; but the receptacle continued luminous for five minutes, the light gradually disapmercury, various gases, water, and in vacuo, considers it solely as an effect of vitality.*

Which of these opinions is the more correct I do not pretend to decide.—But though the experiments of Mr. Macartney seem fairly to bear him out in denying the existence of any ordinary combination of phosphorus in luminous insects, there exists a contradiction in many of the statements, which requires reconciling before final decision can be pronounced. The different results obtained by Forster and Spallanzani, who asserts that glow-worms shine more brilliantly in oxygen gas, and by Beckerheim, Dr. Hulme, and Sir H. Davy, who could perceive no such effect, may perhaps be accounted for by the supposition that in the latter instances the insects having been taken more recently, might be less sensible to the stimulous of the gas than in the former, in which perhaps their irritability was accumulated by a longer abstinence: but it is not so easy to reconcile the experiment of Sir H. Davy, who found the light of the glow-worm not to be sensibly diminished in hydrogen gas,† with those of Spallanzani and Dr. Hulme, who found it to be extinguished by the same gas, as well as by carbonic acid, nitrus and sulphureted hydrogen gases.‡ Possibly some of these contradictory results were occasioned by not adverting to the faculty which the living insect possesses of extinguishing its lights at pleasure. At the same time, however, it may be here observed, that as this luminous substance can be collected in considerable quantities, there can be no difficulty in deciding by chemical analysis whether it is really phosphoric or not; and that till this analysis has been made it is premature to bulld any hypothesis on the assumption of its being so, or to apply this epithet to it, as is so generally done."

It is only necessary to say farther that the more recent researches by M. W. Peters, respecting the luminosity of the *lampyris italica*,* show the internal relation which exists between this faculty and the function of respiration, and that the luminous organ is provided with a copious supply of air at the will of the animal, by means of a plexus of air plugs which, connected with the organs of respiration, form a rich net work of tracheæ, ramifying through every portion of the organ.

It would afford us pleasure to make a more extended notice of this work, but in a Journal like ours, we are in a measure restricted to those subjects in which the mass of the profession are more especially interested. It would, however, be impossible to give any thing like an adequate idea of all parts of the work, and to those who are interested in such investigations, nothing less than the perusal of the work itself would be satisfactory, and we therefore close our remarks by recommending it to our countrymen as a work well worthy of the attention of the lovers of science and nature. W. M. C.

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pearing.—Having extracted the luminous matter from the receptacles, in two days they were healed, and filled with luminous matter as before. He found this matter to lose its luminous property, and become dry and glossy like gum, in about two minutes; but it recovered it again on being moistened with saliva, and again lost it when dried. When the matter was extracted from two or three glow-worms, and covered with liquid gum-arabic, it continued luminous for upwards of a quarter of an hour.

* *Phil. Trans.* 1824. † *Phil. Trans.* 1810 p. 287. ‡ *Phil. Trans.* 1801, p. 483.

* Observations *sur la lumiere que répand le lampyris italica.* Annales des Sc. Nat. 2nd series, vol. 17, Zoologie, p. 254.

IV.—*Proceedings at the Annual Convention of the Connecticut Medical Society, May,* 1846. Together with a list of Members, and the Annual Address. Hartford, Conn.

We are indebted to the kindness of Dr. G. W. Russell, the Secretary of this society, for this pamphlet. There is nothing in these proceedings worthy the special notice of our readers, except the following in relation to the National Medical Convention.

"Dr. Cogswell made an informal report of the proceedings of the National Convention, lately held in New York, and submitted a resolution, which after being amended, was passed as follows:

Resolved, That the county meetings have power to appoint one delegate each to the National Medical Convention, to be holden in Philadelphia, on the first Wednesday in May, 1847; and they are hereby requested to make such appointment, and make return thereof to the Secretary of the State Society."

It seems that this society numbers 393 members, and we suppose has the power to regulate both the teaching and practice of the profession throughout the State.

The subject of the annual dissertation was—"*Observations on Typhus Fever.* By Theodore Sill, M.D." Our limits totally forbid our attempting any thing like an analysis of the paper; and we shall therefore only allude to a few points. The author gives a good and plain account of the *symptoms, diagnosis, prognosis,* and *treatment.* In regard to the latter, he says, "there is probably no disease of equal frequency and importance in our country, in the treatment of which there is less uniformity than in this." He confines himself to his *own conclusions,* the results of *fourteen years experience.* He condemns blood-letting, emetics, and cathartics; and says the rational indications are—"to sustain, and if possible, to *increase* the enfeebled powers of the system,—to equalize the circulation—to allay the morbid irritation and irritability when it exists, and by a prompt, regular and uniform support, to enable the system to react, and to throw off the diseased action." Notwithstanding the usual objections to the use of tonics, except in the latter stages of the disease, Dr. Sill has "every reason to believe that their early and prompt administration increases tenfold the chances of recovery." As regards the possibility of *interrupting* or *cutting short* the progress of the disease, Dr. Sill is clearly of opinion that it may be done by "*prompt and decided medication*"—though he does not give any specific directions as to how it is to be done. The peruvian bark is his favorite tonic, and on a decoction of this, conjoined with galangal, he places "his chief dependence, during the entire course of the disease." He says "I am aware that some substitute the sulphate of quinine, for the bark itself, but after a long trial of it, I find myself wholly unable to depend upon it as a tonic, in febrile diseases." The doctor is evidently unacquainted with the *sedative* and antifebrile powers of quinine—its *tonic* power is a different thing altogether. He speaks favorably of diffusible stimulants, the mineral acids, blisters, and a nutritious and supporting diet. When the bowels are too loose, he gives astringents, both mineral and vegetable; and in that "tympanitic state of the bowels so often attending," he has seen the happiest effects from the use of the *nitrate of silver*. Thus Dr. Sill addresses his remedies to the different symptoms as they make their appearance. Indeed, he boldly proclaims it to be "the golden rule of medical practice," and the great secret of all successful medication, is *to prescribe for the symptoms*." We certainly cannot go with Dr. Sill, the whole length of this. If the physician were to devote his whole attention to the application of remedies to the various symptoms that appear in any disease, instead of endeavoring to trace them all to some *common origin—some general pathological condition* upon which they all depend, he would certainly often find himself at fault. This is certainly the great object of medical science ; to trace all the symptoms to their *true source*, *the pathological condition from which they emanate*, and address remedies to the correction of this—for the cause being removed, the effects will soon disappear.

In regard to a very serious and frequent complication, we give the following extract:

" *Typhoid Pneumonia.*—In relation to that form of this disease called typhoid pneumonia, which prevails with us more or less extensively every year, and which is so often treated as a highly inflammatory disease, I would only briefly remark, I have never bled a case, and have never lost a case. My treatment has uniformly been a prompt and persevering course of opiates, acrid stimuli, mild tonics, expectorants and diaphoretics. Upon opium, sanguinaria, actea, arum, capsicum, lyttæ, galangal and blisters, I have mainly relied."

In support of Dr. Sill's principles and practice, he certainly brings forward the most flattering results—he says that during the whole course of his practice, (14 years,) he " lost but two cases of typhus fever, both of which were brought home out of town, several days succeeding the attack, and one of them after having been greatly reduced by the repeated use of Brandreth's pills, and epsom salts. In farther confirmation of his principles and practice, he is permitted to give us similar results from his respected friends Dr. Elijah F. Reed, of East Windsor, and Dr. Pierson, of Windsor, Conn., whose general views coincide entirely with his own. It seems that Dr. Reed lost only 16 patients out of 500 cases of this disease, and Dr. Pierson, " not more than one per cent."—and in both instances, " cathartics and other reducing agents had been tampered with, either at the commencement, or during the progress of the disease."

This degree of success is certainly unprecedented in the annals of typhus fever, and ought to be made generally known. We thank Dr. Russell for his polite attention, and should be pleased to keep in constant communication both with him and his medical brethren of Connecticut.

E. D. F.

V.—*Observations on Remittent Fever, as it occurs in the Southern parts of Alabama.*—By Wm. M. Boling, M. D., of Montgomery, Alabama. (From the American Journal of Medical Sciences for April 1846.) Pamphlet, pp. 56.

This essay, first published in the American Journal of Medical Sciences as above stated, and now disseminated additionally in *pamphlet* *form*, we look upon as one of the most *remarkable*, as well as most *valuable*, that has ever appeared in this country. We are happy to see that the author has had *extra* copies of it struck off, and hope there are a sufficient number to give it an extensive circulation.

The views here set forth are not altogether *novel* in this region, though we would by no means pretend to say they are generally entertained, but they are certainly received with much doubt and caution by our brethren at the North. What better evidence can we give of this than the fact, that in the very latest "*Treatise on Fever, by Clymer &c.*," which professes to embody "*the received doctrines of their pathology and treatment*," Dr. Boling is complimented on his *excellent description of symptoms* in this paper, but *not a single word* is said about *his plan of treatment*. This was doubtless considered preposterous, or at least too dangerous to be mentioned amongst *received doctrines*. But if we are not mistaken, the time is not far distant when it will take rank amongst the foremost of them. We have expressed the belief that the discoveries made within a few years, in regard to the astonishing powers of the sulphate of quinine, may lead to *a complete revolution in Therapeutics*. We mean that *new light* will be shed upon the *nature* of fever, or rather upon that *organic process* which produces the state of the system called *febrile excitement*, and that they will lead to a *more efficient and safer* method of cure than was ever known or used before. If we see a person labouring under excessive arterial action, excessive heat, severe pain and distressing thirst, we say he has *fever*, with or without inflammation—that the equilibrium of his functions has been disturbed, *somehow*, the circulation and excitement being now preternaturally exalted—and that although we cannot exactly comprehend how this disturbance is produced, and although the patient complains of great *debility* and *exhaustion*, yet the most natural and rational indications are, to reduce the circulation and excitement to the nominal standard as soon as possible, and to restore the disordered functions to healthy action. For these purposes the most powerful depressing agents are used, such as the direct abstraction of blood, the application of cold, the administration of cathartics, &c. The system being sufficiently reduced, we then resort to remedies called *Tonics*, with the view to maintain a uniform standard of action, and to restore the lost energies.

According to our poor conception this is the *orthodox and received doctrine ;* consecrated by the lapse of ages, though *never executed with satisfactory success* The *experimentum crucis* may be said to have been applied to it, for the patient was indeed passed through a *crucible*, as it were, with perhaps as much to dread from his *physician* as from his *disease*. Now, suppose we have a remedy, which by its specific virtues, is capable of reducing excessive arterial action and heat, relieving pain, and removing thirst without the loss of blood, of restoring the disordered functions to healthy action, without the disagreeable operation of emetics and cathartics, and of maintaining the equilibrium of the system, when once established. Suppose farther, that this remedy had been long known to the Medical Profession, but one whose use, it was thought, required to be preceded by blood-letting, emetics, cathartics, &c., before the system could be prepared for it ? Would not the introduction of it into practice, almost to the exclusion of other remedies, be justly entitled *a revolution in Therapeutics* ?

All this is claimed for the *sulphate of quinine*, given in *large doses*, in the *incipient stage* of Remittent Fever : and that too by men of talent and education, unimpeachable veracity, and extensive experience. Some go farther and claim similar virtues for the remedy in the incipient stage of Typhoid Fevers and all others having the least character of *periodicity*. Some go still farther, and say it will exert the same influence on all inflammatory affections occurring in *malarious districts*—whilst a few go *still farther*, and assert that by its specific impression on the nervous system the remedy will exert the same beneficial influence in the early stages of all fevers, whether intermittent, remittent, continued, or exanthematous—and likewise, *probably*, upon all inflammations, whether in *malarious districts, or elsewhere*. This is certainly the *ne plus ultra* ; and if these sanguine anticipations of these greatest admirers of quinine should be confirmed by future observation, it will certainly result in the most remarkable *revolution* that was ever witnessed in any science.

In confirmation of the assertions above mentioned, we would refer to the writings of M. Maillot, of the French military service in Africa ; Dr. Boling, of Alabama ; Prof. Mitchell, of Kentucky ; Surgeons McCormick and Van Buren, of the U. S. Army ; and Drs. Harrison and Beugnot, on the Yellow Fever of New Orleans. For ourselves we do not undertake to endorse *all the claims* set up for the sulphate of quinine, yet we must confess our *astonishment* at the confirmation of our own limited experience, as far as it goes, and our belief that they are worthy the profound consideration of the profession.

By all medical writers up to the present era, peruvian bark and its preparations have been considered as *tonic stimulants*, and it was thought necessary *to reduce* the system by means of blood-letting, cathartics, &c., before it could be safely administered. Now it is discovered to be the most powerful *direct sedative* ever used ; calming nervous excitement, reducing arterial action, and relieving pain with at least equal promptness, and far greater safety, than the lancet, cold water, and purgatives. How strange it must appear to those imbued with the "*received doctrines*," to find Dr. Boling giving large doses of quinine in the *exacerbation* of fever ; and when the excitement is reduced, recommending *opium* as a *stimulant*, to counteract its *depressing effects!* Is it possible that we shall find in *quinine* a *substitute for the lancet?*—*Nous verrons!* It is certainly true that Southern practitioners use the lancet *less and less* as they grow in knowledge and experience. But why are patients ever bled? Is it done for the purpose of absolutely removing a portion of the diseased mass ; for blood is but *fluid flesh?* Or is it done with the view to make a favorable impression on the nervous system, to relieve local engorgements, and re-establish the equilbrium of the excitement and circulation? These questions would give rise to many important reflections. But we must defer their considerations to some more proper time and place.

Let us return to Dr. Boling on remittent fever, though in truth we could never get far away from him, whilst discussing the virtues of quinine. Dr. B. gives a most excellent description of the disease, confining his observations to the *simple and pernicious forms;* excluding from the former, "a class of cases of a very mild character," which require but little attention of the physician. Indeed, we may say they are generally cured by *domestic skill.* His description of the *pernicious or congestive form*, so common in the south, is most graphic, and will be readily recognised by all who have seen the disease. We extract the following interesting commentary on Dr. Parrish's "*strictures on the use of the term congestive.*" Dr. Boling says, p. 18:

"Notwithstanding the small and thready state of the pulse in this variety of pernicious fever especially, the action of the heart will be found strong, as indicated by the loudness of its sounds and the force of its impulse. Dr. Parrish, in "Strictures on the use of the term Congestive," &c., in the *American Journal of the Medical Sciences* for April, 1845, looks upon what is generally recognised as the state of congestion, as "the consequence of *diminished nervous power*, and nothing more nor less than that state which occurs in all cases of sudden prostration." He also observes—"It is not a pathological state peculiar to this form of fever, or necessarily connected with it, except as one of the phenomena of nervous exhaustion, or the dying state when this event happens." "It occurs to a certain extent in fainting, in nervous shock after severe accidents and operations, and under a great variety of circumstances where its existence is not noted as an element in the pathology of the disease. I conclude, therefore, that the use of the term congestive *in this sense*, is no more appropriate in the disease under consideration, than it is to any prostrate condition of the nervous system, induced from other causes. If it be, then we might say that every individual who dies of any disease or accident, is labouring under congestion, because, as the heart ceases its action and the tissues lose their tone, the blood becomes congested, or, more properly, settles in the heart, large vessels, lungs, and other central organs, leaving the capillaries and smaller vessels by the simple force of gravity." Certainly there can be but a very remote analogy between a fainting fit, or the other conditions spoken of by Dr. Parrish, in which the languor of the circulation in the extremities is accompanied by a feeble action of the heart, and congestive fever, in which, even when the pulse may be imperceptible at the wrist, the action of the heart is loud, strong and tumultuous. Prostration there certainly is, but where? In the capillary network alone? Muscular prostration is rather apparent than real, as evinced by the force with which the heart beats, and the fact that there is "oftentimes surprising muscular strength, until within an hour or two of dissolution, the sufferer being able to jump out of bed and walk about while he is pulseless." Can there exist such muscular force with exhaustion of the nervous power?—When penning the above sentence, did any instances occur to the mind of Dr. Parrish in which he had observed it in connection with fainting, or any of the other conditions, to which he has likened the congestion of congestive fever? A state certainly exists in this disease which nothing else so well expresses as the word congestion. This is probably secondary, and consequent upon some modification of the nervous system. It may be perverted innervation, or *irregular distribution* of nervous influence, but certainly not diminished nervous power."

Dr. Boling gives a good description of a modification of congestive fever, "produced by injudicious treatment, in cases where there is an original tendency to assume a bad character, and sometimes indeed it is to be feared, where this tendency is very great. It is most frequently brought about by drastic purgatives." In this the author is fully supported by Dr. J. W. Monette, of Mississippi, whose able paper on *Congestive Fever* was published in the 3d and 4th numbers of the 1st vol, of this journal. In respect to *treatment*, the author says:

"In the *treatment* of remittent fever there are but few remedies necessary or proper; but to insure success, promptness in their administration, and a well-timed application, are all important; and none should be used but such as are intended to, and capable of fulfilling some important indication. None can be administered, as in many other diseases, with a mere negative effect. The stomach suffers greatly, and its powers must be husbanded; and there can be no remedy administered, which, if it fails to exert a favorable influence over the disease, does not produce a more or less injurious effect on this organ. I will enumerate the most useful remedies, and the circumstances under which they are indicated, not so much perhaps in the order of their importance, as in the probable order of their administration."

Here follow his remarks on *blood-letting*, *opium purgatives*, *counter-irritants*, *stimulants*, *calomel*, *quinine*, *the drinks*, *diet*, *emetics*, *diaphoretics*, to which we shall make but a brief allusion, as Dr. B. is himself so *concise* that it would be difficult to abridge what he had said; our present limits will not allow us to give lengthy extracts. He says that *blood-letting* can hardly be considered a remedy of safe and general application, though with proper discrimination, its use is *frequently beneficial*, and under certain circumstances, *imperiously demanded.* "One half of the cases probably, are better treated without it—and when the indications for its use are not decided; when the circumstances are such as create or admit of doubts—the safer course, at least with the inexperienced, a general rule, will be *not to bleed.*" He points out plainly the cases which call for the lancet, and the precautions necessary to prevent ill-consequences where collapse is apprehended; amongst these he recommends artificial heat, sinapisms, hot mustard pediluvia, and a full dose of opium half an hour, or three quarters before the operation, in cases of oppression: "The object is to procure as much blood, with as little shock to the system as possible." Cases of *Comatose* remittent require the abstraction of a considerable quantity of blood, which often has to be, as it were, "*coaxed away.*"

In regard to the general use of the lancet, however, Dr. B. says:—"In an earlier period of my practice, I used it more freely than at present. I have partially abandoned it, not from a conviction of its inutility, but from the fact, that a more extended experience has enabled me to supersede it with a much less dangerous remedy"—(*quinine.*) Dr. Boling speaks rather *disparagingly* of *local-depletion;* but we are inclined to think this proceeds from the imperfect manner in which the operation is performed in the country. One of our *professed city cuppers* will extract from *fifteen to thirty ounces of blood* with his glasses, fully as soon as it can be done by Dr. B's. method of "*coaxing*" it from the arm, and we doubt not with better effect. We place a high estimate upon *efficient* local depletion, in congestive fever; especially when the brain is the chief suffering organ.

Dr. Boling speaks highly of opium and morphine, judiciously administered. He says:

"In that form of pernicious remittent, denominated *congestive*, it is perhaps more useful than in any other. In none is it more required, on account of the irritability of the stomach, while the bowels, in a large majority of cases, require its restraining influence. In combination with quinine, it sustains the force and volume of the pulse and the system, against the *depressing* effect of the latter remedy, during the time of its use, in breaking up the *periodicity* of the disease. The solid opium is here preferable to the morphine."

The author is exceedingly cautious, and in the main, we think judicious, in his recommendation of *purgatives*. As respects the state of the alimentary canal, in remittent fever at the South, the following remarks are very appropriate:

"Although I do not pretend to say that gastro-enteritis is the cause of the disease, or rather that the fever is a mere gastro-enteritis, it is nevertheless present in all, or nearly all; and in a large proportion of cases, is the cause of many of the most prominent and distressing symptoms. The disposition to inflammation of the intestinal canal, where it perhaps may not at first exist, and the slight causes necessary to increase it to a dangerous extent, where it may have been at first but slight, render the utmost caution necessary in the administration of purgatives. It is an erroneous impression among physicians who have not practised in the south, that more powerful active purgatives are requisite here; and one productive of much mischief, as causing the northern practitioner settling in the south to double the size of his purgative dose, when its reduction one-half would probably be efficient, and certainly more safe. On the contrary, the bowels are extremely susceptible to the action of purgatives, which, without great caution, are apt to produce much irritation and hypercatharsis."

He expresses a partiality for *saline* cathartics, particularly epsom salts, in the small doses of $1\frac{1}{2}$ or 2 drachms. We know some very excellent practitioners who concur with him in this; though we believe that saline cathartics are generally condemned in the south. Dr. B. says, he was himself "exceedingly disappointed" in his earlier attempts with epsom salts, on account of the hypercatharsis it produced, and "for a while, abandoned it altogether;" but observing the same tendency from other cathartics, "when prescribed in their recognized doses," he resumed its use, and with the most satisfactory results.

We think that southern practitioners, *especially the young ones*, require to be *seriously cautioned*, in regard to the use of emetics and cathartics in our summer and autumnal fevers. They come from the schools and books imbued with the most exalted ideas of their efficiency in removing crude ingesta, reducing febrile excitement, and correcting disordered secretions, and it generally requires several years' experience, (not without the perpetration of more or less mischief,) to convince them of their error. The condition of the alimentary canal above stated by Dr. Boling, should be duly considered. But we do not think he has been sufficiently explicit in pointing out *the proper time* for giving cathartics; this is unquestionably *after the exacerbation has reached its acme and the paroxysm is on the decline.* Then an effort is made to bring about a *crisis*, and we must *aid nature* in the best we can. Then *less medicine* is requisite, and it will produce its happiest effects.

He offers nothing worthy of particular notice in regard to *counter-irritants* and *stimulants.* He quaintly remarks, that "when that stage of the disease arrives, in which stimulants are prescribed, and relied on as the principal means of cure, the patient's condition is desperate indeed; and the writing a prescription for an ammonia julep, or any stimulating compound, is about equivalent to signing his death warrant."

The author's remarks upon *calomel* are very interesting, and are worthy of serious attention. He does not class it amongst *purgatives*, nor does he esteem it for its *purgative effects.* Its chief virtue consists in its favorable action on the secreting organs, keeping them in operation and correcting their derangements. He uses *mercurials* very moderately and in small doses, giving them in such a manner as would be calculated to display their constitutional effects, evinced by *ptyalism*, though he does not deem this *essential*, or even desirable. He has seen no good effects, but *much harm*, from large doses.

This valuable remedy has been so horribly abused in the south, that we are much pleased to see our best writers, of late, pointing out its *true virtues*, and the proper method of administering it.

Quinine.—We have already made such extensive general remarks, in regard to the power of this remedy, in the beginning of this notice, that we feel inclined to say no more about it at present; yet we cannot, in justice to Dr. Boling, omit alluding again to his views, and to his method of using it in remittent fever. It constitutes his *main reliance;* all other remedies being merely resorted to as assistants. He devotes ten pages of his essay to the consideration of this article, in which he reviews the various opinions that have been entertained in regard to it; makes numerous quotations from standard authors, and gives us the results of his own experience, in a concise but lucid manner. We are compelled to be brief, and shall therefore confine ourselves pretty closely to Dr. Boling's observations. He says·

"Many seem to think, were we deprived of purgatives, the lancet and other depletive remedies, that quinine would be totally inadmissible in the treatment of remittent fever, as a previous course of these is absolutely necessary, in their opinion, to prepare the system for its use. The impression is an erroneous one, for although the use of other remedies will be found materially to advance the cure, there is nothing peculiar in the action of quinine which absolutely requires a course preparatory to its administration. True, it may not be capable of effecting as much alone as with the assistance of other remedies, but in this it is not singular. If in the case of uncomplicated remittent fever, a preparatory treatment has been considered absolutely necessary to the successful administration of quinine, the occurrence of local inflammation in combination with the fever, has been considered sufficient grounds for its total exclusion. On the contrary, nothing so much calls for its prompt and decided administration as this complication.

"Setting aside the vast advantages to be derived from quinine, from its power of controlling the periodicity of diseased action, its prompt, decided and unequivocal influence, as a sedative, over the heart and arteries, renders it peculiarly applicable to all those cases of remittent fever accompanied by increase in the *force* and frequency of the heart's action. It is itself, when efficiently administered, the very best remedy for bringing about that absence of great febrile excitement, which was considered requisite for its successful administration as an anti-periodic. Undoubtedly, there will scarcely occur a case in which the assistance of other remedies may not advantageously be brought to bear. In violently inflammatory cases, where the pulse is full and firm, blood-letting may and should be used in conjunction with it; not as a preparative, but as an adjuvant, acting with and for the fulfilment of the same indication. In other diseases, and with other remedies, the same course is frequently pursued, of bringing several to bear for the production of the same effect. Many treat pneumonia principally with tartar emetic, but preceding or accompanying its administration with blood-letting, is no admission that the former is an improper remedy without the latter, or that a preparative course is necessary for its administration."

He says, that a proper evacuation of the intestinal canal is an important step in this as in all acute febrile and inflammatory diseases. It gives a fairer scope to the operation of quinine, as it does to opium, mercury, and all other important remedies; but he thinks that "great mischief has been and still is the consequence of the impression, that a preparatory treatment—a reduction of the force and violence of the disease—is necessary for the administration of quinine." In dangerous cases, not a moment of time is to be lost; pour in the quinine, and use the *juvantia* as occasion may require. His usual method is as follows:

"Whatever the type or character of the fever, wherever there is reason to apprehend danger, no time should be lost in bringing the system decidedly under its influence. Where the remission is well-marked and of some continuance, "I generally prefer this period for its commencement, probably now from habit, more than anything else. Where the remissions are short, or when the case is urgent, and there is reason to apprehend a fatal termination in the next exacerbation, or where the disease is of so violent a character as to justify fears of the occurrence of any serious organic lesion, or a considerable aggravation of any that may already exist, it seems to me preferable to commence with it immediately, and this I generally do, without regard to the stage of the paroxysm." The fact, that the exacerbations frequently anticipate or come on at irregular and unexpected periods, and that, too, most frequently in cases of such violence, as to require decided and prompt measures for their successful management, is another reason for the immediate administration of the quinine.—Two portions of from eight to sixteen grains each, according to the urgency of the symptoms, given within a couple of hours of each other, will most generally bring the patient under its influence two or three hours after the administration of the second portion. Where the case is of a very violent character, and but a short period is allowed the physician to act, before the time of the expected exacerbation, the whole of the above amount may be given at one time.—Where even the physician arrives during the height of the exacerbation, when the case is one of great severity, the remedy should be immediately administered in full and decided doses, for the purpose of preventing mischief during the existing exacerbation, by at once bringing the arterial system under its sedative action.

"The system once fully under the influence of the medicine, about eight grains every third or fourth hour will generally be sufficient to effect all the good it is capable of. Larger doses, indeed, may be given with safety, but they are rarely necessary, and frequently add much to the present discomfort of the patient. Where any decided inflammatory complication exists, the remedy should be continued until this is completely subdued, as its too early withdrawal, under such circumstances, rarely fails to be followed by renewed excitement of the arterial system, and an increase in the local inflammation. Where, however, the case is one of simple remittent, of quotidian or tertian type, the remedy need be continued but a short time after it has controlled one exacerbation.—Where the type is that of a double tertian, it is absolutely necessary before its withdrawal, that two successive exacerbations should be controlled,—for the mere suspension of *one set* of exacerbations does not secure the patient against a continuance of the other. Under any circumstances, however, when the patient is once strongly under its influence, it should be *gradually* withdrawn, as its sudden suspension is apt to be followed by that state of reaction which so frequently follows temporary depressions of the circulation from other causes."

In cases where the irritability of the stomach is so great, that the remedy cannot be retained, he gives it by enema combined with opium, if the bowels are also irritable. He repeats the assertion, that *the impression is erroneous*, that quinine should alone be given during the *remission* of fever, "and that mischief must certainly be the result of its administration during the exacerbation." He admits that its action is more favourable during the remissions, and less of the remedy will be required, but its virtues are not confined to this stage, and often no time is to be lost.

So much for the *sulphate of quinine*, for Dr. Boling does not speak of the *Ferro-cyanate* which has attracted some attention lately, nor of any other preparation or mode of administration. We fear we have given a very imperfect sketch of Dr. Boling's views, but hope it will serve at least to direct attention to his paper.

Of the other remedies mentioned by Dr. Boling, it is only necessary to say a word. He condemns *emetics* almost *in toto*, and we think correctly. We have often witnessed that distressing sense of fulness in the stomach accompanied by nausea, which causes the patient to beg for *an emetic*, but have seldom seen it afford relief—often great mischief.

Of *Diaphoretics*, he says:—

"If we confine the word diaphoretic, in speaking of this disease, to those agents that really prove such in it, then are they applicable to its treatment.—Blood-letting, quinine, opium, cold ablutions and calomel, all, when properly applied and acting favorably, prove more or less sudorific; but in that large class of remedies generally denominated diaphoretics and sudorifics, there are few that can be said to have such an effect in the fever in question."

Post-mortem appearances.—Dr. Boling concludes his paper with some remarks upon these, but confesses that his opportunities for investigating them have not been numerous. He found nothing characteristic, or peculiar in the condition of the *heart*, *lungs*, *kidneys*, *or bladder*. He generally found traces of inflammation in the *stomach* and *intestines*.—The *spleen* he found generally enlarged, dark, and somewhat softened. In regard to the *liver*, to which his attention had been so much directed by all writers on remittent fever, especially in Southern latitudes, he says that to his *surprise* he *never* found *well-marked fulness or tenderness of the right hypochondriac region* during life, nor any other evidence of affection of the liver, more than that functional derangement partaken by it in common with every other organ during the state of febrile excitement; and which, he ventures to say will be as palpable and well-marked in the kidneys as in the liver. *After death*, he seldom found any alteration in it—"indeed, in a large proportion of cases, the organ, so far as I (he) am capable of judging, was entirely healthy." When otherwise, a *bluish-slate colour* of the concave surface, was the most that he could discover. We regret that Dr. Boling does not say any thing in regard to the appearance of the *biliary secretion* after death. Our own observations at the New Orleans Charity Hospital have led us to the conclusion that there is a *marked contrast* between the amount of bile to be seen, not only in the intestinal canal, but in the gall-bladder and biliary ducts, in remittent and yellow fever—being vastly *greater* in the former. If this is uniformly the case, it is certainly worthy of notice. We cannot but think that Pathologists in their *post-mortem* researches, devote too much attention to the condition of the *solids*, and not enough to the *fluids*,—the blood and secretions.—What have they accomplished by all their arduous labours in the *dead-house?* They show us but the *fatal ravages* of diseases—throwing but little light upon the mysterious processes by which they are produced; and *scarcely any* on the powers of the remedial agents on which we must rely for their prevention. It is like looking upon the ruins of some magnificent temple prostrate in the dust, without being able to discover *how it was brought low*, or *how to guard others against a similar fate.*— We are at this day better acquainted with the *nature* of *pneumonia*, and *consumption, and perhaps fevers*, than our ancestors were; yet these diseases continue to swell our bills of mortality to a dreadful extent, and we must admit that much yet remains *unknown* in regard to them. We are not so *utopian* as to suppose that all diseases may be *prevented* or *cured;* yet we do not know how much can be accomplished. But let us keep in the *right track*, and that certainly consists in *a close and correct observation of symptoms, and the effects of remedies.* Here we may well take pattern after some of our illustrious ancestors.

We have made a rather extended notice of Dr. Boling's essay, but not farther than its merits entitle it; nor have we given such an analysis as will answer in the place of the *original.* We hope it will be generally read.

E. D. F.

VI.—*A Popular Treatise on the Diseases of the Teeth.* Including a description of their structure and modes of treatment; together with the usual mode af Inserting Artificial Teeth. By Robert Arthur, D.D.S., etc., etc., p. 187. Piladelphia; Lindsay & Blackiston, 1846.

Within a comparatively short period, dentistry has risen from a *mechanic art* to the dignity of *a science*, and is numbering among its followers so many educated and enlightened minds, that hardly a year elapses without our being supplied with one or more interesting volumes in relation to it: and this too in addition to a regular quarterly journal of most respectable pretensions, and a monthly "Intelligencer" having a London editor We shall surely soon learn every thing that may be desired respecting the teeth, except perhaps that great *desideratum* of rendering them impregnable to the *tooth of Time.* In our last number we noticed an American edition of Fox's celebrated work on the Teeth, and now we have Dr. Arthur's *brochure*, which is an excellent thing in its line. It is written in a plain intelligible style, illustrated with wood-cuts, aud contains a great deal of useful information as well for the popular reader, as the student of dentistry. Indeed, we think it were well for such a *manual* as this to be among the *hand-books* of every family. They might learn from it *how* to take care of their teeth and the *importance* of doing so, and thus save themselves much suffering and expense.

The following excellent general directions for the preservation of the teeth, may be acceptable to our readers.

"1. Relieve, as quickly as possible, excessive inflammation of the mouth, at the time when the infant teeth are coming through the gums.

2. Attend, carefully, to the health of the child.

3. Keep the temporary teeth clean, by washing the mouth of the infant several times a day, and passing between the teeth some material, such as floss silk, to cleanse the lateral surfaces.

4. Induce the child to form, at an early age, the habit of using a suitable brush.

5. Do not allow the first teeth to be extracted, except when they prevent those of the second set from taking a regular position in the jaws.

6. When the second teeth are succeeding those of the first set, pay especial attention to cleanliness of the mouth, and take the necessary precautions to prevent irregularities.

7. If the second teeth should be irregularly situated, lose no time in having the irregularity corrected; but never allow sound teeth to be filed for this, or any other purpose.

8. Avoid those causes which may affect the health of the digestive organs.

9. Be very cautious in taking medicines, which, by contact with the teeth, can injure them.

10. Make use of no tooth-washes, powders, or pastes, unless you are aware of what they are composed, and then let them be of the most simple materials.

11. From the first appearance of the second teeth, use every means to keep them always clean: by using a suitable brush, night and morning; by passing between them, every day, some material which will effectually cleanse their lateral surfaces; and by always washing the mouth, thoroughly, after eating.

These directions, it will be seen, are verysimple,and yet, if they were rigidly put into practice, few persons would loose their teeth at an early age."

E. D. F.

VII.—*Proceedings of the Medical Society of the State of Tennessee; at the Seventeenth Annual Meeting; held at Nashville, May*, 1846.

Our thanks are due the secretary (we presume) for this little pamphlet. We regret very much to see the physicians throughout the State of Tennessee, display so little interest as they do in their State Medical Society. We are well aware that it was at their solicitation, this society was first chartered by the legislature; and it was hoped it would exert a beneficial influence upon the profession; but it appears that its annual sessions are only attended by the physicians of Nashville, and some of the adjacent counties, who deserve much praise for their persevering efforts in the cause. It must appear a little singular that a State which has with such difficulty, for seventeen years, maintained a *single medical society*,* should at this time aspire to the establishment of *two medical colleges* at once, and at the same place. We sincerely hope that these colleges will succeed in infusing greater spirit and energy into the ranks of the profession, than the State Medical Society, with the united and untiring efforts of the able physicians of Nashville and vicinity, have been able to do. We heartily wish success to them all—such success as will *do honor* to the profession.

We return thanks for Dr. Ford's interesting essay on Gastrotomy, which will be found amongst our original communications in the present number. We should be much pleased to receive for publication Dr. Manlove's case of *simulated Hydrophobia*, Dr. Overton's "*well-authen-*

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* We mean, for the whole State.

ticated case of Hydrophobia, which was cured by the administration of a strong decoction of the root of the Phytolacca Decandra," and Dr. Robertson's case of the same disease, "*which was cured by the administration of Tincture of Cartharides to strangury*"—all of which were reported *verbally* to the society. *Well-authenticated facts* of this kind in relation to so fatal a disease as *Hydrophobia*, should, by all means, be published to the world. We are aware that the Phytolacca Decandra, (Poke weed,) commands considerable *popular* reputation as a remedy in Hydrophobia, and we have seen it given ourselves; but without any good effect.

It seems that the society found some difficulty in determining the comparative merits of *four essays on Scrofula*, which had been offered for the *Prize* announced at its last meeting. It was finally awarded to Dr. W. L. Sutton, of Georgetown, Ky. No prize essay is announced for the ensuing year.

We repeat our best wishes for the success of this society. It deserves high praise for its indefatigable efforts to cultivate medical science, and to elevate the profession throughout the State, which, if properly seconded, would doubtless be followed by the happiest results.

E. D. F.

VIII.—*A Dictionary of Medical Sciences.* Containing a concise account of the various subjects and terms; with the French and other synonymes; notices of climate, and of celebrated mineral waters; formulæ for various officinal and empirical preparations, etc. By Robley Dunglison, M.D., Professor of the Institutes of Medicine, etc., in Jefferson Medical College, Philadelphia; sixth edition, revised and greatly enlarged. Philadelphia; Lea & Blanchard; 1846; one volume, pp. 808.

The learned author of this work must surely have realized his most sanguine expectations in the general approbation and success that have attended it. This is the *sixth edition* that has been published; and every one has been marked by decided improvement. "Nearly two thousand five hundred subjects, and terms not contained in the last," are added to this edition; and we think that "the author's anxious wish to render the work a *satisfactory* and *desirable*—if not *indispensable*—Lexicon, in which the student may search without disappointment for every term that has been legitimated in the nomenclature of the science," has been fully accomplished. Such a work is much needed by all medical students, and young physicians, and will doubtless continue in extensive demand. It is a lasting monument of the industry and literary attainments of the worthy author, who has long occupied the highest rank among the medical teachers of America.

E. D. F.

Part Third.

EXCERPTA.

I.—*Report on the Progress of Human Anatomy and Physiology, in the years* 1844–5. By JAMES PAGET, Lecturer on General and Morbid Anatomy and Physiology, and Warden of the Collegiate Establishment, at St. Bartholomew's Hospital.

The following Report concerns the history of physiology during a longer period than either of the two former Reports, and contains notices of the works published between the 1st of October, 1844, and the 31st of December, 1845.—Its general plan is similar to that of those already published, except in that, for brevity's sake and for convenience of reference, there are appended to each chief section the titles of such essays relating to the subject therein treated of as, for various reasons, could not or needed not to receive any further notice.—For this, as for all the former Reports, the original essays have been read in nearly every instance; and whenever reference is made in the foot-notes to more than one publication as containing the facts to which any part of the Report relates, the first of the books so referred to is that from which the account given in the text was derived.

CHEMICAL COMPOSITION OF THE BODY.

Elementary constituents. Another fact in evidence that copper may occasionally exist, independent of poisoning, in the body, is furnished by Bertozzi,* who has detected it in fourteen biliary calculi of various kinds. The more yellow biliary colouring matter they contained the more abundant was the copper: white biliary calculi contained no trace of it. In bile he never could detect it. But, though these may be facts, yet the question, whether either copper or lead ever exists in the healthy body, must still be held open: in the last year MM. Devergie, Barse,† and others have again asserted that they do thus exist, and MM. Flandin and Danger‡ have as positively again denied it.

Proteine compounds:—Albumen, &c. M. Wurtz§ has rendered a further account of his mode of preparing albumen, in a state of purity, yet soluble without the addition of alkali or any other of the substances hitherto supposed necessary to its solution.

Dr. Ludwig|| has extracted from the materials hitherto vaguely named *Extractive matters* of the blood a principle which is isomeric with the binoxyde

* Oesterr. Med. Wochenschr. Sept. 6, 1845. † Comptes Rendus, 28 Oct. 1844.
‡ Comptes Rendus, 30 Sept. 1844. § Annales de Chimie et de Physique, Nov. 1844.
|| Annalen der Chemie und Pharmacie, Oct. 1845.

of proteine of Mulder. The extractive is obtained by the pressing of defibrinated and coagulated blood, the neutralizing of the fluid pressed out, and again coagulating it, and filtering. To this filtered fluid, now free from albumen, alcohol is added, and the precipitate thus obtained, being washed with water, yields chloride of sodium, phosphate of soda, and a very small quantity of substance like binoxyde of proteine. Boiling alcohol removes fatty matters from it, and ether a crystallizing fat. The body thus purified, both in its general characters and in the results of its chemical analysis, resembles in all essential respects the binoxyde of proteine. It exists in the serum—not in the blood-corpuscles—and, next to the saline constituents, forms the chief part of the extractive matters of the blood of man, and of the dog, ox, sheep, and pig.

But what binoxyde of proteine may be is a question; for doubts are now cast by Liebig † on the accuracy of Mulder's researches from which he deduced the existence of proteine itself. Liebig finds that the supposed binoxyde [tritoxyde ?] of proteine obtained by adding ammonia to be solution of fibrine in dilute hydrochloric acid (the albuminose of M. Bouchardat,) contains really all the sulphur which was present in the fibrine. And he finds that, when, as in the process proposed by Mulder, fibrine, albumen, or caseine is dissolved in potass ley, and the solution is neutralized by acetic acid, this solution contains no sulphuret of potassium or other sulphur compound; but that, if the precipitate formed when the acetic acid is added (and which is the supposed proteine) be dissolved in potass, sulphur may be detected in it by adding sugar of lead to the solution. This precipitate, therefore, Liebig holds, is not proteine; neither does he find the corresponding precipitate obtained from peas to be free from sulphur; nor, finally, has he "yet succeeded in producing a nonsulphuretted substance possessing the composition and properties of the so-styled proteine."

Dr. Buchanan ‡ has found the remarkable fact, that the liquid of hydrocele may be coagulated in five or more minutes into a transparent tremulous jelly-like substance, by adding to it a small quantity of the washed clot of blood.—The same coagulating property is possessed in a less degree by several other animal substances; e. g. the buffy coat of blood in minute shreds, or even when dried and pulverized; the transparent coagulum on a blistered surface; and, in various much less degrees, by muscle, skin, cellular membrane, spinal marrow, mucus, and pus. He thinks that the coagulant power is chiefly seated in the colourless blood-corpuscles, which exist, together with the insoluble parts of the red corpuscles and some fibrine, in the washed clot; and with fibrine in the buffy coat; and in the coagulum from blisters. To these also he ascribes, I think, the usual coagulation of the blood; the colourless corpuscles inducing the coagulation of the fibrine dissolved in the liquor sanguinis, and thus leaving its other constituents as serum, which is only *defibrinated* liquor sanguinis.

Fibrine. Dr. Polli § has confirmed the fact, that when fibrine begins to putrify in water, albumen is among the substances formed from it. He has also found that if serum be mixed with three or four times its volume of water, and then boiled, the milky fluid which remains, and which contains, according to Boudet, albumen combined with soda, is spontaneously coagulable. If left to itself for a few days it forms an opaque-white soft clot, from which a limpid watery fluid separates, in which the clot sinks. But the spontaneous

† Lancet, Feb. 20, 1845.

‡ Medical Gazette, Aug. 8, 1845. Dr. Buchanan argues as if it were proved that the fluid of hydrocele contains fibrine, and that it is fibrinous coagulation by which it becomes jelly-like when the portions of clot or other substances mentioned are added. It may be so, but there is no proof of it; and he is not right, I think, in saying that hydrocele fluid is commonly regarded as analogous to liquor sanguinis. The same coagulability may be seen in other dropsical fluids, e. g. when blood is let into them after death.

§ Annali Univ. di Medicina, Feb. 1845.

coagulation is the only circumstance in which it imitates fibrine, for the clot is not filamentous.

The rest of this paper is occupied with the changes induced in the fibrine of blood by inflammation, especially its diminished tendency to coagulation, and its rarefaction. Several cases of late coagulation are given, similar to that mentioned in the last Report,* but less remarkable than it. They confirm his belief that no blood is really incoagulable or remains liquid out of the body till it decomposes: and that when blood has seemed not coagulable, as in typhus and scurvy, its coagulation has been overlooked, or it has not been kept long enough for an unusually slow coagulation to take place. He considers that the most essential condition of this slowness of coagulation is a peculiar modification of the vitality of the fibrine, other favorable conditions being the excess of carbonic acid and of salts in the blood. And for fibrine thus modified by the influence of an inflammation he proposes the name of *bradyfibrine.*

Fibrine modified in another manner, and peculiarly *rarified*, he calls *parafibrine.* He has found that in the most acute inflammations the specific gravity of the blood or liquor sanguinis is rather increased by the removal of the fibrine; so that, contrary to the rule of health, the serum has a higher specific gravity than the liquor sanguinis. He supposes that this light fibrine, or parafibrine, may be that which is formed during, and as a result of, the inflammation, and which, in severe cases, may be formed in such quantity that it more than balances the effect of the ordinary fibrine and the *bradyfibrine* in increasing the specific gravity of the blood, so that the specific gravity of the blood is increased when both it and they are removed from it. He describes the parafibrine as coagulating very slowly into a mass which has a gelatinous aspect, and consists of very slender filaments, like those which give consistence to the albumen of an egg. When the serum is pressed from such a clot it becomes fibrous, tenacious, and heavier than the serum. Such clots are found in vesications: the fluid they contain is rich in parafibrine, and may be studied for the characters of this substance.

Both these modifications of fibrine may coexist in various proportions with ordinary fibrine in inflammatory blood, and hence arise many varieties in the time of coagulation and the appearance of the buffy coat of such blood. It is this light fibrine in the blood in inflammations, which, according to Dr. Polli, is effused with the serum in inflamed parts, for its tenuity permits it to traverse the walls of the vessels more easily than any other constituent of the blood can; and, once effused, it coagulates and becomes fibrous. Its copious existence in blood indicates the highest degree of inflammation; of this the gelatiniform buffy coat is the best sign. The simply retarded coagulation, due (at least in part) to the formation of bradyfibrine, indicates a lower degree of inflammation. The mere increase of ordinary fibrine is characteristic of the first degree of the same, and the increase of the one is proportionate to the extent of the other. Lastly, the existence of light fibrine in blood explains how a buffy coat may in certain stages of inflammations be formed in blood which coagulates quickly. In such a case the liquor sanguinis is very light, and may have been made lighter by previous bleedings; the corpuscles sink rapidly and leave above a layer which may presently form the buffy coat.

Saline constituents of animal fluids. Dr. Golding Bird † questions the correctness of Enderlin's ‡ deduction that no organic acid is combined with the alkaline bases of any of the secretions except the bile, because no alkaline carbonate is found in any of their ashes. He thus shows that a salt of soda with an organic acid may exist in a solution of phosphate of soda, and yet yield no carbonate on ignition;—nine grains of dry tribasic phosphate of soda being mixed with four grains of acetate of soda, and exposed to a full red heat

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* Page 6 † Philosophical Magazine, May, 1845 ‡ See last Report, p. 8

in a covered crucible, the product is easily soluble in water, and has a strong alkaline reaction; but no effervescence ensues on adding dilute sulphuric acid to it.*

BLOOD.

Corpuscles. Mr. Gulliver† has published a synopsis of all his former observations on the sizes of the red corpuscles of the vertebrata, in tables, stating the measurements of these bodies in no less than 485 species. He has also shown,‡ in reference to the formation of the buffy coat, that the rapidity of sinking of the blood corpuscles is not directly proportionate to the tenuity of the fluid in which they are collected, but rather, inversely proportionate; for it depends mainly on the rapidity and completeness with which they aggregate in rolls and clusters (as described by Hewson and others.) This grouping is promoted by saline solutions, which have been made thicker by mixing gummy matters with them, and is retarded or destroyed by dilute saline solutions. As soon as the corpuscles are aggregated they begin to sink rapidly; and hence, in blood that will be buffed, there is a remarkable acceleration of the sinking of the corpuscles two or three minutes after it is drawn.

Blood. Dr. G. O. Rees,§ extending his well-known observations on the changes produced in the blood corpuscles by the entrance of fluid when they are placed in fluids of less specific gravity than the liquor sanguinis, and by the exit of fluid from them when placed in fluids of higher specific gravity than their fluid contents, has shown in these changes several sources of fallacy in the quantitative analyses of the blood. He states also that, after copious perspiration induced by exercise, the corpuscles are very thin and very like those from which fluid has exuded when immersed in a denser fluid. The change may be ascribed to the loss of water from the liquor sanguinis, and the consequent increase of its density, till it becomes of higher specific gravity than the fluid contents of the blood-corpuscles. On the other hand, the serum of a dog, into whose veins six ounces of water were injected in the place of six ounces of blood just previously drawn, was tinged by the colouring matter of the corpuscles, which had oozed from them into the fluid of reduced specified gravity.‖ In examining corpuscles retained at a temperature of 100 deg. F., he has seen each corpuscle contracting into a hour-glass form, and then dividing into two, usually of unequal size, a process which he supposes to be similar to that by which the corpuscles are naturally multiplied.

Gaseous contents. Prof. Magnus¶ has repeated his experiments to determine the quantities of oxygen and nitrogen contained in the blood, and the

* Among other works relating to animal chemistry in general, the most important are, of course, the Lectures of Liebig in the Lancet of the past year, and the continuation of Mulder's Phisiol. Scheidkunde; and after these,—1. An essay by Dr. Schmidt, 'Zur vergleichenden Physiologie der wirbellosen Thiere,' in the Annalen der Chemie, Juni, 1845; from an essay published at Brunswick. Its main purpose is to obliterate the supposed line of absolute material distinction between the animal and vegetable kingdoms, evidence being drawn from ample analyses of the elementary tissues of the invertebrata. 2. Gobley, "Sur l'existence des acides oléique, margarique, et phospho-glycérique dans le jaune d'œuf," in the Comptes Rendus, Sept. 29, and Nov. 3, 1845. I may here also refer for the compendium of every recent work on chemistry to the Annuaire de Chimie of MM. Millon and Reiset. Paris, 1846.

† Proceedings of the Zoological Society, No. clii, Oct. 14, 1845.

‡ Dublin Medical Press, Dec. 11, 1844, and Edinb. Med. and Surg. Journ., Oct. 1845.

§ Culstonian Lectures. Medical Gazette, March 14, e. s. 1845.

‖ Schultze has made a similar observation in blood drawn after taking large draughts of water.

¶ Philosophical Mag., Dec. 1845, Suppl. p. 563, from the Annalen der Physik u. Chemie, lxvi, p. 177.

extreme quantities of those gases which it is capable of absorbing. The experiments consisted chiefly in repeatedly agitating fresh drawn blood with atmospheric air, and then (with necessary cautions) washing out the absorbed oxygen and nitrogen with carbonic acid. The results of numerous experiments were pretty uniform. The quantity of oxygen obtained from the blood was from 10 to 12·5 per cent. of its volume, that of nitrogen from 1·7 to 3·3 per cent. Reversing the mode of experiment, it was found that blood could absorb $1\frac{1}{2}$ times its volume of carbonic acid, and that, after all its oxygen and nitrogen had been washed out by carbonic acid, it could absorb, at a maximum, 16 per cent. of its volume of oxygen, and 6·5 of nitrogen. Similar experiments were made on old horses' arterial blood, and they proved that such blood naturally contains in simple solution from 10 to 10·5 per cent. of oxygen, and from 2 to 3·3 per cent. of nitrogen. And some further experiments lead the author to believe, that of this 10 per cent. of oxygen in arterial blood, about 5 per cent. are consumed in the systemic capillaries, and 5 per cent. remain in the venous blood, to be completed to the general average of 10 per cent. by the absorption of fresh oxygen in the pulmonary capillaries.

Life: capacity of organizing. Whatever doubt might still remain in the minds of some, whether Mr. Hunter maintained the truth in his doctrine that the blood could be organized, that is, that the whole substance of a portion of blood, being at rest either within or without a vessel, may develope itself into tissue and become vascular, and grow and nourish itself, must be satisfied by the observations of Dr. Zwicky * on the metamorphoses of the thrombus, or clot of blood which forms in a vessel above a part at which it is tied. For observations which, like these, stand on the boundary between physiology and pathology, no more space can be given than to state briefly the result; which is, that, to all microscopic appearance, the organization of the fibrine in an inflammatory condition and of that in a clot of blood is in all essential respects the same process, the presence of the blood corpuscles in the latter having no further influence on the process than that of somewhat retarding it. The whole process of the metamorphosis of the fibrine of the blood into fibrocellular tissue, and of the formation of vessels in it is satisfactorily traced; and thus the only additional evidence which was needed is supplied for the establishment of another of the long-doubted Hunterian doctrines.

Modifications in disease. An account has already been given of the researches of Dr. Polli on the changes of blood in inflammation. The continued investigations of MM. Andral and Gavarret † have in numerous striking instances confirmed their account of the constant increase of fibrine in the blood in direct proportion to the increasing acuteness of the diseases in all cases of inflammation, and of its regular and proportionate decrease in typhoid fever and purpura.

And these cases are confirmed by the laborious investigations of MM. Becquerel and Rodier, ‡ who have prefaced their pathological results by certain facts of more present interest in physiology. They give a standard table of the comparative constitution of the healthy blood of the adult male and female, showing the constant differences between them. As to the differences of constitution of the blood at different ages, they find none that is well-marked in men, but in women it is constant that the proportion of corpuscles is less before menstruation is fairly established than it is afterwards, and that when menstraution has ceased it again diminishes. In both sexes the cholesterine increases with increasing years after the age of between forty and fifty. The corpuscles are more abundant in the robust and well-fed. During pregnacy there is a great diminution in the proportion of corpuscles, a less considerable one in that of albumen, a slight increase of fibrine and of the

* Die metamorphose des Thrombus; von Dr. H. Zwicky. Zurich, 1845. 4to.
† Comptes Rendus, Séance due 18 Nov. 1844.
‡ Ibi . and Gazette Méd., 22 Nov. e. s.

phosphorized fatty matter, and an increase in the proportion of water. The other results obtained by these authors relate more exclusively to pathological states of the blood, but they are in that regard of very high interest. *

GENERAL ANATOMY AND PHYSIOLOGY OF THE TISSUES.

Fibro-cellular tissue and elastic tissue. Distinct filaments are described by Stadelmann † as demonstrable in the transverse sections of fibro-cellular tissue, cut and moistened after being dried. Their cut ends are like exactly circumscribed, pellucid circles, shadowed by sharp lines. Their diameters are all equal, and, as nearly as they could be measured, 1-19000 of an inch. After similar preparation of portions of the elastic tissue, he finds the cut ends of its fibres round, ovate, triangular, or polygonal, circumscribed by pellucid lines, and separated by intercellular substance. They measure in the human legamenta subflava from 1-9174 to 1-7037 of an inch: in the legamentum nuchæ of the horse they are nearly twice as large.

Membranes. Much attention has been given to the arrangement of nerves in the serous membranes. The republished observations of Purkinje, ‡ on the nerves of the pia mater and arachnoid are noticed elsewhere. They are confirmed and added to by those of Mr. Rainey, § on the nerves in the arachnoid; but these have been published since the time to which the Report has reference. M. Bourgery ‖ also has given a long account of what he believes to be nerves of the peritoneum, dissected by the help of weak nitric acid, and briefer notices of those of other serous membranes; but, in regard to the nerves of the peritoneum, since the cords which he dissected were not submitted to minute microscopic examination, and the microscope can discern only obscure traces of any nerves at all in that serous membrane, his description cannot yet be received as sure. So also, it must remain an open question, what amount of nerves the pleura receives, and how they are arranged. ¶

Cuticles, pigment, &c. E. H. Weber ** has noticed that in all cases, but especially in the nasal ciliary epithelium of warm-blooded animals, the ciliary movement is accelerated by warmth and reduced to half its usual rate by cold. In this as well as in its rythmic action the ciliary movements are like those of the heart; and there is no striking difference between them in regard of their period of continuance after apparent death, for he has seen the frog's auricle contracting for sixty hours, and after the blood in which it was placed had become quite putrid.

* Papers on the development of the blood corpuscles have been read to the Royal Society by Mr. Newport and Mr. Wharton Jones, but I cannot gather an intelligible account of their results from the abstract given in the Proceedings of the Society, Nos. 60 and 61.

There is an agreeably written sketch of the present state of the pathology of the blood, by Wunderlich, with the title 'Pathologische Physiologie des Blutes, Stuttgart, 1845,' but there is nothing in it, I think, which is both new and interesting in physiology. Here also, as well as for the anatomy of the tissues, I may refer to Prof. Harting's 'Recherches micrométriques,' 4to, Utrecht, 1845, containing exact measurements of the blood corpuscles and other parts. The physiological part of the work is preceded by a careful discussion of the comparative merits of different methods of micrometry, among which, after minutely testing all, the author prefers the common plan by double sight, because it is the most convenient, always applicable, and, for very minute objects, liable to least error.

† Sectiones transversæ partium elementarium corporis humani; Turici, 8vo, 1844, pp. 10 and 12.

‡ Mueller's Archiv, Hefte iii, iv, 1845.

§ Report from the Med. Chir. Society in the Lancet, &c., February 27, 1846.

‖ Comptes Rendus, 8 Sept. and Gazette Médicale, 20 Sept. 1845.

¶ See hereon Pappenheim, in the Comptes Rendus, Oct. 1825, and Purkinje l. c. p. 293.

** Archives d'Anat. génér. et de Physiologie, Janvier 1846, from a paper read at the Scientific Congress at Naples in 1845.

Dr. Moleschott * has examined the discoloration of the pigment of the skin of frogs when they breathe in pure oxygen, held over water, so that the carbonic acid they produce may be absorbed. In a few hours the blackish-green elongated marks, extending backwards and downwards from the eyes, become lighter and greener. Then for several days the change goes on more slowly, but at the end of twelve days it is very distinct in all the spots; the dark semicircular marks on the thighs can then hardly be seen.

The developement of epidermis has been described by Mr. Erasmus Wilson † as consisting essentially in—1st, the production of "primitive granules" in the blastema; 2d, the collection of four, five, or six of these into "aggregated granules," which become the nucleoli of the complete cells; 3d, in the arrangement of a single tier of aggregated granules around each of these first aggregated granules, so as to form an oval or circular mass, a "nucleated granule," the nucleus of the complete cell; and 4th, the production of another tier of aggregated granules around the nucleated granule, forming a transparent border around it, around which border there is probably a cell-membrane, forming the proper epidermis cell or "nucleolo-nucleated cell." The growth of the cell he believes to be due to successive repetitions of the same process in the development and growth of aggregated granules within it, the full-grown cell containing secondary and tertiary cells and bodies in all the four stages of development above mentioned.

Cartilage. M. Valenciennes‡ has made an extensive examination of the structure of the cartilages in mollusca and cartilaginous fish, but the only facts which it appears important to state here are—1st, that the cartilage cells are generally arranged in such regular plans that it would be possible to determine by microscopic examination the order, or even the genus. of an animal, from the character of its cartilages; 2d, that none of the cartilage cells have, in any species, canaliculi communicating with them; and 3d, that gelantine, not chondrine, is abundant in the cartilages of cephalopods.

Bones: chemical analysis. A number of analyses of bones of various animals have been made by Dr. Stark.§ He shows that, besides their marrow [i. e. I suppose, all the fat that can be easily scooped out of the cancellous tissue], the bones of mammalia contain from 13·5 to 29·2 per cent. of fatty matter. The solid part of the shaft of the canon bone of a sheep contained 4·3 per cent. The bones of almost all birds under one year old contain an oleo-albuminous (?) matter, which, as the bird grows older, is absorbed. No less than 232 analyses of various bones from all the vertebrate classes are given, to show the proportions of animal and earthy matters; and the result is, that in all bones the proportion is nearly the same, the average proportion for all being 66·09 of earthy matter to 33.91 of cartilage. No confirmation is afforded of the notion that the proportion of earthy matter is the larger the higher the animal is in the scale of creation, for the largest proportion (68·74) was in the true bones of cartilaginous fishes—the smallest (61·9 to 62·3) in the bones of the bear and marmozette monkey. The proportion of earthy matter appeared a little greater in the wild than in the domesticated mammalia. There was no evidence found of its increasing with age in either men, cows or sheep. Neither did the hardness, or inflexibility, or want of transparency of bones appear to be dependent on the earthy matter being present in a very large proportion, but on the mode in which the textures of the bone were held together.

Structure. Mr. Goodsir‖ states that in the cavity of each bone-corpuscle,

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* Tijdschrift voor Naturl. Geschiedenis en Physiologie, 1845, St. 2, D. 12.

† Proceedings of the Royal Society, No. 61, June 19, 1845; and Philosophical Mag., Feb. 1846.

‡ Comptes Rendus, 25 Nov. 1844. § Edinb. Med. and Surg. Journ., April, 1845.

‖ Anatomical and Pathological Observations. Edinburgh, 1845 .8vo., No. x, p. 64.

but not extending into the canals, there is "a little mass of nucleated cells of great transparency." This he regards as the *germinal centre* of the texture or of the cells comprehended within the canals of the corpuscle which it occupies, through which canals, also, he supposes that the nutritive material which the mass of cells attracts into the corpuscle is conveyed for the nutrition of the hard parts between them. He also describes the tissue which lies between the blood-vessels and the walls of the Haversian canals as a layer of cellular substance, at least in the fœtus, for in the adult it is replaced by fat.* In another paper† he describes (as a source of fallacy in those experiments in which it appeared that, when a piece of bone was removed and its periosteum was left, the periosteum reproduced the lost bone), how that it is hardly possible to separate periosteum from bone without detaching minute portions from the surface of the bone; and that these, adhering to the periosteum, may serve as nuclei for the production of the new bone, which thus seems to be produced from the periosteum itself.

Development. The early stages in the development of bone are described by Koestlin,‡ from the examination of a thin layer of new bone (osteophyte) which lined nearly the whole skull of a woman who died three weeks after delivery. His account has, perhaps, less of special than of general interest, for it may not be true of the ordinary development of bones, but it confirms the account given by Karsten§ and some others, of the general plan of cell-development. He finds that, in a homogenous membrane formed in the earliest stage of the exudation, there are first formed scattered elementary granules. These gradually increase in number; some enlarge, some coalesce, and the larger granules thus formed separate into a moderately thick wall, a dark contents, and an eccentric nucleus. Thus they become primary cells; and these also enlarge by growth and coalescence. At first their form is nearly spherical, but they gradually become less regular, and, their contents disappearing, they become also less turgid and clearer, and their nuclei more numerous. As their contents disappear so their walls coalesce with the intercellular substance, and their nuclei become imbedded in it. The number of these nuclei increases still more and they become darker by the in-taking of earthy matter; and, moreover, there is formed from the intercellular substance around each cell a group of dark corpuscles like the nuclei, which acquire a regular arrangement in regard both to them and to one another. Their arrangement is such, that if one imagines them connected with one another and with the nuclei by dark lines, and the cell places darkened by the reception of earthy matter, one will have the common bone-corpuscles with the dark network of the so-called calcigerous canals.

Periosteum. Purkinje‖ has found (as Pappenheim¶ also did) a copious network of nerve filaments in periosteum. He examined particularly that of the front of the tibia, and that of the vertebral canal. In the latter, which he describes as formed by an outer layer of the dura mater, divided into two layers at the foramen magnum, he found abundant bundles of fine-fibred nerves, which appeared to communicate through the intervertebral foramina with the sympathetic.

Muscular tissue: its structure, action. and growth. Stadelmann,** in transverse sections, could never detect any appearance of an empty space or canal in the axis of the striated muscular fibre, such as Valentin and others have described.

E. Weber†† has examined the mode of contraction of the muscular fibres

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* I cannot make out what is described as the mode of origin or development of the cells of this layer of substance. † No. xi.

‡ Mueller's Archiv, 1845, Heft. i. § See last Report. p. 37.

‖ Mueller's Archiv, Heft. iv, p. 289. ¶ Last Report, p. 10.

** Archives d'Anat. Gén. et de Physiologie, Janvier 1846, from the Report of the Scientific Meeting at Naples in 1845.

†† L. c. p. 15. The rest of his account of this tissue confirms Mr. Bowman's.

under the favorable condition of continued contraction, into which the frog's muscles are thrown when excited by the electric current from a rotating magnet. He finds that the contraction is always by simple shortening, and never by zig-zag flexures of the fibres; and that these are formed, as Mr. Bowman stated, only when the fibres relax, and by their elasticity tend to elongate. He shows also that, in contraction, the fibres do not become harder, and that it is only their increased tension when contracted which makes them feel harder, as it does also the tendons and other tissues at the same time.

Dr. Helmholtz* appears to have obtained a more direct proof than hitherto existed, that chemical decomposition accompanies the action of a muscle.—With all due precaution he electrified the hind legs (say the right hand legs) of frogs till their muscular irritability was exhausted, and then compared the results of the analyses of the muscles of these and of the other hind legs of the same frogs, which, except in that they had not been electrified nor in any way excited to muscular action, had been kept in the same external conditions. The result was, that in every case the quantity of alcoholic extractive matter was found to be increased in the electrified muscles, and the quantity of water-extractive matter decreased in them. The proportionate quantities of the former in the electrified and in the non-electrified were (on an average of six experiments) as 1·33 : 1, and those of the latter as 0·78 : 1. Similar results were obtained by like experiments with an eel-pout and with a pigeon. Of the other constituents of the muscles;—the changes, if any, in the fibrine could not be determined; the differences in the quantity of albumen were inconsiderable; the fat appeared unchanged. Counter-experiments were made which showed that electricity did not produce similar changes in muscles whose irritability had been previously exhausted, and that the changes were not due to ordinary spontaneous decomposition.

Together with many observations on the minute structure of the muscles of hymenoptera, Prof. Harting† has given an account of the comparative dimensions of the muscular fibres of the new-born child and adult, showing that the average diameter of the primitive fibres in the child is to that in the adult as 1 : 3·64 and the respective average intervals between the transverse striæ as 1 : 1·18. In the child the distance between the striæ is to the width of the fibre as 1 : 4·415; in the adult as 1 : 8·42. It is hence deducible that the ordinary growth of a muscle is due to the increase in size, not in number, of its primitive fibres; but whether this increase of size is due to enlargement, or to increased number, of the primitive filaments, could not be determined. It is evident also, that of the growth of a muscle in its length, part is effected by the increase in the length or depth of the particles (?) on which its transverse striation depends; but this is not enough to account for the whole growth of a muscle between childhood and manhood; so that part of the growth in length must be ascribed to the increased number of these particles (?).

He has also measured the muscles and their elementary parts in the healthy and the paralytic arm of an adult; from which it appears that the diminution of size accompanying the paralysis of the muscles was due to a diminution in the size, not in the number, of the primitive fibres. But with the diminution in the size of the muscles there was corresponding diminution of the nerves: both their trunks and their ultimate fibres were of equal size in both arms.‡

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* Mueller's Archiv, 1845, Hefte i, ii.

† Tijdschr, voor Naturl. Geschied, en Physiol. d. xii.

‡ See further on this subject in the section upon Nutrition. And on the several tissues refer to. 1. C. Bruch, Untersuchungen zur Kenntniss des kornigen Pigments; Zurich, 1844, 4to, containing a clear account of all that is known of pigments, whether healthy or morbid, with some original observations. 2. J. E. F. Knorz, De pili structura et genesi; Marburg, 1844, 8vo, of which a similar account may be reported. 3. Dr. Gregory, On the presence of Phosphate of Magnesia in Bone, and on a new method of obtaining Phosphoric Acid from Bones; in the

CIRCULATION.

Action of the heart. Dr. Mitchell,* in a case of ectopia cordis, watched the movements of the heart for an hour and fifty minutes. The pulsations were twenty-five in a minute before the separation of the umbilical cord; after it they fell to twenty, and then to seventeen. After the auricles were distended with blood they emptied themselves by a gentle flowing motion, and immediately after this the ventricles contracted. The effect of the ventricular contraction was to shorten the heart from base to apex, and to cause a considerable bulge or projection in the centre, giving rise to an evident elevation of the fingers when laid on it. The apex of the heart was *not* elevated. After the ventricular contraction the heart appeared quite flaccid and relaxed, although it was evident that the ventricles were not emptied.

Volkmann† has discussed the relation of the movements of the heart to the nervous system; and, to prove that they do not depend on the brain and spinal cord, he adduces the fact that they continue after the heart is cut out, while all the rythmical movements which do depend on the brain and cord, such as those of respiration and the lymph-hearts, cease as soon as their connexion with parts of those nervous centres is destroyed. He supposes, therefore, that the movements of the heart depend on the *immanent* power of the sympathetic nerve-fibres and ganglia in its substance. His experiments show, that if the auricle and ventricle, while pulsating rythmically and in harmony in a fresh frog's heart, be suddenly separated from each other, though they may both continue to pulsate they will not pulsate in harmony. And when the ventricle is divided by incisions carried through nearly its whole length, some of its portions will continue to pulsate spontaneously and rythmically, while others, just as large, will only move when irritated. He thinks that this shows that, in the former, central organs remained from which impulses for movement might proceed, while in the latter there were no such central organs or ganglia. He concludes, therefore, that the ganglia in the heart are so many central organs, or points from which moter impulses flow out, and that they are suited for action in concert by connecting nerve fibres, forming altogether a system so arranged as to produce, in regular series, the successive contractions of the muscles of the heart.

This theory coincides in many points with that of Kurschner, alluded to in the last report; and he holds that the auricles are the parts to which the reflex influences of the ganglia are always first directed, and that the contraction of the ventricles is the consequence of the contraction of these. In evidence of this he says, that whatever part of a heart, when cut out but still irritable, is irritated, the consequent contraction always begins in the auricle. [But it is certainly not always so: I have many times tried the experiment in cut out turtles' hearts, and have always found that the contraction begins at the part irritated and thence extends over all the rest.] Valentin,‡ also, has made experiments on the same question, but he doubts whether the rythmical contractions of the heart are dependent wholly on its nervous system, and not in

Medical Gazette, April 11, 1845. 4. C. G. L. Bruch, Nonnulla de Rigore Mortis; Magunt, 1845, 4to, confirming by new evidence the established view that the rigor is due to the contraction of the muscles. 5. Harting, Histologische Aaanteekingen; in the Tijdschrift already referred to. Besides the papers on the muscles, lens, and nerves, of which an account is here given, the essay contains remarks on the milk-corpuscles, the action of sublimate on the blood-corpuscles, and other questions of the anatomy of tissues. The continuations of the admirable works of Dr. Sharpey, in the new edition of Quain's Anatomy, of Dr. Todd and Mr. Bowman, in their Physiological Anatomy, and of Mulder, in his Physiol. Scheikunde, are also important contributions to the recent history of the general anatomy.

* Dublin Journal of Med. Sciences, Nov. 1844.

† Mueller's Archiv, 1844, Heft iv, p. 424.

‡ Handb. der Physiologie, Bd. ii, p. 767.

part on the mechanical arrangement of its fibres; since, he says, the continuance of rythmical movements is observed only in those pieces of the heart which remain connected with portions of the tissue intermediate between the auricle and ventricle. Kolliker,* also, while he agrees with Volkmann's explanation of the rythmical action of the heart, states this as a fact which he, like Valentin, has observed in frogs' hearts. [But in the hearts of turtles, the size of which makes the fact evident enough, it is very different; I have often seen the apex of the heart continuing its rythmical contractions long after being cut off from the rest of the ventricle. Both auricles also continue similarly contracting when cut off above the auriculo-ventricular rings; but their contractions are not synchronous. Indeed, I do not remember to have ever seen any difference, in respect of the continuance of their rythmical action, whether the pieces cut from the heart were or were not connected with the tissue uniting the auricles and ventricles.† I believe, therefore, that Volkmann's theory may be held as true; but that there is no evidence at present for assigning a peculiar locality for the chief centres for harmonizing the rythmic actions of the heart.]

Arteries. An account of the structure of the blood-vessels, confirming in all respects that of Henle, and supporting the microscope by chemical evidence, so far at least as the action of acetic acid and potash on the several coats is concerned, is given by Mulder,‡ from examinations by himself and Donders.

The nerves of the arteries are among those which Purkinje§ has submitted to renewed investigation. He finds by the help of acetic acid, all the plexus of arteries in the rete mirabile of the ox surrounded by a close web of nerves.

In an excellent essay on anatomy of the parts concerned in laryngotomy and tracheotomy, Dr. Gruber‖ has treated especially of the middle thyroid (A. thyroidea ima. seu Neubaueri) and the crico-thyroid arteries. The former occurs in about ten per cent. of the bodies examined, arising in general from the innominata, more rarely from the arch of the aorta or the common carotid, more rarely still from the inferior thyroid or thyroid axis, and most rarely from the internal mammary. It probably never arises from the left carotid; when it appears to do so the artery thus arising should be called a second inferior thyroid. Its diameter varies from half a line to two lines and a half; in two cases, with healthy thyroid glands, its trunk measured five lines. It is more frequently on the right than on the left side; and once only in ten cases was double. It may proceed vertically up the front of the trachea to the isthmus or either lobe of the thyroid gland, or it may pass obliquely across to the trachea, even going from its right side to the left lobe of the gland.

In regard to the crico-thyroid artery, Dr. Gruber notices especially that arrangement in which it is very large on one side, having a diameter of from three-qarters of a line to two lines. This is most often observed when there is a middle lobe to the thyroid gland, but the large artery is not always on the same side as the middle lobe. Such an artery occurs about once in every four persons, and is more frequent on the right than on the left side. Its place may be taken by the thyroid branch of the right superior thyroid artery. The large artery in most cases bends down at a right angle in front of the cricro-thyroid membrane, to reach the isthmus of the thyroid gland, and gives

* Die Selbstandigkeit und Abhangigkeit des sympathischen Nervensystems. Zurich, 1844. p. 39.

† It is, however, I think, necessary for the propagation of contraction from one part of the heart to another, that they should be connected together by muscular tissue; probably because there are nerves in it. If two portions of a heart be held together by a very narrow isthmus of muscle, the contraction excited by irritating one will extend to the other; but this will not happen if the connecting isthmus be tendinous tissue, even though it be a piece of the auriculo-ventricular ring.

‡ Physiologische Scheikunde, St. vii, p. 664. § Mueller's Archiv, Hefte iii, iv.

‖ Oesterreich, Medic. Jahrbucher, Mai u. Juni, 1845.

off at its bend a small branch which crosses the membrane. More rarely the trunk of the artery crosses the crico-thyroid membrane and reaches the opposite side. It gives branches to the larynx through the crico-thyroid membrane, to the middle lobe of the thyroid gland, to the lobe of its own side, and when it crosses over, to that of the other side also, and very often to the isthmus of the gland.*

Capillaries. Observations on the development of the capillaries in tadpoles and the tails of young tritons by Platner† indicate, as some others have done, that new capillaries are formed by outgrowths from old ones. First, capillaries are seen which have abrupt blunt closed ends; some of these have no trace of prolongation, but many exhibit a very thin, long *outrunner* which is gradually lost sight of; and in some cases it may be seen how two such thin canals as these unite and form one arch, which gradually increasing becomes a new capillary loop. At first this is filled by finely granular matter, and is too small to admit blood corpuscles. It soon acquires distinct walls, but the nuclei found on fully developed capillaries are subsequent formations.

Veins. Henle described generally the differences of structure of veins in different parts of the body according to the degree in which the longitudinally fibrous coat is developed; that in some it is hardly discernible, in others strongly marked; but he did not indicate the veins thus differently conditioned. The result of many dissections by Dr. Norman Chevers‡ is that, as a rule subject to but few exceptions, the deep-seated veins of the trunk have their proper or middle coat (wherein I suppose he includes all between the striated coat and the external cellular coat) composed almost entirely of circular fibres; while in the external and superficial veins there is in the same situation a strong internal layer of longitudinal fibres, and a thinner layer of circular fibres next external to it.

Venous pulse. Two cases of pulsations in the veins of the back of the hand, coincident with the pulse of the arteries, are recorded by M. Martin-Solon.§—Both occurred in patients who had been largely and repeatedly bled for pleuro-pneumonia. M. Poiseuille, in a discussion on the essays, referred to cases recently recorded in a thesis by Condret, in which the persons presenting the venous pulse were strong young men with full arterial pulses.||

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* For the practical deductions from these anatomical facts I must refer to the orginal paper.

† Mueller's Archiv, 1841, Heft v.

‡ Medical Gazette, Aug. 8, 1845.

§ Bull de l'Aacadémie de Médecine, Nov. 1844, pp. 102–116.

|| All the physics of the circulation are well discussed by Bergmann in his article, "Krieslauf des Blutes," in Wagner's Handworterb. der Physiologie. There is an exposition of the theory of the wave-movement of the blood in the arteries, and of the mechanism of the pulse, by Dr H. Frey, in Mueller's Archiv, 1845, H. ii, iii.—His essay is in full support of the theory as commonly received: against it there are, an anonymous very clever essay, "On the Cause of the Pulse," in the Medical Gazette, July 11, 1845, and one with the same title by Dr. Thomas Williams, in the same, July 25, 1845. The opinions in these two essays differ in degree rather than in kind. It seems to me that the existence of what may be justly termed a wave of blood raised by each contraction of the ventricle, is quite certain: the main question is, how far does the first wave extend? None of the essays answer this question; and it is not possible to give an answer which shall be always true, for both the breadth and height of the wave must, in different cases, depend on very many variable conditions, of each of which we can as yet obtain only an uncertain estimate. Other works of more or less interest in the anatomy and physiology of the circulatory system are,—Parchappe, Sur la Structure . . . du Cœur; Paris, 8vo, 1844. Pliny Earle, Observations [with extensive statistics] on the pulse of the Insane; in the American Journ. of Med. Science, January 1845. Norman Chevers, On the effects of obliteration of the Carotid Arteries; in the Medical Gazette, Oct. 31, 1845.—Bouisson, Sur les lésions des artères fessière et ischiatique; in the Gazette Médicale, Mars 1845, containing measurements of the exact relative positions of these vessels. Prevost et Lebert, Sur le développement des organes de la circulation . . . du poulet; in the Annales des Sc. Nat., t. i, 1845.

Part Fourth.

MEDICAL INTELLIGENCE.

FOREIGN.

1.—*On Valerianic Acid, and a new substance produced from Caseine, and on Vinous Fermentation.* By Baron Liebig, Professor of Chemistry, Giessen.

Mix thoroughly pressed cheese, prepared from fresh or curdled milk, and freed as much as possible from adherent particles of butter, with an equal weight of hydrate of potass (or highly concentrated hot solution of potass;) melt the mixture by the application of heat, and keep it in this state of fluidity until hydrogen gas begins to be evolved, and also ammonia; dissolve the mass now in hot water, super-saturate the solution slightly with acetic acid, filter, and allow the filtrate to cool, when a mass of very fine needles will separate. This mass is very difficultly soluble in cold water, and absolutely insoluble in alcohol and ether; by repeated solution in water slightly mixed with carbonate of potass, and subsequent re-precipitation with acetic acid, the substance of which this mass consists is obtained in crystaline needles of a pure white colour and silky lustre.

The composition of this substance is expressed by the formula $C_{16}\ N\ H_9\ O_5$; the analyses, however, from which this formula results, require confirmation. This substance, although readily soluble in alkalies, combines with acids; the mother liquor from which it is crystallized yields subsequently, upon further evaporation, a copious amount of Leucine.

If the melted mass is super-saturated with tartaric acid instead of acetic acid, and the fluid is subsequently subjected to distillation, a distillate of an acid re-action is obtained; upon saturating this with water of barytes, evaporating the mixture to dryness, and distilling the dry barytes salt in conjunction with phosphoric acid, a colourless, oily acid, and an aqueous acid fluid, possessing the odour, and all the properties of valerianic acid, are produced.—Leucine, when fused with hydrated potass, yields ammonia and hydrogen gas, and the residue contains valerianate of potass. The formation of velerianic acid, upon melting caseine with potass, seems, accordingly, to be preceded by that of leucine. Except one equivalent of hydrogen, the formula of leucine expresses the composition of an ether, which consists of one atom of cyanic acid, one atom of oxide of amyle, and two atoms of water. The transmission of the vapour of hydrated acid through anhydrous fousel oil, causes the formation of a solid crystalline substance, soluble in water and ether; this substance crystallizes readily from aqueous and ethereal solutions, and bears in its external appearance the very closest resemblance to leucine; its solubility in ether, however, distinguishes it from the latter substance.

Long protracted melting of the mixture of casine with hydrated potass causes the formation of a copious amount of butyric acid by the side of valerianic acid. The salt of silver prepared with the oily valerianic acid produced from caseine, left, upon combustion, 51.62 of silver; this removes all doubt regarding the identity of this oily acid with the common valerianic acid. The crude distillate contained, besides valerianic acid, a volatile substance exhaling the odour of human excrements; this substance reduced nitrate of silver to the metallic state, but it contained no formic acid; the alkaline solution yielded a copious crystallization of oxalate of potass, previously to the super-saturation with tartaric acid.

M. Mulder states, that while acting with potass upon albumine, he obtained two greasy, syrupy substances, which he denominates protide, and erytroprotide. I did not observe the formation of any such substances by the action of potass upon caseine, nor do I believe that either M. Mulder himself or any other chemist will ever produce them again of the same composition which he assigns to them. These substances are simply a mixture of intermediate products, the composition of which alters according to the different state of the temperature, the duration of the action of the alkali upon the albumine or caseine, the concentration of the alkaline solution, &c.—*London Lancet*, *June*, 1846.

2.—*Case of Scirrhous Ulceration of the Œsophagus communicating with the Trachea.* By J. P. Harris, Esq., Surgeon to the Eastern Dispensary.

Henry Noble, aged 56, labourer, of good conformation, sanguineous temperament, states that he has always enjoyed good health, until about three years ago, when he had an attack of synovitis of the knee-joint, which ended in ankylosis. He has been and is accustomed to drink freely of ardent spirits, though irregularly.

About a week before christmas, 1844, he experienced some difficulty in swallowing, especially solids. He was not led to seek assistance until a month subsequently, when, after three or four days indulgence in drinking rum, the difficulty of swallowing returned, attended with pain at the epigastrium. He applied for relief at the Northern Hospital, (where he was treated by the daily use of bougies,) and left at the end of a week, seemingly improved, his food meeting with little obstruction in its passage to the stomach. The relief, however, appears to have been of short duration, as a few days after he experienced a relapse, in which instance he does not appear to have sought medical aid.—He continued to suffer from the recurrence of the above symptoms for the space of two months, when he applied to the Eastern Dispensary.

His symptoms were as follows:—Great emaciation; distress of countenance; sense of burning and constriction referred to the œsophagus; cough, with the occasional expectoration of a kind of tenacious mucus, tinged with blood; pain and a sensation of sinking referred to the epigastrium, the result of prolonged abstinence from food; bowels confined, have not been relieved for fourteen days; headache; dimness of sight; great thirst; pulse 90, small and thread-like; temperature of surface much reduced; urine scanty. Percussion yields a tolerably clear sound over the chest, at the upper part of which the respiration is bronchial.

Every attempt to swallow food (which consists entirely of fluids in very small quantities) is attended with much suffering. The food seems to pass no lower than about the middle of the chest, and after being retained for a time, varying from a few seconds to two or three minutes, is rejected by an inverted action of the œsophagus, and the small quantity which seems to reach the stomach is frequently rejected by vomiting; the matter discharged has an offensive smell;

the patient fancies his food takes some unusual course besides that to the stomach.

An elastic tube was with little difficulty passed to the middle of the œsophagus, but any attempt to get it beyond excited so much distress as to compel its disuse. Nutriment was administered by the rectum, which sustained life for three or four weeks, though in a miserable condition, when he appeared to sink from the effects of inanition.

Sectio cadaveris, fifty-six hours after death. The apices of the lungs were thinly studded with tubercles, varying in size from a millet seed to a horse bean: the bronchial tubes were filled with a frothy mucus, and their lining membrane slightly congested; the glands at the bifurcation of the trachea were mottled in appearance, hardened, and much enlarged. On tracing the œsophagus to its middle, it was found very firmly adherent to the spine and surrounding tissues for three or four inches of its course down to its passage through the diaphragm, and to have undergone complete scirrhous degeneration, passing into the ulcerative or carcinomatous state; the surrounding cellular tissue was of a cartilaginous hardness; the aorta, though adherent, seemed free from disease. The trachea was closely united to the diseased gullet, and communicated with it by an ulcerative opening, large enough to admit a good sized catheter; the bodies of the dorsal vertebræ and intervertebral cartilages had began to be involved in the disease; the heart and abdominal viscera were much paler and thinner than usual, but otherwise healthy.—*Edinburgh Med. and Surgical Journal, July*, 1846.

3.—*Influence of the Quality of the Milk of the Nurse on the Health of the Child.* By M. Girard. (*Journal de Pharmacie et de Chemie*, October, 1845.)

A young lady fifteen days after her delivery, found it necessary to procure a nurse for her child, and she succeeded in getting one whose milk was only fifteen days older than her own, and which presented all the physical characters of good milk. The nurse was besides young and in apparently good health. After a few days the child began to sleep ill; it was attacked with a troublesome diarrhœa, and sensibly fell off; shortly after this it rejected all its food, and the mouth got foul with aphthæ. As the ordinary medical treatment failed to relieve the child, the nurse's milk was examined, and was found to present the following characters:—The milk was neuter, of an ordinary consistence, and became slightly viscous by ammonia. The globules were very voluminous, and instead of appearing under the microscope of a brilliant pearly lustre, were of a dull white opal colour. The whole field of the microscope was scattered with rounded particles, of which the form was comparable to that of a flattened raspberry, and presented the characters usually assigned to the corpuscles of colostrum. A new nurse was, therefore, instantly procured, whose milk presented the microscopic properties of healthy milk. Within forty hours after the child had sucked this nurse the vomiting and diarrhœa had ceased, the aphthæ were greatly diminished, and a few days sufficed to restore the child to strength.

In another case where the mother was the nurse, the child, two months old, suffered from constant vomiting, was falling off, and had occasional comatose attacks. The milk of the mother, when examined by the microscope, was found to contain a considerable quantity of mucus. After awhile the child vomited less, and the milk being anew examined was found to contain fewer mucous globules. Soon, however, the vomitings and other disagreeable symptoms became worse, and to save the child it was absolutely necessary to change the nurse. Eight nurses were examined, but the milk of seven was rejected on account of containing globules of mucus, or granular bodies, or from having undergone some other change. The eighth nurse having apparently healthy milk, was chosen, and within two days the vomiting and other symptoms had disappeared, and did not again return—*Ibid.*

4.—*New Researches on the Movements of the Heart.* By Signor D. Gola.—(*Annali Universali de Medicina*, April 1845.)

Most physiologists are agreed that the impulse of the apex of the heart against the thoracic parietes correspond with its systole. The contrary opinion has, however, been maintained by several eminent writers both in this country and in France. Signor Gola's researches were undertaken with the view of ascertaining to what movement of the heart the impulse of the apex against the thoracic parietes correspond. Having with much care exposed the heart of a frog, he found that the contractions of the heart were accomplished in the following order.

The systole of the auricle preceded the diastole of the ventricle; in its dilatation the ventricle increased at its base by three lines in diameter. In proportion as the ventricle became distended, the surface became flattened on its greatest diameter, and the whole heart executed a slight movement of rotation from left to right, in consequence of the dilation being greatest towards the right side of its base. By virtue of this rotary movement the point moved about a line towards the left side, but was not sensibly elevated.

To the diastole succeeded the systole of the ventricle, and during it the transverse diameter of the ventricle at its base diminished by three lines, but this part of the heart gained in height what its lost in breadth and in extent; it took a lengthened form, and the apex was elevated from its plane and sensibly carried forwards.

When a fine straw, fixed at its two extremities, was laid across the heart, so that during the diastole it simply touched the surface, it was sensibly raised during the systole. By placing this straw longitudinally to the axis of the heart, and allowing one end to move over a graduated scale, he found that while during the diastole the straw was only raised to the extent of one-tenth part of a centimeter, during the systole it was raised eleven and one-half tenth parts of a centimeter.

These and other experiments led Signor Gola to conclude that the impulse of the apex against the ribs and the contraction of the auricles coincided. In conclusion he remarks, that he has often introduced his hand into the pericardium of animals while being slaughtered for food, and invariably remarked that the heart during its contraction struck with force against the hand, and that the impulse was sometimes so strong as to cause a disagreeable sensation.

Several valuable papers have appeared in the two last numbers of the British and Foreign Medical Review, on the Natural History of Diseases. These papers have been elicited by Dr. Forbes' article on Homeopathy, Allopathy, and Young Physic. From one of them (Dr. Laycock's) we extract the following remarks:

"I have said that the *natural order* or *law of sequence* of vital phenomena is a point necessary to be known in constructing a natural history of diseases. We never can know the results of medication without a knowledge of the physiological antecedents; you are aware how much careful and earnest attention I have devoted to this point, and you know the result of my inquiries. It is simply this, that starting in the rudimentary germ, vital phenomena advance in waves of greater or less magnitude, the crest of the wave being the morbid period; that the periods of these oscillations vary in length from a few hours to several years; that they can be measured; and that they determine the periods, crises, and duration of diseases, and the order of their phenomona. As you have honoured me with a notice of my doctrines in No. XXXV. of your Review, I need only refer to it. In the history of the more common fevers, the utility of these doctrines is manifest enough. As they run through a known and determinate cycle, the action of remedies may be better estimated than in those in which the periods are not thrust before the eyes of the practitioner. But even with regard to the more common forms, there are many "post hoc ergo propter hoc" absurdities committed. The natural and regular crisis or termination has often been triumphantly attributed either to some paltry medicine, a decillionth globule perhaps, or a few drops of hay tea; or to the effects of some heroic dose,—ten or twenty grains of calomel to wit—or a frightful drastic, in which case it was really nature winning a double triumph, conquering both the doctor and the disease.

"It is very manifest that any medicine whatever (the more inert the better for the purpose) may be so administered that the number of recoveries, stated numerically, shall be more recommendatory of its activity than can be produced in favour of any other medicine. Suppose I was able in all cases of pneumonia to ascertain the day when the first rigor began, and suppose that I treated them all on the expectant method, but on the eve of the 7th or the 14th day I invariably administered one grain of rhubarb. The amendment on the next day would, in the majority of the cases thus treated, be so manifest that, without a knowledge of the law of sequence, the evidence in favour of the impotent grain, as an active and effectual remedy for pneumonia, would be absolutely irresistible. If amendment followed its administration in 28 cases out of 30, the probable ratio, (vide Fleischmann's table), how could you invalidate the "ergo propter hoc?" You could only answer, "your grain of rhubarb is like a literary pirate, it claims merit not its own. When the disease began then began also a series of changes, the purport of which was a restoration to healthy action."

"Now on this point at least I belong to the *old* physic school, the real old school. The ancient physicians and their successors, studied the natural history of disease very closely, and applied this law very systematically to the treatment of febrile affections. Although I specify febrile diseases, please to remember that the course of *all* diseases is regulated by it, because all vital phenomena are. You may not always be able to trace it; several points are requisite for this, as undisturbed action in the organism, the power and habit of acute observation, manifest phenomena, and the like. But be certain that the natural history of all diseases will be fallacious unless this principle be made a leading idea in the observation and classification of pathological phenomena.*

"I might refer for another illustration of the important bearing physiology has upon a true pathology to the phenomena of the nervous system. But we are really on the verge only of the vast field these present to us for observation and analysis, and I fear I should only weary you with long explanations; I therefore forbear. I think, however, I have said enough (I hope not too much) in support of my main argument. Yet would it be believed that there are able, nay, scientific, physicians who virtually, at least, prevent the application of physiology to the elucidation of pathology? You occasionally hear half-educated practitioners speak slightingly of him who devotes his leisure to physiological as well as to empirical reading. They hint that he is a "mere reader," "a theorist," "not a practical man," &c., but this is usually in the way of business; it is an ignorant dealer and chapman trying to extinguish a dangerous rival in the true artist. No great harm can result, as the pretender must needs yield at last.

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* Dr. Graves has published an interesting essay on the law of relapse periods in agues. The foundation of the essay is a case of quartan ague, which continued with intervals of health for more than two years. Dr. Graves found the relapses took place, with one or two trifling exceptions, exactly on the days on which a fit *would* have happened, provided they had recurred regularly. Now this law, which Dr. Graves publishes as a discovery, and of which this case of Dr. Graves' is a good demonstration, has long been known to me, and is simply a corollary deducible from my general law. It is, however, an interesting example of the utility of the latter, for by it Dr. Graves might have *predicted* the facts he observed so accurately.

Not so, however, with the able physician ; such an example as I met with not long ago in a weekly journal. The Editor, in reviewing a book on practical medicine, had animadverted in a passage (I think in the introduction) in which the author, an accomplished physician and a physiologist, observed that scientific men are not and cannot be practical, because they have had no experience. The Editor was "pulled up" by the offended author, and positively the latter, in his letter of reply to the criticism, repeated his offence.

"It is obvious enough that a physiologist *and nothing else* cannot be a good physician ; but a practitioner may be a physiologist and *something more.* While he studies the experiments of nature, he may have an acute vision for the minute shades of disease, and a quick perception of the effects of medical agents. Nothing will escape his prying eyes. He will be as fertile in curative resources as in theories ; as keen a lover of practical information as of scientific knowledge. Further, this practical study of physiology is in itself most interesting. In effecting it you have not the trouble of torturing, and dissecting, and analysing. If, for example, you want illustrations of reflex action, your first patient will probably present them, look closely only. If not seen in your first, they will appear in your second ; at all events, wait patiently, and you need not go and catch frogs or snails to vivisect. Indeed you may study the physiology of the nervous system everywhere, in everybody ; in the man that walks before you in the streets, that stands before you in the pulpit, that struts his hour upon the stage, in people in the ball-room or at a dinner table ; nay, you can make your bachelor friend, who pleasantly chats tête-à-tête with you at your fireside, wagging his leg the while, a subject of physiological research. All these, and a thousand more, are good examples of subjects for scientific investigation, presented by persons in perfect health. But when the proteiform diseases of poor human nature come before you, in all their varied phenomena, what an inexhaustible store of matter for observation and meditation !*

"You will see that I think a physiological practitioner is the man of the age, and that it is of essential importance to the reformation you advocate that the practitioner should be a thoroughly good physiologist. He should have the science as ready at hand as his snuff-box. He should be *trained* to its use ; for every symptom he should at least seek out, if he cannot find, a *rationale.* He should always be curiously catechizing physiology for the why and the wherefore. Much might be done in the training of the student by our clinical lecturers, if they would set their shoulders to the wheel ; but, unfortunately, the empirical method is the easiest to teach as well as to practise. The student might be led on step by step. He might, for example, first have simple pathological problems in muscular action given to him. A man with morbus coxæ

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*I cannot help giving you a small and homely illustration of these views, which has just occurred to me in looking over Dr. Ranking's excellent Half-yearly Abstract. At p. 72 of vol. ii, you will find two modes of treating scabies, the one the consequence of physiological and microscopical research, the other of empiricism. Now, firstly, scabies gets into very respectable families, and is no jest when a guest with them ; 2dly, sulphur, its best cure, stinks. Mr. Stiff, a "*practical* man," and evidently of the right sort, namely, a physiological practitioner, meditates on these two points, and then on the disease. Scabies depends on an insect, the acarus : so the microscope tells us ; the acarus belongs to a class of insects that breathe by tracheæ : this entomology sets forth. Comparative physiology shows that (like men), if insects have their trachæ stopped up, they die. Now, Mr. Stiff argued that if the acari were smeared over with an oleaginous substance, their tracheæ would be stopped up, and they would be suffocated ; consequently that, after all, it is the lard, and not the stinking sulphur, which cures the itch. He tried the simpler method, and has cured more than forty cases with simple inunction alone. The other example is the *empirical* use of veratria alba, solemnly published without a reason in the Annales de Thérapeutique. Now this, as well as half a dozen other *poisons of the acarus,* have been known empirically to vagrants from time immemorial.

steps across the ward before a clinical class. Problem as follows :—Required to describe the bones and muscles entering into the deformity, and the rationale, physiological and pathological, of its production. Or if the students be more advanced, five patients are brought under the notice of the class ; one is in an apopletic fit, a second has scoliosis, a third has valvular disease of the heart, a fourth has phthisis, a fifth, pneumonia. Required to describe the peculiar characteristics of the respiration in each case, and to give a physiological and pathological rationale of them. Would not this method sharpen the diagnostic and therapeutic wits, and be at least one safe and sure step towards a Natural History of Disease ? Alas, for the " practical men," those " dealers and chapmen," that sneer in the way of their *métier* at physiologists as " mere theorists," and " bookworms !" What a rout a few swarms of students so trained would create among them !"

AMERICAN MEDICAL INTELLIGENCE.

CARTWRIGHT ON SOUTHERN MEDICINE.

[Under this title we have just received the following communication from our esteemed correspondent Dr. Cartwright, and we have deemed it our duty to insert it, though it will be to the exclusion of our customary extracts from the American Journals. It appears that Dr. C. only *very recently* met with the number of the *Bulletin of Medical Science*,* containing Dr. Bell's severe strictures on his "Address before the Mississippi Medical Convention ;" and without delay, and whilst labouring under indisposition, he prepared and sent us this reply and defence. It will be read with interest, and will serve as well to correct some "*fallacies*" which prevail at the North, in regard Southern diseases, as to re-kindle admiration for the writings of Hippocrates. As so much time had elapsed since the appearance of Dr. Bell's *critique* without our hearing any thing from Dr. Cartwright, we prepared an article on the subject ourselves ; but cheerfully give place to Dr. Cartwright.

EDRS.]

An attempt, lately made by me in the Mississippi Medical Convention, to direct the attention of Southern physicians to the works of Hippocrates, as the best source of medical knowledge, has been ridiculed at the North, and particularly in Philadelphia, in Dr. Bell's *Bulletin of Medical Science*, as an effort to set up a species of "States-right Medicine." I do not propose to enter upon a controversy, as no good grows out of controversies either medical or religious ; but to explain, a little more fully, some points alluded to in the Address, which its brevity necessarily excluded. In doing so, Dr. Bell and the Northern physicians will find their objections answered, and I hope those of the South will meet with some observations not destitute of practical importance.

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* See number for June, 1846.

For the benefit of those who have not seen the Address and the review, it may be well to premise, that I have been taken to task for asserting, (what the northern physicians call a popular fallacy,) "that many of the diseases of the south have not been well described, nor their best treatment given by northern writers," "that there is but *one book*, which gives an accurate description of diseases daily met with in southern practice; viz: Hippocrates"—for asserting that, "when civilization travelled north, Medicine went with it, and from a science nearly perfect in the climate of Greece, it became a very imperfect science when transplanted into the cold latitudes of the north of Europe, and was there pruned and trimmed to suit the climate"—that "Medicine, thus reformed, may be the best for England, and the north of Europe, but is not as good for southern latitudes as it was before the northern physicians reformed it"—"that more knowledge can be obtained, in regard to southern diseases, by cutting across direct to Greece and drinking at the fountain head, than by seeking it through the circuitous route of Edinburgh, London, and Paris"—"that the north has improved the medicinal agents with which we work, but no improvement has been made in the description that Hippocrates has given of Southern diseases, and the laws governing them, and it is doubtful if any improvement in that respect can be made."

The reason might have been added—"*Description*, like sculpture, painting, oratory and poetry, had arrived at its highest perfection in the days of Hippocrates." When Homer is excelled in poetry—Demosthenes in oratory—Appelles in painting, and Phideas in sculpture, we may look for a better, clearer and more concise description of Southern diseases, one truer to nature, than what Hippocrates has given.

The above contains the propositions in the address, which have been called in question. The assertion, "that the climate of Greece is very similar to the climate of Mississippi and Louisiana," has also been denied.

The North is very prolific in authors and compilers of books on medical subjects.

The assertion in the Address, that I know of but *one book*, which gives an accurate description of diseases occurring in the daily practice of Southern physicians, appears to be the most objected to by the Northern book men.

Dr. Bell, the reviewer, is himself the author and compiler of two large volumes, entitled "Bell & Stokes' Practice," containing some chapters on congestive fevers, intended to enlighten Southern physicians.

Extracts from this work are interwoven in the Review, to convict me of the heresy of "States-right Medicine." It may be "States-right Medicine," to prefer Hippocrates to the very clever work of Bell & Stokes. It may be plead, with much show of argument, that Bell & Stokes, and all other modern writers have had the benefit of the world's experience since Hippocrates wrote. I plead the Hippocratic maxim "*Experientia fallax*," and deny that experience drawn from the icy region of the North, is applicable to the diseases of the South. We must, in the South, begin *de novo*, or go back to Hippocrates and the ancient Southern writers on Medicine, in order to profit by experience in Southern diseases; because, with a few exceptions, about to be mentioned, there has been no experience in Southern diseases since the downfall of Grecian and Roman Empires. Medicine in the South, fell with the ancient Greeks and Romans, because there was no freedom and civilization to nurture and uphold it, in any Southern country in the whole world, after the blood of the free and civilized *white man proper* had all been spilt in civil wars and by the incursions of the barbarians. More than twenty centuries after the fall of the Grecian, and about sixteen from the fall of the Roman Republic, *the civilized white man proper* again made his appearance in the South, first as soldiers and sojourners in the East and West Indies—afterwards making a few settlements therein—and at length establishing himself permanently in Virginia, the Carolinas, Georgia and Louisiana. The colonies which Spain and Portugal had previously planted in the warm climates of Asia and America are not civilized now, much less then, as they, like the present inhabitants of Southern Europe, want those essential ingredients of true civilization, the spirit of liberty, moral virtue, industrious habits and the general diffusion of common school education among the masses.

Up to the close of the last century, Virginia, the Carolinas, Georgia and Louisiana, (with the exception of a few settlements made in the East and West Indies by France and Great Britain,) were the only Southern countries, which afforded a home to the civilized white man proper, and opened the doors to the science of medicine in the South, since its overthow in Greece and Rome, and the downfall of the Republics in the North of Italy. Since the close of the last century, the civilized white man proper has spread himself far and wide in the South—from the Potomac to the Gulf of Mexico, and from the Atlantic to the Rio Grande, and now occupies a Southern territory, which would make a dozen of ancient Greece and Rome. It is true, as Dr. Bell says, that many eminent physicians, (nearly every one of whom were educated in the doctrines of the Edinburgh school,) have written tracts and volumes on Southern diseases. But their observations were mostly drawn from British soldiers in Southern garrisons, and from the sparse settlements of white men, chiefly unacclimated, in the East and West Indies. These authors looked at every thing *Southern* through Edinburgh spectacles. Johnson, who wrote the popular work on "*Tropical climates*," scarcely ever saw a dozen white men, who were natives of the climate he wrote about.

The other writers on Southern diseases, had scarcely any better advantages.

Like Johnson, they were strangers to the climate themselves, as well as most of their patients. Hippocrates' observations on Southern diseases were not drawn, like Johnson's, Jackson's, Chisholm's, Hillary's, Hunter's, and other British writers, from the diseases of a few straggling white men in Southern climates, but they were drawn from the millions of native-born Southern freemen in the Grecian Republican States, and aptly apply to the millions of like people at the present day inhabiting the Southern States of America. For many centuries after Hippocrates wrote, experience proved the truth of his precepts and the correctness of his observations, not only among the millions in Greece, but the millions in the Roman Empire also. In the language of Rome, his works were called the "*aureum opus.*"

Through the dark ages they afforded a constant light, dimly seen through the surrounding mental obscurity, and on the revival of learning, the brightest star was the "*Aureum opus.*" Guided by that, the republics of the North of Italy brought Medicine forth as the first trophy of returning civilization. The inductive philosophy of Lord Bacon is nothing more than a practical application to the other sciences of the Hippocratic method of arriving at truth in Medicine.* Since the fall of the republics in the South of Europe, there has been no opportunity, in Southern latitudes, until very recently, of making observations on the diseases of that portion of the human race who are foremost in civilization; because Southern climates ceased to be the home of the civilized white man proper. That ancient Greece was inhabited by the white race proper, is sufficiently proved by the frequent mention by Hippocrates of the variety of color in the hair, eyes and the complexion of the skin, corresponding so exactly with the appearances of the people of the United States, as to attest the identity of race beyond a doubt.—So long, however, had the white man proper been excluded from Southern climates, that it was supposed, the South was uncongenial to his nature, and that he would soon degenerate therein. Owing to this delusion, which for a long time possessed the British mind, Washington, most fortunately for America, was refused a captaincy in the British service.

Virginia, and the colonies North and South of her, dispelled the delusion, and by union became independent States, as far as political matters were concerned. But in regard to Medicine, both the Northern and Southern States are to this day, under a species of colonial vassalage to the Northern country. In comparison to Great Britain, the United States, North, South, and West, *are all Southern States*, if climate be defined by summer temperature. In summer temperature, they very slightly differ among themselves; whereas the coldest of them, has a summer temperature fully fifteen degrees above Great Britain.—Great Britain differs from the United States in every essential particular. In climate—being cool in summer, and comparatively to our Northern States, warm in winter; instead of intensely hot in summer, as all the States are, and cold in winter, as our Northern States are;—in having a murky, foggy atmosphere, with almost constant falling of light chilling rains;—instead of the clear bright heavens overhead, agitated and refreshed, at long intervals, by the awful tornado and heavy deluges from the clouds;—in government—a limited monarchy in name, but practically a most odious oligarchy, instead of a free and independent republic;—in food—in clothing—in house-room, and fuel. Disguise it, no matter how, the fact is undoubted, that three-fourths of the unhappy people of the North of Europe, (intended by nature for a better destiny,) have not enough of the substantial comforts of life to be consistent with the healthy functions of the human system. Want of a sufficiency of a wholesome food—want of clothing, and bed-clothing—want of fuel, and want of house-room enough to prevent the morbid influences of human effluvia, are the prolific causes of three-fourths of

* "I repeat it: in the writings of Hippocrates you will find the principles of the inductive philosophy. A physician showed Bacon the road to immortality." (*Dr. Stokes.—See Bell and Stoke's Practice, vol.* 1, *P.* 18.) (N. O. Edr.)

the fevers, and surgical ailments afflicting the people of the monarchical governments of Northern Europe. Not one of which causes have any appreciable influence, whatever, in causing disease in any part or portion of the United States, except in a few instances of gross improvidence, mostly from intemperance. Even in Great Britain, the monarchy which boasts of its freedom, surgical diseases and typhus fevers rise and fall with the price of wheat.

Nearly all the surgical complaints, apart from accidents, are caused from an impoverished state of the blood, from inanition and the want of the common comforts of life—the natural fruit of that monarchical device of extorting work by creating want.

Nearly all the surgical maladies, afflicting the laboring classes of Europe, are artificial creations, together with a large majority of the febrile affections, and point directly to monarchy and its oppression as their cause. They tell a tale to the medical philosopher, of the evils of monarchy, more horrible than the freemen of America have ever imagined.

Surgical ailments in the United States are almost unknown; except a few, arising from accidental causes, or occurring in the aged or infirm. But a want of bread and meat, and the common comforts of life, is never here, as it is in Europe, the prolific cause of such affections. The flaming sword of Washington has kept them out of the American Eden. Yet, notwithstanding, the causes of diseases are so different—the government so different—the condition of the great masses of the people so different—and the climate of the North of Europe so different from any part or portion of the United States, our Northern book-makers and compilers for the press, and our medical teachers will persist in holding the medical profession in America under a worse than colonial vassalage to British authority in Medicine. As if to prove to us, the monstrous absurdity of the physicians of the United States, particularly of the Southern States, adopting British medicine. France, just across the channel, a few degrees South, has never adopted it or given it a foothold in that country. France, in all her theoretical vagaries has never lost sight of Hippocrates. If the United States have been wise in adopting the medicine of the hyperborean regions of Europe, France has been most unwise in excluding it; because hyperborean Medicine is much better adapted to her than to us.

Tho original types of fevers, endemical to Southern latitudes, are treated of in our text books as varieties of some original type of fever common in Edinburgh, Dublin and London; as if the original types of fever, observed in those cities, were the only original types to be met with anywhere else. The bilious fever, which Hippocrates treats of as an original type of disease, is common in every part of the United States in hot seasons; but more common in the Middle and Western than in the Southern and Northern States. This fever is never met with in Edinburgh and the high latitudes of Europe, because the summer temperature is too low to produce it. Yet this wide spread malady is treated of in the Edinburgh, Dublin and London text books as a variety of some hyperborean type of fever, and the American physician, bold enough to call it by the name which Hippocrates gave it, would be in danger of losing caste with the profession, even in this country, where it is so prevalent, and to be taken for a humoralist, with antiquated notions behind this go-ahead, steamboat-age of improvement and discovery.

According to Hippocrates, it is a fever varying greatly with the constitution of the seasons; but according to Brouissais, it is no fever at all, but an inflammation of the stomach and duodenum, varying with the greater or less intensity of an imaginary phlogosis.

Dr. Bell (vol. 2, page 640. Bell & Stokes Practice,) informs us, that he was so far under the "*hepatic delusion*, when a student, as to choose the liver and its diseases for his Inaugural Dissertation;" that Dr. Chapman complimented him on the style of the essay, but added, "*you are all wrong*"—meaning that he was all wrong in believing that "the origin or support of those summer and autumnal fevers, called bilious, is in the liver." According to Dr. Chapman, it was not in the liver, but in a kind of mesmerism, called sympathy; according to Dr. Cook of Kentucky, in a plethora of the vena cava; and according to Dr. Chapman's pupils, Dr. Bell among the rest, in an undefined irritation of the mucous membrane of the stomach. To such shifts have the writers and teachers in the United States been driven, to reconcile the phenomena of bilious fevers in America, with the fevers of Northern Europe. Are they not "*all wrong*" in forgetting the important fact, that a hyperborean climate gives an undue preponderance to the *sanguiferous* system, and a hot climate an undue preponderance to the *hepatic ?* —The types of the diseases of the former being laid in a plethora of blood, or a disordered action of the sanguiferous system; and the types of the other being laid in a plethora of bile or a disordered action of the hepactic system. The fevers, we call bilious, are not met with at all in the climate of St. Petersburg, Drontheim, Stockholm, London, Dublin, or Edinburg—because the summer temperature is never high enough to produce them. They require a temperature of at least 70° of Fahrenheit for their production. But the summer temperature of these cities is below 70°. The summer temperature of Edinburg is only 58° 2; Dublin, 59° 5; whereas the summer temperature of Philadelphia and all our northern cities, is from 73° to 78° or 80°.—There is a much greater difference between the summer temperature of Philadelphia and Edinburg, than between that of Philadelphia and Havana. In Havana it is about 83°, in Natchez about 80°, and New Orleans about 81°. It would, therefore, be much wiser in the Philadelphia doctors to come to Natchez, or New Orleans, or to go to Havana, to look for the types of their summer diseases, than to go to Edinburg or the north of Europe. The radical error of squeezing the whole of our bilious fevers into types imported from Edinburg, has vitiated Medicine in America more than any other cause. Abstractions, the most undefined and incomprehensible, are called into requisition, to engraft bilious fevers upon the fevers of the north of Europe. The types of fevers, in a summer temperature of 58 and 80 deg., are different. The former are connected with a plethora of the blood—the latter of bile. The testimony of Hippocrates is positive in regard to the existence of fevers, called bilious, in the climate of ancient Greece. American physicians have only to study his works with attention, and the cord which has so long bound them in subjection to the hiperborean Medicine of Europe, will be broken asunder.

Thus, the CAUSUS of Hippocrates (from καιω, I burn,) a burning hot bilious fever of a high grade, is frequently met with in the Southern States, but never in the north of Europe. It is an original type of fever, differing from the common bilious. European authors, who have never seen it, make it a variety of the synocha or some other northern type.

This was the disease that Dr. Bell's Scotchman died of in Winchester, Virginia, which he mistook for congestive fever. It was not a congestive, because Dr. Bell, (page 620, vol. 2,) tells us, that "the face was deeply suffused, eyes watery, breathing laborious, the voice husky, patient walking about and was covered with a profuse watery warm sweat up to the hour of his death." These are the symptoms of the *causus* or ardent bilious fever. In 7th vol. Van Swieten's commentaries on Boerhaave, Dr. Bell, if he has no faith in Hippocrates, will find this disease described, in which all the symptoms he enumerated, are mentioned as diagnostic of the *causus*.

The CAUSODES is a milder kind of causus, but more common. The fever called EPIALUS by the Greeks, and mentioned by Hippocrates in his treatise on "Places, Waters, &c.," is characterized by a confused sensation of heat and cold at the same time.

It is admitted by Sauvages, that this fever is seldom, if ever, met with so far North as France, though he adds, it is very common in Italy. Alibert, a northern writer, not knowing where else to put it, places it among the algide fevers or malignant intermittents—the gulf into which nosologists cast nearly all the original southern types of fever.

The *epialus* is an original type of disease common in the Southern States—Paul, of Egina, treats of it.

THE AMPHEMERINA of the Greeks is also very common in the southern States. It is placed among quotidian intermittents by northern writers, but it belongs to a different type, because there is not a complete intermission, and the paroxysms do not return at regular periods of the day.

The TRITÆOPHYA of the Greeks, a very *dangerous* disease, is an original type, frequently met with in the South. Northern writers, never having seen it, make it a variety of the tertian intermittent of the North. But it differs widely from a tertian, because the cold, hot and swetting stages do not follow one another in the same order as in a tertian. Thus the cold stage may begin again, before the sweating stage commences, and the hot stage may not come at all. The LIPYRIA (from λειπω, *I want*, and πυρ *the fire*,) is a southern type of fever which northern writers seem to know nothing about.

The ASODES, thus named from a Greek word meaning anxiety, is characterized by Hippocrates, as a fever attended with great restlessness, oppression and sensation of tension about the hypochondria, cold skin, particularly cold extremities, exhausting discharges from the bowels, cold sweat on the forehead and above the nose, and on the neck and back of the head, terminating on the fifth day.

Southern physicians, of late years, have called the three last mentioned fevers by the general appellation of congestive fever. The diseases, thus called congestive, differ among themselves, as several distinct species are included in that term. They agree in one important particular—a deficient development of animal heat, in regard to the external surface of the body, characterizes them. In some of the types, the deficient development, *or want of secretion of animal heat*, extends to the mucous surfaces within, as well as to the skin without. According to Dr. Bell, congestive fever is anything or nothing, and he has written about as well on the subject as any other northern writer, who has gone to the north of Europe, where these types of fever never prevailed, to get information, instead of coming South or turning to Hippocrates. In ancient times physicians kept little statutes of Hippocrates, to remind them of their duties. The Father of Medicine interdicted his pupils from offering sacrifices to him, as to a God—the only sacrifice he required of them was to read his writings with attention, and to learn the force and reason of his precepts ; those who rendered him this homage were promised success in their art.

We learn from Lucien, in the case of the physician Antigonus, how those physicians were punished, who neglected to read the writings of the Father of Medicine, or who deviated from his precepts. Every night, when the lamp burned dimly, the statue of Hippocrates would come down from its pedestal and mix the various drugs together. No one can read the chapters in Bell & Stocks' Practice, on congestive fever, without being convinced that the statue of Hippocrates has been at work—not in mixing the drugs together, as the Greek physicians were punished—not in throwing medicine altogether away, leaving only the dust or ten millionth of a grain, as the homœopathists are punished for their heresy, but in making a complete jumble of all the learned author has compiled on congestive fevers and other southern diseases. This is a punishment necessarily following the wrong direction he took in going to the North of Europe, and to writers of the Edinburg and Dublin schools, to get information on the subject of Southern diseases, instead of going to Hippocrates, or coming South. In the island of Cos, in the extreme corner of southern Europe, the medical school of Hippocrates was established, and there he wrote his principal works. He travelled and even practiced medicine for some time in Greece proper, and also in Macedonia, Thessaly and Epirus. He also visited some of the warm countries in Asia and Africa. But the coldest climate in which he made any observations, was that of Thrace, in southern Europe.

The laws of diseases, derived from observations made in the warm and mild climate of Cos, near Asia Minor, were called Coan Prognostics, to distinguish them from his other observations in more northern latitudes.

Cos lies between the 36th and 37th parallels. In process of time the famous medical school of Cos, and the school at Cnidus in Natolia, and Alexandria in Egypt, were superseded by new schools farther North. Still Hippocrates' works continued to be the text books of teachers and of all students in Medicine, until in the progress North of civilization, the medical schools were removed to Göttingen, Leipsic, Leyden and Edinburg—all from fifteen to nineteen degrees further North than the original school at Cos. Then, and not till then, Hippocrates ceased to be read as a text book. In Great Britain his works were not even dignified with a translation into the English language. The age that witnesses the translation of the entire works of Hippocrates into

English, will witness the utter downfall of hyperborean Medicine in America.

Edinburgh is in 56° North, (wanting two minutes.) It is further to the North of Cos, than Quebec is North of New Orleans, or Philadelphia of Havana. No wonder that Hippocrates' description of diseases, their critical days and the laws governing them, should not hold good in the high latitude of Edinburgh, where a great majority of the diseases he described were not to be found.

At Edinburgh, the reformation of ancient Southern Medicine, to adapt it to a Northern climate, which the Northern continental schools began, was finished. From Edinburgh, the reformed, or hyperborean Medicine was carried direct to Philadelphia, and from thence disseminated throughout the United States, North as well as South. It became nearly the only kind of Medicine known in America. The reformed Scotch, was never appropriate to the summer diseases in the Northern part of the United States, it was not adapted at all to the diseases of the Southern portion, either winter or summer. "Treat the diseases in my family as you did before you went to Europe," said a plain common sense planter to Dr. Fearn of Huntsville, Ala., "or I must change my family physician"—is quoted as an apt illustration of the inapplicability of hyperborean Medicine to the Southern States. Every northern physician, who comes South, has to modify the practice he there learned, in very important and essential particulars, or he is sure to fail in the treatment of diseases. Finding few or no written rules, to guide him in making the necessary modifications, the new-comer is in danger of running into great extremes, in giving calomel and quinine, or some other powerful article in excess. Southern physicians are discouraged from writing on medical subjects. The truths, that long and toilsome experience in Medicine has revealed to them, if published, would only bring ridicule on their heads. The practice which experience has proved to be eminently successful in enlarged spleens, and many other diseases of the South, if published, would only be laughed at, because it is not in accordance with the doctrines and fashionable practice of hyperborean writers. This will ever be the case, so long as America continues her colonial vassalage to Europe in Medicine.

It will ever be the case, so long as compilers of medical books and the professors in our schools, *North*, *South*, and *West*, continue to be blind to the original types of diseases in their own country, and regardless of the differences in the circumstances, the climate and people of America and Europe, continue to echo and re-echo every thing said and done in Great Britain, and France, and to keep all the improvements and discoveries of American physicians, which do not tally with European theories, in the back ground. Both Dr. Perrine and myself, twenty years ago, made known through the Medical Journals, the virtues of large doses of quinine in certain fevers. Although quinine, in large doses, had carried us and also Drs. McPheeters and Metcalfe, of this vicinity, triumphantly through a wide spread epidemic, disarming it of its mortality entirely, yet our medical writers, and teachers waited until somebody in Europe had used quinine in large doses, before they ventured to recommend it, or even to dignify a remedy, which was curing thousands and tens of thousands in their own country, of formidable fevers, with any notice at all; and after it was brought into notice, they gave the credit to some European experimenter, who happened to give one or two large large doses at an early period, instead of to their own countrymen, who had given ten thousand large doses ten times over, before it had been used in Europe in half a dozen cases.

Dr. Metcalfe's brother was in Europe at the time, and it is probable, that through his reports of its vast success in his brothers hands in America, the European physicians ventured on the large doses. Again, the practice that was found to be the most successful in the South-West in the epidemic Asiatic cholera, has never to this day been dignified with any notice, (except a trivial one in Boston,) in any medical book or medical journal since published.

Dr. Bell, in his elaborate chapters on cholera, mentions almost every practice and mode of treatment, except that which experience in his own country proved to be the most successful. It was an American practice, built on a Grecian precedent; but it happened to be directly contradictory to the physiological and fashionable batch of theories, which were last imported from Europe. It is a practice not only successful in arresting watery evacuations in cholera, but also in congestive fever, with almost as much certainty and safety as quinine arrests an intermittent. It was only noticed in Boston to sink its author in the estimation of the profession, as one acting without authority and setting all law and gospel in Medicine at defiance. The translation of Hippocrates into the English language will prove to the Medical profession, that the author has the soundest philosophy, and the very highest authority for the objectional remedies he used in cholera, and still uses in congestive fevers, of any known in Medicine. Dr. Bell asks "what book of Hippocrates gives the accurate description of the diseases daily met with in practice in the State of Mississippi and the contiguous regions?"

It may be answered, that the entire works of Hippocrates were written in a Southern climate, and apply much better to the diseases of Mississippi and the United States, than to the hyperborean regions of Europe.

Dr. Bell will find, however, in the 7 books, called the "*Epidemics*," a description of the several types, so common in every-day practice in the South. Also the epidemic constitutions of the atmosphere and their influence on the original types of disease. Names are said to be things. In modern Medicine there are many names without anything to represent them, but undefined abstractions; but in ancient medicine there are many things without names. Hippocrates was very sparing of names; as names are apt to mislead the mind in the search of truth. Hence, in his Epidemics, he omits names almost entirely. He gives cases of each original type of disease, without naming or confusing it, with narrating the treatment. It is Nature naked and uncovered, even by a name, that he examines from day to-day, and notes down the observations he makes on her mysterious workings. Nothing is taken for granted—nothing imagined—he states what he sees, hears, smells, and feels, but nothing more. He does not even take for granted, that the pleura is inflamed, in the disease we call pleurisy; or the stomach, the seat of phlogosis in gastritis; but states the symptoms, their order and succession, as nature reveals them up to the crisis and through it. The observations thus made on the individual cases, in the several types of disease, afforded the data for a comparison, and enabled him to rise from particulars to axioms, and to discover the laws of each separate type of disease, and to recognize the original type when it put on the garb of another. Hippocrates is a very uninteresting book to the mere medical *reader*, searching for matter to expand and swell the imagination; but it is deeply interesting to the *student*, who is toiling to become great at the bedside of the patient—great in seeing into the future, and to be able to predict, with certainty, the changes that are to happen. True science is foreknowledge—foreknowledge can never be acquired in Medicine, in any other than the Hippocratic method. The *Novum Organum* of Lord Bacon will greatly aid the student in tracking Hippocrates through a labyrinth of cases, apparently so uninteresting, up to the laws which govern morbid action. Indeed, few minds can follow him, or see much merit in his writings, without the help of Lord Bacon's works—the best commentary on the Hippocratic philosophy that has ever been written. In the treatise on "Places, Air and Water," as in the books on Epidemics, Hippocrates strictly confines himself to observation. In the treatise on "*Critical Days*," the treatise on "*Predictions*," the books entitled "*Affections*, and those headed "Maladies," as also in his various other writings, he rises from particulars to axioms, and calls into requisition the reasoning faculties of his mind, keeping reason, however, always subordinate to observation—often not permitting reason, even to confer a name, lest the name should convey some erroneous impression to the mind, not in strict accordance with the truths revealed by nature to the close observer of her works. Hence, most types and varieties of disease are described without a name.

The assertion made in the Address to the Medical Convention, that "Mississippi and Louisiana have a climate very similar to Greece," has been called in question by Dr. Bell, and in a manner to betray the inattention he has given to the subject of medical geography. The Southern and Northern boundaries of Greece are not more than six or seven degrees North of the Southern and Northern boundaries of Mississippi. Every one knows, that isothermal lines do not correspond with parallels of latitude. Greece lies nearly midway between two oceans, the Atlantic on the West, and the Indian ocean on the South-East; Mississippi and Louisiana lie midway between two oceans, the Atlantic on the East and the Pacific on the South-West. Greece lies on a large inland sea, the Mediterranean—Mississippi and Louisiana lie on a large inland sea, the Gulf of Mexico.

Dr. Bell does not seem to be aware of the important truth in Medical Geography, that the climate is never the same on different sides of the same ocean, in the same parallel of latitude. All countries, of the same elevation, lying on the Western side of an ocean, are invariably colder in winter and hotter in summer, in the same parallels, than those countries which lie on the Eastern side of an ocean.

Thus, the United States, Canada and Labrador, on the Western side of the Atlantic, from Europe, are very much colder in winter and hotter in summer, in the same parallels of latitude, than any of those countries in Europe lying on the eastern side of the Atlantic from us. Philadelphia, in 39° 57′ is colder in winter than Edinburgh in 56°, and much hotter in summer. Quebec, in 46° 47′ is colder in winter than Drontheim in 63° 24′ and much hotter in summer Near the margin of the ocean, on each side, the isothermal lines do not correspond with the parallels of latitude by fully 15 degrees. As we penetrate the continent, on each side, the difference decreases. All that region of America on the Pacific ocean is much warmer in winter and cooler in summer, at the same elevation and in the same parallels, than that portion of Asia lying on the western side of the Pacific from us. Our Oregon territory is warmer in winter and cooler in summer, than any portion of Chinese Tartary in the same parallels, on the opposite side of the Pacific, and corresponds in climate with the same latitude in Europe.

Philadelphia and Pekin are each on the same side of a great ocean, and in the same latitude; they are both cold in winter and hot in summer.

The Western coast of Africa is much hotter than the Eastern. The cause is very plain. Moist air is a conductor of heat—dry air is a non-conductor. The earth, in turning from west to east, carries with it the heated moist air arising from the wide expanse of waters and impinges it over the eastern margin of the continent to the east. The difference in climate on the two sides of an ocean, is the natural result of the rotation of the earth.

Greece being only 6 or 7 degs. farther north, would be much warmer than Mississippi and Louisiana, if it were not for two causes—its distance from the Atlantic and the elevation of its mountains. Greece, in its most extended sense, including Macedonia, Thessaly, Epirus and the Ionian Islands, and all those in the Ægean sea, is not as large as Mississippi and Louisiana. A portion of it differs from Louisiana and Mississippi, in being mountainous, but the inhabitants do not live on the mountains, but in the plains. The mountains are only natural barriers, separating the different provinces from one another. In Hippocrates' time, the mountains divided the different republics. He refers to them as modifying the climate and making it much more changeable than Asia Minor. Greece has its marshes as well as its mountains, and like Mississippi and Louisiana, was subject to malaria. The classic reader will remember the Phalerean marshes below Athens, and Ilissus and Cephissus of Attica, which lost themselves in these marshes. Nor can he forget the violet, the primrose and the narcissus, which decorated the banks of the Alpheus; or the jessamine, the myrtle, the laurel, the ancient mistletoe, and the jay-birds screaming in the oaks. If Dr. Bell will come to Mississippi, he will be right welcome and be reminded of these things by seeing them here; proving that the climate of Greece and Mississippi are the same. In southern Mississippi and Louisiana, as in southern Greece, he will find the olive, the orange, lemon and pomegranate; and all over the country, as throughout Greece, he will find the fig tree, the wild cherry, the laurel, the vine, the cypress, the juniper, the locust, the willow, the alder, the poplar, the bay, the ash, the mulberry, the styrax and the family of the terebinthinæ, covering half the country.

If still incredulous, in regard to the similarity, if not the identity of climate, let him go to Macedonia, the most northern province of Greece, and he will find no less than three hundred villages engaged in growing cotton, the staple commodity of that province, as it is the staple commodity of Mississippi and Louisiana—thus proving the identity of climate between Greece and the south-west portion of the United States, beyond all question and dispute. Mississippi and Louisiana, not only correspond in climate with Greece, but the people, their government, their peculiar domestic institutions, their manners, customs and employments, were very similar in Hippocrates' time, to those of the Southern States of this Union. One portion of the people were free and independent republicans, bred to arms; another portion belonging to a different and inferior race, bred to servitude; but both living in plenty and abundance. Dr. Bell, in his Practice, speaks of the high excitement of the brain in the United States, arising from political and religious freedom, social and individual enterprise, mental cultivation and a heated moral atmosphere, without, however, attaching sufficient importance to his new idea. The same high excitement of the brain, only to a greater degree, existed in Hippocrates' time among the Greeks, and influenced diseases no less with them, than with us at the present day. Similar people, similar government, similar climate, must, of necessity, produce a similarity in diseases. What Dr. Bell says of the works of Dr. Jackson, may be with more truth said of the works of Hippocrates, "*as furnishing more available knowledge than will be found in the more elaborate expositions of city teachers, whose personal experience is very slight, and who illy make up for this poverty, by exuberant details of pathological anatomy, not always in strict relation to the matter in hand.*" —(Vol. 2, page 662.) What is dignified with the appellation of "*pathological* and *physiological*," by the present European writers, is scarcely worth more, in American practice, than what has been dignified by a new school in Ohio, with the appellation "Botanical." Botany, physiology and pathology, are all good in their places, but they throw very little more light on the laws, phenomena and treatment of Southern fevers, than algrebra and astronomy. Hippocrates built on no such basis. His system is founded on observations of those phenomena at the bedside, which are tangible in some form, or strike the senses in some clear and direct manner.

His great merit consists in painting to the life the diagnostic symptoms of each particular type of disease, and their order of succession—drawing axioms therefrom, enabling the student to recognise the disease at a glance and foretell the result. With such knowledge, the present, the past, and the future, are all brought to a point. Thus, in a regular intermittent, the present, past, and future is known to the physician, and he can tell what has occurred, and what will occur—not from his anatomical, or physiological, chemical, botanical, or theoretical knowledge, but simply from having observed the symptoms and their order of succession, as revealed by the highest authority—Nature herself in her mysterious workings. The same thing can be told of many other original types of fevers by similar observations, nicer and closer, being made on them; due regard being had to climate, constitution of the atmosphere, temperament of the individual and other circumstances. This is the only rational foundation to build a system of practice upon. Much genius, labor and toil have been lost by attempting any other.

The experience of the last 2,000 years is against any other system than, the one way, which Hippocrates pointed out, *as the only true method.*

In conclusion, let me observe, that I am actuated by no spirit of hostility to my professional brethren at the North, or I would not advocate their interests, by opposing their present opinions, by calling the attention of the profession, not to their editions of hyperborean authors—but to the works of him whom 2,000 years have not deprived of the glory of being the most faithful interpreter of nature that ever wrote on medical subjects.

It is not to be independent *of* them, but to be independent *with* them of the dogmas of hyperborean Medicine, that I would invoke antiquity. I wish to see American physicians not only commentators, but authors, thinking for themselves—to see that delusion dispelled, which requires, even a Philadelphia physician, to put a foreign label on his book, like a foreign label on a bottle of home-made quinine, to make it sell—to see such works as Hippocrates published and commented upon, instead of the *talk* of every flippant lecturer in Dublin, London, and Paris—to see the style of our medical writers, more in accordance with the classic model of the father of Medicine, and trimmed of undefined abstractions, and swelling terms which obstruct, rather than open the road to knowledge. I have only to regret that Dr. Bell, from reading my hasty and defective writings, should form so poor an opinion of the physicians South, as to speak of them as "moles unable to see anything beyond the sphere of their immediate vision." Perhaps it would be as unjust to suppose that the physicians South are as ignorant as I know I am, as it would be too complimentary to the physicians North, to suppose that they are all as wise as Dr. Bell.

Whatever disparity there may be between him and me, the great body of the profession, no doubt, North as well as South, are about equal. It must be confessed, however, that Southern physicians have some advantage over their Northern brethren, by reason of their having to study Medicine *twice;* first in studying hyperborean Medicine, in order to get a diploma, and secondly in studying the diseases of their own country in order to become successful practitioners.

SAMUEL A. CARTWRIGHT, M. D., of Natchez.

An Error corrected in regard to Dr. J. Kearney Rodgers' *Ligature of the Left Subclavian Artery within the Scalenus Muscle.*

In the number of our Journal for May last, we gave an abstract of the above interesting case, from the *American Journal of Medical Sciences,* followed by some *remarks* in which, as we have since discovered, we laboured under an erroneous impression. We thought that the *difficulties* spoken of by Dr. Rodgers, related to *any ligature* of the left subclavian, and we cited *five cases* in which it had been performed by the surgeons of New Orleans; whereas it appears that he alluded particularly to the application of the ligature *within the scalenus muscle.* Upon discovering the error, the writer of our notice took steps immediately to have the correction made, but he was far from home, and the instructions which he gave, it seems, never came to hand. He has much pleasure in making it even at this late date, in *justice* to Dr. Rodgers; and he takes this occasion to add, that upon farther inquiry, he finds that in none of the five cases cited by him was the ligature applied *within* the scalenus muscle.

F.

MEDICAL JOURNALS.

Our highly esteemed cotemporaries, the "Western Lancet," and "Buffalo Medical and Surgical Journal," come to us greatly enlarged and improved. We are very glad to see such evidences of success, and heartily wish them all prosperity. The manner in which these two Journals are conducted, reflects high credit upon their respective talented and enterprizing editors.

NEW ORLEANS, SEPTEMBER 1, 1846.

HEALTH OF THE CITY.

Our city may be said to be in the enjoyment of uninterrupted and *unmitigated* health. So little disease of *any kind whatever*, was perhaps, never known in New Orleans at *any season of the year*, as exists at this time. This is certainly very extraordinary, when we take into consideration the sultry heat of the season, the immense quantity of rain that has fallen, the unusually filthy state of the streets, and the great influx of unacclimated soldiers going to, and returning from the *seat of war*. Nearly all the volunteers from the Mississippi valley, amounting to *twelve* or *fifteen thousand* men, have passed through our city, during the hottest time of the year; they have been generally detained from four or five days, to two weeks; and whilst here, were exposed to the inclemencies of the weather, in a manner to which, for the most part, they were altogether unaccustomed. *Nearly six thousand* Louisiana Volunteers, (not one fourth of whom perhaps could be said to be *acclimated*, i. e. have had yellow fever,) have been sent back from Mexico, in the month of August, and disbanded in this city, where they were necessarily detained a considerable time in the settlement of their accounts; yet we have to record the prevalence of less disease than was ever known before. Some cases of Diarrhœa, Dysentery, and Intermittent Fever, of course have been met with; and as they occurred chiefly amongst the lower class of soldiers, they have generally gone to the Charity Hospital, where they swelled the monthly admissions to a greater amount than customary; but this is every thing that is worthy of notice.

In the number of our Journal for September, 1845, we offered some remarks in regard to the *possibility* of banishing yellow fever from New Orleans, by means of proper attention to *cleanliness*, *draining*, *paving*, *&c.* Another year's experience has served but to strengthen our impressions, and we would respectfully invite the *serious attention* of our municipal authorities and influential citizens to the subject. In a previous number, we had remarked on the same subject, that we might be *just beginning* to realize the benefits that would arise from the great extension of pavement and drainage, which had been made since 1836. The amelioration, as well in the *malignancy* as in the *frequency* of Epidemics within the last ten years, is most evident. Indeed, since 1841, we have had scarcely Yellow Fever enough to be entitled *Epidemic;* and if we escape the rest of this year, it will make *two in succession,* in which there was a *total immunity,* with the exception of three or four cases of doubtful character. The observations of the last two years would seem to indicate, that Yellow Fever must depend upon something more subtile and obscure, than any *appreciable state* of the weather or atmosphere ; for we have had *excessive heat*, *excessive rains*, and *abundance of filth*, yet the disease has not appeared. But let us continue our investigations ; for inasmuch as it is an approved maxim in politics, "*in time of peace prepare for war,*" so should we *in time of health endeavour to guard against disease.* As we have to contend with an *unknown, invisible, and intangible enemy,* it behooves us to fortify our position with all the approved principles and measures of *Hygiene.* This is a subject of vast magnitude and importance to the city of New Orleans, for upon it depends in a great degree its future advancement in wealth and population. We are fully convinced that with the exception of Yellow Fever, this city enjoys a greater exemption from disease, and suffers a smaller annual mortality than any other of its size in the world. That Yellow Fever does not arise from our *parallel of latitude*, nor from the mere *vicissitudes* of the weather, we think, may be, fairly inferred from the fact that the disease is unknown on the neighbouring plantations, some of which have families numbering several hundreds exposed to precisely *the like causes.* It must then depend either upon *something appertaining to the city*, or *infection* imported from abroad. If the *former*, let us see if we cannot escape it by making the city as clean as the plantations; if the *latter*, we can *discover* it, and *prevent* it too. A *spirit of investigation* is abroad, which may give rise to occasional panics and false alarms, but we should be careful how we repress it, for *excessive vigilance* is preferable to the *apathy* which has prevailed here so long.* We shall continue to record every thing that we think has any bearing on the subject before sickness comes, under the hope that we may yet discover a clue to the mysterious cause of Yellow Fever.

The Weather :—By reference to Mr. Lillie's Meteorological Tables, it will be observed that the summer has not been so hot as the last.—The greatest heat he noted was 90°. The number of rainy days in July was *fifteen*, and the quantity that fell reached the unprecedented amount of near *twelve inches and a half.* In August the number of rainy days up to the 23d, has been 10, and the amount of rain, nearly 6 inches.

---

* We have received the customary accounts of the prevalence of yellow fever in the West Indies, and at Vera Cruz, but have heard of no case having been imported into this city during the summer. The intercourse between this place and the West India Ports has been uninterrupted; but Vera Cruz has been blockaded, and of course cut off from us.

The River has not been as high as it was the two last years. It only got within about *three feet* of the *high water mark* at this place, and is now about *twelve feet* below that point.

The Streets and Gutters have been in a far more filthy condition than usual, and evince culpable neglect in some quarter. We understand that the business of *draining* in the rear of the city, is progressing with much energy and activity.

Monthly mortality:—Owing to the expiration of the term of service of the late Board of Health, and the complete organization of a new one, by the Common Council, we have not been supplied with the regular Reports from the Cemeteries. We hope, however, that this defect will soon be remedied. Indeed, whilst engaged penning these remarks, we learn that our colleague Dr. Hester, has been chosen Secretary to the new Board of Health. Of course we shall have no farther difficulty in regard to Reports from the Cemeteries.

HEALTH OF THE COUNTRY.

As far as we can learn, the whole South-western country is universally healthy. We give below such letters as we have received on the subject, and return thanks for the same. Our obliging correspondent at Montgomery, Ala., will please accept our apology for not inserting his letter intended for our last number—it did not reach us in time for publication, from some cause:

St. Mary's, La., August 14th, 1846.

Editors of the New Orleans Medical and Surgical Journal:

Gentlemen.—Little sickness prevails in this region, and most physicians whom I meet, complain that the country is "distressingly" healthy. I have never known so few cases of fever in this month, as at present. And this favorable condition of health exists contrary to the predictions of many wise prognosticators. The Tèche, until recently, has been very high, the lower banks have been overflown, the spring and early summer months were rainy, every pond and marsh was filled with water, and the earth was saturated with moisture. Early in July the Tèche suddenly fell, and left a large surface of the banks covered with vegetable and earthy deposit, exposed to the action of the sun. Since the middle of July we have had but litile rain, the weather has been hot, the ponds and marshes are fast drying up, and the earth has become dry, and yet, notwithstanding these supposed combined causes of malaria, the country has been healthy, even more so than usual.

The first fifteen days of July were warm and sultry, showers of rain were frequent, and the atmosphere extremely humid. During this period I saw an unusual number of cases of asthma—all old asthmatics were laboring under attacks of this distressing complaint, and I saw several cases of it in children who had never shown symptoms of it before. In these cases the attacks came on suddenly, and often without any previous cough or cold, and in some instances were alarming, and almost threatened suffocation—frequently accompanied with slight fever and symptoms of verminous irritation—most cases in children were characterized by great difficulty in producing emesis. The treatment I found most successful was, at the commencement, the administration of a vomit of ipicacuanha or hive syrup (which latter I preferred,) but the emetics were followed by only partial relief. I then administered two or three small portions of calomel and ipecac proportioned to the age of the child, followed by a dose of castor oil—at the same time exhibiting an infusion of spigelia, which often produced the discharge of several lumbrici. Generally as soon as the cathartics operated, the difficulty of respiration was entirely relieved. The attacks seldom lasted over twenty-four hours, and in no instance did the paroxysm return. I have observed that in this country persons subject to asthma are more liable to attacks of it during the summer than in the winter, and I know several asthmatics who look forward with dread to the approach of summer. The warm, sultry, damp weather of our summers, seems to predispose to the attacks.

P. S.—Since my last communication I have met with a case of trismus nascentium; symptoms commencing on the seventh day. I had seen Dr. Sims' article in the American Journal, but did not find that depressed position of the occipital bone of which he speaks. However I had the child placed on its side and adopted the treatment suggested by Dr. A. Eberle in the New Orleans Journal, viz: blister to the umbilicus, cathartics and antispasmoics. Unfortunately the blister was removed too soon, before it had drawn. I found the child worse next morning, reapplied the blister, but it died in the evening. The same woman lost a child two years before from the same disease. This disease is common with negro children in this region. I have seen also cases of it in white children. I have never seen but one case of recovery, and I was doubtful whether that was a pure case of trismus.

Woodville, Miss., August 13, 1846.

Messrs. Editors:—There has been more general sickness during the past two months, than was present at the same time last year. The most of it though has been among children. An influenza has been prevalent throughout the country, entering families and attacking the children almost uniformly, and also adults in many cases. Nothing remarkable is observable in it; the fever being light and the cough relieved by the usual remedies.

The hooping cough is still lingering in some families.

The fevers are mostly the tertian intermittents.

An obstetrical case of some importance occurred here in the country. The uterus was ruptured shortly after the commencement of labour, and the contents entirely expelled into the peritoneal sac. Gastrotomy was performed and the case progressed apparently very well, till the fifth day, when fatal symptoms supervened and she died the next day. A full report of the case will be published by the attending physician.

The summer has been unusually wet; rain has fallen on twenty-four days since the 16th of June. The mercury has never been above 93° in the shade; it was down to 73° at 10 o'clock 16th July, and 74° at 3 P. M. Winds mostly from E. S. E.

HEALTH OF THE ARMY ON THE RIO GRANDE.

We learn from our friends who have just returned, and by letters from the army, that the volunteers are suffering greatly from diarrhœa and dysentery, arising from bad water, bad weather, and diet, to which they are unaccustomed. The chief sufferers are the regiments from Tennessee, Indiana, and other States in that region. This is not to be wondered at, as the change of climate and mode of living, are so marked. It will be very fortunate if the campaign does not prove *disastrous* from sickness. No serious mischief seems to be apprehended from *Mexican bullets or bayonets.* The volunteer regiments now on the southern banks of the Rio Grande, from the States of Ohio, Illinois, Indiana Tennessee, Mississippi and Alabama, are composed, in a great measure, of *the flower of our youth*, who have been suddenly ushered into a *climate and service* to which they are altogether unaccustomed. The wisdom of calling *such* a body of men into *such* a service at this season of the year, except under the most *pressing emergency*, may be questioned; and the "*powers that be*" will be fortunate, if they escape a *reprobation* from those States, akin to that which caused the disappointed and afflicted Augustus to exclaim, in the bitterness of grief, "*O Varus, restore me my legions!*"

Let us not indulge evil forbodings, however, but *hope for the best.* It cannot be denied that the troops best calculated to stand this service at this season of the year, i. e. the Louisiana volunteers, and provided moreover with a *medical staff* best acquainted with the treatment of the diseases in the extreme South, have been disbanded; whilst those who have taken the field, have much more to apprehend from *exposures incident to the service*, than from the *sword of the enemy.* We should be thankful for any communications from the medical staff on the Rio Grande, or elsewhere.

BOARDS OF MEDICAL EXAMINERS.

We are pleased to discover that the Medical Board for the Eastern District of Louisiana, is at last awakened to a sense of its duties, and is determined to have the laws for the regulation of the medical profession carried into full effect. They have published in the city newspapers the names of all the licensed physicians, apothecaries and midwives, they have upon their books, and have given notice that all persons acting in contravention of the law, who do not come forward within thirty days, and comply with its requisitions, will be prosecuted with the utmost rigour. This is perfectly right; the law should be either enforced or repealed. The names of one hundred and ninety-four licensed physicians, fifty apothecaries, and twelve midwives, are published. Many of these have long since died or retired, but there are doubtless a great many now practising in the Eastern district contrary to law. We are informed that a number of suits have been commenced and will be prosecuted with vigor. We hear nothing from the Western Board, except occasionally of an individual who has gone before it and readily obtained licence, after having failed or been deterred from coming before the Eastern Board. The law regulating the profession requires amendment, and it has been intimated to us that the Eastern Board contemplate inviting a Medical Convention, to be held sometime previous to the next meeting of the Legislature, for the purpose of discussing this and other important matters. An intelligent correspondent in the country, fully concurs in this suggestion, and we hope we shall be able to give something more definite on the subject in our next number. The objects for consideration are, 1st, the addition of *imprisonment* to the penalties of the present law; 2d, the abolition of the Western Board; 3d, the repeal of the odious tax upon practising physicians; and 4th, the formation of a State Medical Society.

At the request of a subscriber, we give the names of the members of the Eastern Board, as follows:—J. Labatut, M. D., *President*; P. A. Bertrand, *Secretary*; W. Stone, M. D., J. Jones, M. D., A. Lambert, M. D., H. Landraux, M. D., A. H. Cenas, M. D., P. L. Massay. (Six physicians and two apothecaries.)

HOSPITAL REPORT.

NEW ORLEANS CHARITY HOSPITAL.—*Monthly Report for June and July.*

MAIN BUILDING.

June--Admitted: Males, 476; Females, 110--Total 586
Discharged: Males, 432: Females, 93. " 525.
Died: Males, 49; Females, 10. " 59.
Remaining on the 1st of July, 393.

INSANE DEPARTMENT.

Admitted: Males, 22; Females, 13—Total 35.
Discharged. Males 26; Females, 9. " 35.
Died: Males, 1; Females, 3. " 4.
Remaining on the 1st of July, 76.

MAIN BUILDING.

July—Admitted: Males, 564; Females, 84—Total 619.
Discharged: Males, 508; Females, 79. " 587.
Died: Males, 43; Females, 5. " 48.
Remaining on the 1st of August, 321.

INSANE DEPARTMENT.

Admitted: Males, 24; Females, 3—Total 27.
Discharged: Males, 18; Females, 4. " 32.
Died: Males, 4; Females, 1. " 5.
Remaining on the 1st of August, 67.

N. B. We make the following semi annual statement for the half year, terminating on the 1st July, 1846.

MAIN BUILDING.
Admitted, 3051.
Discharged, 2567.
Died, 311.

INSANE DEPARTMENT.
Admitted, 175.
Discharged, 128.
Died, 13.

It will be perceived that the admissions into this Hospital are on the increase from year to year, notwithstanding the improvement in the general health. This proceeds from bad management, but we deem it useless to expose it any farther than we have done on previous occasions. We had prepared some general observations on the service in this Hospital, but they are necessarily excluded from this number.

ABSTRACT OF A METEOROLOGICAL JOURNAL FOR 1846.

BY D. T. LILLIE, AT THE CITY OF NEW ORLEANS.

Latitude, 29 deg. 57 min.; Longitude, 90 deg. 07 min. west of Greenwich.

WEEKLY. — 1846.	THERMOMETER.			BAROMETER.			COURSE OF WIND.	FORCE OF WIND, Ratio 1 to 10	Rainy Days.	Quantity of Rain. — Inches.
	Max.	Min.	Range.	Max.	Min.	Range				
June - - 27	89.0	73.0	16.0	30.15	30.04	0.11	S.E.	3	3	0.995
July - - 4	89.7	72.5	17.2	30.07	29.83	0.24	S.W.	2¾	3	8.900
" - 11	89.7	77.0	12.7	30.17	29.97	0.20	W.	3	2	0.210
" - 18	88.0	69.5	19.5	30.17	29.91	0.26	E.	3¼	5	2.710
" - 25	89.5	74.0	15.5	30.14	29.97	0.17	S.E.	3	1	1.180
Aug. - - 1	90.0	75.0	15.0	30.17	30.03	0.14	W.	2½	5	0.473
" - 8	88.5	76.0	12.5	30.20	30.03	0.17	W.	2½	2	0.725
" - 15	89.0	70.0	19.0	30.14	30.00	0.14	S.E.	2¾	4	4.516
" - 23	89.5	72.5	17.0	30.13	30.01	0.12	S.W.	2½	4	0.658

REMARKS.—The Thermometer used for these observations is not attached to the Barometer, but is a self-registering one, and is placed in a fair exposure. Regular hours of observation, 8 A. M., 2 P. M. and 8 P. M.

The Barometer is located at an elevation of 19 feet above the level of the ocean, and is suspended clear of the wall of the building.

The Rain Guage is graduated to the thousandth part of an inch, and the receiver is elevated 40 feet from the ground.

During the above 9 weeks, we have had 29 rainy days, producing an extraordinary quantity of rain, say 20 inches and 367 thousandths. While during the same period in 1845, we had 19 rainy days, and only 4 inches and 607 thousandths, being but little more than one fifth of the quantity that has fallen during the corresponding weeks this year. During the 24 consecutive hours ending 4th July, at 8 A. M., there fell the largest quantity of rain, which I have recorded in the last 8 years, say 7 inches and 730 thousandths.

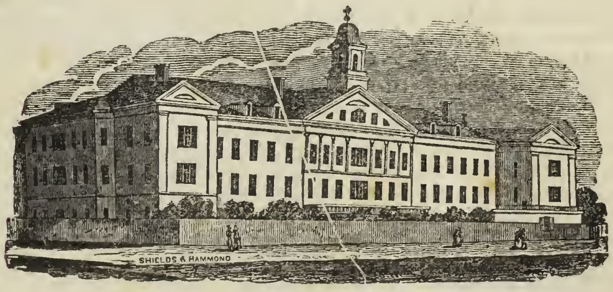
THE NEW ORLEANS MEDICAL AND SURGICAL JOURNAL,

DEVOTED TO MEDICINE AND THE COLLATERAL SCIENCES.

EDITED BY

W. M. CARPENTER, M. D.
E. D. FENNER, M. D.
J. HARRISON, M. D.
A. HESTER, M. D.

"Summum bonum Medicinæ, sanitas."—GALEN.



NEW-ORLEANS CHARITY HOSPITAL.

NOVEMBER, 1846.

NEW-ORLEANS.
PUBLISHED BY S. WOODALL, 49, CAMP STREET.
1846.

TO READERS AND CORRESPONDENTS.

Communications have been received from Dr. N. K. Leslie, of Jackson, La., and Dr. T. C. Brown, of Woodville, Miss.

The following books have been received:

The United States Dissector, or Lessons on Practical Anatomy. By WILLIAM E. HORNER, M. D. Edited by HENRY H. SMITH, M. D. Fourth Edition. Philadelphia: Lea & Blanchard, 1846.

A Practical Treatise on the Diseases of Children. By JAMES MILMAN COLEY, M. D.; Member of the Royal College of Physicians; Author of a Treatise on the Remittent Fever of Children, &c., &c. Philadelphia: Ed. Barrington & Geo. D. Haswell, 1846, pp. 414. (In Bell's Select Medical Library).

Text Book of Chemistry. By Prof. DRAPER, of New York.

Scrofula, its nature; its causes; its prevalence, and the principles of treatment. By BENJ. PHILLIPS, F. R. S. & C. Philadelphia: Lea & Blanchard, 1846, pp. 350.

Human Physiology. By R. DUNGLISON, M. D., &c. 6th Edition. Philadelphia: Lea & Blanchard, 1846; 2 vol. 8vo. pp. 651 and 694.

Medical Formulary. By BENJ. ELLIS, M. D., &c. 8th Edition, by SAMUEL G. MORTON, M. D. Philadelphia: Lea & Blanchard, 1846 8vo., pp. 272.

Professor Paine's Defence of the Medical Profession in the United States, 10 Edition. New York.

The Dublin Quarterly Journal of Medical Science; consisting of original communications, reviews, retrospects, and reports, including the latest discoveries in medicine, surgery, and the collateral sciences. Vol. 1. Dublin and other places, 1846. pp. 582, (From the publishers, through J. B. Steel of N. O.)

An Essay on Congestive Remittent Fever; the Disease of the South, &c., &c. By O. F. MANSON, M. D. Richmond, 1846. Pamphlet. (From the Author.)

We have had access to a number of late Foreign Medical Journals.

We have received the following Journals regularly in exchange—viz:

The American Journal of Medical Sciences. (Oct.)
The Boston Medical and Surgical Journal. (Sept. and Oct.)
The Western Lancet and Medical Library. (Sept. and Oct.)
The Western Journal of Medicine and Surgery. (Sept. and Oct.)
The Buffalo Med. and Surg. Journal. (Sept. and Oct.)
The St. Louis Med. and Surg. Journal. (Sept. and Oct.)
The Missouri Med. and Surg. Journal. (Sept. and Oct.)
Illinois and Indiana Med. and Surg. Journal. (Sept. and Oct.)
The New York Med. and Surg. Reporter. (Sept. and Oct.)
The Annalist, No. 1, New York. (Oct.)
The Medical Examiner. (Sept. and Oct.)
The Bulletin of Medical Science. (Sept. and Oct.)
The Medical News and Library. (Sept. and Oct.)
The Southern Journal of Medicine and Pharmacy. (Sept. and Oct.)
The Southern Med. and Surg. Journal. (Sept. and Oct.)
The Brit. Amer. Journal of Med. and Phys. Science. (Sept. and Oct.)
The Amer. Journal and Library of Dental Science. (Sept.)
The Amer. Journal of Insanity. (Sept.)

☞ The New York Journal of Medicine for October, has not come to hand.

Back Numbers of this Journal wanted.

☞ Those who have full sets of the first volume, which they are not particularly anxious to have bound and preserved, would confer a favour upon the Editors by exchanging them for the forthcoming or any subsequent volume. A few copies of No. 6, vol. 1, and No. 1, vol 2., are greatly wanted; one dollar a piece will be cheerfully paid for them by THE EDITORS.

LIST OF

AUTHORIZED AGENTS FOR THIS JOURNAL:

S. WOODALL, No. 49 Camp st., N. O., Publisher.

Milton Boullemet, - - - -	*Mobile.*
G. S. Taintor, - - - - -	*Natchez, Miss.*
O. O. Woodman, - - - - -	*Vicksburg,* "
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Jordon & Wiley, No. 20 State st., -	*Boston.*
N. D. Labadie, - - - - - -	*Galveston, Texas.*

TRAVELLING AGENTS.

W. H. Weld, John B. Weld, Thos. Waterman, H. N. Lewis.

TO OUR DELINQUENT SUBSCRIBERS.

Since the publication of our last number we have concluded to put into execution a resolve which we have been contemplating for some time, and at which we have repeatedly hinted in our former numbers, namely—*To strike from our list all Subscribers who are in arrears for more than one years subscription.* Much as we regret the necessity of such a step, we see no other that promises to abate the evil of which we complain, and we consider it but doing justice to those of our Subscribers who have promptly and honorably fulfilled their obligations to us, as well as to ourselves, to exclude in future from participating in the benefits of our labours, all such as refuse to contribute to the support thereof.

We are happy to announce to our friends generally, that the prospects of the Journal are such as to justify our declaring it permanently established, and we hope that our endeavours to render it more and more valuable to the profession as we acquire additional support may be rightly appreciated.

To those of our tardy Subscribers who may evince a disposition to help us in the execution of our plans by remitting us the amount of their arrearages, we cheerfully promise to respond in like spirit by continuing the Journal to them as formerly, and will hail them as fellow-supporters of a noble cause, one which tends to add to science the light of experience, by recording the results of patient and earnest investigation of the numerous and often complicated cases of disease, which so often present themselves to the practitioner, and thereby enable him to alleviate human suffering, as well as to gain new laurels in his profession.

We again take occasion to remind our Subscribers that all letters, remittances, or communications on the business of the Journal must be addressed *Post Paid* to

S. WOODALL, *Publisher,*
No. 49 Camp st., New Orleans.

☞ We invite the attention of our Subscribers to the Catalogue of Medical Books in the present number. By having the prices attached, it will enable those desirous of purchasing to remit accordingly.

JOSEPH COHN, Printer, 31 Poydras street.

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EDTORIAL.

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THE NEW ORLEANS

MEDICAL AND SURGICAL JOURNAL.

NOVEMBER, 1846.

Part First.

ORIGINAL COMMUNICATIONS.

I.—*An Examination into the History of Kreosote, and its various medical uses.* By A. Lopez, M. D., of Mobile, Ala.

The tests to which the *Brochierri water* and *Ergotine* have recently been subjected, throughout this country, induce me to review the notes I have made from time to time on the properties of Kreosote, which has not only long preceded them, but is entitled to an equal consideration among medical men.

The dry distillation of organic substances, subjecting them to an elevated temperature after they are deprived of moistuie, enabled a celebrated chemist of Blansko, *M. Reichenbach*,-to discover five substances of greater or less efficiency as therapeutic agents, and one other which has proved pre-eminently so, and is now destined to assume a larger share of reputation, because I am disposed to believe that the proximate principle of all other agents producing decisive *Hemostatic* results, will be found to reside essentially in the *Kreosote*. The five substances to which I have alluded are:

1st. *Eupione:* (from ευ very and πιων greasy) abundant in animal tars, less so in vegetable. It is obtained by distilling the tar of bones.

2nd. *Parrafine:* (from parum and affinis) because it is so unlike any other product.

3rd. *Capnomor:* (from καπνοσ smoke, of which it is a constituent element, and μοιρα a part).

4th. *Picamar:* (in pice amarum) the bitter principle of tar.

5th. *Pittacal:* (from πιττα pitch and καλοσ beautiful) so called because from its rich blue it is used as a fast dye. N. B. To this Mr. Reichenbach claims the exclusive right of discovery.

The other, and by far the most important, is *Kreosote*, obtained from impure pyroligneous acid, and all the tars. It is so called from κρεασ flesh and σωζω I save. The almost universal range which this article takes in its application to practical purposes, places it, beyond doubt, at the head of all other products having a like origin, and but for the imprudent anxiety which governs every one, that the protegé he ushers to the profession should prove too much, thereby subjecting it, through distrust, to proving nothing, I feel convinced that long before this the records of medicine, and surgery more especially, would have abounded with testimonials in its favour, of inestimable value. Notwithstanding this, the journals of foreign countries and our own, afford cases which entitle it to consideration as a detersive, a sedative, a counter-irritant, and in a more eminent degree, as a *Hemostatic.* It ranks yet higher as an *antiseptic*, and has been applied not only to medical uses in that capacity, but to the ordinary purposes of domestic life, and also as an invaluable handmaid to the arts in perpetuating objects, which else would have shared with other perishable matter the ravages and annihilating processes of destructiveness. These antiseptic properties are supposed to reside chiefly in its power of coagulating albumen, and although this rationale has never been thoroughly explained, yet *Fourcroy* attributes it to the absorption of oxygen gas. The reason why its antiseptic property has been supposed to depend upon this coagulating influence, is, because it has been discovered that when albumen is coagulated, *it does not become putrid.* Hence, fresh meats can be preserved indefinitely by being immersed two days in a solution of one part Kreosote with one hundred parts water ; then being dried by fire or the sun, and laid by for eight days, you are provided with as fine smoked beef as you desire. I have never subjected meats to the Kreosote for this purpose, but have frequently prepared them with complete success by means of the Pyroligneous acid. *Kreosote* has been found equally effectual in the preservation of dead animal matter, and has been tested by the fact that the bodies of birds poisoned by it have resisted putrefaction for a length of time. In evidence that it is entitled to the merits we assign to it, late discoveries prove that it is a *vegetable tar* called by the Bedouin Arabs, *Katran*, that the ancient Egyptians were mainly indebted to for their great and unsurpassed skill in embalming. [Asiatic Journal, Feb., 1836.] To the other ingredients, such as camphor, aloes, myrrh, and frankincense, they ascribed only secondary agencies. *Lieutenant Bagnol*, to whom we are indebted for these facts, presented to the Royal Asiatic Society a human hand and a piece of beef perfectly preserved by means of a preparation of vegetable tar found on the borders of the Red Sea, in the vicinity of Mocha, and obtained from the branches of a small tree, a shrub of Syria and Arabia Felix [op. cit.] Further proofs which the properties of Kreosote might establish as being among the earliest of the antiseptics used, are to be deduced from the fact, that the *flesh of mummies* enjoyed at different epochs a high reputation as a medicinal agent, for the same forms of disease, and under the same circumstances, as are now supposed to require the use of *Kreosote*, viz : cutaneous diseases, abscesses, nausea, hæmoptysis, ulcerated lungs, &c., and we are informed that it was much esteemed as a hemostatic. According to *Madden*, the Arabian physicians still rely upon its medical virtues. Indeed, so highly was this mummified substance appreciated, that it was deemed an appropriate demonstration of the court etiquette of the day, for the Persian Ambassador to present Louis XIV with "two golden boxes filled with it, accompanied by this address: L'ambassadeur de Perse dit à Louis XIV que le *Baume momié* etait un specifique pour les fractures des os, et generalement pour toutes les blessures; qu'il etait employé pour les maladies et ulcères *tant internes qu'externes*, en un mot, qu'il avait la propriété *de faire sortir le fer que pouvait être dans les blessures.*" As it is now well known that bituminous properties are comprised in the most valuable of these preparations, and as we have already shown that the portions of the human body noticed by Lieut. Bagnol, owed their preservation to a vegetable tar, there can be little doubt of the participation which KREOSOTE, as an elementary principle in some form, however unknown to them at the time, must have had in those results.

The attention of REICHENBACH was called to this article by having his finger deprived of its epidermis, when he discovered it, and it was thus suggested to his mind, that to this the pyroligneous acid owed its preservative properties. Experiments confirmed the opinion. Pursuing his investigations he further inferred its value as a therapeutic means, from its peculiar and specific influence upon dead animal matter, and his first application with it as a remedy, was to bad suppurating sores and chronic diseases, in which he says "it possesses specific, healing and energetic properties." [Chemical and Medical researches on Kreosote, by E. Miguet.] At that time he was entirely ignorant of its power in arresting hemorrhage, and equally unacquainted with the *aqua Benelli*, which then, and for some time subsequently, played so high a role among the anti-hemorrhagic remedies. The hemostatic qualities of the *Kreosote* have been much more fully developed by those who have followed up Reichenbach's discovery than by himself, and their general results combine to pronounce it not only the best of that class of remedies, but to prove I think very conclusively that their proximate principle, as I have already stated, resides in it. Such is the decided opinion of *M. Seidel.*

As regards the *Benelli Water*, a series of experiments instituted by Dr. J. Davy, led him to a conclusion rather adverse to its claims. He found that compresses wet with it and applied to a bleeding wound "*produced no better effect than cold water*," and all that he could say in its favour was, that "*at least it was harmless.*" [Miguet op. cit.] The trials with *Kreosote* have not yet damned it with such faint praise.*

A brief outline of the various cases to which from the time of its discovery to the present day, the *Kreosote* has been applied, and an attempt to contrast its success with its failure, may not prove uninteresting. The history of diseases teach us their reliable features. The history of remedies teach us to confide in them or to discard them, according to the mass and stamp of testimony. An easy credulity may mislead, as readily

* In July, 1840, Mr. SAMUEL WRIGHT published his mode of preparing *Oil of Ergot*, which was noticed in the Edinburgh Medical and Surgical Journal. After recounting its therapeutic effects in various forms of disease, that Journal thus notices its *anti-hemorrhagic* properties. "But perhaps its greatest value as an external application, is in *arresting hemorrhage.* He has often wounded small arteries in dogs and rabbits, *and subdued the bleeding completely by one drop of this oil.* Hemorrhage from the jugular and femoral veins, has been similarly arrested. The troublesome bleeding which sometimes follows the extraction of a tooth and leech bites, it is equally efficient in stopping."

as an obstinate unbelief may lose us much that is valuable. But these errors respectively are avoided by patient and diligent inquiry, and the physician who does no more than gather such evidences can aspire, to say the least, to the character of a faithful steward.

I proceed then to examine in order the claims of this remedy, and in so doing, commence with a few cases taken from my own practice, and follow them up with such other records as I have from time to time noted.

Case 1. *Epistaxis:* Jane Lockwood, a coloured woman, of intemperate habits, sent for me 10th Dec., 1837. She was bleeding profusely from the left nostril, and this had been the case, with very short intervals, for the last fifteen hours. Eyes very turgid and inflamed, skin hot and dry, notwithstanding the loss of blood, pulse, quick and full, head-ache. Her liver and spleen were both enlarged. *Treatment:* V. S. ℥ XVI. Nitro-saline aperients, semi-recumbent posture. I filled the nostril with a moistened plug covered with *Bonafou's styptic* (equal parts of powdered gum resin, gum arab. and charcoal)* ; cold cloths to the head and nape of the neck. 11th : Bleeding not controlled, plug thrown out, by being saturated with the blood which also escaped very freely by the posterior nares through the mouth. Pulse still full quick and hard: V. S. ℥ XII. Tinct. Digitalis gtts X quaque hor. 6ta.: re-insert the plug and styptic powder: Iced Lemonade ad libitum. 12th, Still bleeding; same remedies and a sponge introduced through the back part of the throat. 13th, 9, a. m. Very little abatement, blood escaping from the mouth and nostrils. Eyes very turgid and inflamed—much pain in the temporal regions. Apply leeches to them and to the inferior orbits. 4, p. m: Eyes and temples relieved, but no abatement of the epistaxis. I now removed the sponge, and introduced by means of a probe, a dossil of lint, on which I poured *two drops of pure Kreosote*, and passed it high and close. In three minutes after this there was no further bleeding, and it did not return.

Case 2. *Epistaxis:* Jim, a colored boy, had been ill with bilious fever. On the 9th day, a violent hemorrhage from the nose ensued. Directions were given by his medical attendant and followed, without avail. This continued for 36 hours, with very short intervals, when so alarming a discharge occurred, that his physician was sent for, and he not being at home, I was summoned. The patient was almost ex-

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Epistaxis be arrested by injecting into the nostril equal parts of Oil Ergot and dilute spirit. [Loco cit].

Ergotine also presents its claims through *Mons. Bonjean*, who affirms its success in experiments made in five cases upon the crural and carotid arteries and the large veins of animals. [Comptes Rendues, July 5, 1842].

A new candidate for experimental favour has shown itself. *M. Dupuy*, of Paris, professes to discover a powerful hemostatic in the *cerebral matter of the common sheep.* It acts by coagulating the blood, which he proved by injecting a solution of the matter into the crural vein of an animal, and found upon examination the blood in the heart and large vessels coagulated. M. M. de Blainville & Rousseau attest.

[Dublin Journal of Medical Science, Nov., 1845.]

* I had long been in the habit of arresting hemorrhage consequent upon extracted teeth, by this preparation retaining it in the socket by means of a phial cork pressed upon by the upper teeth.

hausted, pulse weak, the blood running *pleno rivo* from his nostrils and from his mouth. I inserted *a dossil of lint with kreosote*, as in the preceding case, into each nostril—elevated his head and gave him iced lemonade. In less than 5 minutes the hemorrhage ceased. I remained by him a half hour, and there being no recurrence I left him. There was no return.

Case 3. *Hemorrhage from Leech bites:* Mrs. C——, aged 70, fell and received a blow beneath the eye, and just above the superior maxillary bone. After the usual domestic applications there remained an extensive ecchymosis. Being desirous to remove this, she was directed by her friends to have leeches applied, which she did on the morning of 25th September, 1837. Two of the orifices ceased bleeding in a reasonable time, but the third continued profusely until 7, P. M. Every means advised by her family and an apothecary were ineffectual. Bandage after bandage was saturated, each upon its removal containing large coagula. She became so faint, that I was sent for, at 7½, P. M. Her face, when I arrived, was enveloped in a handkerchief, beneath whose edges the blood was trickling. I removed it, and with it a coagulum large enough to fill the palm of my hand. I applied *one drop of pure kreosote* on a piece of lint, to the orifice, and in less than 5 minutes there was no further hemorrhage, and it did not return. The ecchymosis was removed by a solution of *murias ammon.* in vinegar and water.

Case 4. *Traumatic Hemorrhage:* Tom, a colored carpenter, while at work on his master's plantation, near Georgetown, S. C., struck himself with a broad axe, which completely severed his foot from the ball of the great toe, diagonally upwards, through where the metatarsal bone of the little toe joins the os cuboides. The bleeding was very profuse, and the wound being wrapped up I was sent for. Upon removal of the cloths in which the stump was enveloped, the blood flowed freely from the surface, and the cut arteries threw it out *per saltum.* The time that had intervened between the accident and my arrival was such, that the dismembered portion was perfectly cold, else I would have tried the chances of re-union. I soaked lint freely in a *strong solution of kreosote*, and applied it to the entire wound. Compresses and bandage completed the dressing. The hemorrhage ceased, granulation and cicatrization following in due time, with very trifling suppuration.

Case 5. *Tooth-ache:* Beck, a colored woman, in 9th month of gestation, applied to have a large molar tooth extracted. The pain had been excruciating for two days, and incapable of relief from ordinary applications. After cleansing the cavity thoroughly with tepid water, by means of cotton, on a blunt probe, I formed a cone of finely scraped lint, upon the apex of which I put *one drop of pure kreosote*, and introduced it into the cavity of the tooth. The pain ceased in less than one minute, and did not return for five months, when she re-applied to me, and was again relieved by the same means.

The cases of tooth-ache in which I have since used the kreosote have been innumerable, and I do not recollect a single instance of its failing to afford relief. In some, the same tooth has again caused suffering, but in the majority of cases, the relief has been permanent.

N. B. Upon the manner in which the kreosote is introduced, depends in a great measure the chances of relief or failure. Those who find no benefit from its use, neglect to cleanse the tooth of impurities previously to its introduction ; and are also careless how they introduce the lint. But by proceeding in the manner I have described, that portion of lint upon which the oil is dropped is the first to reach the seat of pain, and the superincumbent lint acts as a covering or plug, thus preventing the access of external air, fluid or any foreign substance from disturbing the coat of albumen, which is said to serve as the future protection of the exposed nerve. In 1838, I recommended this method to the nurse of a large hospital, who had generally failed with the kreosote, but after my instruction, he informed me that he uniformly found it successful.

Cutaneous Disorders.—Case 6. *Porrigo Favosa:* Emma ——, aged 5 years. Head entirely occupied by the eruption in its various stages. I applied warm poultices every third hour, in order to reduce the inflammation. The next day I used the *unguent kreosote, gtts.* v to ℥ i lard. By alternating every eighth day with the *soot ointment* (equal parts of finely sifted soot and lard,) observing a simple diet, and interposing occasionally saline and absorbent mixtures ; she was cured in four weeks.

Case 7. *Herpes Phlyctenodes* : Louisa ——, a colored girl, aged 9 years. The eruption covered her neck, chest, arms and part of the back. Chronic vomiting and irregular bowels, with porraceous dejections. Treatment.—Alkaline absorbent mixtures, with blue mass occasionally, until the stomach and bowels were regulated. Then I applied the *unguent kreosote.* In less than a fortnight the eruption had almost disappeared, but the irritability of stomach and bowels returned. Resort was again had to the constitutional treatment, with a wash of *carb. potass.*, instead of the ointment. The gastric and enteric symptoms abated, and *the eruption re-appeared.* This vicarious action being kept up, I resumed the use of the kreosote ointment. The case continued to baffle from August to October, when it began to yield. A course of mild bitter tonics was prescribed, and she was almost entirely cured when she passed to different owners, and I lost sight of the case.

Case 8. *Herpes Circinnatus :* J. C. L., aged 7. Eruption on the chin, spreading towards the cheek. I applied *unguent kreosote.* Its progress was arrested, and in one week it was cured.

Case 9. *Impetigo Figurata :* Miss A. M., aged 9. The disease presented its most aggravated form, involving the entire left side of the face and ear, and meatus internus, thence to the neck and around the nape, and down the front to the nipples. It was also interspersed here and there on the shoulders. The right ear externally and internally was affected. There were deep fissures yielding a sanio-purulent discharge, high irritative inflammation, and the surface of the interstices presented a deep glistening red. The disease was of eight weeks standing, and the face was much tumefied. March 16, 1838.—I commenced with emollient cataplasms to the parts, and constitutional remedies applicable to such cases. On the 18th, the inflammation having subsided, and the more urgent symptoms meliorated, I commenced with the *kreosote ointment gtts.* viii to ℥ i lard, applied twice daily, bathing the parts each time previously with tepid water and castile soap. *Mass Hydr.* every alternate night, and magnesia the following morning. 25th.—The improvement very perceptible; continue ointment; and in lieu of the tepid ablution, I used a wash, *sulphuret potass.* ʒ i to aq. *distillat.* ℥ viij, which was also injected into the ears. 30th.—Still improving. Change kreosote for *unguent fuliginis.* April 11th.—Perfectly well. Mild bitter tonics.

Case 10. *Porrigo Favosa:* Mary-Ann, aged 4. Same treatment as in case 6th. Cured in thirteen days.

Case 11. *Impetigo Scabida:* H., a blacksmith, affected on the little finger of his right hand for several months. This case, after trying every means, including Hufeland's formula for hydrocyanic lotion, was cured by means of *unguent kreosote*, at the same time using mass hydr., and imposing an unirritating regimen. But the difficulty of restraining the patient, who was a high liver, as well as the constant use of his hand, in the prosecution of his trade, protracted the cure to nearly three months.

Case 12. *Phlegmonous Abscess of the Thumb:* threatening to involve the back of the hand. It had been subjected to temporizing domestic remedies, greatly aggravating its condition. Gangrene had commenced, and the stench was exceedingly noisome. At first, I used the *kreosote solution*, and after the third day, changed it for *unguent kreosote*. In one week he was sufficiently improved to complete the cure with simple ointment.

I will now pursue the examination, by adding to what I have just related from my own practice, the experience of others, who have deemed the subject worthy of attention; and condensing as far as practicable, the uses to which they have applied the remedy.

Firstly.—As A HEMOSTATIC. Reichenbach's experiments with it in this department, was, as I have before stated, limited. We have no further evidence than the following: a. One case of *Hemoptysis*, completely cured by the internal use of kreosote.—*Miguet.*

b. *Recent Wounds and Hemorrhages:* His experience is here limited to seven cases, in all of which, the application of kreosote, either pure or in solution, was instantaneously successful. *None of these cases were of large vessels*, being confined to smaller arteries and capillary vessels. But he adds, "I have seen many cases of solution of continuity and wounds, with loss of substance, *causing abundant hemorrhages*, yield almost as soon as pure kreosote or kreosote water was applied."

[*Op. citat.*

M. DA LUZ relates three cases.—1st. Traumatic Hemorrhage of the *external jugular veins*, in taking up the carotid. Compression and ligature failing, he succeeded in arresting it by a sponge dipped in a *strong solution of kreosote.*

2d. Hemorrhage of *Crural Artery*, arrested by charpie soaked in *solution of kreosote*, ʒ i to ℥ iij of water.

3d. Hemorrhage from the Nose; attendant on the extraction of a polypus. Arrested as above, by kreosote solution on charpie.

[*Eclect. Jour. of Med., vol.* 3, *No.* 8, *June*, 1839.

M. Hoering, of Neustadt, has arrested bleeding from *large arteries and veins*, by dossils of lint soaked in *kreosote solution.*

[*Gaz. Med. de Paris, Dec.* 1834.

M. Bichthauer, of Kungalsen, arrested profuse hemorrhage from leech bites, when all other means failed.

Prof. Corneliani, of Pavia, after a number of experiments, testing the toxicological properties of kreosote, and also relating to its therapeutic qualities, thus expresses himself: "If its external application does not arrest venous hemorrhage, at least of a large vein, it is efficacious in hemorrhage from *an artery of moderate size.*"

[*Jour. de Chimie Med., February*, 1836.

Messrs. Muller & Reiter testify in its favor, by the following experiments, applied directly to bleeding surfaces. a. Division of the crural veins of a dog. A plug of cotton well moistened with *kreosote*, arrested the hemorrhage.

b. In wounded arteries, they say it is necessary for a time to keep up "a certain degree of compression," so that the kreosote might act upon their parietes. Subsequent examination of the cut arteries exhibited them quite closed and obliterated, having outwardly an umbilical depression, corresponding with a conical shaped coagulum within. The coats usually inflamed an inch or so.

c. They pronounce the kreosote "a more decided hemostatic than the far famed Benelli water."

d. They say that the kreosote water is sufficient for oozing surfaces.

e. When they injected kreosote into the veins of animals, "the blood was instantaneously coagulated."

[*Med. Chir. Rev., July*, 1839; *from Schmidt's Jahrbücher.*

Professor Schneider, of Munich, succeeded in arresting a profuse hemorrhage from the mouth of an old man. He had lost "several pounds of blood." All other means failing, *he made him fill his mouth with kreosote water.* After the third mouthful it ceased, and did not return.—*Ibid.*

Dr. Burdach, of Luckan, relates a case, which being of much interest in itself, as well as showing the value of the remedy, I will give more at length, abridging as much as practicable. The *ulnar artery* was divided by a sharp knife. The bleeding repeatedly staunched by surgical aid. In three weeks afterwards it returned, and Dr. B. was called in. The wound presented the following aspect: livid edges, and expanded to the size of the hand by a spongy growth from the bottom. The mass gangrenous, preventing an examination of the wound. Arm swollen from the shoulders to the fingers' ends. Excessively painful and intolerant of motion. *Seeing no chance for a ligature, his alternative was caustic or kreosote.* He chose the latter, and *poured* ℥ ss. *of pure kreosote into the wound.* It gave no pain, and he slept for the first time since the accident. *There was no hemorrhage.* The kreosote was repeated morning and evening. The spongy mass was gradually diminished, and in three days more, by aid of a bandage moistened with *kreosote, ol tereb. and bals. indic.*, it loosened itself from the bottom of the wound. The divided artery was not visible. The swelling diminished, and it proceeded to a final cure.

[*London and Edinb. Monthly Jour. of Med. Sci., May*, 1842.

The Editors of the Southern Jour. of Med. and Pharmacy, [July, 1846, pp. 403 and 5,] report two successful experiments with the KREOSOTE as a hemostatic agent in Traumatic Hemorrhage, produced upon the carotids of sheep. In concluding their very interesting experiments with this article, Ergotine and Brocchieri water, they incline to the opinion, that in as much as no experiments have yet been made upon the larger arteries of human subjects, "*there is no danger of ligatures being supplanted by either of the above articles.*" With our present limited knowledge of the thorough efficacy of kreosote or ergotine (both of which I esteem above the Brocchieri) perhaps the editors are right; and be the auxiliary appliances of surgery what they may, I would be averse from the abandonment, or supplanting of so certain a means as the ligature. Nor do I think it is with such an object that the hemostatic treatment is advocated. But, as physicians are frequently exposed to emergencies, where either from incapacity or inconvenience, the control of operative surgery is denied them, it is certainly a desideratum to be made acquainted with such alternatives as may be at command. Besides such authorities as *Hoering*, who by kreosote has arrested bleedings from large arteries and veins;" *Burdach*, in wounds of the "ulnar artery," (certainly one hitherto the subject of ligature) and *Prof. Corleniani*, in "arteries of moderate size," go far to justify a due degree of dependence upon agents, provided they come to us fortified by good and sufficient guaranty.

Kohler has found the kreosote successful in one case of *Impetigo Sparsa*, and in several cases of *Scabies*. In *Tooth-ache*, he pronounces its effects to be "most remarkable."—*Rust's Magazine, October*, 1836.

In the March number, 1846, of the *Southern and Med. Jour. of Med. and Pharm.*, Dr. WM. WRAGG, of Charleston, reports *fourteen cases* of successful treatment with kreosote, as an *internal remedy*. With all who know that gentleman, this testimony goes far to establish additional confidence in the propriety of further experiments. The diseases were hemorrhage, either of the bowels, lungs, uterus or vagina, and a reference to them would repay the reader, and instruct him in the methodus medendi adopted by Dr. Wragg.

HOW IS KREOSOTE HEMOSTATIC?

Miguet thinks it purely mechanical, and evidently due to the property it possesses of coagulating albumen; because, says he, "If after having carefully wiped away the blood from the surface, kreosote be applied before it has had time to re-appear, we do not prevent it from escaping from the textures; it still flows, until a thin transparent pellicle, formed by the agglutination of small particles of coagulated albumen, opposes an insurmountable obstacle."—[*Op. cit.*, p. 47.] The reasoning which sustains this action finds analogy in the consequences obtained by agitating the white of an egg with kreosote, when, as has already been shown, it instantly coagulates, and if to a large quantity of an aqueous solution of the albumen ovi, *one drop* of kreosote is added, it becomes filled with coagulated albuminous pellicles.—[*Op. cit.*] I am convinced that this coagulating property must exert an important influence in the modus operandi, because it is well known that all of the stronger acids will coagulate the white of an egg, and I have been more confirmed by meeting with a case where *Dr. Arentz*, of Norway, succeeded in arresting hemorrhage of a deep seated vessel, inaccessible to a ligature, by *dropping into the wound* 8 *or* 10 *drops of nitric acid.*

[*Gazette Med.*, *September* 15, 1832.

The same process is generally ascribed to the curative properties of kreosote, when applied to all secretory surfaces, and also the relief it affords to a carious tooth, in which it is supposed to coagulate the albumen of the blood, always more or less present in such cases, thus forming an albuminous covering and guarding the nerve from exposure to the atmosphere and other offending causes, and not, as was supposed, by any caustic influence destroying the nerve, because if this were so, pain would not be renewed as it frequently is. There are cases in which it is difficult to decide, whether the success of this and other hemostatics is attributable to the remedy itself, or to the *compression* which is often super-added; and this doubt is warranted by the experiments of the the Editors of the *Southern Jour. of Med. and Pharm.*, to test the relative qualities of Ergotine, Brocchieri and Kreosote.

[*No.* 4, *July*, 1846, *p.* 402.

On the other hand, the seven cases cited from Reichenbach, and those which I have given from my own practice and that of others, produced their results independent of any pressure, capable of interposing an objection. So, also, the case of *Dr. Arentz*, with nitric acid. Nor, does Da Luz, or any other whom I have quoted, *except Muller & Reiter*, speak of the necessity of compression. From all the testimony, therefore, which I have seen, I am constrained to believe, that although there may have been cases which resisted the application of the kreosote, yet among all kindred remedies, it deservedly holds the highest rank. I am supported in this opinion by M. Miguet, who uses the following language: "It is certain, as a hemostatic, kreosote is a *precious application*, but it must be remarked, it is only efficacious in capillary hemorrhage* and bleeding from vessels of small calibre. Error in this particular should be avoided. The action of kreosote is purely mechanical."

[*Op. cit.*, *p.* 49, *translated by Wetherill.*

The cases of hemorrhage being disposed of, I will now proceed to present, in a condensed form, the various practitioners who have subjected the kreosote to practical tests, in public and private practice, as well as the diseases which it is said to have controlled. I have also endeavored so to arrange the result of my researches, as to present first, the success and then the failure of the remedy.

1. Phthisis Pulmonalis: *Reichenbach* argued from its efficacy in external ulcers, that analogous effects would be obtained by applying it to pulmonary diseases, especially by inhalation. He tried it and was satisfied of its power. He states, that it allayed febrile action, arrested sputa and purulent discharges.—*Miguet.*

Billard, a chemist and pharmacien, at Paris, confirms this, by using it in his own case, inhaling the vapours of the laboratory in which he prepared the kreosote. It relieved him, and he was in no degree incommoded, however dense the vapours.—*Ibid.*

The fact to which I wish to call the attention of the profession is this:

* Some have objected to its use in capillary hemorrhage, that it cannot be used when union by the first intention is desirable, as it prevents this object.

2. For BURNS, its utility is attested by *Goupil & Bertholet.* They attribute its sanative properties to its power of healing, before suppuration (which it arrests) is established.—*Ibid.*

3. TETTER: Bertholet, Martin Solon, Goupil, M. Coster, Duchesne, Dupau, Dauvergne & Alibert, at the Hospital St. Louis.—*Ibid.*

4. CHANCRE AND CHRONIC VENEREAL ULCERS: Bertholet, Le Marquant, Kunckel, Lesseré.—*Bulletin Gen. de Therap., Feb.*, 1834.

5. *Pruritus, Scabies; Tinia Capitis; Syphilitic Ulcers:* In all of these M. DA LUZ used it with infinite success.

[*Ecl. Jour. Med., June*, 1839.

PROFESSOR CORNELIANI, states that it can be used with advantage in the following diseases: Diabetes Mellitus; Polydipsia; Hemoptysis; Chronic Catarrh; Diarrhœa; Palpitatio Cordis; Inflammatory Fevers(?) and *perhaps* Tetanus.—*Jour. de Chimie Med., Feb.*, 1836.

From the equivocal manner, however, in which the Professor recommends its use in the foregoing catalogue, I imagine that the issue is rather conjectural than warranted, but as a *Diuretic*, he speaks unqualifiedly and says, "its action is very prompt, and acts by paralysing the neck of the bladder." He remarks, that with regard to its effects in Phthisis, he has observed, when respired, it produces prostration of the intellectual functions, and muscular movements. Here, the Professor and Mr. Billard are at issue.—*Vide, Billard ut supra.*

DR. ELLIOTSON bears testimony to its value in the following diseases:

Bronchorrhœa, with profuse secretion, *without inflammation.*

Asthma, depending on morbid excitability of the bronchial membrane; used by inhalation.

Hysteria, unconnected with inflammation, generally very useful; used internally.

Palpitation, depending on morbid excitability of the heart; is more readily subdued by it than by any other remedy.

Vomiting, independent of inflammation and structural disease of the stomach. In this, he thinks its power is "perfectly established, and knows of no remedy to be compared with it, succeeding when even hydrocyanic acid fails."

Colic and Enteritis. It arrests vomiting in these long before the bowels are opened. It causes purgatives to be retained, which could not be done before. He has seen it of essential service in vomiting, from *arsenic, pregnancy and sea sickness.*—*Med. Chir. Trans.*, vol. 19, p. 416.

Dr. Elliotson, also, used it successfully as an *anti-emetic* in an aggravated case of jaundice, when all other remedies failed. No nausea or vomiting occurred after the first dose had been taken.

[*Lancet, August* 20, 1836.

Mr. Maddock, used it in *sea sickness*, with almost uniform success.

[*Lancet.*

Cancer of the Face and Palate, Graefe of Berlin.

Cancerous Ulceration of the Nose, Breschet.

Cancer of the Lip, (a perfect cure) Marchal.

[*Edinb. Med. and Surg. Jour., Oct.*, 1836.

Reichenbach and Cloquet add their testimony in cancerous affections. And Dr. *Friese* in the *Berlin Med. Zeitung, No.* 13, 1837.

M. Sigmond, in *irritable itching of the anus*, from long continued hemorrhoids. R. Copaiv, p. 3.; Kreosote, p. 1.—*Lancet, Sept.* 8, 1838.

Mr. Curtis, in *Deafness*, from deficient cerumen. After cleansing the ear and softening the wax, used as follows: kreos. ℈ i. ol. amygd. ℥ ss. Insert a little by means of a camel haired pencil, night and morning.—*Lancet, Nov.* 24, 1838.

Ulcer of Septa Narium: Sir Francis Smith effected a cure in an aggravated case, which had resisted nitrs. argenti, sulph. cupri, and iodine. He at first used a solution 1 part to 60 of water, snuffed up frequently through the day. This not answering, he applied it pure by means of the hair pencil, causing the patient a few seconds after to inhale the fumes of acetic acid, because this was its proper solvent, rendered it more diffusible, and destroyed its unpleasant odour. In *scald head*, he pronounces it almost a specific. [*Dublin Journal, May*, 1837.] I concur fully with Sir Francis, as to its virtues in eruptions of the scalp.

Chronic Gonorrhœa and Gleet: Dr. Dick thinks it more reliable than copaiva. 2 drops per diem, beat up with loaf sugar and water into a syrup.—*Edinb. Med. and Sur. Jour., April*, 1838.

In *Toothache*, I have scarcely met with a dissent from its uniform success, if properly applied, and provided there be caries.

Chilblains: *Reichenbach*; *Hahn* of Stuttgart, and *Cormack*.

Cutanei generally: *Elliotson* and *Prof. Wolf*, of Berlin.

Ulcers, especially apthous,* venereal, scrofulous, and phagedenic.—Used in these by *Dr. Shortt*, of the Royal Infirmary of Edinburgh.

Mr. J. Rose Cormack relates the following singular effects of the remedy while being curatively employed. M. *Tealier* applied a saturated solution of kreosote to the cancerous breast of a woman. As soon as it came in contact, there was "an acute burning pain in the sore, shooting through the right side of the chest, and extending from the head to the tip of the toes." This continued an hour. The pains then ceased, and the patient slept uninterruptedly for ten hours.

In case of ulcer of the cervix uteri, "the application caused the woman to toss about the bed like one in convulsions."

[*Edinb. Med. and Surg. Journal, October*, 1836.

Failures: The cases in which the *kreosote* has failed to produce such salutary results as those first exhibited, are as follows:

Miguet: 3 cases of hemorrhage in which the section of the vessel was complete; also, in chronic pulmonary catarrh, and some tuberculous diseases, and in two cases of simple incised wounds of the same class of vessels as the three cases above referred to.

Lesseré: In syphilitic blotches of the skin.

Breschet: Cancerous ulcer of the nose.

Goupil: Two cases of fistula communicating with caries of the bone. The physicians of Germany also doubt its virtues in such cases.

M. Solon: Five cases phthisis pulmonalis, with cavities in the lungs.

Da Luz: 1 case herpes furfuracea, 1 icthyosis, 1 herpes crustacea; 3 scabies, 1 tinea capita favos., 1 tinea furfur., 1 tinea granulosa, 2 ulcers

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* Magendie's formula for Apthous Ulcers; R. Kreos. pur ℈ ss, gum arab. ℥ iss, mist. camphor. ℥ xss. M. and use as a wash every half hour.

of the anus, 2 condylomata ani, 3 abscesses, 1 suppurating bubo, 1 caries tarsi.—*Eclect. Jour. Medicine, June*, 1839.

Wolff, of Berlin, 2 cases cancer uteri; *Prof. Syme*, lupus.

Dr. Elliotson, was unsuccessful with it in a case of phthisis. He is "not satisfied with it as a remedy for tubercles." He thinks, however, it is rather serviceable when a single ulcer or a small number exist, with no disposition to further tubercular formation, and *cites one case of perfect recovery.* In *epilepsy*, he has found it occasionally successful, *but in the majority of cases, the disease was either aggravated by it or wholly uninfluenced.* In *neuralgia*, he pronounces it "uncertain and variable."
[*Med. Chir. Trans.*, vol. 19, p. 416.

Dr. Kohler, (loco cit.) condemns it in phthisis. He has found the result generally unfavorable, increasing the fever and number of pulsations. It aggravated the cough and dyspnœa, produced no good effect on expectoration, and increased the tendency to hemoptysis. It also diminished the urinary secretion, and in one case so much as to "promote the development of a dropsical affection." He further asserts that in phthisis "it never seemed to act as a palliative, much less as a radical cure." In cancinoma uteri he equally rejects it.

With the merits of the Kreosote thus fairly arrayed I conclude my task, and thus informed, no physician can or ought to proceed unwittingly, either working injury to his patient by an infatuation which shuts out reason and testimony on the one side, or neglect to avail himself of a possible means of relief, with the weight of responsible authority on the other.

II.—*Trismus Nascentium.*

We have been highly gratified at the kind attention of our correspondents to our request for facts and observations in relation to this curious and fatal disease. In addition to what we have already published in our previous numbers, we give below two letters just received; one of which details a *successful case* treated on a *new plan*—the other merely offers *a suggestion* in regard to *the cause* of the disease, which may be taken for what it is worth. We may remark again that the affection is by no means confined to *coloured* children in New Orleans, but is occasionally met with in the most respectable families.—EDITORS.

WARREN COUNTY, MISS., 30th July, 1846.

Messrs. Editors :—I send you the report of a case of Tetanus Nascentium, successfully treated.

I was called to see the child of Mr. M——, on the ninth day after its birth. On the seventh day the umbilicus was dressed, and the nurse said it looked well. The cord sloughed off, and the mother (this was her first child,) thinking there was no further necessity for the bandage, threw it off. The next morning it was very restless and refused the breast. In the afternoon the mother observed twitching of the muscles of the face, and on the following morning I was sent for. I found a well marked case of Tetanus Nascentium. The hands were tightly closed ; the legs contracted, crossed and drawn firmly to the nates ; the muscles of the face were in continual motion, and the jaws closed. No physician that has ever seen a case would mistake this ; he would remember, at least, how futile had been all his efforts to save his little infant.

In previous cases I had tried almost every thing that offered the least hope of success, and I had never been able to cure a single case. I was induced to try the sulp. of quinine in decided doses.

9th.—I gave one grain of quinine with the eighth of a grain of ipecac every two hours—ordered a tepid leg bath every three hours, and opened its bowels by enemata.

10th.—I visited it, hardly hoping to find the slightest benefit from my prescription, but to my great gratification I found it better. I continued the prescription at intervals of three hours.

11th.—I saw it this morning, it was decidedly improved—had but three spasms during the night—the hands and legs were relaxed, and it could swallow tolerably well. But what was very strange, when it was put to the breast it would have a spasm. I continued the same prescription.

12th.—I found it this morning convalescent—it could take the breast without difficulty ; discontinued all remedies. It is now two months old and in fine health.

I do not remember to have seen the disease treated in this way, and if it has any thing of interest in it, you can use it.

About two-thirds of the deaths amongst the negro children, is from this disease, and so uniformally is it fatal, that a physician is never sent for. I am of the opinion that the disease may be prevented by the proper application of the bandage.

Very respectfully,

D. B. NAILER, M. D.

Black River, July 12, 1846.

Messrs. Editors :—I have seen in your journal, papers by Drs. Eberle, Wooten, and Fitzhugh, on the subject of Trismus Nascentium. The doctors appear to agree on the nature, fatality and postmortem appearances, but differ and are in doubt as to the cause of the disease. That the disease originates in the cord, all agree ; yet it is impossible to conceive that the pressure of the bandage, a little more or less tight, should generate so fatal a disease as contended for by Dr. Fitzhugh.

It is reasonable to suppose, however, that the application of a violent poison to the cut extremity of the cord, should produce a disease presenting all the pathological appearances of Trismus Nascentium.

I have but one fact to adduce in support of this supposition, aside from the known laws of the animal economy, and the known effect of the admission of poisons into the circulation.

I saw my first case of Trismus two weeks since. The case presented many of the symptoms mentioned by Drs. Eberle and Wooten, and terminated fatally in twenty-four hours; I did not have an opportunity to examine the dead body.

The fact to which I wish to call the attention of the profession is this: the mother had contracted vaginal disease of a venereal origin, I presume, and had a profuse discharge of acrid matter at the time of her delivery. The case was attended by the negro midwife who attends all such cases on the place, and the disease had not made its appearance at any previous time in her practice.

Dr. Wooten says:—"Sometimes I have found it of such frequent occurrence as to present the appearance of an epidemic; yet I have never seen a white child with the disease."

The disease "existing in an epidemic form" in negro quarters, would not astonish the wise, could it be given a venereal origin; but it is difficult to account for the fact with the "rusty scissors" or "tight bandage" doctrine of Drs. F. and W.

That the disease does sometimes attack the white child, we have the evidence of Dr. Eberle, although some of his cases hardly deserve the name.

My suggestion is that vaginal disease (I will not say venereal) in the mother, is the cause of Trismus in the child. The treatment is obvious. Thorough ablution after tying the cord and before dividing it; and perhaps the application of nit. argent. to the cut extremity after it is separated. I shall have an opportunity of testing the remedy soon, as I have other cases in an advanced stage of gestation.

E. HUGHES, M. D.

III.—*A New View of the Position of the Epigastric Artery and its Agency in Determining the Position of the Internal Inguinal Ring.* By A. J. WEDDERBURN, M. D., Professor of Anatomy in the Medical College of Louisiana.

During the winter of 1842, whilst dissecting the inguinal region for demonstration before the class, I noticed the continuity of the tunica vaginalis communis, with the facia transversalis, and was at once convinced that the internal ring had been erroneously considered by Anatomists, a hole in the facia, instead of what it really is, the mouth of a sac, which is continuous with this membrane. At that time the works of Wilson, Cruveilhier and Curling, had not reached this city, and I found it a difficult matter to convince my medical brethren of the correctness of my observation, as it was opposed to all the received opinions of the day. During the year 1843, I had an opportunity of reading Curling on the testis, and was forcibly impressed with a fact therein stated concerning the agency of the cremaster muscle in the descent of the testicle, during fœtal life, as being strongly corroborative of my views concerning the identity of the above mentioned membranes. In my lectures in the winter of 1844, before I had received the works of Cruveilhier and Wilson, in order to convince the class of the continuity of the tunica vaginalis communis with the facia transversalis, I spoke of the agency of this muscle in the descent of the testicle—of its two points of origin, one from the spine of the pubis, the other from Poupart's ligament—and contended that during fœtal life before the descent of the testicle, that the facia transversalis must certainly be reflected over this muscle to its point of connexion with the testicle, so that this facia which covers the cremaster muscle whilst it is within the abdomen, becomes its lining membrane after it gets without.

By way of illustrating the continuity of these membranes, after the descent of the testicle, I have resorted to the following expedient: unite the forefinger and thumb of the left hand, by way of representing the internal ring—throw over the same hand a handkerchief, which shall represent the transversalis facia. By placing then the forefinger of the right hand upon the handkerchief, and pressing it through the ring made by the union of the two fingers in an oblique direction from right to left, it will be at once perceived that when the handkerchief passes the point of juncture between the thumb and finger, it is thrown into a crescentic doubling which corresponds with the doubling in the facia at the internal margin of the inguinal ring. Now, in the descent of the testicle, this same arrangement takes place, the epigastric artery taking the same situation with regard to the facia transversalis, that the integument connecting the thumb and finger, does with reference to the handkerchief. From what has been already said, with a little reflection, it will be seen, (admitting the continuity of the membranes in question,) that the cord has anterior to it one layer of the transversalis facia, whilst it is in the canal, and at the posterior part two, as will, I think, be clearly shown presently. Now, let us ask the question, what produces this crescentic doubling at the internal ring, and around what is it formed? If we examine a subject, we will ascertain that it is the epigastric artery, and we will immediately see that this artery is *anterior to the facia transversalis*, instead of being between it and the peritoneum, as is asserted by all writers on the subject.

In order that the Epigastric artery may be seen in its proper relation to the facia, let the dissection be made in the following manner: The tendinous portion of the external oblique muscle being exposed, after the manner directed in the books, in the dissection of the parts concerned in the anatomy of Inguinal Hernia, and the scrotum laid open so as to expose the cord and testicle. Make an incision from the external ring along Poupart's ligament to the anterior superior spine of the ilium—dissect the tendinous expansion of this muscle from the internal oblique, by which the cord as it lay in the canal will be exposed. Remove from the cord and testicle the cremaster muscle, then remove the cord from the canal by throwing the testicle upon the outside of the thigh, and we will have the epigastric artery plainly exhibited, anterior to the facia transversalis. The dissection must then be continued, by cutting carefully through the internal oblique and transversalis muscles at their points of origin from Poupart's ligament. Turn the transversalis carefully from the facia with the handle of a scalpel, working all the time towards the middle line of the body. After exposing a sufficient surface of the facia to see the artery coursing upon it, take hold of the testicle, and make traction upon the cord, and a fine view of the funnel-shaped neck of the sac (the tunica vaginalis communis) will be obtained. We will here see that the covering of the cord which is continuous with the facia transversalis, passes into that facia behind the artery—the artery being lifted when traction is made on the cord. Now, if the cord be placed in its proper position, it will be perceived that the facia transversalis coming from the iliac side of the pelvis, runs directly in a straight line into the facia of the cord, whilst that portion of it coming from the direction of the pubis passes up to the outside of the epigastric artery, turns abruptly around it, and is lost in the tunica vaginalis communis—the artery being situated as it were in a crotch. This arrangement makes one layer of the facia at the anterior part of the vessels composing the cord, and two layers at the posterior, the one its proper tunic, the other forming the posterior boundary of the canal. Having seen now the artery anterior to the facia, make a transverse incision from just above the crest of the ilium to the linea alba, and a vertical one to the symphisis pubis—then remove carefully the peritoneum from the facia transversalis, and we will find that from a posterior view, the artery cannot be seen because the facia transversalis is behind it. We may now introduce one or two fingers into the internal ring, and examine from a different view, the continuity of the facia. We will here also see that if the cord is held out at a right line from the facia, that the crescentic doubling, mentioned by some writers, at the internal margin of the ring, entirely disappears, but is exhibited again as soon as the cord is placed in its proper oblique direction.

From what has been said it will no doubt be perceived that the epigastric artery, which is always at the inner side of the cord, (or it would be more correct to say posterior, or both inner and posterior,) determines the position of the internal ring, in the mechanism of the descent of the testicle, the cremaster, before its descent, passing to the testis anterior to the artery.

About the first of last April, whilst at the Charity Hospital with Dr. Axson, of this city, I communicated to him my views on this subject, and at the same time made for him a dissection in which the position of the artery was plainly exhibited, and in this subject had the satisfaction of seeing the two points of attachment of the cremaster muscle. That part arising from the spine of the pubis was a bundle of muscular fibres nearly as large as the little finger.

In concluding this article, I have to state that it has not been my object to give a full description of the region, but simply to call the attention of the profession to the subject, in order that a false chapter in anatomy may be corrected. The difficulty experienced by students in understanding the descriptions of the anatomy of Inguinal Hernia, has evidently grown out of the fact that such descriptions have not been true.

IV.—*Remarks upon the Causes, Phenomena and Treatment of Tetanus.* By A. Hester, M. D.

The more serious and fatal a disease, the more necessary it is to investigate the causes which produce it, and to determine, as far as practicable, the best—the surest means of arresting its progress. Notoriously frequent, and often fatal in this latitude, tetanus has strong claims to our attention, and the object of this paper is to induce others more competent than myself to turn their attention to the subject.

In treating of any particular disease, it is customary to speak of two causes, the one, the *predisposing*, and the other, the *exciting*. In the affection under consideration, these terms possess more than their ordinary weight and signification ; indeed, it was in reference to one powerfully predisposing cause in our climate—viz, heat—that I was induced to select the subject at the head of this article for a few disconnected remarks. Climate (including what is both beneath and upon the earth) controls, in a great measure, the development of organic nature ; thus the luxuriant vegetable of the Tropics will wither and pass away, if transplanted to the cold and ungenial climes of the North.

Man bears, in this respect, as well as in some others not necessary to mention, some resemblance to the vegetable creation. If then, climate alone plays so important a part in the healthy growth and development of man, certainly it may, and does exert an influence, no less obvious, in the production and propagation of particular diseases. Without seeking to explain the mode of action of heat in predisposing the system to tetanic affections, I shall simply name a few points in Tropical or warm regions, where this disease is of frequent occurrence. At Cayenne, tetanus is so liable to follow the most trifling wound, that a law is actually in force there, condemning the proprietor of a house to a heavy penalty, should foreign bodies, such as pieces of glass, nails, &c., be found uopn his premises, calculated to inflict a wound upon the pedestrian. The same liability to the disease also exists at the Antilles and in St. Domingo. In the West Indies, and in fact all the hot latitudes, tetanus may be produced from the slightest causes. In our own country, this affection is much more frequent and fatal in the Southern than in the Northern parts of the country. Along the coast of Mexico, where the heat is intense, tetanus frequently follows the simple operation of cutting from the feet and toes a small insect, called the *Yagers*, which buries itself in the flesh. During the expedition of the French army into Egypt, under Napoleon, Baron Larrey informs us that tetanus often followed the most simple wounds.

It would seem that long protracted heat, whilst relaxing and debilitating the muscular fibre, increases, at the same time, the excitability of the nervous system; hence the loss of balance between the muscular and the nervous systems. The first is not only deficient in tone, but at the same time acquires an aptitude for contraction, which, on the application of the slightest stimulant, may be developed to such an extent as to produce tetanic phenomena. It is also well known, that whatever tends to debilitate the system, must also increase its excitability ; this will place the subject in the best possible condition for tetanic symptoms, on the least disturbance of the economy, from any kind of wound. Of the individual predisposing causes, a susceptible nervous temperament—profound moral emotions—mental disquietude, and in a word, all mental impressions characterized by a pervading perturbation of the nervous system, favour the production of this disease. Hence, those endowed with a nervous temperament, and in whom a train of sympathetic phenomena always announces an impending pathological state, should be watched with the most assiduous care, on receiving otherwise insignificant wounds; else they fall victims to tetanic disease.

We have barely alluded to but a few of the predisposing causes; enough has been said on this point, we think, to answer our present purpose. The exciting causes are as numerous as the temperaments &c. of individuals are different. Among the essential causes of tetanus, are wounds, such as the lacerated, contused, penetrating, and indeed, all solutions of continuity, attended with rupture or laceration of muscles and tendons, and of tough and inelastic fasciæ; compound dislocations, particularly of the ginglymoid joints; partial division of ligaments and nervous cords; foreign bodies, such as glass, rusted nails, spicula of bone, buried in the soft parts, and in contact with nerves, tendons, aponeuroses, &c.;—such are the most frequent causes of traumatic tetanus. The action of a cold damp atmosphere upon the cutaneous surface, in cases of extensive burns, and the sudden recession of the different exanthemata will likewise produce tetanus.

Noël having opened the bodies of many dead of this disease, and finding vermes in the intestinal tube, declared these to be the essential cause of the disease. This was, perhaps, purely accidental, and should therefore have but little weight. The constant irritation, from the presence of worms, in the digestive tube, may certainly act as a predisposing cause; since we know that in children, they frequently give rise to convulsions and spasms. Sudden changes of temperature, particularly from hot to cold, may prove the exciting cause of tetanus. Again the sudden suppression of any habitual or periodical discharge, as the menses,* will, it has been asserted, produce the disease. The causes above enumerated, will be much more likely to determine tetanus in the spring and summer, when the temperature is high, and the changes sudden, than at other seasons of the year; my observations corroborate this statement. I think it highly probable that malaria or marsh miasm, in conjunction with a high range of temperature, may powerfully predispose the system to tetanic convulsions. If, as McCullock asserts tic-douloureux, and certain forms of neuralgia are the offspring of malaria, or marsh effluvia, it would require us to advance but a step further in the catenation of causes, to admit the same agent as playing an active part in the production of tetanus; with this difference, however, that the first is seated in a sensitive nerve, and more local in its character; whilst the latter affects the excito-motory system, and is universal.

Tetanus has been divided by writers into *traumatic* and *idiopathic*,—*chronic* and *acute*.

The first, or traumatic is synonymous with the acute; the latter or the idiopathic with the chronic form. The first is the most frequent, and by far the most fatal; whilst from the latter, the patient often recovers.

* Dazille, Maladies des Negres.

Traumatic tetanus will end in death in twenty-four hours;—again the patient may survive as many days, and ultimately perish. A slave, whilst engaged a few years since in this city, in repairing a dilapidated building, accidentally received a wound in the foot from a broken nail. He was immediately ordered home, a short distance from the spot; and on the way, he was seized with all the terrific symptoms of tetanus, and expired with universal rigidity of the muscles. Dr. Robinson, of Edinburgh, reports in *Rees' Cyclopedia*, the case of a negro, who wounded his thumb with a piece of porcelain, and died in a quarter of an hour afterwards of tetanus.

The progress of this disease is sometimes so rapid that little or nothing can be done, in the way of art, to arrest its course. It then behooves us to use every means in our power to prevent its development—to institute such a prophylactic course as will be most likely to fortify the nervous and muscular systems against a disease so painful in its effects, and so difficult to subdue. "To prevent is better than to cure diseases," is a wise maxim in physic; it applies with much force and truth to the affection under consideration. To foresee, often enables us to forestall disease.

Of chronic or idiopathic tetanus, I shall say but little; of trismus nascentium, nothing, except that it seems to be produced by the action of an unhealthy atmosphere upon an extremely delicate and excitable nervous system. It sometimes assumes an epidemic character, seizing *toutes les nouveaux nées* in an extensive district, the moment they assume an independent existence, and become exposed to the action of light and the atmosphere. Traumatic tetanus will claim more particularly our attention, both because it is the worst form of the disease, and infinitely the most difficult to treat. Celsus writes that when the head is thrown back and fixed to the scapula, it is called opisthotonos; when the chin is made to approximate the sternum, then it is designated emprosthotonos; but *tetanus* when "rectam et immobilem cervicem intendit."

M. Larrey asserts that when the nerves of the anterior region of the body are wounded, we have, in traumatic tetanus, emprosthotonos; but, opisthotonos, when those of the posterior part sustain a lesion. M. L. does not advance any facts to sustain his position; and I am disposed to question the soundness of such an hypothesis. Tetanus is a disease of the true spinal cord. Although the essential nature of the affection is unknown, yet, by the aid of physiology, we are enabled to trace the course of the disease through the system.

During the existence of an equable temperature, whether hot or cold, temperate or inclement, tetanus is rarely witnessed; but sudden transitions from one state to the other, will be likely to favour the development of the disease, as already hinted.

Besides atmospheric vicissitudes, moral causes may prove the directly exciting agents; and when the two co-operate, the danger is increased.

Sudden fright, sharp and shrill sounds, as the clamour and clangour of two armies, when about to engage in battle; explosion of fire arms, particularly of cannon, are said by military surgeons to excite tetanus in those wounded on the field of battle.

The invasion of tetanus is generally characterized by an irrascible temper, horripilations, transient rigours, nervous shocks, and muscular twitchings, in different parts of the body.

It more frequently supervenes upon wounds inflicted upon the lower, than upper extremities; it is more frequent in the young and irritable, and middle aged, than in the old; in men than in women, perhaps from greater exposure to the exciting causes.

A peculiar—shall I call it, a *sui generis* irritation—is propagated from the seat of lesion, by means of some nervous filament, to the spinal cord; and thence transmitted through the excito-motory branches, to the various muscles of volition or relation—and finally to those of organic life, as the heart, &c., causing speedy death. The muscles of the pharynx are the first to be affected, causing dysphagia; then the elevators of the inferior maxilla, as the masseter, temporal, and the like, clenching the mouth, and thus preventing the introduction of food and drink. If the *orbicularis oris* is affected, the lips are puckered; if the zygomatic, the angles of the mouth are elevated, imparting a hideous smile to the countenance; if the occipito-frontalis, the brow is knit and the forehead corrugated, and so on of the other muscles of the face. The rigidity gradually extends down the trunk, reaching the abdominal muscles and those of the lower extremities; the upper are said to be the last of the voluntary muscles affected. A sense of tightness or constriction is experienced about the præcordia and region of the diaphragm. The sterno-cleido-mastoid muscles, are prominent and hard; the pectorals, on percussion, sound like a solid piece of timber; the abdomen as unyielding as a two inch clap-board; the belly of the gastrocnemius, as firm as that in the marble statue of the *Farnesian Hercules.*

At this period, the excretions are with difficulty voided; the urine does not flow for want of consentaneous action of the muscles concerned in its expulsion; the bowels are constipated, and all the secretions, except from the skin, are both morbid and scanty.

The tongue is coated, and the mouth dry and clammy; the patient is tormented with thirst, which he cannot quench but at the risk of redoubling the spasms and augmenting his sufferings.

The disease is one of irregular paroxysms, during which the patient experiences indescribable agony—I have seen them brought on by raising the bed clothes—touching the surface, or permitting a current of air to pass over the patient.

Other animals besides man are exposed to tetanus. It is well known that one of the noblest of domestic animals, the horse, is frequently the subject of this intractable disease. Besides the horse, Hunter mentions the cow, the deer, the monkey, and several other animals as obnoxious to this complaint; Mr. Hunter states that he once lost a stag from tetanus.

The *pathology* of this disease is extremely obscure. But little of a positive nature has been written on this subject; still less that may be relied upon as enlightening us in regard to the best mode of treatment. The pulse in tetanus is at first hard, small, rather frequent, and seldom ranging over one hundred, except during, or soon after a paroxysm of *clonic* spasms. This smallness of the pulse can be explained by the diminished calibre of the arterial tubes, being acted upon, and pressed by the adjacent muscles, when in a state of spasmodic rigidity. The surface is throughout all stages of the disease, bedewed with a rather clammy perspiration, and generally quite warm to the touch. The matter of perspiration is decidedly acid, and emitting an odour of a specific smell, more easily perceived than described. The urine is highly coloured, and deposits a floculent sediment on standing. Of its chemical changes, we are not prepared to speak; but it doubtless contains much foreign matter, as might "à priori," be inferred from the fact that the functions of nutrition and secretion are materially deranged.

There are many diseases, of an exhausting nature, which assume tetanic symptoms before the close of life. Dr. Rush distinctly declares that yellow fever sometimes puts on the form of tetanus. I have observed the same fact in yellow fever subjects a few hours before death. In the summer of '42, I had a stout German, of huge muscular proportions, under my care with black vomit; the night previously I had left him to die; on returning the next morning, I found him as if in the act of rising from the bed, with his head thrown backwards, and to the right, resting upon his elbow; feet extended, and every muscle of the body prominent and as hard as wood; yet his respiration and pulse were but little hurried. The patient was unconscious. On raising the bed-clothes to examine his feet, I accidentally touched the great toe, and instantly such violent tenanic spasms followed as raised his head some distance from the pillow; still, however, the body maintained its inclination to the right side. The shock resembled that produced by a powerful galvanic battery.

To return to the pathology of tetanus. The lesions found after death, in this disease, by no means explain the formidable nature of the symptoms which mark almost every stage of its progress. The real changes wrought by the disease may be supposed to exist in the nervous system, of which the spinal column is the centre; accordingly, Pathologists have examined this tract and its investing membranes with great care, which has been any thing but satisfactory. In some, the spinal marrow is somewhat engorged, vascular, and its coverings injected, with an effusion of serum in the theca; in others, and by far the greater number, not a trace of inflammatory action is discoverable after death.

Dupuytren quotes a case of this kind, and after the most careful examination, pronounced the spinal arachnoid "*parfaitment saine, aussi que tous les autres organs du tronc.*" Not satisfied with the results of the inspection, M. D. began to explore the surface of the cadaver, and remarked a cicatrix on the inside of the forearm; he excised it and found the knot of a whipcord buried in the very substance of the cubital nerve.

The muscles are sometimes found ruptured, particularly the *recti* of the abdomen, and occasionally those on the back of the neck, with extravasation of blood in the surrounding structures. The parts about the roots of the spinal nerves are sometimes the seat of considerable congestion and vascular turgescence. In cases of traumatic tetanus, the nervous cords, near the wound or injury, causing the disease, usually leave traces of inflammatory irritation and some degree of thickening of the neurilemma. The mucous membrane of the air passages is highly vascular and considerably injected. Some of the internal parenchymatous organs, as the liver, kidneys, and lungs, are usually loaded with blood, due, doubtless to the centripetal direction given to the fluids, by the violent contraction of the muscles of relation upon the venous branches. When the tetanic irritation extends to the ganglionic system of nerves, death usually takes place before any material change can be effected in those structures. Death is sometimes produced, and always hastened, by involving the apparatus for respiration. The fixed and immoveable state of the diaphragm; the rigid contraction of the intercostal muscles, and indeed, of all the muscles of respiration, prevent that free expansion and contraction of the chest, so indispensable to the complete decarbonization of the blood. Hence, death from asphyxia is not uncommon, as I conceive, in the latter stages of tetanus. I shall close these remarks, by a few observations upon the treatment of tetanus. Before we begin with this subject, it may be proper to define what means should be adopted in order to obviate an attack of this formidable affection, when threatened. If a patient of an excitable system receives a severe contused or lacerated wound of either the upper or lower extremities, implicating the ligaments, fasciæ, tendons, and nerves of the part, he should be carefully secluded from every thing likely to disturb his system or to affect his imagination.

Confinement in a separate chamber, from which all noise must be excluded, is of the first importance. The temperature of the room should be regulated by the thermometer; on this point, let us hear Celsus: "*cavendum vero præcipue frigus; ideoque in eo conclavi, in quo cubabit æger, ignis continuus esse debebit, maximeque tempore antelucano, quo præcipuè frigus intenditur*," Lib. iv, p. 156. We can add nothing to this sound advice, only that to be more precise, we should prefer a temperature between 75, and 80° (F) as most likely to be congenial to the feelings and less liable to disturb the balance of the nervous system. Perfect repose of both mind and body should be enjoined; and the patient restricted to a light nourishing diet. All the means, both general and local, should be such as are calculated to subdue inflammation, repress excitement and quiet the irritability of the nervous system. From the wound, every thing, such as foreign bodies, spicula of bones, &c., must be abstracted; and emollients, tepid lotions, slightly anodyne washes, detergents and anti-septics applied to the wound.

Tetanus may, and does supervene sometimes within a few days after the receipt of an injury. It is most apt to be developed at a time when the wound is highly inflamed; again, not until free suppuration is established; and occasionally when cicatrization is nearly complete.

If the patient escapes this complication for two weeks after the injury is received, he is generally regarded as secure; but in a case of compound dislocation of the ankle joint, tetanus supervened the fourth week after the accident and the man survived 15 days. It has been recommended to unbridle the parts—to slit up the fasciæ, rendered tense by the accumulation of blood or pus,—to divide completely the half torn tendons and nerves—in a word, to remove all causes of tension, pressure, and stricture.

When inflammation is violent and the pain acute in the part affected—the usual antiphlogistic course will be enforced; but depletion must not be pushed *ad extremum*, else we augment the irritability of the nervous system and favor the developement of tetanus. As I regard a genuine case of traumatic tetanus beyond the power of the healing art, a sound system of prophylaxis—a preventive course of treatment is particularly called for in cases where we have reason to apprehend an explosion of the disease. To illustrate the practice which seems to me best in such cases, I will here introduce an observation, bearing directly upon the question:—A stout man. aged 35 years, when fowling in the month of December, 1844, near this city, over-charged his piece, and when he fired it, about 10 inches of one of the barrels upon which his left hand rested, was detached from the barrel, lacerating his whole hand and wrist in a most shocking manner—all the palm or space between the thumb and first joint of the little finger was desperately wounded, the thumb being nearly torn off;—the flesh hanging in tatters and shreds;—the fascia ruptured and the tendons exposed; several of the carpal bones dislodged and shattered, extending even to the head of the radius and ulna—thus involving the wrist joint. The hemorrhage was considerable, after reaction took place; but in 24 hours all bleeding ceased, to be followed by a most intense and rapid inflammation, with swelling, tension, and heat of the hand.

A free bleeding, purgatives, a number of leeches to the hand and wrist, followed by free scarifications with the bistoury, and emollients soon subdued these threatening symptoms, and placed the patient in a condition for the free use of opiates. For five or six nights and days, he did not enjoy ten minutes sleep; but was nervous—starting at the least noise, had jerkings and muscular twitchings; in fact such a state of system as foreboded the irruption of traumatic tetanus.

The tendons and muscles which had been torn across, and still hung from the wound, were left to slough away spontaneously, aided only by soothing lotions, as the *aqueous solut. opii*—with a little chlorine added; at the same time I gave large doses of *morphine*, as much as one grain every two hours through the night, with an occasional dose of black drop. In this way, I kept the system narcotized for ten or twelve days, giving the morphine and black drop, and applying the watery solut. opii, with but little regard to the dose, rather looking to the effect upon the system, as my object was not only to relieve pain, but to allay the irritability of the nervous system, and prevent the transmission of that morbid—that tetanic irritation, shall I call it, which, when once established is not amenable to our remedies. During the 24 hours—I sometimes gave as high as 8 and 10 grains of morphine, in the forms mentioned, keeping the hand saturated with the watery solution. The weather, although in the winter, was most favourable for the production of tetanus—being hot and damp for a few days, then suddenly cool and dry.

Whether the treatment pursued in this instance warded off an attack of tetanus or not, certainly the nature of the injury taken in connexion with the train of symptoms at an early period, justified the most serious apprehensions and led me to institute a prophylactic course of treatment. The result exceeded my expectations; and a similar injury would induce me to give opium and its preparations, of which I prefer sulphate of morphia and black drop, with a liberal hand. Some of the narcotics are believed to destroy the susceptibility of the system to take an epidemic disease. For instance, belladonna will, it is believed by many, fortify the infantile constitution against the invasion of scarlatina; opium, by diminishing nervous excitement, blunting sensibility, and checking absorption, will sometimes secure the system against contagious maladies. It would be well if a series of experiments, properly conducted, could be instituted to ascertain how far the narcotics, as a class, might be made available in securing the human subject against epidemic and contagious disease.

Practitioners are agreed that during the prevalence of epidemic or contagious disease, persons possessing susceptible nervous systems, are much more obnoxious to an attack than those of a sluggish and phlegmatic temperament. I regret that I cannot pursue this subject.

The indications to be fulfilled in the treatment of tetanus, are three; —first, to remove every thing calculated to react unduly upon the nervous system; secondly, to allay the spasms and violent contraction of the muscular system; and thirdly, to support the system until the disease or the patient is exhausted. I have already pointed out the course to be pursued in order to meet the first indication; viz: such as cleansing the wound of all foreign bodies, expelling from the *primæ viæ* all sources of irritation; confinement in a dark room, remote from all noise, bustle, and excitement, in order to give perfect repose to both mind and body; a uniform and agreeable temperature—caustic and deep and free incision around the part; sedatives and anodynes internally, and emollients and tepid applications to the seat of injury. Amputation of the limb has been practised by military and civil surgeons with success in a few instances. Baron Larrey says, that when it is plain that tetanus is produced by a wound or injury of a part, we should not hesitate to amputate the moment tetanus declares itself. He cites many cases in support of the practice, and among the number, that of an officer whose leg he amputated, and all the tetanic symptoms ceased. When the disease arises from an injury of the extremities, amputation, says he, will dissipate all the accidents "*par enchantement.*"

It was chiefly in Egypt, that the Baron obtained such extraordinary success; perhaps his experience would not differ from that of others in large cities and crowded hospitals.

Dupuytren, removed a leg for a noble lady, in whom tetanus had succeeded a compound fracture of the limb, a few days after the accident. The progress of the disease, says Mr. D., was neither checked, nor stayed for a single moment; and the unfortunate patient succumbed as rapidly, as if amputation had not been performed.

To fulfil the second indication, antispasmodics, diaphoretics, narcotics, and the directly sedatives, must be called into requisition. We shall only notice a few of these medicines; viz: opium, extract of the Indian Hemp, and prussic acid. If any article of the materia medica be entitled to the name of remedial, in this disease, it is certainly opium and its preparations. It will rarely check the disease, but by lessening sensibility and repressing excessive and violent nervous and muscular action, it may enable the constitution to wear out the disease and finally triumph. Opium, if given in large and increasing doses, for too great a length of time, will derange the stomach and bowels, producing a species of dysentery, such as usually carries off opium eaters—that will be most likely to hasten the death of the patient. If opium disagrees, drop it, for a time, and give in lieu, another narcotic ; or combine it with ammonia and other diffusible stimulants. This will restore the tone of the system, and its efficacy will be greatly enhanced.

A valuable medicine—Indian Hemp, has been recently introduced into practice, which really seems to exert a potent sway over the progress of tetanus. If the extract of this plant fulfils the high expectations now entertained of its virtues, the profession will have just reason to congratulate itself, and a disease, the bare name of which is a terror to both doctor and patient, will no longer be classed among the *opprobria medicorum.* Since Prof. O'Shaughnessy, of Calcutta, first directed the attention of the profession to the wonderful virtues of this extract in tetanic affections, others, both in this country and in Europe, have given it a pretty fair trial. The result has been highly gratifying, if we are to credit the cases reported. In an able paper, on the virtues of the Indian Hemp, published by Mr. Donavan, in a late number of the "*Dublin Journal of Medical Sciences,*" a number of cases of tetanus cured by this medicine are reported. Indeed, if we take the testimony of those who have made fair trials of the Indian Hemp, in the treatment of tetanus, we have a remedy as certain in the cure of this affection as quinine is in intermittent fevers, or mercury in syphilis. But let us not be too sanguine, remembering that *experience* often overthrows the illusions of opinion and establishes the decrees of nature! It is best to give this medicine, according to its advocates, in the form of *Alcoholic Tincture*, the resin, when administered in the form of pills, is apt to "pass undissolved through the intestinal tube." Dr. Wilson, of this city, has recently reported an interesting case of tetanus, following a punctured wound of the foot, in which it appears the Indian Hemp entirely subdued the violent spasms, and ultimately placed the patient in a fair way to recover. During convalescence, however, he was attacked with rubeola, and died on the 47th day, with tetanic spasms. This case was under the care of Dr. Farrell, of this city, who was the first, we believe, to recommend this medicine here in the treatment of tetanus. This heroic remedy—this *magnum donum Dei*, as some regard it, seems invariably to produce one effect, independent of its antispasmodic virtues, which we consider of the first importance in the treatment of this formidable disease, viz: it seems to invigorate the digestive organs, increase the appetite, and thus enable the economy to repair that lesion of nutrition in which, whatever may be the nature of the morbid action going on in the system, the disease essentially consists. When death does not proceed from the violence of the spasms, tetanus seems to terminate existence by exhausting the nervous system. In its latter stages, therefore, when the nervous system is precipitated into a state of adynamia, from deficient nutritive action, we must administer stimulants, without regard to measurement, and a digestible and nutritious diet as much as the patient can be induced to take.

Dr. Rush was an advocate for stimulants in this disease. He tells us to give brandy by the pint, yea, let the patient take gallons and even barrels, if the disease does not yield. A writer in the June number of the "*London Lancet,*" recommends *alcohol*, as the best stimulant in the treatment of this disease, and relates a case, in which he prescribed it with marked benefit, though too late to effect a cure.

The acknowledged effects of tobacco in relaxing muscular spasms, have induced some to try an infusion of this poisonous plant in the form of enemas ; but, it has not answered the expectations of those who made a trial of it. It produces extreme exhaustion, hypercatharsis, cold sweats, and a frequent and small pulse, without relaxing in the least, the muscular system. Warm baths I consider objectionable, not only because, in transferring the patient to and from the bath, more or less shock will be communicated to the system, but likewise, on account of the rapid evaporation that must take place from the surface of the body, thereby producing chilliness, spasms, and a train of morbid symptoms. If you must order a bath, give one of *hot oil* (olive I presume) as recommended by Celsus, in his 4th book. Stimulating embrocations, frictions, counter-irritants and the most prompt revulsives may be serviceable in some cases.

In the June number for 1844, of the *Bulletin Général de Thérapeutique*, M. Espezel relates a remarkable cure of tetanus in a little boy, aged 12 years, by *prussic acid.* In eight days, the little patient took three baths, four *pastilles* of calomel, and swallowed one hundred and ten drops of prussic acid. He gave it in the following formula :—lettuce water 120 *grammes ;* Magendie's medicinal prussic acid, 20 *gouttes ;* simple syrup, 30 *grammes*, mix : of this the patient took every hour a table-spoonful.

From the well known and powerful sedative influence of prussic acid, it might be supposed to exercise a controlling influence over the phenomena of tetanus. The aphorism of Hippocrates—*ad summos morbos, summæ ad unguem curationes adhabitæ, optimé valent*, may embolden us to administer the most dangerous and desperate remedies in this disease. We have omitted to mention a number of remedies recommended and given in this disease ; all are familiar with the long list, and each can make such a selection as his judgment may dictate. From a single trial I am disposed to think favorably of the *sulphate of quinine* in the treatment of this disease. The following case will speak for itself:

In October, 1845, a stout German, aged about twenty-five years, entered the surgical department of the charity hospital under my care. His friends stated that about ten days previously he had accidentally driven a large nail through his foot. He suffered but little from the wound, until two days prior to his admission into the hospital, when he began to experience, for the first time, some stiffness in the muscles of the neck, and especially in those about the temporo-maxillary articulation. Medical aid was now requested; the result was a free bleeding in the arms, cups to the inter-scapular region, and an active cathartic. But partial and transcient relief followed this treatment; the rigidity rapidly extended over the entire trunk,—finally, the upper and then the lower extremities became permanently extended, and in this painful condition he was received into the hospital. At this time he complained bitterly of cramps and spasm in the ileo-lumbar muscles. More to pacify than with the hope of relieving him, I ordered cups to the seat of pain. A few ounces of blood were abstracted, and the pains abated. On examination, I found the nail had been driven quite through the right foot, about the middle of the second phalanx. The wound on the dorsum of the foot was circular, rugged and as large as a five cent piece. Neither swelling nor inflammation existed about the part. The wound below had healed perfectly. I immediately cauterized the wound, and covered the dorsum of the foot with an emollient cataplasm. The patient was ordered to be screened from the light, and strict injunctions given to the nurse to prevent currents of air from striking the patient. My first object was to place the patient in the best possible condition to struggle with this terrific disease, without which precaution, the constitution must speedily succumb in spite of all our boasted medicaments. In the meantime, meeting with Dr. Harrison, he suggested the use of quinine in the treatment of this case. To this suggestion I readily yielded, not only because this article was achieving wonders throughout this country, but because I regarded tetanus as primarily a nervous irritation, traversing the system, having irregular but distinct remissions and exacerbations, and more easily developed in warm latitudes, where malarial fevers abound, than in cold. Accordingly, the following day, I gave 60 grains of quinine; on the day after 70 grains, and so on for four or five days. The effect was to reduce the pulse, from over 100 to 80—to excite a free, a copious perspiration—to diminish the rigidity of the muscles, and lessen the frequency and violence of the paroxysms of clonic spasms, which are so distressing and exhausting to the patient. To procure sleep, small doses of morphia were prescribed at night. Under the influence of large doses of quinine during the day—a nutritious diet, and *fractional* ($\frac{1}{2}$ gr. during the night) doses of the sulphate of morphia, convalescence was soon established.

Towards the close of the case, brandy was freely administered, and a more nourishing diet allowed. In fifteen days, the patient was on his feet, and soon left the hospital.

Whatever may be the immediate cause of tetanus, certainly a high degree of nervous irritation characterizes its progress and symptoms. This irritation interferes materially with nutritive action, and the most we can do is, to repair as far as possible the lesion of nutrition which deranges the entire organism; and, finally, if not counteracted by a nourishing diet and stimulants, ends in the extinction of the vital principle.

Tetanus will sometimes subside without any kind of treatment, for I have seen more than one case recover, in the Charity Hospital, with but little assistance from art. Indeed, I am inclined to believe, that if the patient be placed in a suitable situation, remote from all excitement, with a proper allowance of food and drink, the recoveries would be, perhaps, more frequent than under the present established mode of treatment. A soldier was wounded at the battle of Wagram: in evacuating Vienna, the surgeon-major received with this man a number of others, badly wounded. Tetanus soon seized the wounded soldier, and the surgeon regarding his case as hopeless, turned his attention to others, and abandoned the tetanic man to what he considered inevitable death. Guess his surprise to find him, a few days afterwards, almost recovered from his disease. In a short time he completely convalesced, without a particle of medicine. This case is extremely instructive, and justifies what I have already said on this subject. In cases, when the spasms extend to the muscles of the glottis, and threaten suffocation by closing the air passages, tracheotomy, or even bronchotomy may be justifiable. I do not know whether this practice has been attended with success or not; certainly the step could hardly endanger life—it might protract—even preserve it, under favorable circumstances.

V.—*Four Cases of Fever.* Reported by W. G. WILLIAMS, M. D., of Rodney, Mississippi.

Case 1st, July 10th, 10 A. M. Called to see J. H., aged about 35, a large muscular man of sanguine bilious temperament. Was attacked with fever on the 8th. Type double tertian. Says that the paroxysm of the 8th was severe, that of the 9th lighter, and returned at a later hour of the day, than the preceding. I found him with high fever, skin hot and dry, pulse 110, of augmented volume, but compressible. Complains greatly of thirst and pain in the head and back. Has not taken any medicine except a little quinine on yesterday, and has had no action on the bowels for 24 hours. Prescribed sulph. morphia $\frac{1}{4}$ gr., epsom salts ℥ iv, calcd. magnesia ℥ ss, mix in half a tumbler of cold water, and give immediately. I saw him two hours after, and found him much more comfortable; quite relieved of pain, and considerable reduction in the frequency of the pulse. 8 o'clock, P. M.—Medicine has operated well. Skin soft and moist. Fever has passed off. *Pulse* 45 *and still* of *augmented volume.* Prescribed 2 grs. of quinine every 2 hours and directed him to drink port-wine and water. This was continued during the night, and at 10 next morning, on visiting him, I found him up.—Advised the continuance of quinine and wine, which he used during the day, and a part of the day following, and he had no return of fever.

Case 2nd. A. B., aged about 30, temperament nervous. Attacked with fever, preceded by aching in the limbs, and sense of chilliness on the 1st of September. My first visit was at 6 P. M., of this day. I found him with considerable fever; skin hot, dry, and harsh to the feel; complains of severe pain in the head, which is very hot. Pulse 95 and did not seem to indicate the use of the lancet. Says his bowels are regular; had an action on them last night; none since. Administered epsom salts and magnesia—cloths dipped in ice-water to be applied to the head, and mustard to the neck. I saw him again early in the morning of the following day, (September 2nd,) and found him comparatively free of fever. Skin, though not hot, retained its dry harsh feel, and he was still complaining of his head. His pulse was rather quicker than natural, but not increased in frequency. Medicine operated well last night. Ordered a blister to the neck, cold cloths to the head to be continued, and administered 10 grs. of quinine. I saw him again at 10 A. M. Blister had drawn, but the head was not relieved. Says he vomited the quinine up with some bile shortly after I left, and that it increased the pain in his head. Is yet free of fever. Continue the cold applications to the head, and let him rest for the present.—

4 o'clock, P. M.—Fever has returned; pain in the head more intense; considerable intolerance of light; pulse 100; will bear the lancet but does not seem to demand its use so far as regards force or tension.—Bled him to the extent of 16 oz. which induced incipient syncope.—Administered ¼ gr. sulph. morph., and directed the cloths to the head continued. On my return about 10 o'clock at night, I found him sleeping. His fever had passed off, and his head was relieved; *pulse* 60.—Reduced below the natural standard. Directed him to take quinine on the following day, which he did not do, but had no return of fever.

Case 3d, July 13th. Called to see ———, aged about 26, temperament nervous. Has had a paroxysm of fever, every day, for three days, and is now in the third; complains of severe pain in the head; intolerance of light; pulse 100,—has volume, but is deficient in force. Considerable thirst; face seemed a little flushed; extremities cool, as well as surface generally, except the head, which is hot. Much troubled with nausea. I understand that the preceding paroxysms have been of the same character; the surface, except the head, not being hot at any time; the pain in the head having continued from the beginning of the attack, with slight abatement, occasionally. This individual was ptyal ized before I saw her, having taken a small quantity of mercury in form of blue mass, to the constitutional impression of which she is extremely susceptible. Ordered cold applications to the head, mustard to the neck, and large sinapisms to the extremities. The impression of the latter to be kept up by shifting their position. Administered 1-6 grain of sulph. of morphine, and directed 2 grs. of quinine to be given every two hours. If nausea is troublesome, apply mustard to the epigastrium, and try *aqua calcis*. Saw her again a few hours afterwards, when she informed me her head was relieved, and that she felt better than since first attacked. Ordered the quinine and mustard to be continued, and if there is no action on the bowels by bedtime, administer an injection of molasses and warm water.

14th. 10 A. M. Saw her again. The injection had been used as directed; and had the desired effect. Seems doing well. Discontinue every thing but the quinine, which continue, and if it affects the head, lengthen the interval and gradually withdraw it. Advised porter and water as a drink. Convalescence continued without the occurrence of any untoward symptoms.

Case 4th, August 20th, 9 o'clock, A. M. Called to see W. W., aged about 35; sanguine lymphatic temperament, and quite corpulent. Was attacked on the morning of the 17th with high fever, preceded by a severe chill and vomiting of bile. Fever abated on the morning of the 18th, and he remained better until about 4, P. M., when fever returned without a distinct chill, but a coldness of the extremities. On the morning of the 19th, he was better, but did not think that he was free of fever, as he had thirst; was restless and troubled with nausea, when he drank water in any quantity. About 10, A. M., of this day, fever returned, preceded by a slight sense of chilliness about the back and shoulders. "Used him worse," (to give his own expression) than any fever he ever had. On this day he took an emetic and some patent pills, which operated so actively on the bowels, and produced so much prostration, that it became necessary to resort to the use of paregoric and toddy to counteract their effects. On my first visit, August 20, 9 o'clock, A. M., I found him with a cool skin, pulse of augmented volume, and the pulsations of unequal force, indicative of a loss of balance in the circulation. Says he has thirst, and feels restless, with an uneasiness about his head, which he says he has never been entirely free of since attacked. His tongue is rather dry, and covered with a dirty muddy looking coat. Says he is troubled with nausea, when he drinks water. Directed mustard to the Epigastrium; administered sulph morphia $\frac{1}{4}$ gr., quinine 5 grs. I told him I thought his fever would return again in the afternoon, but to continue the quinine, 5 grs. every three hours. If fever returned, I directed him to send for me immediately, as I prefered treating him during the paroxysm. His fever rose in the afternoon of this day, but three hours later than before. I was sent for, and arrived about 10 o'clock at night. I found him with high fever. Said his head did not pain him as much as in the preceding paroxysms, but he had more pain in the back. He was very restless; pulse 100, of augmented volume, and the pulsations of a throbbing character, though the artery was easily compressed by the finger. Administered quinine 12 grs., blue mass 10 grs., Dover's powder 5 grs., combined, at one dose, and directed the quinine to be continued, 2 grs. every two hours. I remained with him two or three hours, and before I left he had ceased to complain of pain, his pulse was reduced in frequency from 100 to 80, and his tongue was moist. I visited him next day, and found him free of fever with a good pulse, good state of the surface, and a tongue cleaning rapidly. Directed him to take the quinine during the day and the day following, and if it affected his head to lengthen the interval, and gradually discontinue it. He had no recurrence of fever.

I have frequently seen the tongue become moist, pain in the head and back relieved, and the restlessness of fever subside, in half an hour after the administration of the combination used in this case. I gave it to bring about a speedy solution of fever, and to prevent the circulation being left on the subsidence of fever in the state I found it; in other words, to equalize it. It was this state of the circulation that induced me to believe, when I first saw him, that he would have another paroxysm of fever, so brief as to be scarcely perceptible. In some cases these remissions are of frequent occurrence, the condition of the patient varying every few hours. Under such circumstances, if you wish a perfect remission or intermission, you must bring it about by the judicious use of quinine, or it may never occur.

It was my wish, when I transcribed the above cases from my note book, to offer some comments on them, but I shall be compelled to be more brief in my remarks than I intended. It may seem strange to those who have been unaccustomed to use opium or its preparations during the height of febrile excitement, that they should be administered under such circumstances. But why should it? Have not opium and its preparations been freely and successfully administered in convulsions, to lessen the sensibility of the brain, and thus shield it against the effects of reflected irritation, as well as to allay that irritation. Though opium and its salts are inadmissible when there is a want of energy in the brain, and the excitability of the nervous system is exhausted, they are of great utility when the integrity of these organs is unimpaired in this respect, in allaying irritations and tranquilizing the system. Irritation is the proximate cause of fever, and becomes, in its course, its most conspicuous element, creating local accumulations of blood, and eventuating, when uncontrolled, in inflammation. Quinine exerts a peculiar anodyne and sedative influence on the nervous system when properly given, which is most conspicuous when administered in large doses. I have seen the senses of vision and audition temporarily suspended by its action. Its sedative effect is further manifest in the controling influence it exerts over the heart and arteries when given in the height of febrile commotion, speedily reducing the force and frequency of the pulse.* Its chief excellency seems to consist in its power of controling primary irritation, thus preventing local engorgements and inflammation when early administered in fever, by the influence it exerts on the function of innervation. If this view of its *modus agendi* be correct, we are prepared to understand why it is so important to keep the system decidedly under its influence, if the object be to subdue local engorgements or inflammation when established. If given early, and duly assisted by other suitable remedial agents in the forming stage of fever, before irritation has firmly fixed itself, and disease has established "a local habitation and a name," it promptly removes all difficulty and danger.

"*Ubi irritatio, ibi fluxus*," is a maxim in physic, and when the irrita tion of fever is permitted to fix its abode, and a focus of attraction is formed, accumulations of blood and inflammations follow. Quinine, by the power it exerts over the function of innervation, will not only avert these consequences, but fulfil an important indication in the removal of local engorgements and inflammation. From this it appears that quinine is to be considered as the chief remedy in the treatment of fever, and the lancet, the preparations of mercury, opium and its salts, purgatives, &c., as auxiliaries.

VI.—*Successful removal of a Stone weighing* 84 *Grains, from the Bladder of a Woman.* By Thomas C. Brown, M. D., of Woodville, Mississippi.

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

Gentlemen, I send you the following case which, if you deem worthy, you will give a place in your excellent Journal.

On the 16th December, 1845, I was called to visit a negro woman, about 45 years of age, belonging to Mr. Lessley, suffering with pain in the bladder, and (as she said) inability to urinate. The attending physician was absent. I introduced the catheter and drew off about two

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* It will relieve bone-ache, so common a precursor of fever. I have seen tooth-ache relieved by a 15 gr. dose in as many minutes. It exerts a powerful controling influence through the nervous system over the circulation, which is most conspicuous when it is given in large doses in the height of febrile excitement. It has been used in pleurisy, pneumonia, and other inflammatory diseases with success, and a very intelligent friend of mine, who is in the habit of trying it on all occasions, represents it to be a very good *remedy for biles.*

ounces of water, when I felt the bladder contract firmly on the catheter, making it grate against a rough stone, producing much pain and uneasiness, which was relieved by injecting two ounces of olive oil into the bladder. The owner being informed of her condition requested me to continue my attention, and to do what I could for her relief. I commenced the introduction of wax bougies for the purpose of expanding the urethra, which was continued up to the 18th of January, 1846.—The irritation and pain in the bladder was kept down by frequent injections of olive oil, and at this time the constitutional irritation was so great as to render an immediate operation necessary. On introducing a long slender pair of forceps, grasping the stone and bringing it up to the urethra, I found it not sufficiently dilated to permit its passage, I then introduced the little finger of the left hand into the urethra, passed a probe pointed bistoury on the finger, and divided the upper part of the canal on the right side, I then dilated the orifice with my right index finger, introduced the forceps, grasped the stone, brought it to the external orifice, grasped it with a short pair of forceps and removed it. I was assisted in the operation by Dr. Harris, a relative of the owner. The stone was rough and granular externally, and *weighed* 85 *grains*. Its greatest length $1\frac{3}{8}$ inch; its greatest breadth $\frac{7}{8}$ inch, its greatest thickness $\frac{1}{2}$ inch and a line. Thirty grains or more had been broken off in grasping it with forceps, the external surface of the stone being softened by the oil. Internally the stone had a slight rose colored tinge.

The wound healed readily, and the woman had no difficulty in retaining her water after a few days.

With respect,
THOMAS C. BROWN, M. D.,

Woodville, September 10th, 1846.

VII.—*Contributions to the Natural History of the Alligator, Crocodilus Mississippiensis*. By Bennet Dowler, M. D.

—— Ambiguous between sea and land,
The scaly Crocodile —— *Milton.*

The Fauna of even the most enlightened countries, seems to labor under the same evil which for ages retarded the progress of medicine, namely, an undue bias in favor of artificial classifications and nosological systems. The Crocodilian family affords a strong example of this arbitrary and illusory method of creating orders, genera, subgenera, species, and subspecies, in advance of exact physical data. The integumentary osseous plates, the feet, the claws, the toes, or the teeth, cannot be assumed as the classific criteria, until these shall be examined analytically and synthetically; an achievement which remains for the future, as the sequel will show.

That the Alligator is identical with the Crocodile, can scarcely admit of a doubt. Even those naturalists who have labored most to establish a difference, have admitted directly or indirectly, that there is none of a radical character. As this animal is, nevertheless, modified to some extent by climate, it may be advantageous to adopt names characteristic of the same, or at least, of the locality where this great Saurian is found—as the Nilotic Crocodile, (*crocodilus Niloticus*), the Gangetic, (*c. Gangeticus*), the Mississippi, (*c. Mississippiensis*), and so on. This topographical nomenclature will, for the present, leave the question of scientific classification open, as it ought to be, until vague and contradictory descriptions shall be replaced by exact observations.

The aborigines of America, called the Alligator *Cayman* , the Spaniards, *Lagarto* or lizard ; the English, by a corruption of the Spanish, *a Laga'to ;* and finally Alligator. The Nilotic Crocodile appears to have got its name from two Greek words signifying saffron and fear, literally, saffron-fearer. Hence the Egyptians placed saffron near their bee-hives to drive off the Crocodile. "The sovereign power of saffron," says Fuller, "is plainly proved by the antipathy of Crocodiles thereunto."

In this paper it is not intended to give the anatomy, physiology, and habits of the Alligator in systematic detail, but to point out some important facts in its history, freed from the trammels of artificial classification, and to correct certain errors, which, for several thousand years, have been accumulating, until the herpetological account of this saurian has, at length, become as fabulous as that of the Griffon itself.

Men who have but one idea—be that calomel, quinine, or venesection, and who, under the pretence of being practical, reject every other inquiry as "stale, flat and unprofitable," will, no doubt, think that crocodilian investigations are unworthy of their attention. It were easy to show that comparative anatomy, physiology, and pathology, afford an inexhaustible mine of useful knowledge, especially to the practical physician.

Dr. Good, an accomplished scholar and a voluminous medical writer, speaks of "Zoology as something on which we may perpetually dwell with new and glowing delight, and new and growing improvement ; a combination of allurements that draw us, and fix us, and facinate us with a sort of paramount and magical captivity."

The dying moments of Sir Humphrey Davy were devoted to reflections upon the *electrical fishes*, the electricity of which he supposed to be *sui generis.* Being unable, then, to test this experimentally, he enjoined upon his brother, Dr. John Davy, to perform experiments with that view, upon the torpedo, the gymnotus, and the silurus electricus. A correspondent of the Western Journal of Medicine for August, 1846, writes from London, that the distinguished Professor Grant is delivering a course of lectures on Zoology in that city, in which he speaks of the habits of the oyster, and the circulation of a lobster, with all the fire of a temperance lecturer.

In modern times, expeditions, at the national expense, have been sent to explore the natural history of different countries—one of the most remarkable of which, was that under Napoleon, in Egypt. That learned and colossal work, *Description de L'Egypte*, was written, as it were, amid the clang of arms. If that was a sublime thought to which Napoleon gave utterance just before the battle of the Pyramids, when, pointing to the summits of those mysterious monuments, he told the army that forty centuries were then looking down upon them :—"*Songez que du* *haut de ces monumens quarante siècles vous contemplent!*" how much more sublime was the spectacle within the walls of the Egyptian Institute, where the soldier laid down the sword for the pen, and after a hard fought battle, resumed the profoundest studies on the sciences, arts, and natural history of ancient and modern Egypt. It is remarkable that these mammoth folios, descriptive and pictorial, left an unexplored field in which Samuel George Morton, M. D., of Philadelphia, has gathered lasting honors—the field of Skulls—the crania Ægyptica, from which is deduced the ethnographic characters of the primitive races of that country, the caucasian and negro, with their varieties. The invading army under General Taylor, might now accomplish for Mexico,* what the French Expedition did for Egypt, and what the United States Exploring Expedition has done, at the expense of millions of treasure, for the icy continent of eternal sterility in the antarctic ocean, and what the authorities of New York have done, on a scale of surpassing magnificence, in the illustration of the geology, and Fauna of that opulent State. Honor to the citizens who authorised, and to the naturalists who executed, this last mentioned work! The other twenty-eight sisters may look upon it with envy, or rather with generous emulation.

The Fauna of Louisiana is still among the *desiderata*. Our birds have found an able historian in Mr. Audubon, whose work has shed an imperishable lustre on the nineteenth century. A foreign writer, in alluding to the imperceptible insects, exclaims—"there is not a single species that does not of itself deserve a historian!" Ehrenberg devoted ten years to the Infusoria alone. Is such a study dry? Is it not rather the Pierian spring—the true Helicon? If we admit with one author, that man is to be distinguished from brutes by his power of laughing or smiling,† has not the author of the Wandering Jew said, "there are so many kinds of smiles, who can discover the false from the real?"—if with another, that language is the most distinguishing trait—has not Talleyrand said that "language was given to man to *conceal* his thoughts?" Such are not the moral lessons derived from the *inferior*, or, as they are scornfully called, *irrational* animals. Their natural language is as sincere as it is true.

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* It is now nearly half a century, since the learned Humboldt cast a scientific glance over Mexico; and, although much of his account, statistical, social, political, and scientific, is now obsolete, it is, nevertheless, for general reference, the best that can be found. Messrs. Stephens and Norman, have explored some portions of the Mexican territories. As it regards *antiquities* and *ruins*, their discoveries have thrown their cotemporaries in the back ground. Palmyra and the Pyramids, are probably destined to become secondary objects, if we may judge from the presageful glimpses of these researches. Mr. Kendall's interesting sketches of another portion of that Republic, present views of the domestic and social condition of a population, in which little more than the *prestige* of civilization actually exists. The country has been immortalized historically, by the pen of Mr. Prescott. But where is its Flora, its Fauna, its mineralogy, its geology? Mexico, in those respects, needs an exploring expedition more than even Louisiana, for which, neither Royal nor Republican masters, have, as yet, done any thing worth mentioning.

† Milton seems to have adopted this theory:

—— "*Smiles from reason flow,*
To brutes denied."

Without entering on questions of orders, genera, and species, I will give, in a desultory manner, descriptions of the Alligator, as taken from five of these animals placed at my disposal, in the months of March and April; or rather, I will, for convenience, restrict myself to the two largest of these, as affording fairer results. They were from ten to eleven feet long, and from three to four feet in circumference, in the thickest part of the body. They corresponded with others, some larger and some smaller, which I have casually examined. The two I allude to, were examined several times daily, with much and prolonged attention. They were kept in cages or boxes, the bottoms and a portion of the sides of which were watertight, the residue being lattice work, or rather bars, which admitted wind, rain, and sun. The animals were sometimes kept partly immersed, and sometimes quite dry, during the periods of examination. During many of the observations on temperature of the gullet, aud on the digestion of food, the mouth was opened, and was retained so by strong levers, in order to facilitate the experiments, and to prevent the crushing of the arm, &c.

The *upper jaw* is wider than the under, which it overlaps. The latter has forty teeth, none of which are *grinders*, as asserted by Professor Owen—none are cutting or incisor teeth, as they are described to be by Goldsmith. The teeth of the upper jaw are similar in number and structure.

The Cuvierian classification is based on the teeth, which this author says, "are for the Alligator, thirteen on each side of the upper jaw. The fourth tooth, on each side of the under jaw, enters a hole in the upper."

Professor Edwards, of Paris, in his work on Zoology, (p. 367) characterises the Nilotic Crocodile by its dental organization, but in the very same page, gives these identical characteristics, by which to distinguish the Alligator. Both are recognised by the fourth tooth, one on each side of the lower jaw, as entering sockets in the upper; an excellent example of a distinction without a difference, not unlike Shakspeare's two lovers:

"Two *distincts*, division none."

Professor Owen, of London, is quoted in the British and Foreign Medical Review, for January, 1846, as maintaining, in his recent work on Odontography, that "the Crocodile has as many as four generations of *molar teeth*." Buffon's account of the teeth agrees with Cuvier's. Geoffroy St. Hilaire, naturalist to the Egyptian Expedition, enumerates 36 in the upper, and 30 in the lower jaw, all of which, according to his engraving, (pl. 2, croc. vulg.) are long and conical. Now, the facts are these: in both jaws there are 80 teeth, nearly half of these, that is 36 or 38, are short blunt teeth, rising but little above the gum, wholly different from grinders—never being worn—occupying the interspaces between the long conical teeth, which latter amount to 42 or 44, and are round, white, polished, tapering, salient, and project from the gum nearly an inch, usually exceeding a quarter of an inch in diameter. As the lower jaw is less expanded than the upper, its long teeth, 20 to 22 in number, are received, not only within the dental range of the upper jaw, but *fit into as many holes in the latter*. Instead, therefore, of two long teeth fitting into two sockets, there are never less than 20 long teeth fitting into as many sockets in the roof of the mouth—an arrangement which totally prevents the possibility of using grinders, did any really exist. Moreover the teeth of the two jaws are not *opposite each other*. Hence, grinders would be wholly useless. It is evident that these, as well as all the other naturalists whose works I have seen, are wrong in every essential particular relating to the dental apparatus.

Both sets of long, pointed teeth, penetrate plank and wood of all kinds, unless extremely hard. The crushing power of the jaws is *vertical*, not *lateral* or *grinding*. Both jaws present, along their dental or alveolar margins, an undulating or curving line, which, in the Nilotic Crocodile, seems more salient, if I may judge from the engravings of St. Hilaire, and a few others. The teeth correspond to this undulation, as does one jaw to the other. The general bearing of this line is several degrees above the horizon, commencing at the muzzle, and running backward to the posterior angle of the mouth. The form and situation of the dental organs, together with the osteological configuration of the jaws, render *grinding* operations quite impossible. The animals found in the stomachs of Alligators, examples of which will be given, show that their prey is killed by penetrating bayonet-like wounds, and are swallowed without mastication. The crushing and prehensory power of the jaws and teeth, is as remarkable as it is unquestionable.

To classify the crocodilian family by its dental organization, is altogether erroneous, so long as the shape, situation, arrangement and number of the teeth are not as yet ascertained. Scarcely any two authors agree in so simple a matter as the number of the teeth. Goldsmith says there are 27 in the upper and 15 in the lower jaw, and the authors already quoted, all give different aggregates.

As this animal has no *lips*, its teeth, especially in the upper jaw, are naked and salient, even when the mouth is shut, contributing much to its hideous physiognomy, and have probably prejudiced naturalists against its character.

Herodotus, Pliny, Aristotle, and many more modern *savans*, including certain French academicians, assert that the upper jaw moves independently of the head, though both are known to constitute a continuous mass of bone, without any flexible articulation. I have for hours forced the jaws asunder by levers, elevating the upper jaw, and with it the head. The cranium, and the superior maxillary bone, constitute a continuous pyramidal mass of osseous matter, the base of which is the skull, and the apex the muzzle.

Here a digression becomes necessary, the propriety of which can hardly be called in question, by any one who may do me the honor to read the same with attention. My crocodilian researches have led me to attribute most of the errors (so servilely copied for twenty-two centuries), to Herodotus, whom Cicero so justly calls the Father of History. From what this author has said concerning the ears of the Crocodile, I infer that he never saw one of these animals. His account is very brief, and may be found in *Euterpe*, a name which his second book received in a manner so flattering to himself, and so honorable to the discrimination of the Greeks, who, having heard his nine books read at the Olympic games, named them by acclamation after the nine Muses.

In the huge folios of Natural History, produced by the French expedition into Egypt, there is an elaborate history of the Crocodile, and which might be entitled, A Defence of the Errors of Herodotus; by Geoffroy St. Hilaire, naturalist to the expedition. It is doubtful whether any of the *savans* of the expedition saw or examined a Crocodile in Egypt. Certain it is that they have added nothing original to its natural history. St. Hilaire appears to have picked up all his information at the fisheries, from people more likely to deceive him than otherwise.

This able physiologist, lately numbered with the mighty dead, may have excelled his predecessors in certain branches of natural history, especially that portion so peculiarly his own, relating to *Monstrosity*, or the deviations of nature in the animal kingdom, which he has reduced, in a great degree, to order, regularity and harmony. With all his reverence for Herodotus, he sometimes differs from the old Greek, but never when the latter is wrong, and nearly always when he is right. Herodotus says, the Crocodile is truly amphibious; no, says St. Hilaire, not "*un véritable amphibie.*" And how does the French Herodotus prove this? Answer, ye who import facts, philosophy, and logic from Paris—the modern Athens! The Crocodile is not a true amphibium. Hence, says he, it is in a false position among animals! It is unsuited by nature either to live in the air or in the water! Hence, it is never satisfied, and is always restless; and this, says the great naturalist of the expedition, is the reason why the Crocodile is always ferocious, always cruel! And this is the argument of one of the principal *savans*, whose works, otherwise very learned and valuable, have on the title pages the following words: "*Publié par les ordres de sa Majesté L'Empereur Napoleon, Le Grand.*"

Herodotus satisfied St. Hilaire, and St. Hilaire has satisfied the later naturalists, who continue to copy the blunders of the former and the latter, occasionally adding some on their own account, as will be seen hereafter. These errors have increased, are increasing, and ought to be checked, or rather, consigned to oblivion.

Herodotus declared the Crocodile could move the *upper jaw* only. Pliny copied the statement. "The Crocodile only moveth the upper jaw or mandible, wherewith he biteth hard. (Holland's Pliny b. VIII.) St. Hilaire is much embarrassed with this statement, which he does not fully admit, and which he tries to explain in a very unsatisfactory way.

Herodotus denied a *tongue* to the Crocodile. Pliny says, "the river Nilus nourishes the Crocodile, a venomous creature, as dangerous upon water as upon land. This beast alone, of all that keep the land, hath no use of a tongue—*unum hoc animal terrestre linguæ usu caret.* (Lib. VIII). Scarcely dissenting from Herodotus, St. Hilaire says that the Crocodile *seems to have no tongue.* The Professor of Natural History to the Royal College of Henry IV, H. Milne Edwards, in his new work *Elémêns de la Zoologie*, says that the tongue is indistinct—"*peu distincte!*"

The tongue at its tip, including its outer third with its frenum is pale, thin, flabby, wrinkled and adherent underneath, along its whole width, appearing to have but little motion. It is truly tonguetied. The middle third becomes massive, and begins to assume a roseate hue. The base or inner third is enormously developed, being thick, wide and strong, filling the mouth, and being moveable upward and backward. When the mouth is forcibly opened, even to the greatest extent, the posterior portion of the tongue is thrown up against the roof of the mouth, just before the palatine arches, so as to act as a valve, completely closing the passage to the pharynx, presenting from one angle of the mouth to the other internally, an even horizontal line. This arrangement must completely exclude water and the like from entering the posterior fauces—a wise provision of nature, because, having no lips, the water must always enter the mouth, when the animal is in its favorite element. It is very seldom that this valve falls, even when the mouth is widely opened for a long period, as an hour or more. This pressure I have often overcome, with a slight force, when passing the thermometer and food into the posterior fauces and gullet. The upper surface or dorsum of the tongue is rough, from large papillary elevations, which are less developed at the tip, but larger or redder towards the base, where, also, the *salivary secretion* begins first to show itself, but the isthmus of the palate, and the posterior fauces only, are well supplied with that fluid. The *roof* of the mouth is white, dotted over with a few dark spots, rough, firm, almost leather-like, and almost dry, except near the velum or palate, where it is lubricated with mucosity.

Herodotus says, that *insects* (*βδελλα*, *hirudo*,) or, as translators have it, leeches, by getting into the Crocodile's mouth, suck its blood, and it dies exhausted. In good faith, he naively relates, that the Trochilus* is the only animal that lives in peace with the Crocodile, into whose mouth it is in the habit of going to pick out these insects—in consideration of this service, the grateful Crocodile never injures the *Trochilus*. St. Hilaire believed, nay proved this story, if we are to credit the Royal Professor of Natural History, in the College of Henry IV, at Paris. He says in his Zoology (1837), That the enemies which the Crocodile fears are feeble insects; but, singular thing! little birds go to deliver him from this plague, and entering his mouth without fear, destroy these insects. "*Ce fait, observé par Hérodote et ensuite traité de fable, a été confirmé de nos jours par M. Geoffroy Sainte Hilaire qui accompagna l'Empereur en Egypte. C'est une espèce de pluvier qui rend au Crocodile du Nil ce service intéréssé, et aux Antilles le todier a des habitudes analogues !*" (p. 367). A modern sailor, who, returning home, told his mother that in his travels he had seen flying fish, was reproached for telling a falsehood, whereupon he said, that one day in drawing up his anchor in the Red sea, he brought up one of the wheels of Pharaoh's chariot, a statement which his mother admitted without hesitation. *Verbum sat sapienti.*

It is a pity to spoil so good a story—one so honorable to the politeness of the feathered race, and so creditable to the reptilian character. An English Baronet, Sir G. Wilkinson, in his late superb work on Egypt, (London, 1843,) avers "*that leeches do not abound in the Nile!*"

That insects should kill Crocodiles may be true. Indeed Dr. Young, of New Orleans, informed me that a young Alligator, which he had kept

*In that magnificent work, "The New York Fauna, Mr. De Kay, gives *Trochilus* as the generic name of the humming bird. Mr. Anthon, in his antiquities says, "The *Trochilus* is the *motacella regulus*, or golden crested wren.

some time, was killed by ants, which, collecting on the top of the head, perforated the skin. It was supplied with water, which it refused to enter.

The soft *Palate* has not, as in the human subject, a central, pendant uvula, but an arching fissure, on each side of which, and nigher the angles of the mouth, is found a structure somewhat similar.

Upon the whole, the tongue is large, excepting only its tip, and, if the Egyptians worshipped the crocodile because it was *tongueless*, they were not wise. "The crocodile was *adored*," says Bishop Warburton, "because having no tongue, it was made in hieroglyphic writing the symbol of the Divinity."

Herodotus says, that the *Eye* of the crocodile is like that of the hog!—a most faulty comparison. The eyes of the Alligator are rather prominent than large, being situated but little below the summit level of the head. The *Pupil* is greatly elongated—scarcely less than an inch vertically, exceeding the horizontal diameter ten or fifteen times—an arrangement, which in connection with the salient position of the eye in a projecting socket, must give it an immense range of vision—probably from the zenith to 45 degrees below the horizon. Contrary to the assertion of Herodotus, it is highly probable that it sees well in water. When it wishes to protect its eyes, a dense, semi-transparent blueish, nictitating membrane is thrown over the globe. The Cornea comprises all that part of the eye, which is visible under ordinary circumstances. But when the neck is twisted, so as to give pain, a very small crescentic part of the white of the eye, (*tunica albuginea*), is sometimes seen. The eye, so far as I have observed, is constantly open, even during apparent sleep.

The *Iris* is dark, variegated with brown. The cornea occupying the whole front, is exceedingly transparent and lustrous. Excepting the imperfection of the eyelids and the absence of lashes, the eye is well worthy of the attention of the poets, as well as the Gazelle's;—indeed, Dr. Aikin has ventured to write poetry on the crocodile's "*burnished eyes.*" As this was all theory with him, and right only by accident, he deserves no credit for it. He had adopted, like many critics, an erroneous opinion, namely, that the *Leviathan*, described in the forty-first chapter of Job, is the crocodile of our era. "His eyes," says Job, are like the eye-lids of the morning."—No one has a right to dispute with Job on natural history. Nor is there any reason so to do. The scales of the animal, *he* describes, cover *all parts of the body*, "as with a close seal. One is so near another, that no air can come between them. They are joined one to another, they stick together that they cannot be sundered. The sword of him that layeth at him cannot hold: the spear, the dart, &c. He esteemeth iron as straw, and brass as rotten wood. The arrow cannot make him flee; slingstones are turned with him into stubble. Darts are counted as stubble; he laugheth at the shaking of a spear. Canst thou fill his skin with barbed irons?" [see what is said of the skin and osseous, integumentary plates in the sequel.] "He maketh the *deep* to boil like a pot; he maketh the *sea* like a pot of ointment"—a proof that he was not of the crocodilian family, which is only found in lakes, rivers, lagoons and swamps, not in the ocean.

The Edinburgh Encycl., the Encycl. Americana, Scheuchzer in his *Physica Sacra*, Dr. Young of England, and the author of the Bridgewater treatise on Animals, in asserting the identity of these animals, and, that the description of the one "is clearly" the description of the other, have committed a grave error, instead of serving the cause of the faithful, or elucidating the natural history of the book of Job.

Among the many fabulous accounts of this reptile, not the least defamatory and false is that concerning its want of *Sincerity*. It is said to be a hypocrite, and that its tears are false. Hear an old poet:

> "As cursed crocodile most cruelly can tole,
> With *truthless tears* unto his death, the silly pitying soul."

Fuller declares that "the crocodile's *Tears* are never true, unless forced by the influence of saffron." I have seen the detestable juice of tobacco tried, by a negro, who, spirted his saliva in its eyes, as correctly as Boz could wish, but without producing any tears; it only enraged the animal—an example worthy of the imitation of the great Primate, concerning whose spitting Mr. Dickens has written so well.—An alligator has no deceit. If he hates you, he will hiss you to your face.

This reptile's eye has not, even in the midst of fear, any of that wild, staring appearance, in which the white portion is displayed by both men and animals. Nor is there any of that fiery, injected, rolling appearance expressive of anger and ferocity, which poets and painters dwell upon so frequently.

The *Ear*, if we follow Herodotus, must be a very prominent organ. An Alligator sent to Louis XIV, was examined by the French *savans*, who were quite astounded at not finding the Herodotian ears, though it is doubtful whether they believed their own senses in opposition to so great an authority!—The ear is found with difficulty by the uninitiated. It begins near the posterior angle of the eye, just above the *Suborbital Fissure*—which latter commences anterior to the globe, running below it, in a horizontal, longitudinal direction, diverging backward, corresponding to the expansion of the head, being nearly an inch in depth and in width. The ear is a valve-like slit, fitting so accurately, that probably no water can enter. The animal seems either to have no power, or no inclination to open this valve. It must be opened with a thin edged lever as an oyster. It is about two inches long, the same in depth, presenting a reddish white, flat surface, inclined to the horizon 25° to 35°, corresponding to the plane of the cranium. There is no external auricle or cavity—no external pavilion—no apparatus suited for the reception of pendant ornaments, with which fable has loaded this organ.*—Both the ears and eyes are from three to four inches apart.

The *Nostrils* are very small—less than half an inch in their greatest diameter—oblong, opening about one and a half inches from each other, and the same distance from the tip of the snout, are used in breathing equally with the mouth.

The *Neck* is less than a foot in length, being much larger at its cranial than at its dorsal end. Near the mouth there is an integumentary *Pouch-like* enlargement, which might, perhaps, be called a cheek. But,

* The Rev. Dr. Russel, in his book on Egypt, (Fam. Lib. v. 23d, 1836,) says, that the crocodile's ears are *broad!*

as it is opposite the pharynx, it may be a kind of crop—or reservoir, in which food is temporarily deposited when the animal does not wish to swallow. It will be seen, that I have placed large masses of meat and bone in this region, quite behind the palate, where they remained until withdrawn.

Judging from external appearances, the neck has five *Vertebræ* or articulations. The head, neck and trunk do not equal the caudal extremity in length; the aggregate number of vertebræ in the former may be estimated at 25 or 30—that of the latter still higher. The first ten or twelve caudal joints are nearly level for two inches in width, over their superior surface, along the outer margin of which, that is on each side, a serried, acute row of bony processes arise about two inches high. As these recede from the trunk, they converge until they fuse into one perpendicular, central, crested ridge, while at the same time the sides of the tail become more and more compressed, until the whole ends in a thin edge, having a vertical diameter of several inches.

The *Leg* seems wholly disproportioned to the size of the body, and is really very feeble, being easily held, so that the animal cannot withdraw it from the grasp. The foreleg is from twelve to eighteen inches long, and is not as thick as a boy's arm at ten years old. The hind legs are a little larger. There seems to be no great credulity implied in the belief of the story often repeated and pictured, even in scientific works, by which a man is represented in South America as mounting on the back of an alligator and using the two forelegs, which he drew over the back, as a bridle. But as to guiding the animal with this sort of bridle, every one must think for himself.

The *Hands*, feet, or paws bear some resemblance to those of man and of some birds. Tbe forefeet have five fingers, of which the three first or inner, have long bird like claws; the two outer, none. The hind *feet* have four fingers, the three first or inner of which have strong, curved, tapering claws. There is a slight webbing between the second and third, and a full one between the third and fourth fingers of the fore feet, counting from within. The outer or little finger of the hind legs, joins the next or ring finger, with a web for half its length or one inch. Now whoever will take the trouble to consult authors, will find nothing but confusion and contradiction on this simple matter—even by those who base their classifications on the feet.

In the London Encyclopædia of 1845, there is an incorrect engraving of the Alligator, representing *all the toes completely webbed*. In the new Parisian editions of the works on Natural History, by Lacépède, and by Prof. Edwards, not a vestige of webbing is seen among the toes at all! The Encyclopædia Americana says, *all* the fingers or toes have *claws!* "Their feet," says Cuvier, "are only semipalmate," None of these accounts are correct.—The *Skin* has numerous, longitudinal, transverse seams, dividing the integument into square figures. Notwithstanding these seams or fissures, which render the skin uneven, it is rather smooth, polished and not very hard, except where the bony plates are found, that is upon the upper part of the trunk.

Omitting the microscopic history of the *Cuticle* for the present, I come to the osseous, integumentary *Plates*—a most prominent feature, and one that has been resorted to for criteria, in giving the distinctive features of this animal. In an elaborate treatise on Herpetology, in Brewster's Encyclopædia, these plates serve as the basis of scientific classification: "Crocodilus Mississippiensis: Muzzle broad and flattened: *four* carinated scales disposod in a square upon the neck." This is the entire description, and is again repeated, and applied to the Alligator.

I will here give an account of one of these plates, taken from the middle of the body. It is of a medium size, much smaller and more regular than those of the neck, though larger than some others which belong to the inferior rows along the sides. It was taken from a skin which had been dried for many years. A maceration of six weeks caused all the soft tissues to separate. The plate proved white and heavy; smooth internally, though apparently marked with the cutaneous fibres—perforated with a few minute holes admitting a bristle only; the general configuration (seen from within), square; each border about two inches long; having rough, suture like margins, from one to three lines thick. The exterior surface is so curiously figured, and withal so irregular as to defy description. The plate rises gradually on three sides; abruptly on the fourth, forming a carinated ridge, being in the central portion nearly one inch high, having about 75 blind holes or depressions sinking from one to three lines into the bone, some of which are large enough to receive half of a small pea; others look like shallow sockets from which teeth have been extracted. On the summit of the carinated ridge are innumerahle small holes, not half as large as a hair.—This plate was dried in the sun, and after fourteen months, it weighed 686 grains (Apoth. weight). Its specific gravity was ascertained, most carefully, by Mr. Curtius,* (apothecary,) and myself, and was found to be 2.057.

The skin from which this plate was taken, presented ten larger, with some smaller ones on the neck; the first row on the trunk, nearest the spine, amounted to 16; the second to 15; the third to 11; the fourth to 6. In another alligator, about 11 feet long, I counted on the right side: first row, 17; second 17; third 8; left side, first row 17; second and third rows, irregular in arrangement, situation, size and number. In another, upon which I experimented, the first row gave 15; the second 15; the third 18; the fourth 8. M. Geoffroy St. Hilaire's plate, so far as I can determine, gives for one side: first row, 12; second, third, fourth and fifth, each 16; the sixth, 6; with five larger and some smaller on each side of the neck. (Pl. 2. *Croc. vulg. Hist. Nat.*) Are not the number of plates in some degree proportioned to the age of the animal, as the number of rattles are in the rattle snake (*crotolus horridus*)?

The skin of the alligator, denuded of its cuticle, is white, and, excepting that portion protected by plates, is so penetrable, that I have found no difficulty in dividing it in the living state, and, that too, with the common lancet, the edge of which was not injured or blunted by the opera-

* This young gentleman, (whose knowledge is not restricted to the compounding of drugs, but embraces a large portion of the physical sciences,) adopted a very simple and satisfactory method of expelling the air from this most porous of bones, in order to give its specific gravity with accuracy.

tion. Here, at the least, the maxim, that what every body says, must be true, fails completely. In Brewster's Encyclopædia it is said, that "this animal's hide is generally impenetrable to a leaden musket ball. It is, however, more vulnerable in the belly, and a bullet discharged down the throat or in the eyes, is fatal. The negroes in the river Senegal attack this huge animal either when asleep, or in shallows where its swimming is impeded, and by forcing an oxhide in its mouth, the water flows in, while heavy blows are given on the head to stun it, and it is drowned."

"In Louisiana, the natives contrive to thrust a piece of wood, pointed at both ends, into the throat, or when rushing upon the assailants, its wide mouth is met by a large stake, which is forcibly thrust down, and it is speedily destroyed. Several leaden bullets, even when they penetrate, are sometimes insufficient to kill, unless when they reach the brain, the spine, or some of the large blood vessels. Iron balls are recommended."

St. Hilaire maintains that lead will not kill! So does the Encyclopœdia Americana, in which, it is said, that "the body, above and below the *entire length*, is covered with plates"—"impenetrable to a musket ball." Goldsmith's Natural History, (a text book in our schools), is, if possible, still more extravagant in its details: "A negro goes into the water armed with a knife, having his arm bound with a cow's hide; the animal swallows the arm most greedily; a hole is cut in the throat; the water rushes in and drowns it, bloating it as big as a tun." Examples, of a similar kind, might be multiplied, were it necessary. So much for theory, which here, as in many other cases, is stronger than the testimony of the five senses!

Now I will engage to kill an Alligator, not merely with the "bare bodkin" of which Hamlet speaks, but with a common lancet. The animal is easily killed. Dr. Lindsay, formerly a resident in the country, recently informed me, that some years since, while he and his two companions were pursuing a wounded deer, ten miles from Baton Rouge, in Louisiana, the party came on a den or army of Alligators, all of which not only appeared indifferent to their approach, but incapable of being frightened. The gentlemen dismounted, secured their horses, and divided their ammunition, which, though abundant in powder, was restricted in lead to 450 buckshot. It was determined to use only three of the latter for a charge. Each man had, therefore, fifty rounds. Each standing quite near the animals, began the work of destruction; each shot proved fatal. The animals died quickly, in from two to five minutes. They jerked, trembled, turned on one side, and held up one of their quivering hands and died. When the last shot was fired, the survivers lay quietly, unterrified, unconcerned. This spot was often visited afterwards, but no Alligators were ever seen there for years. Dr. L. estimates the number of these animals which he has killed, at different times, at five hundred! Mr. Audubon, the ornithologist, says—that a Louisiana negro will kill a dozen of large Alligators in one evening, cutting off the head with a single blow of the axe, for the purpose of obtaining the oil. (Buffon's Nat. Hist. Am. Edit.) On some occasions he holds a language somewhat dissimilar. He says, "When Alligators are about to go into winter quarters, a child may mount them as a wooden rocking horse." Several gentlemen have informed me that in digging canals, and in making roads, they have opened the burrows of Alligators, and have been obliged to remove them out of their way: on one occasion, an Alligator had been wounded by a man who was not aware of it at the moment—the animal ran some distance with the man, who had accidentally fallen astride on its back.

I am credibly informed, that when hunters camp out in the forest, the Alligators of the neighboring waters watch for the offal of the camp, during which they are often easily noosed with the lasso, and are then dragged from the water by horses.

The following case, which may be fully relied on, shows that Alligators do not bear herculian doses of physic: Mr. I., an educated gentleman, engaged in the study of medicine, living near Fort Pike, in Louisiana, having observed, in 1845, a recent "Alligator's wallow," and having at the same time killed a snake, he opened its abdomen, into which he inserted about three grains of strychnine, carefully enveloped in several folds of letter paper, which, being properly secured, the snake was left for the Alligator, which, the next day, was found dead, with its abdomen turned up. The snake had disappeared. The Alligator had been poisoned.

Alligators commit errors of diet. The following is a fatal instance: A gentleman of the State of Mississippi informed me, that having been, with others, on a hunting excursion, one of the party finding that the whiskey bottle, which he had been carrying, was now empty, he threw it to an Alligator which was swimming near, in a lagoon. The animal suddenly seized and crushed it. On returning to the same place in a few days after, the animal was found dead, with its abdomen greatly distended and turned upward. A physician being present, it was determined to make a post mortem examination. Broken fragments of the bottle, with putrid fish, were found in the stomach and bowels. These organs were, in many places, quite mortified, and emitted a fœtor, so horrible, that my informant was nauseated, and which, in his opinion, caused the doctor's sickness and death—occurrences which took place soon after the post mortem examination.

I have examined several wounds which Alligators had received during the conflict in which they were captured. The following is a good example of Crocodilian *hyperæmia* or inflammation: A torn and contused wound, of two or three inches in length, between the fingers, was tumefied, but without *redness*. Granulations appeared, coated over with a dense transparent exudation, not flakey, but resembling half coagulated albumen. On touching these, the animal expressed great pain, withdrawing its limb and blowing loudly. Another foot which had been bruised and swollen, without any breach of the skin, presented extensive exfoliations of the cuticle, leaving the true skin *white*. Some recent bruises on the muzzle and in the mouth, together with an incision which I made in the back with the lancet, discharged a little thin, pale, scarlet colored blood. The general hue incidental to inflammation in man, did not occur. It was *white*—analagous types of which do sometimes happen in ordinary practice, as in white swelling, phlegmasia dolens, and in some fatal cases of glottidian and laryngeal hyperæmia, in which the submucous tissue is white, though swelled and infiltrated with lymphy, serous, and purulent matter. I have found the epiglottis a mere sack, containing pus, though *blanched*. Hence, the necessity of changing the technology of pathological anatomy. Inflammation is, to some extent, a theoretical word, implying redness and so forth, which may not be essential to its physical history, an evil which may be greatly lessened by using words designating *physical changes only*, as cohesion, softening, brittleness, induration, size, figure, vascularity, injection, collapse, infiltration, and the like. In medicine, words, [prescriptions], are things, which blacken the body with leeches, blanch it with venesections, or modify its organization with the concentrated preparations of medical chemistry.

The Crocodilian *Respiration* is very irregular; I might say, sometimes altogether suspended for indefinite, or at least, very long periods, when the animal is not disturbed. The method I have adopted to prove this, is as perfect as could be desired. For several days, two large Alligators were so placed in their cages, that the water covered the mouth and nostrils completely. They lay perfectly still. There was no movement of the walls of the trunk. The least movement must have agitated the water. Every steamboat or dray that came near, caused slight undulations or waves—the Alligators none, when left unmolested, which, however, seldom happened, as persons frequently came near. On several occasions no interruptions occurred for half an hour, or even an hour. When they are annoyed, and wish to scold or frighten their enemies, they make deep inspirations, inflating their bodies very largely—this air they discharge in low bass notes, or rather with a bellows hissing sound, several times in a minute. There can scarcely be a doubt that one inspiration supplies a stock of air for hours, if not for days. Herodotus was right in considering this animal as a true amphibium, and, of course, St. Hilaire is wrong in denying it, as is Mr. Kirby, in his Bridgewater treatise on Animals, wherein, he asserts, that "the Crocodile cannot remain more than ten minutes under water," (p. 418). Some illustrative facts might be produced. "Tortoises have lived more than a month with their jaws closely tied, and their nostrils stopped with wax." (Ed. Ency.) "The hedge-hog, according to Professor Mongili, respires from five to seven times in a minute; but in a room, at 54°, it becomes torpid—respiration is then periodical, being suspended for fifteen minutes at a time, and this, too, in April and May, after it had naturally revived from its winter lethargy." The same author noticed in the dormouse intervals of suspended respiration for sixteen minutes. By maltreating the Alligator, its inspirations and expirations may be produced at pleasure, but contrary to the chemical doctrine of pulmonary combustion, animal heat is not thereby augmented, as will be shown in the experiments on the temperature of this saurian.

The *Circulation* in this animal, after all my attempts to investigate it, appeared to me, at least, a perfect enigma. On several occasions I explored different regions, wherein I expected to find arterial pulsations, but without much success. This seemed the more surprising, as the axillæ, flanks and limbs were sufficiently soft and flexible, to induce the belief that the pulse might readily be detected. The muscles of the limbs are small, cord-like, and pliable. Either from policy or politeness, the animals allowed the fullest examinations without resistance.

I will give the details of one experiment: April 3d; noon; air, 68°; the axillæ and groins, each 65°: a search for the pulse began, and continued for three hours without intermission or disturbance. The whole attention was directed to this one object. In the first half hour I felt three strokes like those of an artery, in the part corresponding to the wrist. Similar pulsations were noticed in the hind leg, near the foot, amounting, in all, to fifteen in three hours—none were felt in other regions. When a stroke occurred, two or three followed in as many minutes or less. The animals were now irritated. The limb was held in my hand. They puffed and raged, but no increased arterial action was perceived. Is their circulation voluntary, paroxysmal, suspensible? Does the blood flow equably, without arterial impulsion, as in the veins and capillaries? Is not the quantity of red blood, very small in this animal? A wound which I made with the lancet, was barely moistened with blood. Doctor, now Professor Le Conte, of Georgia, in decapitating an Alligator, on which he made some interesting experiments, recently, noticed that "not more than *two ounces* of blood flowed from the wound. (Vide N. York Jour. Med. Nov. 1845).

With respect to certain hybernating animals, "Spallanzani and others are of opinion that the circulation of the blood is entirely stopped in the remote branches of the arteries and veins, and only proceeds in the trunks of the larger vessels and near the heart."

The *Digestive* function of the Alligator, seems but remotely illustrated by that of man. Herodotus asserts, (with great probability), that during the four winter months, Crocodiles eat nothing—a postulate which St. Hilaire, with all his bias for that historian, denies. From some of the officers, crews, and passengers of a Bremen ship, I learned that two large, healthy, Louisiana Alligators, for two months before going to sea, for two months during the voyage, and for two weeks after its termination, ate nothing whatever. In another case, an Alligator was known to have eaten nothing for forty-nine days. A physician who kept a young Alligator for a considerable period, never could discover that it ate, though food was put in its mouth. Dr. Davy's experiments on poisonous snakes at Ceylon, show that these animals become more and more active without eating. The tic-pologna (*vipera elegans*) 4½ feet long, which he kept 146 days, took no food. Its poisonous bite killed some animals instantly—others in a few seconds. Spallanzani kept frogs, salamanders and snakes, in a torpid state in an ice house, for three years and a half, and then readily revived them by the atmospheric warmth. Of course they fasted all this time. A turtle which Dr. Davy killed at Malta for experiment, had lived two months without eating, and without any impairment of its activity.

I have several times put into the mouths, posterior fauces and stomachs of Alligators, flesh, bones, and the like, with cords attached; in nearly all cases, I have found these, on the following day, just where I left them, and without any other alteration than that incidental to maceration. In but a single instance did I find that the animal had swallowed any thing left in the mouth or pharynx; though it is difficult to believe that these substances could remain a day or more without causing irritation or strangulation. I tied a cord to a portion of the spine of a hog, with considerable flesh adhering: This was put by force into the mouth. On returning, in half an hour, I found that the mass had been swallowed. The rope was cut within a foot of the mouth. The next day the rope was pulled—it slipped over the bone, stripping off some of the meat, which, together with the knot of the rope, was coated with a transparent, tasteless, scentless mucosity, without acidity on being tested by litmus. I took a strip of the skin, fat and flesh of a hog, about two feet long, which, being secured in like manner, was put in the mouth ; on returning three hours after, I found that the mass was just where I had left it. I then forced it down the throat, leaving the string out as before. Twenty hours after, the whole was drawn up unchanged, except a little blanching, and a coating of mucous matter, as in the last case ; though this mucosity slightly reddened litmus.

"Mr. John Hunter conveyed pieces of worms and meat down the throats of lizards, when they were going into their winter quarters, and keeping them afterwards in a cool place—on opening them, at different periods, he always found the substances he had introduced entire, and without any alteration ; sometimes they were in the stomach, at other times they had passed into the intestines, and some of the lizards which were allowed to live, voided them towards the spring entire," A torpedo was kept by Dr. Davy many days ; when it died, a fish was found in its stomach much in the same state in which it was swallowed ; no part of it had been dissolved : (Researches, v. 1, p. 37).

A curious fact is mentioned by Mr. Audubon, and is directly in point, though shocking to the true disciples of Isaac Walton, namely—that the ornithologist was in the habit of killing Louisiana Alligators, for the purpose of getting fresh fish out of their stomachs. He says, "in those I have killed, and I have killed a great many, when opened to see the contents of the stomach, *or take fresh fish out of them*, I have regularly found round masses of hard substance like petrified wood. These masses appeared to be useful in the process of digestion, like those found in the maws of some species of birds. I have broken some of them with a hammer, and found them brittle and as hard as stones, which they outwardly resemble. And as neither our lakes, nor rivers, in the portion of the country I have found them in, afford even a pebble as large as a common egg, I have not been able to conceive how they are procured by animals if positively they are stones, or by what power wood can become stone in their stomachs." May not these masses be indurated clay ? Are not Alligators, to a certain extent, dirt-eaters ? Dr. Lindsay informs me that he has had many opportunities of knowing that these animals defecate large indurated masses, having all the physical properties of the mud banks in which they make burrows or dens.

The *Diet* of Alligators appears to consist chiefly of fish. That they should swallow tigers, oxen, mules and horses, is altogether ridiculous and impossible. How a man of Mr. Audubon's accuracy, could directly or indirectly give countenance to statements of this kind, appears quite incomprehensible. According to him, "the drovers of Louisiana, when driving horses, cattle and mules, go first into the water and drive off the Alligators, which would otherwise attack the cattle, of *which they are very fond*. They will swim quickly after a *horse* :" (Buffon's Nat. Hist. Am. Edit.)

Any one who will examine the unyielding osseous boundaries of the palato-pharyngeal, and maxillary regions will find, that their diameters absolutely forbid the idea of this animal swallowing a horse, an ox, or a tiger, any more than a 74 gun ship; though a small calf, a dog, or a pig, is quite a different matter.

The engineer of the Water Works, of New Orleans, formerly resident in the country, was one day, during a visit to a neighboring planter, called upon to witness the manner in which an alligator catches its prey. The planter had just seen it catch one pig; and for his friends amusement, determined to let it catch another. The pig, which was small, continued to root about the alligator's head, until it came nigh enough to be caught. After many efforts, the alligator swallowed the pig. The planter shot the alligator immediately, and directed his negroes to open it, with large knives. The two pigs were found quite dead—not masticated, but marked by penetrating wounds, from the teeth—an eel, quite natural in appearance, and about a peck of cotton seed were also found in the stomach.

Many authors assert, that alligators cannot *swallow* under water. In offering some facts to disprove this assumption, the sagacity of these animals will be more or less illustrated. A gentleman, on two occasions, watched alligators when catching sunfish, which were swimming in shoals, in shallow water. The alligator placed his long body at a suitable distance from the shore. As soon as the fish came between him and the land, he curved his body, so that they could not pass; the tail was moored on land; the mouth was opened under water, and brought so close to the shore, that the fish had no method of escaping, but through the mouth, where they were entrapped. *Incidit in Scyllam, qui vult vitare Charybdim.*

Dr. Lindsay has often observed, in midsummer, when the inundation is subsiding, and swamps, lakes, lagoons, and bayous, are becoming dry or too shoal, for not only alligators, but the fish, that a general migration commences. When thousands of square miles, submerged for several months of the year, are about to become desiccated, these knowing animals begin to travel. When the water subsides rapidly, there are currents through narrow channels, from the higher to the lower basins and streams, to which the alligators repair, in great numbers, and turn their heads up stream. The large buffalo, and still larger cat-fish,* with many other fishes of the lower Mississippi, in their migrations,† through these straits, are thus devoured; often, very few escape.

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* Family *siluridæ; genus pimelodus.* Some of these silurians, attain a colossal size, in Louisiana.

† "The action of beasts," says a French writer, (M. Bayle,) "are among the profoundest mysteries upon which human reason can dwell; and, that so few people should perceive this, is to me a matter of surprize." Yet, like the great Primate, they sometimes commit mistakes; even in late years, alligators have come into New Orleans—an *error loci*—fatal to the wanderer, as he never returns again to his reedy, splashy den. Poets and philosophers tell us, that man is governed by *reason*, but when they speak of brutes, they ascribe all their actions to *instinct*. I will not stop to examine what is gained by using that convenient word; but will simply say, that *inspiration* or *Divine afflation* would seem much better to accord with their descriptions and explanations as usually

All these enormous fish are *swallowed under water*—the power of doing which, Cuvier expressly denies them. He says, "that they first drown their prey; then they carry it to some submerged crevice, to putrify before they eat it." Hence, the Cuvierians hold, that alligators live on rotten flesh. The truth is, these animals live mostly on fresh fish—a fact asserted by Sir G. Wilkinson, with respect to the Nilotic crocodile.—(*V.* 2*d* p. 124, *London*, 1843.)

The learned and the unlearned, seemed never tired of telling about crocodilian *ferocity*—Cuvier, among the rest. Professor Edwards, in his new work on Zoology, says "this animal is very ferocious and dangerous, even to man." So says the new London Encyclopædia, which gives a very dramatic story about an alligator, that invaded a South American city, and in the presence of the governor, carried off, in his capacious jaws, a living man! Mrs. Trollope's story, which follows, has become classical, and is quoted as authority. The scene is laid in Louisiana, the hero is a squatter. The poet is a lady: "towards daybreak, the husband and father was awakened by a faint cry, and looking up, beheld relics of three of his children scattered over the floor, and an enormous crocodile, with several young ones around her, occupied in devouring the remnants of their horrid meal. He looked around for a weapon, but finding none, and aware that he could do nothing, he raised himself gently on his bed, and contrived to crawl from thence through a window, hoping that his wife, whom he left sleeping, might with the remaining children, rest undiscovered till his return. He flew to the nearest neighbor, and besought his aid; in less than half an hour, two men returned with him, all three armed; but, alas! they were too late! the wife and her two babes lay mangled on their bloody bed." (Six killed.) Captain Alexander, a voluminous writer of travels, who visited Louisiana, in 1831, says, the people "are obliged to keep a sharp look out lest their children should be snapped up by alligators." In Lacépède's Natural History, just from the French press, an engraving is given, representing an alligator as swallowing a negro! This work, quotes M. de la Coudrenière's account of the Louisiana crocodile, (*Journal de Physique*, 1782), in which he sets forth, that this animal *feeds on men, particularly negroes*—"*particulièrement les nègres*"—and that it roars as loud as a bull! Other writers say, that this animal prefers negroes to all other kinds of diet. If this be true, the fondness is mutual. A gentleman of New Orleans, once a planter, assures me, that his slaves were in the habit of eating alligators, which, invariably made them sick. All his authority was insufficient to prevent this practice. The sickness was so frequent and so peculiar, that he could readily recognize it without difficulty. He gave emetics for its cure. The suspected substance was always brought up; though the negroes always denied having eaten the

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given—none of which is, perhaps better, poetically or philosophically speaking, than that of Pope, on the migration of birds:

"Who bade the stork, Columbus-like explore
Heavens not his own, and worlds unknown before?
Who calls the council, states the certain day,
Who forms the phalanx, and who points the way?

For practical purposes, the syllogisms of Aristotle are often not so satisfactory as instinct, nor so logical, nor so certain.

same. This fondness extends to dogs, which are often fed with the tail of this animal, which is the choicest part. A physician, who once tasted this animal's flesh, informed me that its flavour, in some degree, resembled that of fish, though unpalatable.

Goldsmith says, that the crocodile "*unpeoples* countries, making navigable rivers *desert* and *dangerous*." Let that suffice for the moderns. Rollin and others spend much learning to show, that the Egyptians worshipped this animal, because it defended their nation from all their enemies, particularly the Arabs. Let this suffice for the ancients.* Truth requires me to say, that there are several examples on record, showing that alligators have bitten persons, while the latter were wading or swimming in the water. Mungo Park, in his second expedition to explore the Niger, says, "that his guide, in crossing a river, was seized by the thigh by an alligator, and dragged under the water. The man put his fingers into the eyes of the animal, which caused him to let go his hold; but soon afterwards he seized him by the other thigh, and the guide took the same method to save himself, and succeeded, having suffered two bad wounds." (v. 2, p. 130.) In 1835, the Jacksonville Courier, (newspaper) in Florida, details the case of a young man named Norton, who was bitten by an alligator in the hand and arm, one of the bones of which was broken. The man *gouged*† the animal, causing it to let go its hold. It was killed, and measured ten feet in length.

Admitting these statements as altogether true, it may be truly said, that there is scarcely an animal, wild or domestic, which has committed so few injuries upon man—a position worth illustrating, as even twenty-two centuries cannot make a falsehood, true. Besides, it is right to give the alligator, as well as the devil, his due.

As illustrative of the *pacific* and friendly character of the alligator, I make the following quotation from the Missionaries' letters, reviewed in the London Quarterly, volume the eleventh. At the Nicobar islands, in the Bay of Bengal, these animals are numerous. "Mr. Hænsel was walking along the coast of Queba, looking on a number of children who were sporting in the water, when he saw a large crocodile proceeding towards them, from a creek. He screamed, and made signs to some Chinamen, to go to their assistance. The Chinamen laughed at his

* It seems, that both ancient and modern historians have had a *carte blanche*, to say what they pleased about crocodiles and the Egyptians. Rollin and others say, that the latter worshipped the ichneumon, because it killed the crocodile: "it leaped into his mouth, ran down into his entrails, cut out a passage, and returned victorious over so terrible an enemy!" and yet, the same authors pretend, that the crocodile was worshipped because it defended the Egyptian nation. If the ancients had some faults, they were not wholly destitute of common sense. Why should they worship the ichneumon, because it killed the defender of their country?

† "Gouge.—A joiner's tool. The word is used by the North Americans, who in their savage quarrels, not unfrequently *gouge* out eyes."—*Richardson's Dictionary*. Now this definition is, no doubt, intended to have a damaging effect on Americans; it is, besides, an Americanism; and a writer in the National Intelligencer says, that *American* writers, dread nothing so much as an Americanism. This word is not only necessary, but excellent, though the practice it designates, is bad, except in the case of crocodilian fights.

fears, and presently he saw the crocodile playing among the children, while they diverted themselves by pretending to drive him away."

Sir G. Wilkinson, in his late work on Egypt, says, "the Nilotic crocodile is, in fact, a timid animal, flying on the approach of man; and generally speaking, only venturing to attack its prey on a sudden."—(V. 2, p. 124.)

Audubon, while traversing Louisiana, in pursuit of birds, became much acquainted with the habits of the alligator. He says, that these animals were so numerous on Red River, before the introduction of steamboats, that hundreds might be seen at once—the smaller riding on the backs of the larger, groaning and bellowing like thousands of mad bulls, about to meet in fight; all *so careless of man*, unless *shot at or positively disturbed*, that they remained motionless, suffering boats or canoes to pass within a few yards of them, without noticing them in the least. Thousands of the largest were killed, while the mania of having shoes, boots, or saddle seats made of their hides lasted. Many of the squatters and strolling Indians followed, for a time, no other business. The discovery that their skins are not sufficiently firm and close grained, to prevent water or dampness long, put a stop to their general destruction.

When alligators, (continues this gentleman,) are distant from water, and perceive an enemy, they drop and lie flat, with the nose on the ground, watching the intruder's movements. Should a man then approach them, they do not attempt to make away or attack, but merely raise their body for an instant, swelling themselves, and issuing a dull blowing, like a blacksmith's bellows. *Not the least danger need be apprehended.* You kill them with ease, or leave them. The chief means of his attack or defence is his large tail. Woe to him who goes within its reach. Mr. Audubon often waded through lakes and lagoons, with no defence but a stick, to drive the alligators out of his way. If, says he, you go towards the *head* of an alligator, there is *no danger*; and you may safely strike it with a club four feet long, until you drive it away, merely watching the tail, which, at each blow, throws to the right and left most furiously. In company with a friend, he killed an alligator of extraordinary size, which appeared to be centuries old, many of the teeth of which measured three inches, and which served for powder chargers. The body was seventeen feet long.

I have been credibly informed, by actual observers, that on meeting an alligator in narrow paths, among canebrakes, briars, and other thickets, there is no method by which the animal can be induced to retreat or turn aside. The traveller must either kill him or turn back himself.

Dr. Lindsay, of this city, related to me the following occurrence: A vast number of alligators being congregated in a confined situation, in the water, a man in a canoe attempted, for his own amusement, to hem them in, so as to force them to go on land, which they were determined not to do. In the melée which ensued, a large alligator got so elevated from the water, by placing his arms on the side of the vessel, that he fell into the same, at full length, to the great dismay of the boatman, who being at the stern of the canoe, and finding the animal's head towards him, and fearing to jump out in the water, among the many alligators all around, concluded to paddle his way to the shore. But as soon as he dipped his paddle in the water, he found that the animal began to move towards him. The canoe was narrow; there was no room for the animal to pass him. The parties were mutually embarrassed. The man hoped that his reluctant passenger would not attempt to kill him, unless he should obstruct the way. A happy thought struck him. He mounted with one foot on each side of the canoe; whereupon the alligator crawled between his legs, until from the stern of the boat, he plunged into the water, to the great joy of the man. Had the man disputed the right of way, a crocodilian battle would have resulted.

The brevity at which I have constantly aimed in this monograph, excludes many facts which I have either observed, or have derived from persons of good character, of accurate information, who have no theories but truth in view, in making statements concerning this animal and its habits. It certainly has great sagacity in distinguishing a friend from a foe. In many of my experiments, it showed not the least excitement or anger; while a negro, who had spit tobacco saliva in its eyes, could not approach without raising a storm of indignation, manifested by the animal rising on its legs, puffing and blowing like a bellows.

The absurd story, that alligators eat their own young, cannot be believed for a moment. A gentleman informed me, that one of his negroes having caught a young alligator, which whined like a young puppy, the parent came towards the negro with a rapidity he had never witnessed on other occasions—a kind of jumping motion, which caused the boy to run, after dropping his captive. I have been assured, when danger is imminent, that very young alligators run into the parent's mouth for safety. I have this statement from a highly respectable physician.

The following experiments, illustrative of the temperature of the alligator, made with an accurate thermometer, which was tested by freezing, boiling, etc.; and may be relied on. I have omitted to enumerate the duration and repetition of the experiments, for the sake of brevity. The thermometer was seldom changed short of ten or fifteen minutes, and never until it appeared stationary. These experiments, which might have been greatly augmented, are, if I may judge, quite sufficient to show, that Cuvier and his disciples greatly err, when they assert, that this animal approaches the hot blooded quadrupeds in temperature. It approximates not the hot blooded animals, but the mercurial column of the thermometer!

March 31st, noon—air 62°; one alligator in the groins, etc., 57°—another 57½°; the water in which they reposed, about two inches deep, gave 57°. At 6, P. M., air 62°; the flanks, axillæ, under the tongue, pharynx, and gullet, each 61°; both the water in which their abdomens rested, and other water near at hand, gave exactly the same temperature. The day was cloudy.

April 1st, sunrise—cloudy, humid, air 60°; alligator's flanks, etc., 59½°; gullet nearly 60°; a little water in which they lay, 59½°; other water, near, 60°. Noon, air 63°; alligators, and the water in which they lay, 61°; other water 62°. At 5, P. M., air 67°; alligator's 65°; water two inches deep 64°; other water 65°.

April 2d, sunrise—air 59½°; alligator's flanks and gullets, posterior fauces, each 63°; the water in which they lay, and which was now removed, gave 58½°; other water 59°.

April 3d, 7, A. M.—the animals and their cages are quite dry; air 64°; groins, gullet, etc., each 63°. Noon, air 68°; alligators 65. Sunset, air 64°; alligators nearly 65, and dry.

April 4th, 1, P. M.—air 63°; flanks 60½; gullet 61°. Sundown, air, flanks and gullet, each 64°; animals dry.

The following experiments are deemed relevant to this subject:—While engaged in making a most extensive series of thermometrical observations, illustrative of the diurnal and annual temperature of the Mississippi River, I have had a few opportunities of experimenting on its fishes, immediately after they were taken out of the water—an example of which is here subjoined. 1845, July 29, air at 5¾, at 6, and at 6½, A. M., 76°; River 85½°; a fish (*perca**) weighing about three pounds, was (after crushing its brain) placed on a plank, in a shade, with a thermometer thrust into the gullet. In two minutes, the temperature was 81°, in 3 m. 81°, in 5 m. 80¾°, (dead) 5 m. 80°, (body flexible) 10 m. 80°, 10 m. 79¾°, 20 m- 79½°, (now one hour—body somewhat rigid) 10 m. 79½° (universal rigidity) 10 m. 79½°. During these experiments, the air of the spot had raised to 83°; and had now, in one and a half hours, begun to communicate its caloric to the fish.

It may, perhaps, be said, that the crocodile of modern times differs from that of the ancient world. Fortunately there is a history extant, written with a pen altogether infallible; it reaches to the earliest era of our planet—THE FOSSILIFEROUS VOLUME OF NATURE. The author of the *Recherches sur les Ossemens Fossiles*, formed a new era. He had surveyed the face of the old, scarred earth, and numbered its animals. He penetrated below its surface—*he disinterred the* FOSSILIZED PAST. Thus Cuvier, like Columbus, discovered a new world! What the inimitable Bulwer says of the mysterious *future*, may, with little modification, apply to the *past*—anterior to Cuvier's great revelation concerning the fossil remains of eras so remote, as to bewilder the imagination: "upon that unknown and voiceless gulf of inquiry, brooded an eternal and impenetrable gloom—no wind breathed over it—no wave agitated its stillness;—over the dead and solid calm there was no change propitious to adventure—there went forth no vessel of research, which was not driven, baffled, and broken again upon the shore."

Thanks to Cuvier! Zoologists can now say with Dr. Buckland, in his Bridgewater Treatise, "that the fossil remains of the crocodilian family do not deviate sufficiently from living genera, to require any description."—(V. i., p. 191.)

MICROSCOPIC ADDENDUM.

The epidermis, cuticle, or scarf skin of the alligator, consists of a laminated horny substance, which, nevertheless, transmits the sense of touch with rapidity. A live coal placed on the skin, directly over the spine, caused an instantaneous manifestation of pain, which seemed to be acute.

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* Family *Percidæ, genus Pomotis.* Mr. De Kay, New York Fauna, 4to.

The epidermoid laminæ appears to be arranged like the slates or shingles on a house, one over-lapping another, as may be demonstrated by raising their edges by the point of a lancet, and pulling them from their connections, as in micacious plates. Each lamina, though inappreciably thin, is without doubt, an aggregation of other similar laminæ.

The superficial surface of one of these plates, is various and uncertain, being sometimes nearly as broad as the human nail. A lamina does not lie exactly horizontal, but dips or inclines slightly; its outer terminal margin, being black, and possessing an absorptive power over the spectrum, little inferior to charcoal itself.

This laminated stratum, is opaque only to a very limited extent along its margins, at which, will often be found, interspaces possessing more or less of that essential element of miscroscopic research, transparency. The plate constantly becomes more pellucid towards its inner termination.

The specimen here examined, was taken from a skin which had been dried for many years, portions of which, were used in that state, as well as after maceration from one day to six weeks. Maceration and trituration of the cuticle, with a suitable quantity of water, give a mixture, which, to the naked eye resembles black vomit.

The microscopic analysis of this substance when conducted in the *direct rays of the sun*, presents, like all other substances, which I have examined, (and they are not few,) certain ultimate phenomena, which will be described presently. And, although, the alligator's scarf skin is not a good type, or point of departure for the illustration of the general doctrine—the germ of which, only, I wish here to delineate, still it is sufficiently adapted to my purpose. Great as is my distrust, in my abilities in this branch of science, I cannot persuade myself, that my observations are mere optical illusions, or the figments of fancy—erroneous they may be. But as my method is, as I suppose, peculiar—the solar microscope, so called, having nothing to do with it, and as it has been practised at intervals for nearly two years, it cannot be considered altogether premature or hasty. Besides, I am far from thinking that my observations conflict with the general laws of light and of optical instruments.

An epidermic plate of the alligator, presents the three elements, which, so far as I have observed, constitute the structure of animals and plants, both liquid and solid, upon examination, by what, I shall call *solar analysis.* It would be, I repeat, utterly impossible in this place to give even an outline of the experimental proof, which I have accumulated in less than two years, and which has induced me to draw this conclusion. I cannot enter upon the objections which professed opticians, and microscopists may urge against a method, (I shall call it the solar method,) which is universally interdicted, involving as they do, the profoundest speculations concerning the refrangibility, the irisation,* the reflexion, the absorption, the entrance, the transit, the emergence, the color, the concentration—in a word, the general doctrines of light or the solar

* *Irisation.*—Webster has not admitted this necessary word, though he has *irisated* and *irised.* The dictionary of the French Academy, has the adjective *irisé*, (that which presents the colors of the rainbow,) but not *irisation !*

spectrum, all of which may be sources of error with careless observers, and which, if they apply to my method, apply with, perhaps, more force (as I could show) to that usually pursued by indirect light, especially that of the lamp, so much recommended, though it is wholly useless, for the purposes of minute and exact observation. The method I have pursued, whether good or bad, is not followed. It is sufficient to mention the opinions of two of the most eminent, recent writers on this subject: Sir David Brewster says, (Ed. Ency.) "Microscopic objects are commonly seen *better in candle light than in day light, a fact which is particularly apparent when very high magnifying powers are employed;* and we have often found that very minute objects, which could scarcely be seen at all in day light, appeared with tolerable distinctness by candle light. So far as we know, the cause of this has not been investigated." Now, this is very plain to myself. In a good light, that is, in the direct rays of the sun, a focal point never can arise from an entire vessel or organ of any animal, even including those called the infusoria, but only from some portion or molecule of such vessel or organ. Hence, all structures as nerves, muscles, heart, &c., are, in a certain sense, annihilated, or become, as it were, structureless. This gentleman gives a list of what he calls *microscopic objects,* which he recommends, without indicating any preparation, as most suitable for examination, among which are the spider, bug, mole-cricket, glow-worm, leech, corals, sponges! Now, the true focal point for one hair of the glow-worm, will be extremely limited for that part, but cannot answer for another hair, much less for the animal's entire body, which, were it sufficiently pellucid, would present an indefinite, nay, almost infinite number of focal points. Dr. L. Mandl, lays it down as a microscopic axiom, to *avoid the direct light of the sun,* chiefly because it produces *irisation!* (MANDL ET EHRENBERG, *Traité du Microscope*). Dr. M. admits that a lamp has the same effect. I am unable to see any thing in his statements which justifies him in his love for a *dim,* and his *dread of a bright light.*

When the observations are properly conducted, the calorific rays, combined with the solar spectrum, including desiccation and certain molecular movements incidental to the same, so far from vitiating, really aid the experimental operations. The proper management of the light is the fundamental point. It is not *in the spectrum,* but upon its margins or boundaries where the eye must be fixed, in order to avoid the dazzling and other bad effects of a concentrated light.

Omitting all matters which are not essential to the intimate structure of animal and vegetable microscopic objects, as color and the like, three distinct things are seen in the direct solar light, and which I must explain, as they have no reference whatever to the terms used by others:

First, Infusoria: these animals have no resemblance to any of those called or described by the same name, or represented pictorially by Mandl, Ehrenberg, and others, and which have been arranged by Müller into 2 classes, 17 genera, and 378 species. I may call them *solar infusoria,* because seen in sun only. They are filiform, cylindrical, lead colored: all are exactly alike. They endure boiling and all degrees of heat short of incineration, and remain desiccated for indefinite periods, and may be triturated without destroying their vitality, that is, their power of motion. They always move in both dead and living matter, whenever the intermedium, in which they are imbedded, is sufficiently *thin* or dissolved. Thus, in the cuticle of the Alligator, dried for many years, the infusoria were found abundant in all parts which were sufficiently transparent for examination, and, of course, rigid, but on triturating this lamina with a drop of water on a glass, the infusoria were set free and became active: so of many other substances. The bodies of the infusoria of authors, afford some excellent examples of this, owing to the great transparency of the former. If a portion of the body or limbs be selected, and be sufficiently thin, the solar infusoria will be distinct, and if the cohesion be sufficiently diminished, active, even while the animal which they help to constitute is yet alive. But if the animal be disintegrated, then the solar infusoria *all* become active. The infusoria of authors, are generally quite moist enough to allow this action, as soon as they are triturated or broken up. Could shells, and the like, in which they abound, be broken up, pulverised, and dissolved completely, these animals would, no doubt, become active immediately, as I have always found them to be when so treated.

A drop of blood, black vomit, milk, or the juice of berries, will afford a most satisfactory view if it be spread out sufficiently *thin*—a drop may be regarded as a "great globe," flattened, it may be, in which these infusoria swim, one greatly above another, each having a different focal point corresponding to its position.

The solar infusoria move without ceasing, so long as the intermedium will permit. As the cohesion of the latter augments, the motions of the former diminish. Their motions are serpentine, presenting only a few curves. They do not possess the power to stem a strong current. They are types of perpetual motion, and of immortality—always moving, all being alike, and all apparently incapable of dying. Desiccation appears to be their hybernating condition.

The second general microscopic element, which the direct, concentrated solar spectrum discloses, is a spheroidal body, remarkable for its luminosity. These spheroids are numerous, and have nearly the same appearance in all substances. They possess no power of locomotion, or other indications of life, unless, perhaps, in a few cases. I have seen, several times, numerous spheroids of a similar size, which travelled in various directions, in a voluntary manner, like ants, with, sometimes, a rotatory motion.

The third element, in this solar analysis, is the most abundant of all, and may be called the *intermedium*, and is both solid and liquid in different substances, both in the natural and artificial states, appearing to be as structureless as water. It serves as a menstruum, bed or cement to the other two elements. It is rarely, perhaps never, as transparent as water. If it be liquid, desiccation will very generally cause fissures, like those of the Mississippi alluvion, after the subsidence of the water, during the action of the sun and wind. It seems to be homogenous, and has, with respect to light, various degrees of absorptive and transmissive power.

Whenever the cohesion of the intermedium is sufficiently overcome by the solvent action of water, or by the disentegration of the tissues, of which the infusoria form a component part, the latter invariably pass from a state of rest or sleep to that of perpetual activity. When the intermedium passes from a liquid to the solid state, by slow or rapid desiccation, or when a live coal is placed on the opposite side of a plate of glass, so as to heat the substance nearly to ignition, no effect is produced on the infusoria, or spheroids, unless the intermedium be coagulated, or otherwise altered, so as not to be again susceptible to solution.

The solar infusoria are very distinct to all eyes—young, old, and middle aged. I have known children to express uncommon admiration on seeing them in minute portions of fresh blood, milk, the juice of a blackberry, black vomit, &c. It would really be strange, that all these persons, amounting to a considerable number, should be deceived.

The microscope used, though an exceedingly good one, possesses but a moderate magnifying power—a fact of great importance, with respect to distinctness of vision, in this method of examination. In a microscope of great magnifying power, all the objects here described, have been identified, but, less clearly, particularly with respect to the motion of the infusoria, which was seldom seen satisfactorily.

It is worthy of remark that the father of Microscopy, the most successful of all discoverers in this department of science, Leuwenhoeck, (who was born in 1632), never used microscopes of high power. "In all his microscopes," says Brewster (Encyc.) "the lenses had *not a very high magnifying power*; and there is reason to believe that most of his discoveries were made more from the *distinctness* arising from the accurate figure and good polish of his lenses, than from the *greatness of their power*. Of the 26 microscopes which he presented to the Royal Society, only *one* had a focal length so small as one-twentieth of an inch, and all the rest were below *half an inch* in focal length."

VIII.—*Case of Rupture of the Heart, and Compound Fracture of the Thigh, in which the Patient survived twenty-eight hours and a half.* Reported by E. D. Fenner, M. D.

Wounds of the heart, almost inevitably fatal, are still very curious and interesting from the fact, that death does not always immediately take place; but life may be maintained for hours, nay even several days, after a severe injury of this vital organ. Several such instances are recorded in the writings of the Profession.

In the case I offer, death did not take place until about 28½ hours after the accident. Perhaps it may be doubted whether the auricle was ruptured at the moment of falling; it might possibly have given way subsequently. Of course this must remain a matter of conjecture. I can only give the symptoms, and the appearances after death.

John Lugo, a robust Italian sailor, aged about 30 years, was brought to the Charity Hospital at 2 o'clock, A. M., Aug. 28th, 1843. He had fallen from a window in the fourth story of a house, down upon the pavement below. This occurred about two hours previous to his entering the Hospital.

The house-surgeon found him very much prostrated—pulse very small and weak—he was bathed in cold sweat, but was perfectly clear in his intellect, and gave a correct account of the accident. The right thigh was broken just above the condyles, and the upper part of the bone, exposed about three inches, rested over the Patella.

The leg being flexed upon the thigh, extension was made; the exposed end of the bone was drawn within the lacerated integuments, and the limb put in as comfortable a position as possible. A stimulant was ordered, to produce reaction, and the patient was left till morning.

In the morning considerable reaction had taken place—he was feverish and thirsty—body hot, but extremities rather cool—very restless, but did not complain of his head—intellect still clear. The visiting-surgeon thought him in such a desperate condition that there was no hope of benefitting him. The stimulant was discontinued, and cool drinks ordered in its stead. The limb was adjusted well enough, and he was not disturbed. During the morning he rather overloaded his stomach with fluids, and vomited. The ward-nurse informed me that he now complained chiefly of his right shoulder. He had pain there, though nothing was observed amiss in the part.

In the evening he became much worse—after drinking some tea, he vomited again. As the night advanced he gradually sank, and died with but little struggle at half past four o'clock.

Autopsy.—The examination of the body was made at 11 o'clock, A. M., August, 29th.

The broken thigh was livid, and very much swollen up to the groin.

As mentioned above, the bone was broken obliquely, just above the condyles. These last were completely split apart. There was no fracture of the patella, or bones of the leg.

Chest.—Upon raising the sternum, a large quantity of dark fluid blood was discovered in the right pleural cavity, in immediate contact with the lung. In searching for the source of the extravasated blood with my hand, I discovered *an orifice large enough to admit the end of a finger, opening directly into the left auricle of the heart. This orifice was behind the pericardium—between the mouths of the pulmonary veins, and so close to the upper one, as almost to involve it in the rupture. It was somewhat oblong, and looked as if it had been torn. The heart was in other respects perfectly sound.*

I measured 17 oz. of fluid blood out of the chest, and there was, perhaps, an ounce more that was not taken up. The lungs seemed perfectly sound. I now examined the abdominal viscera, but found nothing extraordinary or unsound, unless, perhaps, an unusual friability of the liver. The gall bladder contained ℥ iv. of healthy bile, and there was yellow bile found in the stomach and intestines. The extraordinary lesion of the heart above described, was witnessed by three other physicians, who were in the dead-house at the time.

The brain was not examined—the symptoms during life indicated no serious lesion of this organ.

It may not be uninteresting in connection with this report to give the following extracts translated from *Roche & Sanson's Elements of Pathology.*

"Rupture of the Heart.—Rupture of the heart is sometimes the consequence of softening of its tissue, or of its ulceration; and consequently one of the terminations of its inflammation; but it sometimes takes place without antecedent morbid state of this organ, and examples of it are not very rare. Sometimes it is produced by external violence, such as the passage of the wheel of a carriage over the thorax—anon it is the consequence of a violent effort—sometimes it is brought on by excess of fury, or by an attack of epilepsy, or by coition—in fine, in several cases it is in some way spontaneous, and happens without appreciable cause, whether it succeed a morbid state of the heart, or happens spontaneously without anterior alteration of the tissue of this organ.

"It is nearly always in old persons that it is observed. It is superfluous to add that when it is the product of external violence, a fit of passion, an attack of epilepsy, or the venereal act, the age of the subject is immaterial. The promptness with which this affection produces death ordinarily, does not allow of a diagnostic, and renders it morever useless.

"M. Bland thinks, however, that one can recognise the following symptoms, viz: the sudden manifestation of a sensation of boiling heat, of acute and deep seated pain, of a sort of constriction, or of disagreeable weight in the region of the heart, immediately followed by great anxiety, extreme weakness, palor of the countenance, alteration of the features, the frequency and smallness of the pulse, which can scarcely be perceived, and finally by speedy death.

"Are these symptoms pathognomic? It is permitted to doubt them; and were they so—the rapidity with which death supervenes in nearly all cases, does not even allow them to be collected.

"Rupture of the heart takes place most commonly in the left ventricle, and towards the point, probably during its contraction. We often find the tissue of the heart about the rent, pale, slightly red or grey, softened, easy to crush between the fingers, or to tear; the borders of the opening uneven, ragged, and ordinarily parallel to the direction of the fibres of the heart.

"It is not rare to meet with the ventricle thickened at its superior part, and attenuated at the point, where the rupture takes place—sometimes the fibres of the heart are corroded or ulcerated in the portion broken or torn. In short the rent sometimes appears under the form of a crack or narraw fissure, more or less oblique, and without any morbid alteration of the pariates of the heart. Often there exists many of these fissures at the same time, and then, excepting that which has produced death, they are superficial. In all cases we find a greater or smaller quantity of clots of blood in the pericardium.

"Rupture of the heart may, however, not bring on death; it is necessary for this that it be very inconsiderable. It may then happen that a clot closes the opening, hardens there, adheres, and that even the edges of the orifice contract adhesions with the pericardium; there exists an example of this fact. The symptoms will be then those of carditis, or pericarditis, and the treatment that of these affections.

"Except in this case, all treatment is useless—nevertheless if the death is not sudden, we may have recourse to abundant bleedings, general and local, as is practised in wounds of the heart.

"There is another rupture of the heart which bears only upon the columns of the ventricles: much more rare than the preceding: it was described for the first time by Corvisart: it is ordinarily after violent efforts that it happens. The symptoms are only well marked when it occurs on one of the pillars, which proceed from the free borders of the tricuspid and mitral valves. Then we see the individual suddenly pass from a state of health to the most serious morbid state; he is taken immediately with an extreme suffocation, and cast into a state of inexpressible anguish; his pulse becomes small, intermittent, irregular, and in placing the region of the heart, one only feels a confused pulsation. The patient ordinarily dies in a few hours; but sometimes he sinks slowly, with the symptoms common to all the diseases of the heart.—This rupture is nearly always mortal. The treatment is the same as in carditis, and consists principally in the employment of bleedings, general and local, absolute diet, diluent or acidulous drinks, and perfect repose of the body and mind.

" Wounds of the Heart.—An opinion which some surgeons entertain with the vulgar is, that wounds of the heart are necessarily and instantaneously mortal. Others, approaching nearer the truth, think that those only are fatal which penetrate the cavities of the heart, and that we can cure those which implicate only the thick part of the muscular parietes of the cavities. To support this last assertion they rely upon observations, the most remarkable of which is beyond doubt that which has been reported by M. Latour, and of which the subject was a soldier, in whose heart was found six years after the cure from his wound, a ball *imbedded* in the right ventricle, near the point of the organ, covered in part by the pericardium, and placed upon the septum medium.

" But the most of surgeons, relying upon numerous and authentic observations, think that wounds of the heart are not always either instantaneously or inevitably mortal, what portion of the organ soever, be the seat of the wound. Individuals have been seen who have survived many hours, even many days, the opening of one or the other ventricles; and what is very remarkable is, that in some of these cases the wounds were of very large size. Thus Paré saw a man, who, having been wounded in a single combat, continued to pursue his adversary and only fell at a distance of 200 paces, although he had in his heart a wound large enough to admit a finger.

" Courtial saw a man in whom the thrust of a sword had traversed the left ventricle, making a wound so large that at the post mortem examination one could introduce the little finger into it; and who, nevertheless, went 500 paces, and only succumbed after about five hours without having experienced oppression, or difficulty in the exercise of speech.

" Other observations, very numerous, prove that narrow wounds only cause death after many days.

" Saviard saw one which passed from the right to the left ventricle, traversing the septum, which only produced death after the fifth day.—Among these one of the most interesting, without doubt, is that reported by Le Range, in one of his notes to the edition which he has given of the Observations on Surgery by Saviard. The subject of this observation is a young man, who having been wounded by the stroke of a sword, and having immediately presented all the symptoms of a lesion of the heart, was happily put beyond danger of the first symptoms, and commenced leaving his chamber and walking about on the seventh day, but expired on the eleventh day from having indulged a wish to go to a cabaret. The opening of the body, done in the presence of Ferrand, proved that the stroke of the sword had traversed the lung, the pericardium, the right auricle, and the aorta through and through. Finally, other observations proved that wounds of the heart can be cured.

"Without speaking of animals slain in the chase, in the hearts of which have been found portions of arrows, or balls, which have been fixed there long since—without speaking besides, of individuals, who having presented all the rational symptoms of wounds of the heart, have been nevertheless cured; we borrow from Durand, an observation which proves incontestably this assertion, viz: "In the month of December 1769, during a very cold time, a cavalier of the royal regiment, after having received the thrust of a sword in the breast, and lost much blood, remained in a moribund state from Tuesday till Saturday, couched upon a ladder, in the midst of the ruins of a demolished quarter.—The cold was such that this unfortunate individual had both legs frozen by it, and the mortification which followed was the cause of death—he had been hurried into death by the loss of blood, of his strength, and by the cold. The right lung had been pierced, and the right ventricle of the heart opened. The wounds had cicatrized during the five days that these viscera had ceased their functions. He lived yet ten days at the hospital, and would have withdrawn from it, if he could have proceeded methodically with the treatment of the gangrene of his legs."

"There results from these facts, that although the prognosis of wounds of the heart, is generally very serious, nevertheless we should not completely despair of the life of patients when the wounding instrument is small and sharp. The position of the heart renders the wounding of certain portions of it more easy than others, thus the right ventricle is undoubtably, the most frequently affected by wounding instruments; after this, the left ventricle—and after these, the auricles.

"The diagnosis of wounds of the heart is always easy. The observations of Courtial that we have cited above, prove that the state of the wounded cannot present satisfactory signs of it; nevertheless in the most of cases, besides the conjectures that can be drawn from the situation, the direction of the wound, and from a knowledge of the depth to which the wounding instrument has penetrated; the wounded man presents certain symptoms, which are regarded as signs of wound of the heart; such as dyspenœa, anxiety, frequent faintings, smallness and irregularity of the pulse, pain behind the sternum, pallor, cold sweats, and the ordinary symptoms of effusion in the cavity of the pericardium, or the breast."

Since the foregoing observation was made, another very remarkable case of *wound of the heart* has occurred in this city, the notes of which have been kindly furnished me by the attending-surgeon, Dr. A. Mercier, and are as follows.

Case.—In 1845, a young man of this city, aged 23 years, in a personal recontre, was stabbed with a double edged dirk, which divided the cartilage of the seventh rib, and *penetrated the left ventricle of the heart, in an oblique direction, making an opening more than an inch in width.* As soon as his antagonist had inflicted the blow he attempted to escape, and ran off at the top of his speed. The wounded man followed in close pursuit for a distance of about 125 yards, when he sank upon the ground very much exhausted, but still able to tell who wounded him. A friend who was going out to the spot where the altercation took place, but had not reached there before the chase commenced, followed the combatants as fast as he could, but only overtook the wounded man after he had run the distance above mentioned. The hemorrhage was profuse from the instant of the stroke, for the blood could be traced along the pavement the whole way. He was taken home, and Dr. M. arrived to see him about three quarters of an hour after the accident occurred. Dr. M. found him in a state of insensibility, his heart beating feebly, and only about *four or five times in the minute.* There was no pulsation to be felt at the wrist. On removing his clothes, about two pounds of clotted blood were found within the shirt—the wound was closed by a clot, and the hemorrhage thus stopped. Stimulants were poured down his throat, and other means used to produce reaction, which began to be evident in fifteen or twenty minutes—he spoke, answered questions, recognized his father and friends, and was conscious of his perilous situation. His pulse now became perceptible at the wrists, beating about twenty-two to the minute. He remained in this state for more than two hours after Dr. Mercier first saw him, and then suddenly expired, *more than three hours after receiving the fatal stab.* At the instance of the Coroner, Dr. Mercier made the *post mortem* examination.

Autopsy.—On raising the sternum a very firm coagulum was observed to extend in the course of the wound, from the external orifice clear into the ventricle, closing the wound completely. As before stated, the wound was more than an inch wide, and would freely admit any of the fingers. The pericardium contained six ounces of clotted blood. No blood was found in the pleural sac, as it had not been opened. The examination was not carried any farther, as it was already perfectly satisfactory.

Thus have I detailed the particulars of two cases, almost as remarkable as any given in the foregoing interesting quotation. The records of surgery abound in instances where men have survived for a longer or shorter time the most dangerous wounds, whilst on the other hand, many have been known to die from apparently the most trivial. The above cases are only given to show what serious injuries so vital an organ as the heart may sustain without causing immediate death.

IX.—*On the Topical Application of the Sulphate of Quinine.*

[The following correspondence having been submitted to us, we have thought it might be interesting to our readers, and therefore insert it in this place.—Edrs.]

To A. J. Wedderburn, M. D., *New Orleans.*

Dear Sir:—Having observed, with much interest, some remarks by you in the last number of the New Orleans Medical and Surgical Journal, on the subject of the treatment of ulcers, with quinine, I take the liberty of suggesting to you, that during the last five years I have frequently treated chronic conjunctivitis and urethritis, by direct applications of quinine dissolved in distilled water—in the latter cases, believing that the inflammation usually extended to the bladder, I have directed the injection to be thrown into it.

I would beg leave to remark also, that for ten or twelve years I have treated chronic ulcers, especially those resulting from burns, with a preparation of quinine, prep. chalk and pulv. rhei, with very marked benefit.

I am, most respectfully,
Your obedient servant,
RICHARD LEE FEARN, M. D.

Mobile, Sept 5th, 1846.

NEW ORLEANS, Sept. 10th, 1846.

DEAR SIR :—In answer to your letter, dated the 5th instant, I have to inform you that I have never used the sulph quinine, in the treatment of conjunctivitis, or in urethritis, as a local application, but I have frequently noticed from its internal administration the relief afforded, in the course of a few hours, in a case under treatment for a soft cataract, the person being subject to frequent violent attacks of inflammation of the Conjunctiva. In these attacks, I have always used opium with the quinine, when the latter has been given, and I have, at times, used the opium alone, but never with the same effect as when given in combination. I have administered this remedy in cases of urethritis, for chordee, in ten gr. doses at bed time, with marked success, when the same quantity of camphor has failed to give relief.

I cannot doubt, for a moment, the good results that must attend the topical application of quinine in the affections mentioned above. I have often thought of its application in such cases, but have not resorted to the treatment, in consequence of the relief I have always seen follow the use of the nitrate of silver in inflammations of the Conjunctiva. If you will refer to the January number of the New Orleans Journal, you will find a case reported of a sloughing ulcer from primary syphilis, in which the ulcerative process was immediately arrested by the topical application of quinine, when other remedies had failed.

Erysipelas prevails in the Charity Hospital, during the winter, in the form of an epidemic. During the last winter, after a number of cases in which I had used the knife, were attacked with this disease, I was induced to resort to dressings saturated with a solution of quinine, about 5 grs. to the ounce of water, immediately after an operation, and in no case in which this remedy was used, did the disease occur.

I have lately used an ointment of quinine in a case of Eczema Capitis ! The excessive inflammation attending the eruption was relieved in a few hours, and the disease entirely subsided in four or five days. The ointment used in this case was composed of sulph. quinine ℈j ; laudanum gtt. xv ; ol. lavender gtt. iij ; simple cerate ℥j. The laudanum was used chiefly with the view of dissolving the quinine.

I shall take the earliest occasion to follow your treatment, and inform you concerning the same.

I am, very respectfully,
Your obedient servant,
A. J. WEDDERBURN.

To Richard Lee Fearn, M. D., Mobile, Ala.

Part Second.

REVIEWS AND NOTICES OF NEW WORKS.

I.—*Animal Magnetism, or Psychodunamy.* By THEODORE LEGER; Doctor of the Medical Faculty of Paris—late Professor of Anatomy at the Practical School—Fellow of the Society of Sciences and Arts of Dep. de la Marne—late Professor of the Medical College of Mexico, &c.

Such is the title of an exceedingly clever book, recently from the press of Appleton & Co., New York. The author is, at present, residing in that city, and from this production, the only means I have of judging, I should form a very favorable opinion of him. His book certainly gives evidence of talent and extensive reading—is written in a simple, forcible, pleasing style, and withal possesses such a pervading air of candor, that to me it has afforded much pleasure and instruction.

The volume before me, (a modest octavo), is confined to the *History* of the Science, and is soon to be followed by a *second*, in which will be given the "rationale of its practice."

He very truly says that "the historical part of Animal Magnetism, is much more important than history in general to other sciences, for in it is involved the question of its very existence." He has, therefore, treated this portion of his subject with some minuteness, and particularly its Academic History, which is but little understood in this country, though full of interesting and instructive matter. From the want ot accessible facts, the proceedings of the Royal Academy on this subject, have been much distorted by both parties, and Dr. Leger deserves our thanks for publishing the reports, which, it is to be hoped, will do something towards bringing the belligerants to a proper understanding.

Our author, aware of the strong prejudices engendered by former contests, and the real objections made against the many names which have been employed to designate this mysterious agent, of which he is treating, and believing that the science has been mainly retarded by these prejudices, has coined the new word *Psychodunamy*, which he derives from two Greek words signifying *Power of the Soul.* This new name, we think, is not free fiom objections, and if there were no other, its uncouth sound alone would prevent its adoption.

"The word Psychodunamy, he says, defines, as exactly as possible, the power which man possesses, of materially acting upon man independent of touch" .... "the name itself designates only that special faculty of the living man which the commissioners of the Academy, in 1784, have been themselves *compelled* to acknowledge, and to prove this, I will here quote their own words." Hear what they say :

"That which we learned, or at least ascertained, in a clear and satisfactory manner, by our examination of the phenomena of *Magnetism*, is, that '*Man can act upon Man*' at all times, and almost at will, by striking his imagination ; that signs and gestures, the most simple, are then sufficient to produce the most powerful effects ; this action of man upon his fellow may be reduced to an art, and successfully conducted after a certain method, when exercised upon persons who have faith in the proceedings," and they might have added with truth, persons who have *no faith*. Again the report says :

"Nothing can be more astonishing than the spectacle of these spasmodic affections. Without having seen, one can form no idea of them ; and while witnessing them, one is alike surprised at the *deep repose* of some and the excited state of others ; at the various incidents that repeatedly occur, and the *sympathies* that are established. *All are submissive to the Magnetiser ;* however drowsy they appear, his voice, *his look, a mere gesture* rouses them. *No one* can help acknowledging, in these constant results, the manifestation of *a great power* which agitates the patients, nay, completely subjugates them, and of which the Magnetiser is apparently the *depository*" ! ! !

Such is a specimen of this celebrated report, made in 1784, by Bailly, and signed by Franklin, Lavoisier and others. This is the awful and decisive condemnation of Mesmerism, of which we hear so much. All the important phenomena for which Mesmer and Deslon contended, are here fully and explicitly admitted—the only dispute being as to the *agent* which caused the phenomena. The commission said it was *Imagination*, the others *Magnetism*. I repeat that all the phenomena contended for by Deslon, were admitted by the commission, for it was not till after this epoch that *Magnetic Somnambulism, with Clairvoyance* and other phenomena, were discovered and promulgated to the world by the Marquis de Puységur, "a nobleman whose birth, mental acquirements, extreme benevolence, as well as large fortune, had secured him the highest rank in society."

The experiments of the commission of 1784 were continued through *five months*, and although they consumed much time and labor, the Academy, as we shall see on a subsequent occasion, after a full discussion of the subject, decided that the investigation was not conducted in a manner to do full justice to the question.

In a lecture which I delivered some months ago on "*Animal Magnetism*" in Mobile, I stated that "Dr. Franklin was ill, and did not witness the experiments," though he signed the report. Dr. J. McN., now a resident of Mobile, published in the last number of the New Orleans Medical Journal, what he calls a "Review" of my lecture, in which he takes exception to the above assertion, and with that delicacy and decency which characterises him, charges me with being *guilty of suppressing the truth*, and instead of appealing to an original source for information to condemn me, quotes from a short and imperfect sketch by Prichard, in the Encyc. of Pract. Medicine.

Unfortunately for this honorable gentleman, I am in possession of all the reports of the Royal Academy of Medicine on this subject, from 1784 to the present time. The experiments were continued *in Paris*, as I have stated, during five months, and the report states distinctly that Dr. Franklin was confined *at Passy*, about two leagues out of the city, with chronic disease, and mentions but *two occasions* on which the committee, with M. Deslon, went to Passy to make experiments before him, but I am willing, if he likes, to *grant him half a dozen sittings.* Most of the experiments were unsuccessful, but some of them were the experiments quoted to prove the conclusions of the commission which I have quoted above. One of these very experiments, strangely enough, is quoted by our *Reviewer* to convict me—an experiment that proves all I am contending for, "the young man fell into a *crisis*,"* "he lost all consciousness, became rigid, was carried to a neighboring grass plot, and restored by M. Deslon, &c., &c. The dispute again at Passy, was not as to the reality of the phenomena, but the "*primary theoretical cause*."

Now for the "*suppressio veri*"—could two occasions, or half a dozen, qualify Dr. Franklin for deciding a complicated question which occupied this commission for *five months*, and a subsequent one for *five years*, and could any one, under these circumstances, say that Dr. Franklin did "witness the experiments" conducted by the commission ?

Magnetism vs. Prejudices, is the caption of our author's 2nd chapter, in which he very ably shows that Magnetism has only been struggling against the same unphilosophical prejudices and persecutions which have been opposed to all reforms in religion, politics, laws and morals, as well as discoveries in science, and particularly Medicine. I might say to our Catholic *Reviewer*, that the new fangled religion of Luther, "*had* received a due degree of attention" from *his* sect for a long series of years, and althongh Dr. Franklin witnessed the experiments without being converted, yet *Protestantism*, like Mesmerism, holds the even tenor of its way.

This chapter deserves to be carefully read and remembered, though its wholesome truths may be somewhat mortifying to our professional vanity. I recommend it particularly to those doctors who descant so much on the Baconian philosophy when Mesmerism is mentioned, and who, with an air of self-satisfaction, tell us that the facts are both false and impossible, or they would have been received long ago. But a word from our author :

"For nearly *fifteen centuries*, the works of Galen made law in Medicine. During that long period not one dared doubt the word of the master—his theories were held sacred. Still Galen, ignorant of Anatomy, describes in the human body, organs and humors which *are not to be found there ;* and to the disordered action of these supposed humors and organs, he alledges the causes of diseases" ! !

"If any one should believe that experience came at length, and came to undeceive and teach a theory, more correct, more rational, and founded on accurate *observation* of *facts*, he would be egregiously mistaken. It was to pass from error to folly that they abandoned Galen—it was to adopt with enthusiasm the mad reveries of Paracelsus, who pretended to

* The word *crisis* was employed by Deslon, and adopted by the committee, to express the assemblage of phenomena contended for.

have discovered the philosopher's stone, and to keep it on the hilt of his dagger."

We have neither time or inclination at present to follow our author through his clever sketch of medical history, which is teeming with follies and falsehoods more absurd, and infinitely more dangerous than Mesmerism, in its most exaggerated forms. We all know that in succession the Medical world was ruled by the *chemical philosophy* of Sylvius—the *mechanical* of Hoffman and Bœrhaave—the *soul* of Stahl—the vagaries of Cullen, Brown, Pinel, Broussais, &c., all of which we now look back upon as the "eccentricities of genius." Even now the demons of red pepper and cold water are struggling for mastery over the mild and harmless spirit of Homeopathy.

Pass to the Materia Medica, and we see that "there is no substance in the three natural Kingdoms that has not been praised as a specific, there is nothing, however absurd, disgusting, or even poisonous, that has not been enthusiastically recommended by some physicians."

Do we not remember too the history of the circulation of the blood, vaccination, antimony, mercury, &c., &c.—but the reader need not be told that there has been far more stability in the opinions of Magnetisers than of those of our profession. The fundamental phenomena established by Mesmer, are still acknowledged by all, and many new ones have been discovered and added which are also established amongst those who have fully investigated the facts. No science, of course, should be charged with the exaggerations and falsehoods of charlatans and knaves.

We now come to the most important and interesting part of the volume before us, viz: a succession of chapters, containing the various *reports* and discussions of the Royal Academy of Medicine on Animal Magnetism, since those of 1784. The French Revolution, and the stirring events which followed for a long series of years, diverted the public mind from this curious subject, and it was not presented to the Academy again until 1825. In this year Dr. Foissac, a gentleman of distinction, wrote a letter to the Academy proposing another investigation, and stating that he had a good subject for experiment at its disposal. A very animated discussion was excited by this letter, on the question *whether it was proper that the Academy should again investigate it*, and finally a committee was appointed to report on *this* question.

The committee, after full deliberation, made a very eloquent, manly, and dignified report, the conclusion of which I give below, and beg that its contents may be noted.

"The following summary, therefore, gentlemen, embodies the sentiments of your committee:

"1.—That the judgment passed in 1784, by the commissioners appointed by the King to inquire into Animal Magnetism, by no means dispenses with the obligation to investigate the subject anew, because in the sciences, no decision whatever is absolute or irrevocable.

2.—Because the experiments on which this judgment was based, seem to have been made in a *desultory manner*, without the simultaneous and *necessary* assembling of all the commissioners, and *in such a spirit*, as according to the principles of the subject they were called on to examine, *could not but cause their complete failure.*

3.—That Magnetism thus denounced, differs entirely in theory and *modus operandi*, from that which exact, honest, attentive observes, enlightened, industrious and persevering physicians have studied for some years past. [The *theory* and *modus operandi* are, of course, all speculation, and the only difference in the *facts* is, that many *new ones* have been *added* to those of Mesmer.—J. C. N.]

4.—That it concerns the honor of the French Faculty, not to remain behind German physicians in the study of phenomena, which are announced by enlightened and impartial advocates of Magnetism, as having been produced by this new agent.

5.—That considering Magnetism as an occult remedy, it is the duty of the Academy to *study* and *experiment* upon it, in order to wrest the use and practice thereof from persons altogether ignorant of the art, *who abuse this means, and make it an object of lucre and speculation.*

"Upon all these considerations, your committee are of the opinion that the section ought to adopt the proposition of Dr. Foissac, and appoint a special committee to devote itself to the study and examination of Animal Magnetism."

Signed, HUSSON, *Reporter.*
ADELON,
PARISET,
MARC,
BURDIN-AINÉ.

These are *all* members of the Academy of *Medicine*, and are as well known to fame as any of the medical profession in France—while, on the other hand, the commission of 1784 was a *mixed one* of four physicians and five from the Academy of *Sciences*. The three names too which have most celebrity, and which are most frequently quoted, are those of Bailly, who *drew up the report*, Franklin and Lavoisier, all three from the *scientific* department.

The celebrated Jussieu, who is represented to have devoted himself more fully and fairly than any of the commissioners, drew up a report as long and quite as able as that of Bailly, which he opposed in detail.

Let me now put the question to every intelligent and candid physician, whether any body of *scientific* men, are competent to investigate fairly, phenomena of the nervous system so infinitely varied as these, and which belong alone to the province of the medical profession? But let us return to our narrative:

After, as might have been expected, an animated discussion, the report was approved of, and a permanent commission from the Academy of Medicine appointed, by a large majority.

This commission commenced the duties assigned, in the spring of 1826, and its report was not made until 1831, after *five years* of laborious, cautious, philosophical investigation. After examining a large number of subjects from every source they could be obtained,—after varying their experiments in every possible manner, and throwing around them every guard against deception which men accustomed to experimental inquiry could devise, this protracted investigation was brought to a close, and a report presented, which is not surpassed in dignity and ability by any similar document on record. Academic history affords no parallel to this investigation.

And what does this commission decide? To form a just opinion, the reader must peruse the report at length; I can here touch but a point or two. Besides asserting the reality of some mysterious agent, the catalepsy, insensibility to pain during surgical operations, foretelling of crises to happen in *their own* bodies, and other phenomena, CLAIRVOYANCE, they say: "We have seen two somnambulists distinguish, with their eyes closed, objects placed before them; point out without touching the cards, their color and value in the game; *read words written by hand, or several lines from books opened at random.* This phenomenon has taken place even when the eye-lids were *firmly closed by the pressure of fingers upon them.*"

Not trusting even to the accuracy of each other's experiments, though before their own eyes; each took the precaution, in rotation, to repeat the same experiment. How is it possible for men to be deceived in an experiment like this?

This report was signed by Husson, *reporter;* Bourdois de la Motte, Fouquier, Gueneau de Mussy, Guersent, Itard, J. J. Leroux, Marc, Thillaye. There was no dissenting voice.

"This Report (which takes up 70 pages) was listened to by the Royal Academy of Medicine, with the greatest attention and interest. In vain did some violent opponents of magnetism endeavor to disturb the deep silence of the assembly; an immense majority repressed indignantly the attempt; and loud and general applause, repaid Dr. Husson's courage and ability." There was an eloquence, a dignity, a philosophic precision and simplicity about it, which was irrisistible.

It may now be asked, if human testimony can establish any fact in science, what does all this mean. Here is incomparably the fullest and fairest investigation which Animal Magnetism has anywhere received—an investigation continued experimentally for five years, by nine members of the Academy of Medicine, distinguished for character, learning and ability, and the facts are asserted by them to be true! Yet we hear, not only ignorant Irish brawlers, but many honorable, intelligent members of the profession, railing against it as all nonsense and imposture! Is it possible, I again ask, that men like these could be employed five years in recording phenomena which had no existence?

Such a great and glorious triumph as this of Animal Magnetism was, of course, not to be tamely submitted to by its opponents—although silenced for the time, they waited deliberately for another opportunity of attack. This did not present itself until 1837, when Dr. Berna wrote to the Academy, that he had a good subject under his charge, at its command, and requested that another commission should be appointed, for further investigation of the subject. The opportunity was seized upon by the opposition members; and a commission was, after some discussion, named, *entirely from their ranks*, at the head of which was M. Dubois d'Amiens, who had long been, to use his own language, "a sworn enemy of Mesmerism," and who had deeply committed himself against it by his violent writings.

As reasonably might be expected from a committee appointed under these circumstances, the experiments were not conducted with the calmness and fairness which a subject of such intricacy demanded. After a short and hurried examination, in which they violated the rules laid down by their opponents, after having treated Dr. Berna, (a gentleman of standing) with a degree of indignity which compelled him to close the experiments at a point which, he "declared to be but the beginning of what he proposed to show them," they made a report of *six pages;* which, though, condemning Magnetism *in toto*, shows an entire ignorance of its commonest phenomena, even those *established* by the commission of 1784. This Report was signed by Dubois d'Amiens, *reporter*, Bouillaud, H. Cloquet, Emery, Pelletier, Caventou, Cornac, Oudet.

Dr. Berna sent to the Academy a protest, in which he made charges of unfairness against the Reporter, and challenged a denial—it remained unanswered.

The Report too was replied to on the floor, by the venerable *Husson*, who drew up the celebrated Report of 1831; it was thoroughly dissected and exposed at every point. Near the close of this reply, M. Husson used the following language:

"So, the whole Report amounts to this; *gross historical omissions—numerous and culpable concealments*—experiments already known, and which prove nothing—erroneous conclusions—a style, the levity of which is highly indecorous, *even in the opinion of the friends of the Reporter*," etc.

"The success of this speech (of Husson) was complete. The Report of M. Dubois d'Amiens was *overruled* and annihilated; and his confusion during the eloquent, dignified, and severe lecture of his adversary may be more easily imagined than described. He did not venture a single word in reply; and our cause instead of losing, gained ground among scientific men."

Here we have evidence of the mortifying fact, that even in this grave and dignified body of the Academy, there are men so blinded by prejudice as to be utterly unable to bring their minds to the proper investigation of a grave question, which is dividing the scientific world—a question too of facts, involving results of immense importance to mankind.

I have thus given a brief *resumé* of the proceedings of the Royal Academy of Medicine, from the first to the last, on this subject, and am not now afraid to put the question to every candid and competent man, whether I was justifiable in using, in my lecture, the following language, which has drawn from Mc——*sneers*, but *no attempt at refutation*—he is wholly incompetent to meet the issue—hear what I said:

"Now I assert and can *prove* that if the *commissions* of the Academy have done anything towards deciding the question, the weight of their authority is largely in *favor* of Mesmerism."

I must here close my notice of our author's volume, but not because its value and interest are exhausted; on the contrary, a very important portion must be left untouched, viz: the discovery of Magnetic Somnambulism—experiments in the public hospitals of Paris—the progress of the science in England, the United States, etc., all of which is full of instruction; and I hope the reader may be induced to give these chapters a careful perusal.

It has often been asked why Magnetism should have been so long in establishing itself firmly, if it really be true. The reasons can be properly understood only by those who are *practically* acquainted with its difficulties. The phenomena and *rationale* of Mesmerism are just as impossible to describe satisfactorily as those of hysteria, to one out of the profession—in both cases the phenomena must be *seen* again and again, in *many* subjects, and in all its protean forms. These phenomena vary with the "peculiar nervous system" of each individual; and to an inexperienced observer, they would appear contradictory and destructive of the whole science; but *hysteria*,, sleep, *insanity, delirium*, neuralgia, and other groups of nervous phenomena, with which we are all familiar, when analysed, offer the same inexplicable complications and difficulties.

It should be remembered, also, that Mesmerism has had to contend from the beginning against the most bitter animosity from our profession, and from the church. The Pope long since crushed it in his dominions; and this is, perhaps, the best reason our *Catholic Reviewer* can give for his opposition, as he has so far been unable to produce *any other*.

Another reason why it has not been more generally received is, that its phenomena in some subjects are so curious, I may say so miraculous, that Mesmerism has been seized upon by dishonest persons, with the view of making money. One of the peculiarities (as I stated in my lecture) of the mesmeric state, is the condition of the mind—the subject is often in a dreamy or delirious state, in which the mind runs wild, and imagines things as wild and extravagant as "Gulliver's Travels" or the Arabian Nights—in this state a person the most honest when awake, may be led on by questions or hints to do anything—they will, without hesitation, attempt to prophecy future events, prescribe for diseases of which they know nothing—go with you on an intellectual journey, etc., etc.; and when aroused, the subject has no more recollection than he would have of ideas which have passed through his mind in the delirium of fever. In this way, for selfish purposes, falsehood has been mingled with the true phenomena by designing men; and not knowing what to believe, many intelligent persons have abandoned it, believing nothing. How often have we seen persons with the best intentions, in the plenitude of ignorance, with the desire of seriously *investigating the science of Mesmerism*, walking off deliberately to a show room, and paying a travelling mountebank 50 cents, to see a series of experiments; and after sitting two hours and watching the experimentor with a philosophic eye, returning home, pronouncing the *fellow* an *unadulterated scoundrel*, and *ergo*, the whole science a humbug!!

The French Academy did not think five years too much time to devote to it; but our Mobile critics are so smart, that they go at night to see somebody put a negro to sleep; and next morning are well enough informed to write Reviews about it in the newspapers and periodicals! inspired, I suppose, by Moses and Saint Patrick!

In the face, however, of all this opposition, from his holiness the Pope down to these *small potatoes*, Animal Magnetism, since the Academic Report, in 1831, has been progressing with a firm and steady march. It would be a tedious and useless task to rehearse the long list of great men in Europe, who have boldly expressed their opinions in favor of this science. From the best information I can procure, belief is general throughout the German States; though the profession is much divided in France, the advocates of Animal Magnetism may well be proud of their ranks, and the line of division may truly be said to divide those who *have* investigated from those who *have not*. The instances must be few and far between where individuals have experimented fairly, and not been converted to some extent. I can't find one in my reading. There are some people in this world, upon whom reason and evidence are thrown away; they are very soon discovered; and it is, of course, folly to talk reason to these on any subject.

Is it not a remarkable fact, that while men are every day changing their religious, political and medical opinions, no intelligent man, after full investigation and conversion to Animal Magnetism, has ever come out and *renounced it?* Is not this a powerful and conclusive argument in its favor? This is only another evidence, that a fact once fairly demonstrated to our senses, can never be abandoned.

In the United States, ten years ago, the word Mesmerism fell upon our ears like a foreign language; and now, there is not a town or village in the Union which does not number many believers amongst the "*first class*;" and we almost daily meet with records of surgical operations performed in the mesmeric sleep, and testified to by witnesses so respectable as to leave no room for doubt; although Mr. Liston and Sir Benjamin Brodie might disbelieve it for two reasons—first, because they have cut off legs without the patients hallooing; and secondly, Mesmerism cannot be true, because *it is not possible!!!*

In England, Magnetism has, perhaps, made less progress than in any other part of the civilized world, except in the Pope's dominions, where it has been horned to death by a "bull." It has here been met by ridicule, and not by that philosophical scrutiny which has governed France and Germany. In the learned article of Prichard, (in the Encyclopedia of Practical Medicine) which our Reviewer has quoted for *honorable purposes*, he remarks, "On the whole, when we consider the degree of suffering occasioned by disorders of the class (nervous) over which magnetism exerts an influence through the medium of the imagination, and the little efficacy which ordinary remedies possess of alleviating or counteracting them, *it is much to be wished* that this art, notwithstanding the problematical nature of the theories connected with it, were better known to us in actual practice, and that some of the foreign operators would *introduce it more extensively into this country.*"

[*Article, Somnambulism.*

This is the language of a man of science, who, though he has little personal knowledge of the matter, feels and knows the value of *reason, evidence, and honesty.* How different has been the course of the Royal Medical Clique of London? Sir James Johnson, Physician Extraordinary to the Crown, and Editor of the most influential Medical Periodical in the world, met it on the threshold with all his talent, and all that bitter sarcasm which has overawed the profession around him. Then comes Sir Benjamin Brodie, Surgeon to the Crown; who so far from investigating, indulges prejudices so strong, that when driving through a particular street, always turns a corner, to avoid passing by the house of that *fool Elliotson, where Mesmeric experiments are made!* Then comes the Editor of the Lancet, whose connection with the matter is a little curious, but too tedious for present purposes. Elliotson gives evidence to show, that *he* changed sides to keep from losing subscribers, and grossly misrepresented facts. He stated that the Okeys (two of Elliotson's subjects) were impostors, and acknowledged the fact themselves. Elliotson had them under his charge for a long time after this date, and not only pronounces, but proves the *charge to be false.* Dr. Caldwell saw them afterwards, in London, and corroborates the statement of Elliotson, as do all those who had the best opportunity of knowing the truth.

I have before me a little volume, by C. R. Hall, the mouth piece of the clique, on "Mesmerism, etc." It is really a literary curiosity to one who knows anything about the matter. The beauty of this book, which he styles "A Critical Inquiry into the Assumed Merits and History of the Mock Marvellous, Hallucinations and Frauds of Mesmerism," is, that when it is carefully read, and all his admissions put together, he admits all that rational Mesmerists, (Mr. Elliotson for example) contend for.

I have no inclination to sift a volume of such trash, and will content myself with giving simply his "recapitulation of what are, *at present*, his own views on Mesmerism." I will leave Mc—— to watch him, and tell us what Mr. Hall's opinions are next week or the week after. Now for his *creed*—

"I believe" (not in the holy Catholic Church, but that) "there are"

"*Proved*—Quietude, composure, sleep—"

"*Probable—but requiring confirmation*—Traction, muscular rigidity, convulsions, heightened sensibility, diminished sensibility, double consciousness—

Possible—but not probable—Insensibility to severe pain for a given length of time, *at pleasure*, etc.

Now I can vouch for these phenomena myself, having produced them, probably a hundred times, and am very well satisfied to get from such a source, the words *proved*, *probable* and *possible*—it wont be very long before the rest is choked out of him. John Bull, though he may kick and cuff his perverse Irish subjects for pastime, is always ready to rise up in defence of his own rights. Mr. Elliotson, who was hunted from his professorship, and crippled in his practice, through incessant ridicule and persecution, says in his Physiology, "I will now stand more ridicule with the same firmness, and the same silent pity or contempt which I have always felt for my opponents, till I see, *as I shall, the truth of Mesmerism established*." If his life is spared many years longer, he will see his prediction fulfilled. He is now backed boldly by talent and numbers. There has been in England, for the last three years, a periodical called the *Zoist*, devoted to Animal Magnetism and allied subjects, which is sustained with great ability, and is extensively patronized.

I give below some information which has come to hand while I am writing. Here we see lords, distinguished physicians, surgeons and clergy, all combined in the great cause of humanity, and promulgating to the world the truth of Mesmerism; and when truth has thus once obtained a fair hearing, it cannot be kept down.*

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* The following extract is from the *London Times*:—"MESMERIC INFIRMARY.—At a meeting held at the Earl of Ducie's, No. 23 Belgrave Square, July 9th, this meeting being convinced of the vast benefits derived from mesmerism in the cure of diseases, even the most intractable, as well as in the prevention of pain in surgical operations, and being desirous that its benefits should be more widely extended to the poorer classes than individual exertion can accomplish, it was resolved—

"1. That with this view a Mesmeric Infirmary shall be established, by

I will here close what I have to say on the *History* of Animal Magnetism; and I hope I may be indulged in a few words about myself, which I would much have preferred to omit, but circumstances which will be alluded to as we go on, have rendered it necessary.

It so happened, that until about three years ago, I had never seen an experiment in Mesmerism; no occurrence had happened to bring it directly under my notice, and hearing it mentioned occasionally, only as a subject of ridicule; I, like most other persons, looked upon it as one of those day dreams that would go down, in history, with astrology, alchemy, and witchcraft. At length, my attention was aroused by a letter from a medical friend, in whom I could fully rely, containing some experiments which he had made. Many of his experiments were of a nature which precluded all idea of deception or delusion—they made a deep impression on me, and I forthwith procured the proceedings of the French Academy; the works of Teste, Foissac, Elliotson, and some other medical men of high standing; and after reading them carefully, I saw plainly the evidence was too strong for the belief that all was fiction.

The phenomena, if true, formed a part of the physiology of the nervous system, and were also important as a means of alleviating human suffering. Under this conviction, I regarded it as a professional duty to investigate the subject more thoroughly; and, accordingly commenced a course of private experiments in my own house, which were gradually extended as occasion offered, to the parlours and sick chambers of friends. My experiments, without developing many of the marvellous results spoken of, were successful beyond my expectations; and, although I wished to avoid notoriety, they began to be talked of through the town, and were, of course, much exaggerated. Some of my friends even, who witnessed many of my experiments, not acquainted with the tendency which the subjects in this peculiar dreamy state have, to deceive, often attached more importance to results than I did. I had determined not to admit any fact until I could demonstrate it by varying the experiment in every possible way—many that others admitted, were excluded by this procedure.

Matters proceeded thus until about the commencement of the present year, when I was waited on by a Committee from the Franklin Society of Mobile, and *requested* to deliver a lecture *on the subject of Mesmerism*,

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voluntary contribution, for the application of mesmerism to the cure of diseases, and the prevention of pain in surgical operations.

"2. That this Infirmary shall be under the management of a president, vice-presidents, trustees, a treasurer, secretary, and a committee.

"3. That the Right Hon. the Earl of Ducie be President.

"Vice-Presidents—Baron de Goldsmid, Viscount Morpeth, M.P.; R. Monckton Milnes, M.P.; J. H. Langston, M.P.; the Rev. G. Sandby, jun.; the Rev. T. Robertson. Treasurer—Mr. Briggs. Committee—Dr. Ashburner, Dr. Buxton, Dr. Elliotson, Mr. Clarke, Mr. Chandler, Mr. Flintoff, Mr. J. Hands, Mr. D. Hands, Mr. F. G. Johnston, Mr. Symes, surgeons; Major Buckley, Captain James, Mr. Blyth, Mr. Fradelle, Mr. Kingdom, and Mr. Topham."

Then follows a lengthened list of subscriptions received, in which we find the Earl of Ducie, Baron de Goldsmid, and Mr. Langston, to have subscribed £100 each; Mr. Murray and Dr. Elliotson, £50 each; and many others who were present, donations or annual subscriptions of £5, or as many guineas.

in their public hall—stating at the same time, that my experiments had been talked of, that the community had confidence in my integrity, and knowing that I had no interest in deceiving, they thought would be gratified to hear, to what conclusions my experiments had really led me.

I very willingly consented to this request; the more so as it afforded me an opportunity of "defining my position," and defending myself against the exaggerated opinion swhich had been erroneously imputed to me.

I accordingly on the 18th day of February, 1846, delivered the unfortunate lecture, which was afterwards published in the Charleston Medical Journal, and which drew forth a notice, entitled a "*Review!*" in the last number of the New Orleans Journal, signed by no less a personage than J. McN., whom I shall drag out of his *peat bog*, and dress up a little for exhibition.*

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* This lecture has been doubly unfortunate, for it has brought down upon me the wrath of both Jews and Gentiles. First came, "like the south wind o'er a summer lute blowing," a very graceful, feeble little *anonymous* attack in one of our city papers, which by its *inimitable style*, I had no hesitation in pronouncing the offspring of the distinguished Dr. L., of Mobile. Some of your readers may recall the name from its association, with a queer and highly wrought little *spider story*, which he published several years ago, in the Amer. Jour. of Med. Sciences, and which afforded the faculty in Mobile a good deal of innocent mirth at the time. We regarded it as a very ingenious little romance, but it turned out that the Doctor had intended it in *earnest*, and he became so sensitive on the subject that we were really afraid to pronounce the word *spider* in his presence—he would bristle up like a *Tarantula.*

Why the doctor should have treated me so cruelly, I could not imagine as I had never done him anything but kindness. I did not, of course, notice this little spider work—these webs do very well to catch flies, but I always prefer walking round them to keep from getting soiled.

If any of your readers have forgotton our author, they will soon have another occasion for forming his acquaintance. I see him formally announced for the next number of the American Journal of Medical Sciences. The doctor has had bottled for some years a magnificent little blighted fœtus, which was the happy subject of an essay some two hours long, before the Medical Society of Mobile—the great object of this essay was to prove points which are *long since perfectly established*—but then we were repaid for all this loss of time by the most graphic and gorgeous description of the act of copulation ever penned by mortal man—the old lecher Jove, with Apollo to back him, could not approximate it. I presume this is the forthcoming article.

Next comes at me *your* bloody little countryman and disciple of "Gulliver," Mac, at present a resident of Mobile, but late, all the way from "*swate Ireland*," and as he don't like the word "first," we will suit him better by saying one of the *last* physicians in Mobile. Until his "*Review*" appeared, I had not thought about him, or heard any thing of him for several months. About a year ago, I had a *rencontre* (professional) with him, which I will relate as it will elucidate the *motives* of the reviewer, as well as his chivalry and character.

A poor, miserable Irish journeyman taylor, whom I looked upon as an object of charity, and whose name I never enquired, came to my office some four or five days in succession to be prescribed for; being taken suddenly very ill during the night, and not knowing where to find me at that hour, he had our learned Reviewer knocked up and summoned to his bed side. The next morning I was sent for by the patient, who informed me of what had happened and apologized for sending for another physician, stating the reasons, and further that he had no confidence in him. As the patient was *mine*, and not dreaming that any-

The first sentence of the Review, coming from one who makes such a display in the secret of *philological* learning, is really rich and rare.—If such a display had burst from one just imported from Cork into a land where English is spoken, there might have been some excuse, but Mc. has been here long enough to have learned better. Hear now this memorable onset—

"There *is a class* of books, so completely beyond the pale of critical examination, that *they* safely bid defiance to it;—occupying that place, which is said to intervene between the *sublime* and the *ridiculous*, *they are* secure from either. To this class, we are of opinion fairly belongs the "*Lecture on Animal Magnetism*," &c.

I presume the reader will be satisfied with a "*critical examination*" of this *single* specimen, though like the "*razor strops*" "there are plenty more left.

The word *class* is first preceded by the singular verb *is*—then referred to by the plural pronoun *they*, and lastly followed by the plural verb *are*—so much for grammar!" Next our Reviewer speaks of a "*place*" which "is said to intervene between the *sublime and the ridiculous*."—I should be glad to see some authority for this "place!" I am sure no one else ever dreamed of such a place. I have often heard it said that there was but a step from the sublime to the ridiculous, but as Professor Cooper once remarked, they are just as "dissimilar as *Hail Columbia and a bunch of grapes*, and the idea of an intervening place is an absurdity.

In the next place he informs us that this class *are* "secure from either" the *sublime* or the *ridiculous!!* This I opine is rather an original use of terms. Lord Kames would not say that a book was "*secure from the sublime*," though he might say that it was secure from the adjective *ridiculous*, with an *a* and double *s* after it. Query: did our Reviewer learn all these marvels out of his favorite book, "*Gulliver's Travels?*"

So much for Mac's philology! We will now amuse the reader with a choice specimen of his medical learning, and logic, which places him in a "very enviable position."

When attempting in my lecture to account for the infinite variety of

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body could think differently, I prescribed for him without ceremony. An hour or two afterwards, Mac called to see him, and finding I had been there, and prescribed, became as furious as a famished wolf, and wrote me a note which smelt so strong of gun powder, that I have hardly yet got over my alarm. This note was handed to me by Dr. F. A. Ross, (a gentleman with whom I was on very friendly terms,) who stated that he bore the note with reluctance and only consented to do so, because he was satisfied there was some mistake which I would explain—he thought me incapable of a breach of etiquette so gross as that imputed to me. I related to the doctor a plain statement of the facts, and convinced him at once that if either party had a right to complain it was myself. The next thing I see from him is this "*black dose*" which he had mixed up for me under the name of a "*review*."

I am perfectly aware, Messrs. Editors, of the impropriety of introducing such things into a scientific journal, but when members of the profession so far forget themselves as to substitute malicious feelings and personalities for scientific inquiry, timely rebuke is necessary to teach them better.

phenomena, seen in different subjects under the influence of mesmerism, I used the following language:

"No two human beings, probably were ever created alike—they differ in external appearance—they differ in their intellectual and moral characters—they are differently affected by morbific causes; and *each has his peculiar nervous system.* What physician can pretend to write a complete history of Hysteria, Paralysis, Neuralgia, Insanity, and other affections of the nervous system? They are just as full of anomalies, and contradictions, in their causes, symptoms and treatment, as mesmerism. Inoculate a dozen patients with small pox virus, and the phenomena which result will be as opposite as those exhibited in the same number of mesmeric subjects."

Here is simply an enunciation of a few *truisms* which are taught in our common text books, of which we have the following Irish version:

"Indeed! we were under the delusion, and we believe it is still taught by professors of anatomy, that there is a *fixed* uniformity (the italics are the doctor's) in the distribution of the nerves of the human body;—that although there is *occasional* variation in other structures, the blood vessels for instance, such is never the case in the nervous system. So with the virus of small-pox, it has been held that, however variable its effects seemed in different persons, it was but a difference in degree; the effects of the virus being always the same; the same precursive fever, followed by the same peculiar eruption, &c."

All this too I presume was found in "Gulliver's travels," for nothing like it can be found in medical books.

According to a different doctrine laid down in another place by our Reviewer, there must be some "relation" between cause and effect, and if so, we might conclude that all sorts of temperaments, nervous, phlegmatic, &c., could not spring from a "fixed uniformity" of nervous structure and function. It is moreover taught in England and France, that the *brain* constitutes a part of the "nervous system," though there is not a "fixed uniformity in the distribution" of *its* parts. It is further "taught by professors of anatomy" that there is a certain *relation*, between certain portions of the brain and certain functions—for example, when the forehead is *well sloped*, and the head comes to a point at the vertex, somewhat after the fashion of a potatoe hill, the victim has a great deal of vanity, but *divil the bit of sinse at all, at all.* In open defiance too of this "fixed uniformity," it is taught that when the brain falls below a certain weight, the possessor is always *idiotic.*

Now it so happens that I have sitting in front of me in my office, amongst some other busts, those of Napoleon, Cuvier, Dupuytren, and Broussais, all of which have remarkably *large* heads—they are from the hands of a Parisian artist of some celebrity, and are said to be accurate. The brains of Cuvier and Dupuytren, (the weights of which I have,) were enormous, and the others are, perhaps, of nearly equal magnitude; but there "*is a class*" of spherical excrescences, occasionally observed on the "*sublime*" extremity of the spinal column, which has no more agency in intellectual operations than the adipose protuberance at the other, and *this* class may be supposed "to occupy that place," "which is said to intervene between" a head of cabbage and an apple dumpling, and "*are* secure from (the excellence) of either."

Another remarkable fact "taught by professors of anatomy," is the want of "fixed uniformity" in the *thickness of skulls.* I recollect, a very thick one of an Irishman, belonging to the Wistar museum, which was exhibited by Dr. Physic to the class. An attempt was made to trepan this unfortunate individual, but in vain. One surgeon bored into the place where brain is commonly found, for a considerable time, but becoming alarmed at the depth he had gone without getting through, desisted—a second, and a third, took a turn at it, but "it was no go"—hunting brain in that direction was like looking for snakes in Ireland—the "fixed uniformity" was not there. The poor fellow was at last left alone to die a natural death from the effects of a fall, as Pat would say. They then determined to make another desperate effort, and by dint of of hard work with a handsaw, chisels, mallets, &c., they managed at last to find a small cavity filled with something very much like mashed potatoes *without any butter* in them.

On the Reviewer's *original views* of small pox, I will merely remark that if, when he had the Ency. of Practical Medicine in his hands, reading to so little profit the able article of Prichard on *Somnambulism,* he had turned over a few pages to this subject, he would have seen that it is "taught by professors," that there is no "fixed uniformity" in the "precursive fever," and that patients often die of this disease *without a single pustule.*

The Reviewer "presumes" "that Dr. Nott has viewed these things with a mesmeric eye." Now the effect of mesmerism is to sharpen some people's wits and senses wonderfully, and if I could only get *him* fully mesmerized, I have not the slightest doubt that I could make him see, as Liston says, "with the back of his head or his belly" better than he sees with both eyes open in the broad light of day—even with his present vision he has seen "things which passeth all understanding," but if he will let me *rub him down well,* I will promise to make him go ahead of Gulliver.

And now Messrs. Editors I have done with our Hebrew and Irish Reviewers—they have wantonly and maliciously provoked a reply, the necessity of which I deeply deplore. It is now nearly twenty years since I embarked in the medical profession, and I venture to assert that there is not one of its members who during this time has had fewer professional altercations—it is not because I have not sometimes been unjustly treated, but because I have been so ashamed of the charge, justly made against us of too much wrangling, that I have made it a rule to bear more from my professional brethren than others. Previous to these ungenerous attacks on my mesmerism, I cannot now call a single instance of interruption of friendly intercourse with a respectable member of the profession. I have always believed too, and acted upon the principle that the only way of making our body respected, is to treat each other with respect, and to preserve dignity, decency, and veneration for science—medical associations, and legislation against quacks, and quackery are of no avail—it is the conduct of individuals alone which elevates our position.

On the subject of Mesmerism I have contended for no miracles. I have said that I had demonstrated a hundred times to my own satisfaction and that of intelligent friends, that by certain manipulations, a peculiar sleep could be induced, resembling natural somnambulism; that a cataleptic state could also be induced at pleasure, in these subjects, with various degrees of insensibility to pain, which in some is perfect; that this sleep and other effects may be removed in a few seconds, by passes or by command; and that nervous headaches, and many forms of neuralgia may be promptly alleviated or cured by these manipulations. I stated that other experimenters, of the highest intelligence and honesty, had asserted the reality of still higher phenomena, as *clairvoyance*, the influence of the *will*, etc., which it would be unfair and presumptuous in me to pronounce false, simply because I had not met with subjects of this degree of susceptibility. I stated that, although, I had seen many instances of what are termed Mesmeric phenomena being produced by the imagination, and was willing to grant this power all the influence which could be claimed, still, I had in some instances seen phenomena, which I could not by any ingenuity account for in this way, and that I was, therefore, compelled to believe in some other and unknown agent, which had been designated by the name of Animal Magnetism; and further, that I was not disposed to dispute about a *name;* if other gentlemen preferred the word *imagination* I had no objection, as I only wished to insist upon the reality of certain phenomena, the investigation of which belonged properly to the medical profession.

Knowing that a lecture, without experiments, would afford little interest, I presented two subjects, on whom the phenomena contended for were demonstrated. The subjects had no money to gain, no motive to deceive, and I had every reason to believe in their perfect good faith.

Being aware that it would be objected, that some of the phenomena, as sleep, rigidity of muscles, influences of passes, insensibility to pain, etc., might be feigned, and therefore not be satisfactory, I made rigid the arm of one of the subjects (a very delicate, feeble young man) and placed it in a horizontal position, where it stood for nearly or quite an hour, until the close of the lecture; at the end of the time it was as rigid and as free from fatigue as during the first minute; and I presume it would have remained in the same altitude all night, had I not relaxed it.

I contend that there were no phenomena in the mesmeric state (with the exception, perhaps, of the control of the operator) which do not occasionally occur *spontaneously* in certain states of the nervous system, as sleep, catalepsy, insensibility to pain, the *sleep-walking* state, etc.; this is the ground taken by Elliotson, and there is certainly nothing irrational in the belief, that *artificial* means may induce phenomena which often occur *spontaneously* in the human system.

As a curious piece of medical history, intimately connected with the subject before us, I will make a brief allusion to what was called Perkinism, a therapeutic means first employed by Dr. Elisha Perkins, of Norwich, Connecticut, about the close of the last century, and named after him. It consisted in drawing over the affected or other parts, the pointed extremities of needles, called *Metallic Tractors*, which were bound together, and composed of two metals.

These *Tractors* became all the rage in London, produced many very remarkable curative effects, and yet were driven out of use by a pamphlet of Dr. Haygarth, "*On the Imagination as a cause and as a cure of disorders of the body, exemplified by fictitious tractors.*

These fictitious tractors were made of wood, and produced all the remarkable effects which had been attributed to the metallic ones. I will give below, as examples, two of Haygarth's cases.

April 20th. I requested Mr. Barton to operate on Peter Seward, aged 32, who had for four years been troubled with pain and weakness in the right arm. * * * I learnt that he had experienced a good deal of pain during the operation (of drawing the tractors downwards so as barely to touch the skin) * * * The next day I was assisted by Mr. Bernard and Mr. Lowe, jr., and as the case is *rather curious*, I shall copy *verbatim*, the notes taken on the spot. In one minute, 'feels pain coming on at the same place as yesterday; the limb feels warm, the pain higher up and sharper;' in two minutes, 'pain increases;' in three and a half minutes, 'very acute, darting towards the collar bone, and begins to give him so much uneasiness, that he will not have it done any longer;' 'perspires profusely, and is gone to bed.' It was fortunate for me that the above gentlemen could bear witness to the *remarkable effects of the imagination*." * * *

"On the 25th, however, in the presence of Messrs. Joliffe, Barton, Gaisford, Emery, and Wilde, Dr. Lovel made use of one bit of mahogany, whilst I gently drew down the arm the point of the other. When he sat down, 'he was perfectly easy;' in a few seconds, 'the pain commenced as before;' in two minutes, it was 'very acute at the elbow and collar bone;' in four, 'he became very uneasy, looked very red in the face, and begged the operation might be discontinued.' His request was complied with, and he went to bed with a pulse at 120. Three-quarters of an hour after (being still in bed) I asked him how he felt? He replied, that he was in more pain than when the surgeon took five pieces of bone from his leg, in a compound fracture which he had met with. *It may, perhaps, be thought that all this was feigned.* I cannot assert that he did not; but he could have no point to gain by such conduct; and he certainly must have been a very *excellent mimic to deceive so many people*.

(If Dr. Elliotson had performed all this on the Okeys, the editor of the Lancet and others, who learn philosophy by intuition, would have sworn it was all *feigned!* N.)

"The experiment was again repeated on the 2d of May, in the presence of Dr. Moncrieffe, Messrs. Noble, Yeatman, Clayfield, Probert, Notcutt, Lax, and Joliffe. The patient felt spasm, and pains in the arm and collar bone; and the pain continued as before, for some time. He could scarcely be persuaded to submit longer to the use of the tractors, although he confessed, that 'on the whole, he *had received much benefit*.'"

On the 24th of May, I requested permission to attempt the relief of a patient of Mr. New's, by the new operation. He had great stiffness and occasional pain in the shoulder. Mr. Gaisford, therefore, began to treat him in the usual manner, with the pieces of pencil. In a few minutes he was seized with a tremor in his limb, and so violent a shivering fit, that it was judged prudent to desist for the present. Next day I wished to repeat the trial; but he refused, *alledging that his arm was quite well*, and therefore it would be putting him to useless pain," etc.

Here we see a class of diseases (which in our ignorance we are always treating by violent empirical remedies, and usually without benefit!) mitigated or cured by simple manipulations, but which are *kicked out of use because they are supposed to operate on the imagination!*

Where is a particle of *proof* adduced to show that it was the imagination? Where is the proof that it *may not* have been Animal Magnetism in both instances, acting independent of either the metal or wood? Its influence is said to pass through the air or anything else, without obstruction.

Again, what matters it, *if the patients are cured*, whether the agent is Imagination or Magnetism? Can any man give a reason why the imagination should not be employed to relieve human suffering, and will any physician deny, that he is daily and designedly enlisting and influencing the mind of patients for their own good? Do we not, with this view, send them to places of amusement, send them to travel, and advise occupation? Are we not often compelled to conceal from timid patients their true condition, and by little pious frauds inspire confidence, and thus give them the soothing and invigorating effects of all-powerful hope. Are we not often compelled to administer remedies almost or quite inert, in order to bring into play *the imagination*; and are patients not often relieved in this way? Are patients not greatly influenced, not only by *faith* in remedies, but by faith in certain physicians?

Is not the whole system of Homœopathy based upon *faith?* and is it not *faith*, with the aid of the *vis medicatrix*, which too often has to meet in desperate conflict our *heroics?*

Mr. Editor, it is time that this unreasonable opposition should cease, and although I may meet the same illiberal persecution which has been heaped upon others, I will never cease to say boldly, that it is our solemn duty to alleviate human suffering, and prolong life by every honest means within our reach. J. C. N., of Mobile, Ala.

II.—*Animal Chemistry, with reference to the Physiology and Pathology of Man;* by Dr. J. Franz Simon, translated by T. J. Day. Part 2nd, 8vo., 430 pp.; Philadelphia, Lea and Blanchard, 1846.

We were much pleased at the reception of this second part of Dr. Simon's admirable work, the first part of which was reviewed in the last volume of our Journal.

So rich is this work in new and interesting materials that a mere bibliographical notice can by no means convey a just idea of its value, and our only aim is to call the attention of the medical public to its valuable contents.—This part is devoted principally to the consideration of the normal secretions and products of organs, and the modifications in their composition which are observable in various morbid conditions of the system.

The first chapter of this part (the third of the work) is on "the secretions of the chylopoietic viscera, and the theory of digestion. No addition is made, to our knowledge, of the properties and relations of saliva, some remarks are however added by the translator on Dr. Wright's researches on this subject:—

"Dr. Wright believes in the existence of the principle called ptyalin, though he separates it from saliva by a new process. This process is "to pass saliva through ordinary filtering paper, and, after filtration shall have been completed, to exhaust the residue with sulphuric ether; the ethereal solution contains a fatty acid ptyalin.* It is to be allowed to evaporate spontaneously,

* A referen e to page 31, will show that Wright's ptyalin differs in several respects from the ptyalin described by Simon. In truth, little is known regarding this constituent.

and the residue left by evaporation is to be placed upon a filter and acted upon by distilled water, which dissolves the ptyalin and leaves the fatty acid. If the aqueous solution be carefully evaporated to dryness, the "salivary matter will be obtained in a pure state." "Ptyalin," he says, "as thus prepared, is a yellowish-white, adhesive, and nearly solid matter, neither acid nor alkaline, readily soluble in ether, alcohol, and essential oils, but more sparingly soluble in water. It alone possesses the characteristic odour of saliva; it is unaffected by galvanism and by most of the reagents which coagulate albumen. It is abundantly precipitated by sub-acetate of lead, and nitrate of silver; feebly so by acetate and nitrate of lead, and tincture of galls; uninfluenced by bichloride of mercury and strong acids; the latter considerably heigthen its proper odour and impair its solubility, while alkalies render it more soluble, and give it the smell of mucus. Moderate heat and oxygen gas also increase its odour, but a more intense heat or cold diminishes or entirely destroys it. At a suitable temperature, ptyalin may be preserved for any length of time without risk of decomposition. The salivary fluid from which ptyalin has been removed, possesses a sickly mucous smell, decomposes much sooner than ordinary saliva, and, in the process of decay, invariably evolves ammonia. If the fluid be heated, the mucous smell will be increased until the evaporation shall have been continued nearly to dryness, when a slight salivary odour may be recognised, due to a portion of ptyalin being liberated from the mucus with which it was previously in combination."

Dr. Wright says that sulphocyanogen is an invariable constituent of healthy human saliva. He advises that it be sought for in the alcoholic extract of the residue left by the careful evaporation of the fluid, as the mucus, unless removed, offers considerable impediment to the action of reagents. The sulphocyanogen occurs in combination with potassium, the salt constituting generally from .051 to .098 of the secretion. "The proportion," he says, "is temporarily augmented by local stimulation of the salivary glands, as by smoking, chewing sialagogues, &c. It is also increased by the internal use of prussic acid and salts of cyanogen, and remarkably so by the use of sulphur."

Respecting the part performed by the saliva, it is added:

"Very little is known with certainty regarding the part taken by the saliva in the process of digestion. Spallanzani fancied that he had observed that food enclosed in tubes pierced with numerous apertures, and moistened by the saliva, was more rapidly digested than when simply moistened with water. Berzelius, however, found that the saliva exerts no greater solvent power than pure water, and Müller confirms his statement. Hünefeld, on the other hand, believes that the object of the saliva is to destroy the tenacity of the food, and he thinks that it has the power of reducing fibrin to the condition of a viscid fluid.

[The services which the saliva performs in the animal economy are classified by Dr. Wright as follow:

Active.—1. To stimulate the stomach and excite it to activity by contact. 2. To aid the digestion of food by a specific action upon the food itself. 3. To neutralize any undue acidity in the stomach by supplying a proportionate alkali.

Passive.—1. To assist the sense of taste. 2. To favor the expression of the voice. 3. To clear the mucous membrane of the mouth and to moderate thirst.

Mialhe* has recently announced the discovery of an active principle in the saliva analogous in its physical and chemical characters to diastase. It is solid, white or grayish-white, amorphous, insoluble in alcohol, but soluble in water and spirit. The directions for obtaining it are the following: Filter saliva and treat it with five or six times its weight of absolute alcohol, adding it as long as any precipitate occurs. This animal disastase is insoluble,

* Lancette Française, 1845, Avril.

and falls in white flocks, which must be collected on a filter and dried. It forms about .2% of the whole saliva.]

Leuchs* was the first who observed that saliva converts boiled starch into sugar."

Some interesting observations are made in reference to the change in the saliva in diseased conditions, which changes have as yet been but imperfectly studied.

"Morbid saliva sometimes contains a free acid; this is most commonly lactic acid, but, in some cases, acetic acid is likewise present. The acid reaction may be at once detected by test paper; while normal saliva communicates a blue tint to red litmus paper, this, on the contrary, reddens blue paper. I have frequently seen the saliva acid in acute rheumatism, and in cases of salivation. According to Donné,† the saliva has an acid reaction in all cases of irritation and inflammation of the stomach, in pleuritis, encephalitis, intermittent fevers, acute rheumatism, uterine affections, and amenorrhœa. Brugnatelli ‡ detected oxalic acid in the saliva of a phthisical patient. The secretion of saliva is sometimes increased to an extraordinary degree, constituting salivation; in such cases, the chemical characters of the saliva are also more or less affected. In a specimen of saliva forwarded to me for examination, which was obtained from a patient who had just terminated a course of mercury of some weeks' duration, I observed an acid reaction arising from the presence of free acetic acid. It was very viscid, of a yellow colour, and possessed a sickly disagreeable, acid smell. It contained no mercury. After evaporation to dryness, all the acid reaction had disappeared: thus showing that it contained no free lactic acid. This saliva contained a very large quantity of semifluid fat, a considerable amount of albumen, and traces of caseous matter. Under the microscope, an immense number of fat-vesicles were seen, some epithelium-cells, and a very few partially-destroyed saliva-corpuscles."

It has frequently been a question, what changes were produced in the saliva during ptyalism, and though some interesting results are given, they do not appear to be constant, and among other things the presence of mercury in the saliva of mercurial ptyalism seems not to be constant, as will be seen from the following extracts:—

"L'Heretier gives the mean of three analyses of this secretion during mercurial ptyalism. He found:

Water - - - -	970·0 in place of	986·5
Organic matters - - -	28·6	12·6
Inorganic matters · -	1·1	1·9

The mean amount of ptyalin was 2·6, or very nearly the normal quantity. He attributes the large amount of organic matter to the increased quantity of mucus secreted by the buccal membrane.

Dr. Wright also found that the saliva of mercurial ptyalism contained an unusual amount of mucus. It consisted of:

Water - - - - - - - - -		988·7
Ptyalin - - - - - - - - -		1·9
Fatty acid - - - - - - - -		·4
Albumen with soda, and Albuminate of soda - - - - -		·6
Mucus with a trace of ptyalin - - - -		3·8
Lactates - - Phosphates - - Muriates - - Hydrosulphocyanates	Potash Soda Lime - -	2·4

He could not detect the slightest trace of mercury in it.]

* Kastner's Archiv. 1831 † Arch. Génér. de Méd. 1835, Mai.
‡ Stark, Allgemeine Pathologie, p. 1074.

Gmelin* has examined saliva discharged in consequence of salivation produced by mercurial inunction. In one case it was brown and turbid, and contained a large quantity of fat, but not much albumen; in another it presented a yellow tint: it contained a large quantity of yellow fat, and when heated, gave no perceptible indication of coagulation. In both cases, but most decidedly in the latter, indications of mercury were obtained.† Thompson ‡ found the saliva resulting from the administration of mercury, turbid; it deposited flocculi of coagulated albumen. It was not precipitable by tannic acid, had a specific gravity of 1003·8, and contained coagulated albumen 2·57; mucus, 3·67; chloride of sodium, ·9; water, 992·8 Bostock analyzed the saliva of a man who was secreting about two quarts daily in consequence of mercurial salivation. It was of a clear brown color, neutral, viscid, but not stringy, and barely transparent. It became clear, however, after the deposition of the minute flocculi suspended in it; the application of heat, and also the addition of corrosive sublimate, gave indications of the presence of albumen. It yielded 2$\frac{0}{0}$ of dried residue. After the discontinuance of the mercury, the saliva was found to be less transparent; it reddened litmus paper, contained more albumen, and more solid constituents generally. Vogel || analyzed the saliva of a man with spontaneous salivation; it contained ·991·2 parts of water; 4·4 of ptyalin, osmazome, fat, and albumen; and 4·4 of salts of soda, potash, and lime; hence, in respect to the amount of solid constituents and ptyalin, this saliva did not differ very much from the normal standard. Mitscherlich also found that, in the salivary flow excited by nervous irritation, the amount of the solid constituents was not increased, that the ptyalin and sulphocyanogen were even below the normal standard, while, on the other hand, the extractive matters were somewhat increased. A similar observation has been made by Guibourt."

In regard to bile, our author does but little more than to give an abstract of the researches of others, respecting this interesting secretion. In 1837, Demarçay announced that the bile consisted essentially of an organic acid combined with soda. This acid he called *choleic*. Dr. Kemp's experiments afterwards led to the conclusion, that bile consists of "a mere simple solution of a salt of soda," the acid of which differs from the choleic acid as characterized by Demarçay, in several respects; he terms it bilic acid. Liebig, in a memoir based on Kemp's experiment, arrives at very similar conclusions, but regards bilic acid as identical with the choleic acid of Demarçay and the bilifellinic acid of Berzelius. The new researches of Theyer and Schlosser go to sustain Liebig's conclusions. The author does not regard it as sufficiently proved that the principal organic constituent of the bile is

* Pogg. Ann. 41, p. 438.

† Gmelin employed Smithson's method for the detection of the mercury. A large quantity of saliva was treated with nitric acid, and evaporated; the residue was digested with nitric acid and dissolved in water; and, after the removal of fat by filtration, a stream of sulphuretted hydrogen was passed through it. The precipitate obtained by this process contains sulphuret of mercury; it must be collected, digested in nitro-muriatic acid, evaporated, dissolved in dilute hydrochloric acid, and a bit of gold-leaf enveloped in tin-foil, or encircled by iron-wire, suspended in the fluid. The gold is tarnished if mercury be present. No tin-foil should be used that has not been itself tested for mercury. In place of the gold-leaf I have employed the blade of a knife with advantage.

‡ Annals of Philosophy, vol. vi. p. 397.

|| Lehrbuch der Physiologie, von R. Wagner, p. 212.

positively an acid, but thinks that, like alumen, it may combine with acids as well as bases.

The action of bile in the process of digestion is briefly considered, without any very definite conclusions. A brief abstract is given of the conflicting views and observations of Brodie, Tiedeman, Gmelin, Berzelius, Theyer, Schlosser, Enderlin, and Schwann, and our author inclines very strongly to the view, that the bile is not merely an excrementitious fluid, but that it is a secretion essential to the animal economy, and performing some important part in the process of digestion. He throws out the following suggestion in regard to its action:

"If the bilin becomes decomposed in the intestinal canal into various constituents, through the influence of the acid chyme, then a wide field of investigation is open to us respecting the function of the biliary secretion in relatian to chylification. No explanation has yet been afforded of the discrepancy in the amount of albumen contained in the chyme absorbed by the intestinal villi, and in the chyle discharged by the absorbents, (even without passing through the mesenteric glands.) May it not happen that a constituent of the bile acts on some hitherto ill-defined protein-compound of the chyme, and converts it into the form known as uncoagulated albumen?"

OF MILK.

In addition to the general composition and relations of milk we have some particulars given in relation of the modifications which it undergoes in nurses. In regard to the influence of temperaments on the quality of the milk, we have the following observation:

"It has been long believed that the milk of fair women is inferior in its properties to the milk of brunettes. As far as I am aware, the only analyses bearing on this point are those of L'Heretier. He selected two females of equal age, and made them submit to the same diet and mode of life. The following are the results of his analyses:

	A Blonde, aged 22.		A Brunette, aged 22.	
	1.	2.	1.	2.
Water - - - - -	892·0	881·5	853·3	853·0
Solid constituents - -	108·0	118·5	146·7	147·0
Butter - - - - -	35·5	40·5	54·8	56·3
Casein - - - - -	10·0	0·5	16·2	17·0
Sugar of milk - -	58·5	64·0	71·2	70·0
Salts - - - - -	4·0	4·5	4·5	4·5

He appears to have selected the analyses that presented the most marked contrast; for he observes, that if he had taken the mean of all his analyses, the difference between the amount of the solid constituents in the two cases would have been less marked, the average ratio being 120 : 134.

Also:—

Changes in the milk, corresponding with the age of the infant.

It seems probable that certain changes will be observed in the milk when the progress of development of the child indicates the necessity for other food. The question is one of considerable physiological interest, and in order to elucidate it I made analyses of the milk of a woman during a period of nearly six months, commencing with the second day after delivery, and repeating my observation at intervals of eight or ten days.

The results would doubtless be more decisive if the experimentalist were able to exclude all disturbing influences: but in almost all cases the exercise of a strict control over the method of living and the nature of the food of the mother, is just as impossible as the exclusion of exciting moral forces.

The fourteen analyses (the colostrum being excluded) gave the following results:

Analyses.		Specific gravity.	Water.	Solid constituents	Casein.	Sugar.	Butter.	Fixed salts.
66	31st Aug.	1031·6	873·2	126·8	21·2	62·4	34·6	0·84
67	7th Sept.	1030·0	883·8	116·2	19·6	57·6	31·4	1·66
68	8th Sept.	1030·0	899·0	101·0	25·7	52·3	18·0	2·00
69	14th Sept.	1030·0	883·6	116·4	22·0	52·0	26·4	1·78
70	27th Oct.	1034·0	898·2	101·8	43·0	45·0	14·0	2·74
71	3d Nov.	1032·0	886·0	114·0	45·2	39·2	27·4	2·87
72	11th Nov.	1034·5	914·0	86·0	35·3	39·5	8·0	2·40
73	18th Nov.	1033·0	880·6	119·4	37·0	45·4	34·0	2·50
74	25th Nov.	1033·4	890·4	109·6	38·5	47·5	19·0	2·70
75	1st Dec.	1032·0	902·0	98·0	39·0	49·0	8·0	2·08
76	8th Dec.	1033·0	890·0	110·0	41·0	43·0	22·0	2·76
77	16th Dec.	1034·4	891·0	109·0	42·0	44·0	20·0	2·68
78	31st Dec.	1034·0	861·4	138·6	31·0	52·0	54·0	2·35
79	4th Jan.	1032·0	873·6	126·4	40·0	46·0	37·0	2·70

A glance at the three columns of casein, sugar, and butter, will show, that, with few exceptions, 1st, the quantity of casein is at its minimum at the commencement; it then rises considerably, and ultimately attains a nearly fixed proportion; that, 2d, the quantity of sugar is at its maximum at the commencement, and subsequently diminishes; and that, 3d, the butter is a very variable constituent of the milk.

Also:—

Other changes in the milk.

Certain substances which are not included in the ordinary constituents of the milk are sometimes detected in it, after having been taken into the system, either as food or medicine. It is not to be expected that all the substances which enter the circulating fluid and are separated by the kidneys, should be found in the milk, since the absorbents appear to exert a sort of selective power, and would thus reject those substances which occur in the blood, but which would produce an injurious effect on the tender frame of the infant, if they entered into the milk.

I have sought in vain for ferrocyanide of potassium in the milk of women who were suckling, and to whom I have given it in doses of six drachms. This salt is known to enter very readily into the circulation, and is found after a very short interval in the urine. After the lapse of two days I gave the same woman twenty-three grains of iodide of potassium, but I could detect no trace of this salt in the milk. Lastly, I attempted in vain to detect sulphate of magnesia in the milk of a woman who was suckling, and to whom I had administered it in a sufficient dose to act as a laxative.

For the particulars of these experiments I must refer to my essay 'On the Milk of Woman, in its Chemical and Physiological relations.' From these observations I think that I am justified in the conclusion that energetic substances, which are foreign to normal milk, either do not enter into that secretion at all, or if they do, they undergo modifications, which render them more compatible with the organism. Although I could not detect the sulphuric acid of the sulphate of magnesia in the milk, it is very probable that the magnesia entered the milk as a lactate, while the sulphuric acid was carried off by the urine as a sulphate.

Peligot, however, has detected iodide of potassium in asses' milk; and Herberger in the milk of woman. [I have on several occasions observed the ordinary indication of iodine on the addition of xyloidin, or of starch and a drop or two of nitric acid to the urine of infants at the breast during the period of the mother taking three grains of hydriodate of potash thrice a day—a convincing proof that the salt has entered the milk.] Mercurial medicines used by women who are suckling have never been traced in the milk, [although their effects on the infant are undoubted.]"

Milk is observed to undergo some change when allowed to stand sometime after being drawn. Some of these changes are familiar to all, but others are less common and are curious.

"The milk is sometimes observed to become blue on its surface after standing for 24 to 48 hours, and the tint gradually diffuses itself through the whole fluid: the milk has also been observed to turn yellow in a similar manner. Fuchs* has carefully investigated this phenomenon, and has detected in milk of this nature a peculiar infusorium, to which he has applied the name *vibrio cyanogenus;* it is not of a blue colour itself, but it appears to have the power of gradually changing the milk to this tint. When removed from the milk, and placed in an infusion of marsh-mallows, these animalcules increase in size, and communicate a faint blue tinge to the fluid; in this way they may be preserved for a long time. Closely allied to this animalcule is the *vibrio xanthogenus;* they are sometimes found together in milk, and Fuchs had also an opportunity of observing them in milk which had become yellow, a much more rare change than the former."

Our author devotes a large portion of this part of the work to the urine and urinary deposits, but a full notice of the subject would carry us beyond the limits of our review department, and we will limit our remarks to one or two interesting particulars.

It will be remembered, that a few years ago, Nauche announced the discovery of a peculiar substance, which he termed *kystein*, in the urine of pregnant women. The presence of this substance he regarded as an infallible sign of pregnancy, and he has been followed in his belief by a large proportion of the writers on this subject. It was, however, supposed, that *kystein* was only some modified form of albumen. Dr. Kane, however, convinced himself that its occurrence was independent of the presence of albumen, and showed that it occurred in the urine during lactation as well as during pregnancy.

"During the first weeks of pregnancy, Kane only rarely observed it; it was most commonly noticed during the seventh, eighth, and ninth months, and up to the period of delivery. In eighty-five cases of pregnancy it was absent eleven times, and was present in thirty-two out of ninety-four cases examined during lactation.

I have examined the urine during the second, third, fourth, fifth, and sixth months of pregnancy, but have not invariably detected kystein. In the cases in which it was formed, as in the second, fifth, and sixth months of pregnancy, the urine on emission was clear, yellow, faintly acid, and not affected either by nitric or acetic acid, or by heat. Usually, in about twenty-four hours, the whole urine became slightly turbid, the acid reaction disappeared, a white viscid sediment was deposited, and soon afterwards the surface of the fluid became covered with a pellicle at first extremely delicate, but after from twelve to twenty-four hours becoming tough, thick, opaque, and with a glistening appearance in consequence of the light reflected from numerous minute crystals of ammoniaco-magnesian phosphate with which it was studded. On examining this pellicle in its early state under the microscope, it appeared (when magnified 300 times) to consist of an amorphous matter composed of minute, opaque points, such as are presented by sediments of phosphate of lime or urate of ammonia, except that in the latter the individual particles are usually darker, more clearly defined, and larger than in kystein. The whole field of vision was likewise bestrewed with numerous vibriones in active motion, and crystals of ammoniaco-magnesian phosphate. When the pellicle became thicker, precisely similar phenomena were observed, but the vibriones were supplanted

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* Beiträge zur näheren Kenntniss der gesunden and fehlerhaften Milch der Hausthiere. Magazin für die gesammte Thierheilkunde, Jahrg. 7. St. 2.

by a considerable number of monads; on the addition of acetic acid the crystals disappeared, while the amorphous matter remained unaffected. On digesting the pellicle in acetic acid, and adding ferrocyanide of potassium to the filtered solution, a comparatively slight turbidity ensued, but on macerating the pellicle in a dilute solution of potash, acidulating the filtered solution with acetic acid, heating, and adding ferrocyanide of potassium after a second filtration, a more decided turbidity was observed. From these experiments I concluded that a protein-compound was present. The white sediment that occurred after the urine had stood for some days, possessed a disagreeable, pungent, caseous odour: under the microscope it presented the same appearance as the pellicle. After repeatedly washing a portion of the sediment with water, and then heating it with alcohol and a little sulphuric acid, it developed a disagreeable, fruit-like odour, reminding me of butyric ether. [We shall presently show that the accuracy of this observation has been thoroughly established by Lehmann.] It results from the above observations, that kystein is not a new and distinct substance, but a protein-compound, whose formation is undoubtedly and closely connected with the lacteal secretion. From the observations of Kane and myself, it seems to follow that pregnancy may exist without the occurrence of kystein in the urine; if, however, there is a probability or possibility of pregnancy, and kystein is found in the urine, then the probability is reduced almost to a certainty. We are unable to draw any positive inferences respecting the stage of pregnancy from the appearance of the kystein.

A deposit of caseous matter and earthy phosphates was frequently observed by Golding Bird in the advanced stages of pregnancy. The sediment is probably similar to Nauche's kystein.

Every urine left to itself forms a pellicle, more or less resembling that of kystein. If formed soon after the urine is discharged, it consists of earthy phosphates, which, from the urine being alkaline, are, for the most part precipitated, but likewise form a delicate film on the surface. When this is the case, the pellicle is very thin and readily sinks to the bottom. Under the microscope crystals of ammoniaco-magnesian phosphate, and an amorphous matter very similar to kystein, but consisting of phosphate of lime, are observed: this likewise differs from kystein in being soluble in free acids. A pellicle of fat on the surface of urine may sometimes be mistaken for kystein: films of this nature are very thin and usually iridescent, and the microscope reveals the presence of numerous fat-globules.

The membrane formed on the surface of urine six or eight days after emission, usually consists of a species of mould; under the microscope there may be seen innumerable filaments matted together, and interspersed with sporules.

I once observed a pellicle on the surface of a man's urine three days after emission, which both in chemical and microscopical characters presented the closest analogy to kystein.*

[Lehmann † frequently examined the urine of a pregnant woman from the second to the seventh month. It was of a dirty yellow colour, and more inclined to froth than usual; it generally became turbid in from two to six hours; but the morning urine, after standing for thirty-six or forty-eight hours, was always covered with a grayish-white film, which often, in two or three days, sank and mixed with the sediment that formed when the turbidity appeared, but sometimes was a longer period before it broke up. By means of ether he could always remove from this film a considerable quantity of viscid fat, which formed a soap with potash, and then, on the addition of sulphuric acid, developed

* [A similar appearance has been observed by Prout in the urine of a delicate child, fed chiefly on milk. (On Stomach and Renal Diseases, 4th edition, p. 555, note.]

† Lehrbuch der physiologischen Chemie, vol. 1, p. 252.

a well marked odour of butyric acid. On treating a large quantity of this urine with sulphuric acid, and distilling, he obtained, after treating the distillate with baryta water, brilliant crystals of butyrate of baryta. The substance taken up by ether, when gently evaporated with nitric acid and exposed to the vapour of ammonia, was not in the least reddened; with concentrated hydrochloric acid, on the other hand, it assumed a blue tint; dissolved in potash, boiled, and treated with hydrochloric acid, it developed sulphuretted hydrogen; it dissolved tolerably freely in acetic acid, from which it was precipitated by ferrocyanide of potassium. These reactions left no doubt of its being a protein-compound. The portion of the film insoluble in potash consisted chiefly of phosphate of magnesia, [ammoniaco-magnesian phosphate ?] with a little phosphate of lime. Hence Lehmann concludes that the kystein of Nauche is not a new and distinct substance, but a mixture of butyraceous fat, phosphate of magnesia, and a protein-compound very similar to casein. He likewise mentions that, in examining the urine of a woman who was not suckling, and was kept on very low and sparing diet, on the third, fourth, sixth, and ninth days after delivery, he found a large quantity of butyric acid taken up by ether from the solid residue; and on dissolving the ethereal extract in water, adding sulphuric acid, and distilling, he obtained a further quantity. The urine in this case was always rather turbid, of a dirty yellow colour, very acid, and contained a very small amount of uric acid.

Möller* relates two cases in which the urine of women who were not pregnant was covered with a film exactly resembling kystein; in one case there was considerable hypertrophy of the uterus; in the other no affection of the generative organs could be detected. The film of kystein consists, according to his observation, of fat, earthy phosphates, and a caseous matter, which differs, however, from the casein of milk in being held in solution by a free acid. When the urine becomes neutral or alkaline, the caseous matter ceases to be held in solution, and separates as kystein. Every thing checking the decomposition of the urine hinders the formation of the pellicle, and if the recent secretion is treated with a free acid (mineral or organic), no separation of kystein takes place even if ammonia be added to saturation, or decomposition allowed to proceed to any extent.

In a case of decided pregnancy, no kystein was formed during the period of a severe cold, attended with a copious deposition of urates; but when the urine became natural, the kystein reappeared. He twice detected cholesterin in kystein.

Kleybolte† has examined the urine in ten cases of pregnancy, and invariably found kystein on the fifth day. The morning secretion was used, and after being slightly covered to protect it from dust, was allowed to stand at an ordinary temperature, for ten days. The following appearances were observed in the tenth week of pregnancy: urine peculiarly yellow, with a greenish tint. 2d day, mucous sediment; 3d day, no change; 4th day, turbidity ascending from the bottom; 5th day, white points and leaflets on the surface, turbidity ascending from all parts of the bottom, and the sediment almost gone; 6th day, kystein distinctly observed on the surface, like lumps of fat on the surface of cold broth; 7th day, no change. From the 8th to the 10th day, the kystein disappears, the turbidit again descends, and the sediment noticed on the 2d day is reproduced. The nine remaining cases are in most respects similar to the above.

A few observations on kystein have been recently published by Audouard,‡ but contain nothing of importance, except that in six specimens of urine

* Casper's Wochenschrift, Jan. 11-18, 1845.

† Casper's Wochenschrift, April 26, 1845.

‡ Journal de Chemie Méd, May, 1845.

passed by young women suffering from amenorrhœa, he found kystein in five.*]

I shall now give a short abstract of Becquerel's researches. During pregnancy, the general state of the system is liable to great variations, and the urine presents differences of corresponding importance. If good health is enjoyed during pregnancy, the urine remains normal; if, however, any thing should happen to excite the vascular system, it readily changes, becoming dark-coloured, acid, sedimentary, and diminished in quantity. During the latter stages of pregnancy the urine often assumes the anæmic type, that is to say, it becomes pale, contains only a small amount of solid residue, and the specific gravity does not exceed 1011. The observations which were communicated by Donné in a letter addressed to the Academy of Sciences, dated May 24, 1841, in reference to the urine in pregnancy containing less free acid, and less of the phosphate and sulphate of lime than normal urine, were not confirmed by Becquerel. Neither could Becquerel observe kystein."

Many substances, incapable of assimilation, which enter the circulation, are thrown off by the kidneys. Among the inorganic substances, many have been detected, among which are the following, viz.: iodine, bromine, sodium, potassium, and numerous salts, arsenic, antimony, mercury, &c., inorganic acids, as nitric, hydrochloric and sulphuric. Numerous organic acids, bases and their salts have likewise been detected, among which are the active principles of opium, &c., and also quinine. Many of the indifferent organic substances have likewise been detected, and some of these are observed to undergo curious changes in their composition. Of these our author remarks:

"As the modifications that these substances undergo in the organism are of extreme interest, let us see what are the most probable changes that can take place. We select salicin, by way of illustration, as a substance whose chemistry is pretty well established.

Is salicin converted in the organism into sugar and saliretin ?†—a change that occurs on digesting salicin in dilute acids: or is it converted into salicylous acid and water?‡—as occurs on treating salicin with bichromate of potash and sulphuric acid. Or, instead of salicylous acid, is hydrated benzoic acid (which is isomeric with it) produced,|| and the benzoic acid then converted in the ordinary manner into hippuric acid? Or does the salicin yield salicylous acid which appears to be isomorphous with, and convertible into oxide of omichmyle?§ Or, finally, does the salicin undergo the same

* Many other communications have recently been published on this subject, which I do not deem necessary to notice, as they are, for the most part, simply confirmatory of the above observations.

† This change is illustrated by the equation—

$$\underset{\text{Salicin.}}{C_{42} H_{29} O_{22}} = \underset{\text{Sugar.}}{C_{12} H_{14} O_{14}} + \underset{\text{Saliretin.}}{C_{30} H_{15} O_{8}}.$$

‡ Symbolically—$\underset{\text{Salicin.}}{C_{42} H_{29} O_{22}} + \underset{\text{Oxygen.}}{O} = \underset{\text{Salicylous acid.}}{3C_{14} H_{6} O_{4}} + \underset{\text{Water.}}{11HO}.$

|| Symbolically—$\underset{\text{Salicylous acid.}}{C_{14} H_{6} O_{4}} + \underset{\text{Hydrated benzoic acid.}}{HO, C_{14} H_{5} O_{3}}.$

§ It appears from the researches of Scharling that the oxide of omichmyle belongs to a series having a compound radical analogous to that of oil of spiræa, or salicilous acid; at least he found that chloromichmyle is isomeric with chloride of salicyl or chlorosalicylic acid, $C_{14} H_{5} O_{4}$, Cl. Oxidy of Omichmyle does not produce a violet colour with nitrate of iron in the same manner as salicylous and salicylic acids; moreover, salicylous acid and salicin do not enter the urine as oxide of omichmyle, but as salicylous acid, as has been found by Lehmann in eight experiments.

changes as when oxidized by fusion with caustic potash, and become converted into salicylic, oxalic, and carbonic acids, and water?* In sixteen experiments made by Lehmann with salicin in doses of 20 or 30 grains, he never detected saliretin, but always salicylous acid, which was taken up by ether with the oxide of omichmyle, and yielded that characteristic violet tint on the addition of nitrate of iron; in most of the experiments there was also a small quantity of hippuric acid, and of oxalate of lime. Similar experiments have been made by Laveran and Millon.

After taking phloridzin, Lehmann also found hippuric acid and oxalate of lime in the urine. After taking a scruple of thein at bed-time, no trace of it could be found in the morning urine, but the urea was considerably increased, amounting to 58.195 § of the solid residue.† He did not remark any unpleasant symptoms, but two of his pupils, after a similar dose (obtained from coffee) experienced great excitement of the nervous and vascular systems generally, and especially of the generative organs. This is perfectly in unison with Mulder's‡ statement, that it produced abortion in pregnant rabbits.

Under the head of fæces, our author notices some peculiar secretions found in stools, among these are the green evacuations observed in some cases.

"In certain morbid conditions of the system calomel is frequently given in considerable quantity; its administration is succeeded by numerous, very green, bilious stools. I endeavoured to determine by an experiment whether the bile and its pigment is the actual cause of the colour of these evacuations. The fifth stool that was passed after the administration of a large dose of calomel, was made the subject of the analysis. It was fluid, perfectly green, had no fæcal odour, exhibited a mild acid reaction, and showed, under the microscope, a great number of mucus-corpuscles and epithelium-cells. On evaporation it gave off an odour resemblidg that of saliva or extractive matter under similar circumstances. Ether extracted from the solid residue a considerable amount of fat which had an acid reaction, contained cholesterin, and was covered with biliverdin. All other substances which were separated from it by water and alcohol were more or less coloured by bile-pigment.

Bilin with bilifellinic acid and biliverdin were found in large quantity; by digestion with sulphuric acid the bilin became entirely converted into biliary resin. From a quantitative analysis it appeared that 100 parts of the solid residue of this evacuation were composed of:

	Analysis 153.
Green fat containing cholesterin - - - - - - -	10-0
Salivary matter soluble only in water, and slightly precipitated by tannic acid and acetate of lead - - - - - - -	24-3
Bilin with bilifellinic acid and biliverdin, collectively soluble in anhydrous alcohol - - - - - - - - - - - -	21-4
Extractive matter soluble in spirit - - - - - - - -	11-0
Albumen, mucus, and epithelium-scales - - - - - -	17-1
Salts - - - - - - - - - - - - - - -	12-9
	100-0

Scharling hints at the existence of a widely-diffused radical, which, in the vegetable kingdom, in warm climates, is the starting point of the benzoyl and cinnamyl series; in cold climates, of the salicyl compounds; and, in the animal kingdom, presents itself as omichmyle.

* These changes may be thus explained symbolically—

Salicin. Oxygen. Salicylic acid. Oxalic acid. Carbonic acid. Water.

$$C_{42}H_{29}O_{22} + 29O = 2C_{14}H_6O_6 + 6C_2O_3 + 2CO_2 + 17HO.$$

† Lehrbuch der physiolog. Chemie, vol. I, p. 97.

‡ Natuur en Scheikundig Archief, 1839, p. 458.

Various attempts that I made (by Smithson's method) to detect mercury in calomel-stools proved unsuccessful."

Dr. Golding Bird recently published an analysis of the green evacuations so frequently observed in children. The specimen examined by him was passed by a hydrocephalic infant whilst under the influence of mercury. Its composition was as follows:

Water - - - - - - - - - - - - -	900-00
Biliverdin, alcoholic extract, fat, cholesterin, with traces of bile	24-50
Ptyalin, aqueous extract coloured by biliverdin - - - - -	11-25
Mucus, coagulated albumen, and hæmatin - - - - - -	56-00
Chloride of sodium, with traces of tribasic phosphate of soda -	5-50
Tribasic phosphate of soda - - - - - - - - - -	1-75
Peroxide of iron - - - - - - - - - - - -	1-00
	1000-00

Professor Kersten of Freiberg has recently published a paper on the cause of the green evacuations observed after a course of the Marienbad waters for fifteen or twenty days.

The occurrence of these evacuations is regarded as critical and highly favorable. Kersten denies that the tint is in any degree dependent on the presence of bile, and ascribes it to the formation of green sulphuret of iron.

In the paper referred to, he first shows that on the addition of very dilute hydrochloric acid to an evacuation of this nature diluted with thrice its weight of water, there is a development of sulphuretted hydrogen, indicating the presence of a metallic sulphuret; moreover, on the addition of ferrocyanide of potassium to the filtered acid solution a bright blue precipitate is observed, which becomes darker after exposure to the air, indicating the existence of protoxide of iron. This experiment shows that the green pigment is destroyed or decomposed by dilute hydrochloric acid, and further, that it is a compound of sulphur and iron. He accounts for the presence of the sulphuret of iron in this way. The sulphate of soda present in the water is reduced in the stomach to a sulphuret of sodium by the deoxidizing power of the organic matters with which it is in contact, aided by a temperature favourable to such a change. The bicarbonate of iron in the water is decomposed at the temperature of the stomach, and the iron precipitated either as a protoxide or as a hydrated peroxide, and immediately redissolved by the free acid of the gastric juice. This reacts on the sulphuret of sodium, and sulphuret of iron is the result.

Since the publication of Kersten's paper, a very similar view has been propounded by Dr. Bley, namely, that the green evacuations observed after the use of calomel are dependent not on the presence of bile, but of sulphuret of mercury. Unfortunately for this theory the mercury cannot be detected by analysis, and Pettinkofer's test reveals the presence of bile.

Dr. Frankl has published a paper containing various arguments in opposition to Kersten's views, and criticising his conclusions.

Berzelius, on the other hand, writes thus: "It never entered my mind to suspect that this coloration arose from sulphuret of iron, but I always believed that it might be attributed to the black oxide of iron. It is, however, quite natural that as sulphuretted hydrogen is usually produced during the progress of digestion, the oxide of iron present in the intestinal canal should be reduced to a sulphuret, no matter whether sulphates have been given or not."

Berzelius renders Kersten's view more general, observing "that every chalybeate water, whether it contain sulphates or not, produces a similar appearance in the evacuations." On this Kersten remarks: that "the coloration may be most intense when sulphates are present, because by their decomposition during digestion an excess of sulphuretted hydrogen will be generated."

Certain morbid productions of the alimentary canal are likewise ejected sometimes by vomiting, among these none are more singular than the organic productions thrown up in pyrosis.

"The most remarkable of these is the appearance of a microscopic cryptogamic plant (*sarcina ventriculi,*) and of acetic, lactic, and carbonic acids in the liquid. The first case in which these were found, occurred to Mr. Goodsir, and was published by him in the 'Edinburgh Medical and Surgical Journal' for April, 1842. Since that period a case has occurred in the practice of Mr. Benjamin Bell of Edinburgh, who allowed Mr. Goodsir and Dr. Wilson to examine the fluid ejected by his patient, in which the same organism and acids were discovered; and Mr. Busk, of the Dreadnought hospital ship, Greenwich, has published the history of three cases where the *sarcina* presented itself, but no analysis was made of the fluids in which it appeared.

On examining the fluid with the microscope, the sarcina is found to present the following characters.* In every instance the organisms presented themselves in the form of square or slightly oblong transparent plates, of a pale yellow or brown colour, and varying in size from the 800th to the 1000th of an inch. They were made up of cells, the walls of which appeared rigid, and could be perceived passing from one flat surface to anothar as dissepiments. These dissepiments, as well as the transparent spaces, were, from compression of contiguity, rectilinear, and all the angles right angles; but the bounding cells bulged somewhat irregularly on the edges of the organism, by reason of the freedom from pressure These circumstances gave the whole organism the appearance of a woolpack, or of a soft bundle bound with cord, crossing it four times at right angles, and at equal distances. From these very striking peculiarities of form, Mr. Goodsir has proposed for it the generic name of SARCINA.†

The balance of the work is dovoted to the composition, bones, cartilages, muscles, and organs; to concretions, calculi, and some morbid products on which no additional light seems to us to be thrown, and we will therefore omit their notice.

We will conclude our crude notice of this work by recommending it most cordially to those members of the profession who wish to obtain a more extended knowledge of this subject than can be had from text books. It embodies an immense mass of matter, the result of careful analysis and extended research, as well as many interesting and important remarks on physiology, pathology, and therapeutics.

W. M. C.

III.—*Experimental Researches on the Post Mortem Contractility of the Muscles, with observations on the Reflex Theory.* By BENNET DOWLER, M.D. (Re-printed from the New York Journal of Medicine, for May.) New York: R. Craighead, printer.

This work, as its title indicates, is a reprint from the pages of one of our contemporaries. It will also be seen by the title, that it consists of two parts; one experimental, the other speculative. All additions to the facts of medicine and its associate sciences must, and even will be re-

* The reader is referred to the 'Edinburgh Medical and Surgical Journal' for April, 1842, for a more minute description of the sarcina, and a detailed account of the chemical analysis of the liquid containing it.

† *Sarcina*, a pack or wool-pack.

ceived with thankfulness; and Dr. Dowler has certainly called attention to some very curious phenomena which may be produced after death, upon the muscular system; but particularly after death from yellow fever. We are sorry to say, however, that we cannot give much praise to the speculative portion of the work. There is a good deal of reading displayed, many sneers and objurgations indulged in—and a great many subjects touched on and impressed into service, which one would have thought had been quite out of hearing, but we have very little logic visible throughout the whole. For our very industrious and estimable townsman, we have the highest respect, both for his talents and character, and it was with surprise and regret, that we were called to read a medico-poetical rhapsody, rather than a philosophical essay.

The author seems to be particularly anxious to overthrow the reflex theory of nervous action, attacks it in very many ways and phrases; and unquestionably thinks, that the result of his experiments are entirely irreconcilable with the theory; that in short, they completely overthrow it, and leave it in its death agonies. In order that our readers may judge of the correctness of such a conclusion, we give the entire experiments as published, together with some observations of the author, elicited by them:

Miscellaneous cases illustrative of the general phenomena of post-mortem Contractility:—

Case I.—O. D., an Englishman, aged 26. From fifteen minutes to several hours after death, raised his fore-arm to the perpendicular as often as its flexors were struck; the motion was slow, the ratio of contraction and relaxation appeared uniform, occupying about half a minute.

Case II.—E. M., an Englishman, aged 37. At thirty minutes after death, afforded similar results. The experiments were often repeated, at prolonged periods; the contractions and relaxations were supposed to occupy about 30 seconds each. The flexion ceased at the perpendicular in every instance. When the experiments ceased, the muscular force continued at its maximum, having lost nothing.

Case III.—O. S., an Englishman, aged 27. Like the last.

Case IV.—W. O., an Englishman, aged 27. Dead five hours; neck moderately stiff; brain, 93°; epigastrium, 100°; chest, 93°; thigh, 99°. The arms strongly contractile, but the motion was slow and uniform, requiring several seconds to produce flexion and relaxation.

Case V.—H. O., a German, aged 32. Presented one hour after death, active muscular contractions of the arms, which soon disappeared after a few blows, the neck being stiff; other parts flexible.

Case VI.—J. Q., a German, aged 24. Fifteen minutes to one hour after death, everywhere flexible; powerful muscular contractions; the neck became stiff; the legs and arms soon after.

Case VII.—S. G., a New Yorker, aged 22. Precisely like the last case.

Case VIII.—J. M., a Virginian, aged 31. Like the last.

Case IX.—J. R., a Marylander, aged 25. Was observed for one hour after death, during which time the biceps, at intervals, contracted vigorously.

Case X.—D. H., a German, aged 22. For more than an hour after death, carried his hand to his nipple as often as the extensors were struck—the jaws being rigid; 109° to 106°, for two hours.

Case XI.—Miss S., aged 26. An hour after death, presented slight rigidity of the arm; one was extended; a blow caused it to rise quickly to the perpendicular, from which it slowly returned, requiring many seconds, perhaps a minute or two. Three similar operations destroyed the contractility. The fingers soon became stiff, then the arms, next the legs, but the neck was limber for three hours. The heat generally 107°, descended in the axilla, at four hours after death, to 103½°.

CASE XII.—F. L., a Frenchman, aged 58. Dead about one hour; heat 107½° to 103°; possessed the contractile function strongly, carrying the hand to the breast; also performing pronation and supination according to the percussion. The abdominal muscles were rigid.

CASE XIII.—This subject had been dead two hours; he lifted his arm quickly and often, several feet from the table, sometimes striking himself in the face with force.

CASE XIV.—J. A., an Irishman, aged 39. Dead three to four hours; heat 107° to 103°; neck very rigid; arms highly contractile.

CASE XV.—J. G., a Baltimorean, aged 24. An hour after death, like the last; 108°.

CASE XVI.—J. P., a Scotchman, aged 22. Dead one hour and a half; raised his fore-arm perpendicularly twice.

CASE XVII.—R. C., a Kentuckian, aged 25. In two hours after death, when the arm was extended to an angle of 45° from the trunk, and was struck with my hand, or still better, with the side of the hatchet, carried his hand to his epigastrium; but when the arm was extended upon the floor, so as to form a right angle with the body, he slapped himself upon the mouth and nose. The contractility began to decline in the third hour, and by the fourth hour all motions of the limbs ceased, though the pectoral muscles assumed the ridgy or lumpy form when percussed. An hour after death the thigh was moderately contractile. The leg hung down near the floor; its flexors, after being struck, drew up the heel against the buttock. Heat for seven hours, from 111° to 102°. Five hours after death contractility ceased, and rigidity prevailed.

CASE XVIII.—J. H., a Virginian, aged 30. Bled enormously, and cupped much; dead a few minutes; had contractility less than a quarter of an hour; dragged a hatchet along the floor, not being able to raise it. The heat, which was usually low, continued as long as observed, that is one hour and a quarter, without varying materially, not exceeding 102½°.

CASE XIX.—N. E., an Italian, aged 25. Dead about one hour; heat 107° to 106°; recti muscles had contracted into indurated masses like knots of wood; neck rigid; the arm raised a weight, say 2 to 3 lbs. fastened in the palm, carrying the hand to the umbilicus several times.

CASE XX.—*Remittent.*—Mr. S., aged 45. Dead two hours; legs becoming rigid; struck the flexors of the arm with the inferior edge of my hand; the cadaver raised his arm with a regular, slow movement, placing his hand upon his breast: as soon as the muscles relaxed, he carried his arm back, extending the same. The experiment was repeated three or four times, when the arm fell back exhausted. The blows were now made with a piece of wood. The biceps gathered up into a lump, at the place where the blow was given, but failed to move the fore-arm.

Without attempting to give all the details of post-mortem contractility, it may be necessary to enumerate a few of its leading features.

Béclard asserts, that the most remarkable and best established fact in the muscular action is, the shortening of the muscle during contraction; that its swelling is caused by its shortening, each compensating the other mutually. [*Gen. Anat.*

It is natural to every muscle, says Haller, to shorten itself, by retracting its extremities towards its belly or middle; when in action, becoming shorter and thicker (cccci.). Whether this doctrine be true of galvanic muscular contraction, I do not know, but I am certain that it does not apply to post-mortem contraction,—because it often happens that both the fore-arm and elbow-joint are stiff and immovable, while the biceps may be powerfully contractile, its belly swelling up into a hard lump; relaxing and contracting repeatedly, and unavailingly, owing to the rigidity at and below its insertion in the arm. Here, it is evident that the indurated swelling is not owing to the approximation of the two extremities of the muscle, for they are fixed. The increase of volume seems to be owing to an expansion with rigidity among its elementary fibres; a zigzag oscillatory motion upon the summits of the contracting masses is visible to the naked eye. Much evidence might be produced besides the following:

Cases of Post-Mortem Muscular Contraction without shortening in Yellow Fever Subjects.

Case XXI.—H. F., a German, aged 28. Dead four hours, and dissected. The fingers and toes flexed and rigid; the biceps and gastrocnemius contracted, but without causing motions, as the limbs were rigid at their extremities.

Case XXII.—R. P., an Irishman, aged 24. Dead ten hours; fore-arm rigid; the biceps contracted strongly, but the rigidity in and below the elbow-joint prevented flexion. Blows with the knuckles over the pectoral muscles caused them to heave up into knotty masses.

Sometimes, post-mortem *rigidity sets in during the paroxysm of contraction*, producing a very singular phenomenon, a hard mass, which, continuing for hours, feels like bone. I have known this to be mistaken for a fracture badly set, or for a bony tumor.

Case XXIII.—J. C., an Irishman, aged 26. From one to three hours after death, the arms performed complete flexions and extensions; heat from 109° to 102°; six hours after death, the arms could not be extended without cutting the muscles, which were indurated and elevated at the site of the blows, having assumed the same arrangement as at the maximum of contraction.

Case illustrative of the Increase, Declination, and subsequent Resuscitation of Post-Mortem Contractility; all being Yellow Fever except the last.

Case XXIV.—From four to five hours after death, and two hours after dissection, and the removal of the entire viscera, the *cadaver* being warm and rigid, great force was found necessary to extend the arm to a right angle with the body—extensions, flexions, and frictions were used, after which, a blow caused the fore-arm to rise until the hand pointed to the zenith; the motion was slow and equable; a second blow caused a slight motion without elevating the arm.

Case XXV.—A. G. G., a New Yorker, aged 30. In a few minutes after death, presented but feeble contractions, which, in half an hour ceased for a time. But soon after the contractile function returned to the same arm with much force, but after repeated blows it was exhausted a second time. Again, after a similar interval, it returned with a like force a third time. In three hours the rigidity, beginning in the neck, extended itself to most parts of the *cadaver*. The blows were found to have caused well-marked contusions, cellular ecchymoses, etc.

Case XXVI.—C. L., an Irishman, aged 31. Was observed from five minutes to an hour after death; the contractility was found to be active, but after appending a weight of 2 to 3 lbs. to the hand, the arm was unable to raise the weight more than twice; a third blow caused the biceps to be convulsed; it gathered up in a knotty heap, which by forcible extensions and frictions, was, at length, removed temporarily, but in half an hour it returned, and was unchanged as long as observed, being to the touch like a bony tumor.

Case XXVII.—C. S., a German, aged 50. Observations from fifteen minutes to two hours after death; heat from 109° to 102°; the contractility, though strong, was extinguished in the right arm by five or six blows. The left arm, about two hours after, was somewhat rigid, but frictions and extensions caused the contractile function to revive again.

Fracture of the Leg—Gangrene—Death.

Case XXVIII.—J. A., a German, aged 33. Dead two and a half hours; winter, room $54\frac{1}{2}^{\circ}$; a single blow with the flat side of the hatchet produced flexion, but exhausted the contractile force for half an hour, when it slightly returned. The supinators and pronators acted for an hour longer than the flexors of the arm; the experiments ending five hours after death.

The Destruction of the Contractile Function in one arm does not affect the other; this rule has no exceptions, and is illustrated in the following Yellow Fever Cadaver:

Case XXIX.—S. F., a Cincinnatian, aged 24. Dead half an hour when the experiments began. The right arm was struck with the flat side of the hatchet, the hand was carried to the ear; the second time to the mammary region; a third to the perpendicular; a fourth caused but a slight motion, and two more, more severely laid on, completely killed the contractility, leaving the print of the instrument as upon dough. In an hour after, the left arm was found to be contractile as the right had been, if not more so. The thigh, two hours after death, and an hour after the removal of the viscera, gave nearly 107. Other cases illustrative of this point are deemed superfluous.

If several blows on the same spot follow each other rapidly, there is but one contraction, but they exhaust the contractile function more than a single blow; if the force be greatly augmented, the contractility may be killed, almost immediately, in the muscle struck, without impairing the action of any other part.

This force has no resemblance to that purely physical property of bodies, called elasticity. A man may stand on the hand of the cadaver while the blow is being made on the biceps; let him then step off the hand, and the contraction will follow as usual. If the power be feeble, the relaxation will be quick in most cases; the arm will fall back upon the plane from all points short of the perpendicular, each elevation being less than the preceding one, until the force is expended. Should the hand pass its meridian, gravitation will of course aid the contraction in bringing it to the trunk.

The blood has no appreciable influence upon post-mortem contractility, because when the limb is severed from the trunk and drained of its blood, its action is not thereby diminished. Hence the following statement by Sir Charles Bell, in his work on the hand, requires, at least in the dead subject, some modification:—

"The muscular tissue has a living endowment, a power of contraction and relaxation, which is supplied by the circulation of the blood, the source of all vital power."

In some instances, as in the following, great losses of blood before death were not preventive of the most intense post-mortem coutractions.

Much Ante-Mortem Hemorrhage, moderate Post-Mortem Caloricity, with powerful Contractions:—

Case XXX.—M. D., a female, aged 30. Dead half an hour; had an enormous sub-mammary effusion of blood, forming a bed of two feet in circumference, amounting to several pounds; the fore-arm was raised, and the hand brought to the umbilical region after each blow; in three hours slight rigidity was noticed in the neck; in five hours, when burial took place, every joint not included in the neck, was perfectly flexible. The axilla and vagina did not range about 100°.

The difficulty, perhaps I should say impossibility, of showing any exact and necessary connexions between cadaveric contraction, flexibility, and temperature, will be best illustrated by giving an outline of a few cases. It will be seen that contractility may be present where the heat is, at least, very moderate, and where it is greatest for seven hours after death, that rigidity may take place much in advance of the maximum heat. Rigidity is at first local, often irregular, and sometimes absent as long as looked for, and may possibly never take place, as I might more fully show were it necessary. Contractility may take place during the rise or fall of the temperature, indifferently and in one region, while rigidity may prevail in another, and is sometimes absent in cases where great heat and flexibility are present.

The following are Cases illustrative of Contractility in connection with Post-Mortem Calorification and Partial Rigidity; the maximum and minimum temperatures being included:—

Case XXXI.—J. K., a Philadelphian, aged 25. In fifteeen minutes after death presented the contractile phenomena in its most intense form, but declined wholly in one hour, the body being everywhere flexible, in half an hour after rigidity set in. This body, which before death had been remarkably cold, had a temperature after death as high as 109°, and which did not refrigerate below 104° in three hours after.

Case XXXII.—L. S., a Swede, aged 48. In three hours after death, was rigid in his legs, neck and jaws. The arms had lost nearly all contractility, while the axilla and perineum, each, gave 109°; at five and a half hours the heat was still 103°, long after rigidity had become general.

Case XXXIII.—J. H., an Irishman, aged 36. Of gigantic frame, estimated at 200 lbs.; in fifteen minutes after death, the neck, jaws and recti muscles were immovably fixed—the contractility of the arm being very powerful. In an hour and ten minutes the rigidity became universal, the thigh being at 107°. In one and a half hours the epigastrium was at 106½°, and the brain 101°.

Case XXXIV.—S. G., a New Yorker, aged 22. In fifteen minutes after death, presented contractility in its most active form; the entire body being flexible, but soon after the neck became stiff, then the legs, arms, etc. The temperature in the axilla and thigh exceeded 107°; in an hour and a half it was nearly the same; in three hours the thigh was 100°.

Case XXXV.—J. Q., a German, aged 24. In three-fourths of an hour after death was universally flexible, and contractility was powerful, temperature 109°; in an hour rigidity took place. In two and a half hours the heat was nearly 107°.

Cases without Contractility, showing the temperature to be high with or without flexibility.

Case XXXVI.—J. S., a German, aged 29. One hour after death, rigid in the neck and legs, moderately so in the arms; axilla, chest and epigastrium, each 107°, thigh 104°; in about four hours universal rigidity, thigh 105°.

Case XXXVII.—C. B., a New Yorker, aged 23. In one hour after death, neck rigid, legs and arms moderately so; universal rigidity in two hours; the thigh did not reach its maximum, 104°, until sixteen minutes after.

Case XXXVIII.—M. H., a German, aged 32. In about fifteen minutes after death was everywhere flexible; in half an hour the limbs, which were without contractility, became stiff, the thigh being nearly 107°; two hours later, the latter was 106°.

Case XXXIX.—G. L., a German. At two hours after death had flexibility of the neck and rigidity of the limbs and recti muscles, with a heat of 104°; in four hours the body, excepting the neck, was very rigid, the heat being only one degree less in the centre.

Case XL.—J. F., a female. In two hours after death was extremely rigid in the superior extremities; the neck, legs, etc., being limber; the heat being 104°, and in an hour after 105°; at three and a half hours, neck limber, legs, etc., stiff; heat 104°, and at the fifth hour 102½°; at the seventh hour the thigh was 100°, the body rigid, and the neck still perfectly limber, and as at first.

The continuance of, or rather the degree in which post-mortem heat is evolved bears no proportion, I repeat, to the intensity of post-mortem contraction. The great heat developed in the dead body, I have endeavored to illustrate in the medical journals of our country, and will not, therefore, dwell upon that subject at present. I find, however, on examination of the original papers not yet published in detail, that for the most part, when the heat had declined, the contractility was exhausted, but that the presence of great heat, ranging as high as 113°, did not by any means imply the presence of contractility, nor the absence of rigidity. Authors seem not to have been aware of the augmentation of animal heat after death; some have, it is true, noticed an increase of heat after death from cholera, compared with the extreme coldness of the surface during the last hours of life; but has any one hinted, that this post-mortem heat ever rose as high as even the healthy standard, to say nothing of 14° or 15° beyond that? I mention this merely to explain what MM. Orfila and Nysten mean by *animal heat*, when they assert "that rigidity is incompatible with its presence.—(*Todd's Cyc. Anat. & Phys.*) Dr. Beatty advances the same doctrine (*Cyc. Prac. Med.*); so does Béclard (*Gen. Anat.*), not to name many others. Nothing can be more erroneous. The heat often increases with the rigidity.

*Post-mortem contractility, in the human cadaver, has no connection with or dependence upon the spinal marrow. This may be received as an axiom, though directly opposed to the reflex theory, and is easily proved by amputating the shoulder in a proper manner.** In performing this operation, it is best to leave a few portions of the skin undivided, so as to tie the arm down to the trunk, so that the former during action may not turn quite over. I select the following yellow fever cases for illustration.

Case XLI.—A. M., a German, aged 30. More than four hours after death, and one hour after dissection, possessed the contractile function in a high degree. An arm was amputated so as to include the scapula and its muscles; after which, blows were repeatedly directed upon the flexors of the fore-arm, which was often elevated to the perpendicular; one of the inferior extremities, an hour after amputation at the hip-joint, being generally rigid, presented, nevertheless, contractility in the form of massive ridges at the places of contact from blows.

Case XLII.—P. L., an Irishman, aged 20.—Dead four hours before the experiments were begun. Both fore-arms raised themselves to the perpendicular several times. The left arm was amputated so as to include the scapula and its muscles. Twenty ounces of blood, by estimation, were rapidly discharged by the operation. The temperature had ranged for more than four hours in different regions, at from 34° to 33° of Réaumur (nearly 109° to 106½° of Fah.). The arm was more contractile after than before its separation from the spinal marrow, rising after each blow without any loss of motor force as long as observed.

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* We have Bentham's authority, "that no man doth wrong for wrong's sake;" but still, it would seem, that some will argue for argument's sake. It has been said that the biceps, after cutting off the shoulder, and all the accessible nerves of the arm, and all the muscles not devoted to flexion, *still has nerves* blended with its texture. Hence its contractility! Now, at the least, this separation of the spinal marrow and division of all the nerves, ought to lessen in a corresponding ratio the force and duration of the contractile function. Yet this never happens. The nervous connexions and influences reduced to zero, leave the muscular force at its maximum. Defeated in the spinal reflex action, these men substitute some invisible nerve! Drowning men catch at straws. Alas. for reflex logic!

Case showing that Amputation of the Shoulder, and the subsequent Division of all the Nerves, Vessels and Muscles of the Arm, except the perpendicular Flexors of the Fore-arm, do not in the least impair the contractile function either in force or duration:—

Case XLIII.—J. G., an Irishman, aged 32. Dead three hours; dissected one hour; viscera all removed from the body; the right arm was extended, and slapped with a piece of shingle, the force being scarcely sufficient to injure a living person. This blow was repeated at intervals five or six times; each time the man struck his hand against his shoulder with great celerity and force. A heavy hammer was then tied in the palm of the hand, but even this failed to counteract the muscular force; he struck himself heavily against the breast. A hatchet, estimated at $2\frac{1}{2}$ to 3 lbs., was substituted for the hammer; with this he struck the breast several times, when the contractility suddenly ceased. One hour after this, and more than four after death, both arms, including the scapulary muscles, were amputated, discharging enormous quantities of blood—hot blood—for the post mortem temperature had been ranging from 36° to 34° of Réaumur (or 113° to 109° of Fah.); the left arm was extended on the table, resting on the hand and on the olecranon, on the angle of the scapula and the sternal end of the clavicle, presenting the deltoid uppermost. The most active contractions followed every blow, the arm twisted about like a wounded snake, the muscles of the scapula were convulsed. The shoulder having lost its *point d'appui* upon the trunk, the whole arm sometimes turned over, so that the deltoid rested on the table. Gradually, as in the former case, the contractility began to decline. I now severed all the muscles of the arm, in its lower third, except the biceps and brachiales, including all the accessible nerves and vessels, but I found the contractility as active as before. It was, of course, moderate from the previous exhaustion. The amputation at the shoulder, the cutting of all muscles, etc., but the flexors had not impaired the contractile functions in the least. The amputated limbs remained very warm about three hours."

It will be seen from the above experiments, that the following *facts* are developed:

1. That percussion on the muscles, from within one to fourteen hours after death, will excite muscular contractions in many yellow fever subjects.

2. That the contractions of the muscles may or may not cause motion of the limbs.

3. That severe contusions destroy the contractility of the muscles.

4. That the destruction of the contractile functions in one set of muscles does not affect other muscles not subjected to percussion.

5. That great losses of blood before death do not impair the contractility of the muscles—at least in some cases.

6. That as rigidity sets in contractility disappears, but the heat of the body generally rises at the same time.

7. That the muscles of the arm will contract when smartly struck, even after the arm has been removed from the body.

We believe the above is a fair summary of the most material facts elicited by the experiments of Dr. Dowler. They are curious and interesting, but how they bear upon the reflex theory, we are at a loss to divine. Our author seems to have laboured under some strange delusion with regard to the reflex theory, or he certainly could not have written such a sentence as the following, put in italics by the author, for the purpose of emphasis, but of which we shall only put in italics the portion to which we would call attention: "Post-mortem contractility, in the human cadaver, has no connection with or dependence upon the spinal marrow. This may be received as an axiom, *though directly opposed to the reflex theory, and is easily proved by amputating the shoulder in a proper manner*." That muscular contractions in an amputated limb are not caused by reflex action is certainly very true, but how such facts are opposed to the reflex theory remains to be seen.

The muscles may be excited to contraction in various ways: by volition; by mechanical or chemical agents applied to the spinal cord; by direct impressions upon the compound nerves; and by direct impressions upon the muscles. It is evidently to this last class that the contractions, produced in the experiments of Dr. Dowler, belong.

It was this capability of contracting when removed from the body, that induced Haller to maintain, that the contractility of the muscles did not depend upon the nervous system, but on a peculiar property resident in the muscular tissue, and which he termed "irritability.' But we thought it had been long acknowledged, that the dispute was an idle one, as the nervous matter cannot be separated from the muscular fibrils, so that the latter could be acted on solely. At any rate, no person that we know of has ever denied, that muscles removed from the body might be excited to contraction by galvanism or some other stimulus. That "the destruction of the contractile function in one arm does not affect the other" is, therefore, not at all wonderful; nor is it at all surprising, that the contractions should cease after the muscles have been disorganized by heavy and repeated blows.

But, besides the modes above mentioned, and some others, that it would be out of place here to consider; there is another way in which muscular contractions may be excited. These contractions are excited in a peculiar manner, they are going on constantly during life, are absolutely necessary to our existence and comprise a class of facts, to the observation and study of which, Marshall Hall first directed the attention of the profession and in doing so, acquired a very enviable and deserved reputation. These contractions are produced not by volition, nor *direct* impressions of mechanical or chemical agents on the cord, nerves, or muscles, but by *indirect* action on the cord; from it on the motor nerves, and finally on the muscles. Divested of the cobwebs of hypothesis and pedantry in which it has been entangled, the reflex theory is a mere expression of facts—facts derived from observation and experiment. These facts may be thus stated: *in certain cases* impressions on the organs of sense—or rather on the nerves of sensation, cause a change in the nervous substance, which change is transmitted by the nerves to the spinal cord. The condition of the cord is changed, not partially but throughout;* certain motor nerves are in turn affected; their affection is transmitted to certain muscles, and they contract in consequence. These actions may or may not be accompanied by sensation. The quotations cited by Dr. Dowler, from the Medico-Chirurgical Review, are too general in their tenor, and it is on that account I have put the words "in certain cases" in Italics.

Such is the reflex theory, and Dr. Dowler is entirely in error when he

* The experiments of Volknaer prove this.

writes, that "the reflex school maintains, not only that the integrity of the spinal cord is indispensable to transmission, but that the division of the anterior roots is a complete barrier to muscular motion."

We need not go abroad or resort to experiments for exemplifications of the reflex theory, for the series of phenomena as above detailed is going on continually in our own bodies; it keeps up our respiratory movements, particularly in sleep;—it is exercised every time we swallow. What points in common have such phenomena as the muscular movements in respiration, parturition, sneezing, hiccough, etc., with the contractions excited by the blow and slaps given in the experiments of Dr. Dowler? Surely, our author, during the course of his life, has taken a pinch of snuff, and had a good sneeze therefrom. How can he account for the actions of the abdominal muscles in such a phenomenon? Surely, not by the *percussion* of the snuff on the schneiderian membrane.

There are many other things in this Essay that we object to, but we are obliged to confine ourselves to the chief purpose for which it appears to be written. J. H.

IV.—*The Influence of Tropical Climates on European Constitutions.* By James Johnson, M. D., Physician to the late King, &c.; and James Ranald Martin, Esq., late Presidency Surgeon, &c. From the sixth London Edition, with Notes by an American Physician. New York: Samuel S. and William Wood, 1846; 1 vo., pp. 624.

This work has filled so large a space in the eyes of the medical profession, and enjoyed such an exalted reputation for a quarter of a century past, that it would be an act of superogation to enter upon a minute analyses of its contents at this day. With a remark about *tantamount to this*, it appears that all the late Medical Journals have ushered the present edition to the notice of the profession, deeming it unnecessary to say more; but, for *ourselves*, we cannot permit the occasion to pass without the expression of a more full, free and decisive opinion as to its real merits, and the influence it has had upon British and American practice since it first appeared.

Who has not read "Johnson on Tropical Climates?" And where is the Southern physician, in whose library, be it ever so scant, it has not occupied the place of a *text-book?* Written in the animated and fascinating style peculiar to its young, talented and enthusiastic author—abounding in bold and novel observations carefully made in the dangerous field of *actual service*—and combining, moreover, admirable critical analyses of the ablest productions on Tropical diseases—the work was every where welcomed with gladness and devoured almost with the eagerness of a romance. It appeared, too, in the great *Revolutionary Era*, when the human mind, bursting asunder the fetters with which it had long been bound by the influence of ancient systems and principles, was boldly launching upon unknown grounds and untrodden paths, in search of scientific truth. The revolutionary spirit of the age was as strongly displayed in Medical Science, as in politics or religion. No *system*, however consecrated by the lapse of time, could stand the *fiery ordeal*, unless founded in true philosophy; and the associate sciences had recently made such immense progress, that no medical author could have written a quarter of a century previously without the most glaring errors or defects. The systems of Bœrhaave and Stahl, Cullen, Brown, Darwin, *et id omne genus*, had been unconditionally condemned; and the young and brilliant star of Broussais, had just risen in the ascendant. This bold and eloquent innovator had just proclaimed from the metropolis of Europe the most beautiful, plausible, and scientific system of Medicine that had ever been offered to the world. The rising generation of the profession was completely captivated by its simplicity, and the apparent demonstration of its truth afforded by *post mortem* research; whilst those more advanced in years and knowledge, were not unwilling to accept any rational or plausible clue to the mysteries of pathology and therapeutics. Yet there were many who would not yield assent to the *new doctrines;* some from stubborn adhesion to preconceived opinions, and others because they did not think the explanations sufficiently satisfactory, or substantiated by experience. Among these latter was Dr. Johnson, who was from the first a formidable opponent of the *Physiological Doctrines*. Johnson never aspired to be the *founder of a system;* but in the morning of life, with a bold, discerning, and highly cultivated mind, he went forth into the field of practice, determined to examine fairly the doctrines and precepts of the schools and books, and to support none that could not bear the test of experiment. He soon discovered the utter fallacy of relying upon *authorities* in practical medicine, and then determined to observe for himself, and boldly to act upon the dictates of his own judgment. When he first encountered the Remittent Fever of Bengal, Dr. Johnson says: "All things considered, I thought myself fortunate in having in my possession, the works of two celebrated authors, (Clark and Lind), containing a full account of this fever, drawn from personal observation on the spot. I accordingly studied them with great attention. In short, I was quite ready to grapple with this Hydra disease, and show the power of medicine over this scourge of Europeans. Many days did not elapse before I had an opportunity of trying my strength against so formidable an opponent, and a very few trials convinced me I had calculated without my host, and that I must use other weapons than those furnished me by Drs. Lind and Clark, if I meant to be victorious in the contest:" (p. 103.) Dr. Johnson treated the first violent case that fell under his care according to the plain and easy directions laid down by Dr. Clark, but, to his astonishment and mortification, his patient died on the third day. A *post mortem* examination convinced him "that it would have required some ingenuity to devise a more injudicious mode of treatment than that which I (he) pursued." He continues: "But it taught me an important lesson—it opened my eyes to my own folly, and *pace tantorum virorum*, to the oversights of my teachers."

As before remarked, Dr. Johnson now commenced to observe for himself, and to study carefully the influence of a Tropical Climate upon European constitutions, as well in health as disease. The result was the formation of original theories on the production of diseases incident to the South, and the adoption of a novel and bold practice which has been admired and received in every Southern region that acknowledges allegiance to the British or American flag, and the consequences of which have been of the most serious and important nature. It is of this *theory and practice*, founded by Dr. Johnson, that we propose now to speak, and to show the grounds of our conviction, that it has been "weighed in the balance and found wanting." Nay, we deem it our duty to go farther, and to express the deliberate opinion that no *theory and practice ever promulgated have been followed by more baneful consequences.* This is the work to which may be mainly attributed the great *mercurial practice* that became so general throughout the South. Dr. Rush had given a wonderful impulse to the free use of Calomel, by his treatment of the Yellow Fever of Philadelphia, in 1793. He gave it in repeated *ten grain doses;* but Dr. Johnson gave it by the *scruple*, and proclaimed the *fatal principle*, that no Remittent Fever could withstand the *constitutional effects of mercury*—he considered the patient *perfectly secure after ptyalism was established.* This might be accounted for in different ways. Dr. Johnson did not contend that the entire beneficial influence arose from the specific effects of mercury on the salivary glands, but rather that the ptyalism was only the evidence that the secretions throughout the system had been "unlocked" by the remedy; thus opening the "flood-gates" for the discharge of morbific matters. Others, however, accounted for it in a different manner, viz: *that the system could not maintain two distinct diseases at the same time, and that by establishing the mercurial affection as soon as possible, the original fever was compelled to give way.* Hence, they determined to "*throw in the mercury*" (as some of the old writers remarked more fortunately of Peruvian bark), as rapidly as possible; but who can give any adequate description of the awful results? Upon the heels of Rush and Johnson, though differing from both, came the bold and daring Professor Cooke, of Transylvania and Louisville, who sometimes ventured to administer from *the half to an ounce of calomel at a dose.* This was the *ne plus ultra* of medical folly; and, fortunately for mankind, the doctrine has been finally, though only recently, completely exploded. Dr. Cooke had no confidence in the specific effects of mercury in the cure of fevers, but relied on it *solely as a cholagogue cathartic.* The best part of Johnson's practice consisted in his *liberal use of the lancet;* thereby rendering much less calomel necessary, to produce the desired ptyalism; but his followers lost sight of this; and, carried away by his extravagant praises of mercury, they placed their main reliance upon it.

In the first case of ardent remittent fever (before mentioned) that fell under the care of Dr. Johnson, in Bengal, he expresses his disappointment and chagrin at seeing his patient die on the third day, notwithstanding his implicit obedience to the instructions of Clark and Lind, in giving purgatives and Peruvian bark: how many hundreds of young physicians in the South-Western United States alone could attest similar disappointment and mortification on seeing their patients die of bilious remittent fever, *after following the plain and easy directions of Dr. Johnson, and having ptyalism fairly and fully established?* Nay more, how many have found it impossible to arrest the deleterious effects of the remedy, after the original fever had been completely subdued? Hundreds have been thus hurried to untimely graves, whilst thousands have been made to linger out a miserable existence. But let us not dwell upon these unpleasant recollections; let us rather congratulate ourselves that *they are but recollections of errors and follies long past.* We have now arrived at a more rational and successful pathology and therapeutics, yet much remains to be discovered in regard to the causes, nature, and treatment of Tropical fevers. This task is imposed upon *Southern* physicians, and must be performed by them, or remain unaccomplished. It is particularly from the young, vigorous and enthusiastic members of the profession, that we must expect contributions; for, as Dr. Johnson justly remarks in his analysis of Dr. Annesley's work on the "diseases of India," "unless observations are made in the vigour of life, by medical men in hot climates, they will never be made at all! After a certain number of years, in hot as well as in cold climates, the current of zeal, in the minds of medical practitioners, is too often dried up or frozen up—and it is replaced either by a sharp look out for the 'main chance,' or a settled resolution to take things easy—to enjoy the short span of existence with as little incumbrance as possible—and to leave to others the trouble of observing for themselves, as they themselves were obliged to do:" (267).

The present edition of this work may be considered more in the light of a compilation of the labours of the latest and ablest writers on the diseases of Tropical Climates, than of an original work—and hence its chief value as a book of reference.

Dr. Johnson's theory of the origin of fevers, dysenteries, &c., is very simple and very pretty, yet, it has never given entire satisfaction. It is too summary in its explanations. He attributes all the phenomena to "*a derangement in the balance of the circulation and excitability*" produced by both "the remote and exciting causes."

"The manner *how*, and the reason *why*, these various causes, predisponent and exciting, act on the human frame, producing the phenomena of fever, are equally inscrutable as the manner *how*, and reason *why*, tartrite of antimony should have a tendency to act on the *upper*, and aloes on the *lower* portion of the alimentary canal. Let any person demonstrate the *modus operandi* of these two simple substances, and then I will engage to demonstrate the *modus operandi* of human and marsh effluvia. The nature or essence of many of these causes themselves, is totally beyond our comprehension at present. Some of them are even *ideal*, as the various depressing passions, &c. Yet we must not cease to investigate the effects."—(91.)

We must think he has attributed too much importance to the various sympathies denominated by him cutaneo-hepatic, cutaneo-gastric, &c., excited and disturbed by vicissitudes in the weather. Nothing is more demonstrable than that people may enjoy excellent health in all climates, and all sorts of weather. We are therefore compelled to conclude, when epidemics prevail, that some mysterious poison has been generated in the atmosphere and imbibed into the human system. Although we have been hitherto foiled in our efforts to detect these subtle causes, we must not be disheartened, but continue our investigations.

Without entering farther into Dr. Johnson's views in regard to "*fever in general*," which are doubtless familiar to the most of our readers, we will simply quote a *note* affixed to the sixth edition of his work, which certainly displays the sincerity of his own deliberate convictions.

"After a lapse of 20 years since the foregoing doctrine of fever was sketched out in the first edition; and after carefully watching the disease in various climates, since that period, I am unable to offer a more probable explanation of the nature, phenomena, and treatment of fever than that which is stated above."

It is somewhat remarkable that two systems of theory and practice, so widely different as those of Johnson and Broussais should have been promulgated about the same time, and each obtain such general and extensive popularity; yet it is not less remarkable that within the short period of a quarter of a century, *both* should have had their day, and been almost universally abandoned! However, the two great parties in the medical profession, which were led by these distinguished men, in their day still exist; and it would not be extravagant to say that one half of the medical world at this day treat diseases *almost without medicines*; whilst the other still boldly resorts to *heroic remedies.* The great question, whether it is safer to rely upon the *vis medicatrix naturæ*, or remedies that must *kill or cure*, is not yet settled; nor ever will be. For ourselves, we believe "*ibis tutissime in medio.*" Quinine is now the Sampson of the materia medica in the treatment of southern fevers; but notwithstanding our great admiration of this remedy, candour compels us to declare, that *its full remedial powers* have by no means been *fairly and conclusively settled yet.*

Dr. Johnson makes the *liver* the great seat of offensive as well as defensive operations in the production and cure of southern fevers, dysenteries, &c. The healthy action of this immense gland is undoubtedly of primary importance to the animal economy; yet it is not to be denied that *post mortem* examinations in fatal cases of fever, dysentery, &c., generally reveal more extensive lesions in the gastro-intestinal mucous membrane and the brain, than in it. Indeed its derangements are generally of a *functional* nature—it hardly ever displays any marked organic lesion. It is known too, that the medicine which exerts the most powerful action upon the liver, viz: mercury, if carried *too far*, produces the most deleterious effects upon the gastro-intestinal mucous membrane, the blood, the nutritive powers of the system at large.—Hence the importance of taking an enlarged and philosophical view of *fever*, a disease in which every part of the system is more or less involved; so that in resorting to remedies potent for good or evil, whilst we shun *Scylla*, we may beware of *Charybdis*. We feel convinced that where the *remote cause* is not so concentrated and powerful as to *overwhelm* the system at once and stealthily effect *irreparable injury* ere the patient succumbs, as is sometimes the case, the discerning and judicious physician may so combine and administer remedies now familiarly known, as almost to ensure a successful issue. Bloodletting, mercury, emetics, cathartics, quinine, stimulants, blisters, cold, &c., &c., all possess unquestionable virtues; yet there is not one of them that may not speedily produce death if improperly applied. So the true merit of our profession does not consist in the number or power of our remedies, but in the judicious application of them.

As we said in the beginning, we shall not attempt to review this work regularly, nor can we notice but very few of the topics discussed in it; yet we cannot lay the volume down without directing attention to some interesting and unsettled points in the history of Yellow Fever.

Dr. Johnson introduces the subject under the general head of the Western Hemisphere; where it seems his own personal observations were extremely limited; but he has supplied the deficiency by most admirable critical analyses of the works of Bancroft, Dickson, and Fergusson, to which the American editor has added the *post mortem* discoveries of Louis, and some extracts from late American writers. Every thing relating to yellow fever—all the interesting points, questions and facts are condensed and embodied in these papers, and the reader will find them exceedingly convenient for reference. The introduction of one or two new remedies or rather modifications of old ones, is about all that has been added to the history of yellow fever since Bancroft, Dickson, and Fergusson wrote. The questions of contagion or non-contagion, and whether it is a disease *sui generis*, or only a variety of miasmatic fever, still remain unsettled, with able and obstinate contestants on each side. No writer ever discussed the question of contagion with more ability or ingenuity than Dr. Bancroft; nor has any thing been added to the strength of his facts and arguments. On glancing at the number of writers, weight of their authority and their opportunities for investigating this question, the balance would appear to be decidedly in favour of *non-contagion*. And this has been the doctrine of the schools ever since the days of Rush; yet every where may be found intelligent and consciencious men, educated in this belief, who have come to *change their minds* after observing for themselves the progress of the disease.

We have thought that the following abstracts from the Review of Dr. Bancroft's work, in relation to certain interesting points which are topics of daily conversation among us at this time, would be acceptible to our readers; viz:

1. *Is Yellow Fever of Miasmatic origin?*

"Our author, therefore, next endeavours to establish the *identity*, or *near affinity* and *connexion* of yellow fever with the fevers which are indisputably and notoriously produced by marsh miasmata. These latter have certain *characteristic peculiarities*, which are pointed out by the author, and afterwards compared with those phenomena which accompany the yellow fever, to show the very great similarity and near resemblance between the two diseases. These characteristic peculiarities of marsh fevers, as stated by Dr. Bancroft, are, 1st. That of occuring in their simple and mild form of intermittents during the Spring. 2nd. That of being exasperated, converted to *remittent*, and apparently to *continued* fevers, by excessive summer heat; and this generally with a great increase of malignity, (especially in low and marsh situations,) when this excessive heat is long continued, and accompanied with *a total or very unusual deprivation of rain* 3rd. That of their being re-converted and brought back to their mild intermittent form, at the approach or commencement of winter, and afterwards extinguished, or suspended by a continued frost. 4th. That of most frequently and violently attacking strangers from colder climates and more salubrious situations. And 5th. That of never being communicated from person to person by a contagious property.

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"Those of my readers who, by a love of truth, may have been induced to follow me attentively in the *view* which I have now taken of the yellow fever in different parts of America, and whose minds are unbiassed, will, I am confident, clearly recognise in that disease, *all the peculiar features and characteristic marks* by which *marsh* fevers are distinguished in all parts of the world. And they will naturally conclude, that though it be the most aggravated and violent of the fevers arising from miasmata, this aggravation and violence are produced only by a greater concentration or virulence in the latter, joined to a greater intensity of atmospherical heat, acting on persons little accustomed to bear it, whilst they retain the excitability of cold or temperate climates, together with an habitual disposition to generate that portion of animal heat which such climates require. They will have seen that the yellow, like other marsh fevers, is always exasperated by great heat, and extinguished or mitigated by cold; that, between the tropics, it prevails *simultaneously* with the milder forms of marsh fevers, violently attacking *strangers* from cold climates, whilst the natives or long residents are at most only subject to intermittents or mild remittents.—They will have also seen, that in temperate situations this disease, in the early part of summer, before the atmosphere has become intensely hot, is commonly preceded by, *or rather shews itself in*, the forms of intermitting or remittent fever; and that when, being exasperated by excess of heat, it has assumed, and for some time prevailed under, the appearance of an epidemic yellow fever, the accession of cool weather speedily reduces it again to its milder forms, and that a freezing temperature soon puts an end to its appearance, even in those forms, as it commonly does to other fevers occasioned by exhalations from marshes, *and to no others.* And they will also have seen, that the common bilious remittent of hot climates, which is universally admitted to be the effect of miasmata, differs from the yellow fever only by being a little less violent; that, at the utmost, their symptoms vary only *in degree;* and that, in truth, even this difference is often so imperceptible, that the College of Physicians in Philadelphia, when anxious to assign a distinction between the *yellow* and the *bilious remittent* fevers, thought it necessary to allege *one*, which is not only *invisible*, but without *existence*, i. e. contagion. In fact, there is no difference between these fevers, excepting the greater violence, and consequently, greater danger attending the former than the latter; for the yellow colour appears in both; and supposing the fatal *black vomit*, with profuse hæmorrhages, and petechiæ, to occur only in what is called *yellow* fever, (though they are sometimes seen in fevers known and admitted to rise solely from marsh effluvia,) they cannot be included among its essential or distinguishing symptoms, unless *death* be also considered as essential to the disease. Nor can any exasperation of symptoms, which has been preceded by a great degree of heat, give any reason to suspect that a fever whose symptoms are thus exasperated, did not originate from miasmata, because such an exasperation is invariably produced by that *cause* in marsh fevers; and by it they are susceptible of the most dangerous and malignant appearances.

"With so many proofs of identity in their cause, and of the nearest affinity in their symptoms and reciprocal conversions into each other, as well as in their effects on the human body, and their changes by heat and cold, &c., it would be highly unreasonable not to consider them as being only *varieties of one disease.* And I think, with Dr. Rush, that we might as well 'distinguish the rain which falls *in gentle showers* in Great Britain, from that which is *poured in torrents from the clouds in the West Indies*, by different names and qualities, as to impose *specific* names and *characters* upon the different *states* of bilious (of marsh) fever.'"—(482.)

2. *What are the diagnostic symptoms of Yellow Fever?* Dr. Bancroft points out the easy diagnosis between this fever and plague, typhus, &c., and then proceeds to controvert the diagnostics claimed for yellow fever by Sir William Pym, as follows:

"The author begins his inquiry by controverting the diagnostics by which Dr. Pym distinguishes his Bulam from the bilious continued, and bilious remittent fevers; and we are of opinion, that he has undeniably proved that no *specific* difference exists between these forms of fever; that the points on which Dr. Pym has attempted to found a diagnosis, are merely differences of degree, and that (excepting the last, the black vomit,) they are not peculiar, uniform, nor essential to the fever in question. Indeed, it appears to us, that they obtain more or less in most dangerous fevers, as we conceive, must be evident not only to all personally and extensively conversant with yellow fever, but even with fever in general; and further, that Dr. Pym has himself proved the futility, and destroyed the foundation of such diagnosis (if we were to grant his assumption, of which, however, an *ipse dixit* is the substitute for proof,) by asserting that even Dr. Rush himself mistook the bilious remittent for the Bulam fever.—*Pym's Obs.* p. 209.

"Of these alleged diagnostics, the two first, the appearance of the eyes, and the nature and seat of the head-ache, the author satisfactorily shews, from various authorities, to be vague and indeterminate, and therefore, perfectly useless in diagnosis. With regard to the absence of remissions, constituting the third diagnostic of the Bulam, Dr. Bancroft adduces a mass of evidence to prove "the simultaneous appearance of both forms of the fever, and their reciprocal *conversions* into each other at particular places and seasons; together with the invariable appearance of remittents at the same places, both *before* the high atmospheric temperature has operated sufficiently to give them the continued forms, and also *after* the effects of this high temperature have ceased to exist. "Further, Dr. Pym has derived the epidemics of Gibralter by importation from those of Cadiz, Malaga, and Carthagena, and has thereby identified them with the fevers of those places; and Sir James Fellowes states that Arejula, Gonzales and Flores, are "the three most eminent physicians in Cadiz, and he believes in Spain." Now, unfortunately for this principal diagnostic, all those writers distinctly mention remissions in their descriptions of the Spanish epidemics; and as regards the fever in Gibralter, remissions are proved by evidence of seven medical officers of that garrison in the epidemic of 1814. The fourth, or the infrequency and paleness of the yellow colour of the skin, cannot be viewed otherwise than a relative expression; and it will be sufficient to state, that from the accounts of Sir James Fellowes, Sir Joseph Gilpin, Mr. Donnet, and others, the suffusion of the skin is observed in every intermediate shade between a lively yellowness, and a dingy, or dark hue. The author also rejects the fifth diagnostic, the duration of the disease, on the principle of the want of uniformity. Dr. Pym says it runs its course in from one to five days; it is admitted, that it commonly does so in its most aggravated form; but it is proved from Arejula, Sir James Fellowes, Dr. Burnett, Labat, and Dr. Chisholm, that it often continues much longer: further, Dr. Pym states "the *remittent* sometimes proves fatal on the second or third day;" and according to Dr. Hunter, it even runs its course in twenty-four hours. We have ourselves witnessed death on the third day, in a violent remittent imbibed in the month of September, in one of the most *northern* rivers of the United States. Lastly, respecting the sixth alleged diagnostic, the gangrenous state of the stomach, and the appearance of black vomit, Dr. Bancroft exposes the futility of such criteria, the first of which can only be known after death; and the latter "is the almost unerring harbinger of death." The chief value of a diagnostic is to enable us to ascertain the true nature of a disease; but this refers to its consequences only. Neither is the black vomit peculiar to the continued form; for the authorities of Pringle, Cleghorn, Hunter, Rush, and Burnett, prove its occurrence in the remittent.

"I shall only add concerning this black vomiting, that as it is a mortal symptom, never occurring, it may be said, *in those who recover*, and one which is often wanting among those who die, its appearance in this disease must be much rarer even than death; and this circumstance joined to that of its *not* being 'peculiar' to the fever in question, render it very unfit to be produced as a diagnostic thereof."—(488.)

3. *Can persons have Yellow Fever more than once?*

"We have always protested with Dr. Bancroft, against the subtilty of making the black vomit a criterion of the Bulam fever, and regulating the admissibility of the proofs of future attacks by that assumed standard. By acknowledging the legitimacy of such a criterion, as few or none recover after that symptom has appeared, a difficulty nearly tantamount to impossibility is incurred, of ever adducing in the course of even a long life, an unobjectional instance of a second attack. When black vomit, and its usual immediate sequel, death, take place, the patient is relieved from future attacks of any kind; but in less aggravated forms of yellow fever, where there has been no black vomit, and the patient has recovered then, in the event of a second attack, say the advocates for the Nova Pestis, the original one was not a case of Bulam, for one of our diagnostics was wanting; there was no black vomit!—and vice versa. Accordingly, we find this subterfuge incessantly resorted to. Against such sophistry, arguments are vain; and facts, for the reasons we have assigned, difficult to be applied. The Report of Inspector Fergusson from Barbadoes, amongst other cases of second attacks, contains, however, one decisive instance of even black vomit occurring twice in the same individual. A patient of Dr. Caddell, a physician of the greatest experience in Barbadoes, miraculously recovered from yello v fever with distinct black vomit, "and died some years afterwards of the same disease, and with the *same symptoms*."—Against a fact of such decisive import, we know not what reply can be opposed, unless it be "*Non persuadebis, etiamsi persuaseris.*"—(492.)

On the other side of the question, however, the American editor has supplied the following testimony:

["A commission to inquire into the possibility of a second attack was appointed at Gibralter in 1828, and after a careful consideration of the cases presented by several different physicians embracing about 27,000, they ascertained that only thirteen of the whole number were presumed instances, and that of these only one was allowed to be well authenticated. They considered one attack of yellow fever, to be as effectual preservative against a repetition of that disease, as one attack of small pox preserves from a second of that disease.—They also inferred from their researches that the circumstance of a person having suffered from an epidemic in one climate, was sufficient to prevent his being liable a second time, when exposed to its influence in a different part of the world. "The preservative influence of a first attack of yellow fever is not destroyed after a considerable lapse of time, 24 years for example." Louis on Yellow Fever, translated by Dr. Shattuck.

"Dr. Dickson of South Carolina, says "if the university is not perfect, it is nearly so, and we may venture to affirm that any person having once been assailed by yellow fever, will not have it again in any of its endemic states provided he remain there a constant resident."

"Dr. Barton of New Orleans, in the Amer. Journal of Med. Science, vol. xv. p. 47, says "I know no instance where it was taken a second time. In intertropical countries the disease is rarely taken twice, unless the acclimation may have been lost by a contirued residence from the climate for some time in more northern latitudes. Ed."]—(475.)

4. *The true nature of Black Vomit.*—Our late essayists need not take to themselves too much credit for determining the true nature of this fatal discharge, for it appears to have been well known to Dr. Bancroft, as the following extract will show.

"If, then, the black vomit is not bile in a morbid state, nor contains any portion of that fluid, whence is it derived? It must proceed from the stomach itself, and appears to be in most cases, a consequence of inflammation of that viscus. Some physicians have entertained an opinion that the black vomit is a particular morbid secretion by the inflamed vessels or glands of the stomach; Dr. Bancroft thinks that "it is merely blood which has been suffused from some of the small arteries, ruptured in consequence of the separation of certain portions of the villous coat, and has coagulated within the general cavity of the stomach, or on the surface over which it was effused; and having been afterwards detached and triturated by the violent and frequent contractions of that organ in the efforts to vomit, has had its appearance as a coagulum of blood altered, and its colour darkened by the gastric juice, or by some chemical decomposition, either spontaneous, or produced by the action of the air, or other matters contained in the stomach." In confirmation of this opinion, it is stated that in many cases, portions of the inner surface of the stomach have been covered with a coat of thick, blackish matter, and upon removing this coat, the parts beneath it, and no other are found inflamed. The substance thus obtained was exactly similar to black vomit, and there is reason to believe that it must have been derived from the vessels of the inflamed part. At those spots moreover, where the villous coat had been abraded, the extremities of arteries have been frequently seen filled with this dark-coloured matter; and collections of the same matter have been discovered immediately under the villous coat.—A relaxation of the vessels of the stomach may give rise to hæmorrhage from that viscus, as we find happens in some cases of extreme debility, and probably this may take place in some very few instances of yellow fever, where the coats of the stomach remain entire; but the author concludes with great reason, "that the black vomit is much less frequently the consequence of a relaxation of vessels, than of a separation of some portions of the internal coats of the stomach."—(459.)

We feel admonished that we should here close our remarks upon this interesting book, but we cannot do so without saying a few words about *dysentery*. We believe Dr. Johnson's *theory and treatment* of this affection to be the best thing in his book. We have tried it, and found it to succeed admirably. The only abatement we would make, would relate to his favourite idea of the importance and necessity of salivation. This has become almost an obsolete idea at the present day.

The additions to the work by Mr. Martin, a distinguished surgeon in the service of the East India Company, will be read with interest. Mr. Martin says in his preface: "The materials of the brief sketches now incorporated in Dr. Johnson's well known work, appeared generally in an official report on the climate and diseases of Calcutta, of which two editions were ordered to be printed at the public cost by the Supreme Government of India."

The notes by the anonymous American editor are very brief, but quite interesting.

This work, the favourite production of his younger days, first gave Dr. Johnson notoriety and a name; but we deem it far from being the most valuable of his labours. Indeed, if his fame had to rest upon this alone, it would probably be at least of a *doubtful character*. This book has unquestionably been the *cause* of much suffering, if not mortality, but who will deny that it has also done much good? We believe a man may attain wonderful success under a certain theory and practice, which, if attempted to be proclaimed, and imitated by others, will prove altogether abortive. It is as the founder of the Medico-Chirurgical Review, his great ability as a critic, and his indefatigable efforts as a Journalist, that Dr. Johnson has done the most good, and will be longest remembered. It has been said by some wise one, that the evil which men do lives after them, whilst the good is buried with them. We hope this may not prove true with Dr. Johnson. He now sleeps in the dust, after a long, laborious, and honorable career. Peace to his manes!!

E. D. F.

V.—*Clinical Lectures on Surgery, delivered at St. George's Hospital.* By Sir Benjamin C. Brodie, Bart., V. P. R. S., *Serjeant-Surgeon to the Queen; Surgeon in Ordinary to His Royal Highness, Prince Albert; &c. &c. &c.* 8vo., p. 350, Philadelphia, Lea & Blanchard, 1846.

Amid the numerous works and new editions of medical literature, with which the press is constantly teeming, we are not often over-interested in the announcement of a new work, whether it be brought forth under the imposing title of cyclopædia, dictionary, library, specific disease, or compendium. But when one makes its appearance stamped with the authorship of such a name as Brodie, we feel naturally inspired with a commendable curiosity to know what a mind such as his may have added to the resources of the profession. Sir Benjamin Brodie is at present one of the oldest surgeons in Great Britain, and since the death of Sir Astley Cooper, has stood at the very head of his art—a station, to which he is eminently entitled, from the soundness of his judgment, the high culture of his mind, and the ample scope he has had to make observations, during a long and extensive practice, both public and private. This little work is not put forth by himself, but is collected from various periodicals and at different dates, by the American Editor, and offered as a service to the public. It consists of a number of clinical lectures, delivered in St. George's Hospital, on surgical subjects, but strung together in somewhat a desultory manner. They however abound in clearness and perspicuity, and are emphatically of a practical nature, drawn from his own ample experience and observation, and enriched with his own improvements and additions. Although delivered to young men in the Hospital, we believe nevertheless, that those already experienced in surgery, will rise from the perusal of them with enlightened understandings and with feelings of a hallowed nature for the dignity and elevation of the healing art.

It is not our intention in this brief notice to review the work, but chiefly to call the attention of the profession to its intrinsic merit, humbly conceiving, that we can do it no greater service; and at the same time, hazarding the assertion that, whether student or physician, he who shall give it a perusal, will be amply repaid.

It would be a desideratum to have the whole works of Sir Benjamin Brodie embodied in one, and we trust ere long to see a complete edition, under the supervision of the author himself. He owes it to his own well earned fame; he owes it to the profession, of which he is so conspicuous and useful a member; and he owes it to his country, which has so munificently rewarded his exalted merits.

J. B. S.

VI.—*New Medical Journal.*

We have just received the first number of a New Medical Journal, entitled "*The Annalist; a Record of Practical Medicine in the city of New York.*" Edited by William C. Roberts, M. D., Fellow of the College of Physicians and Surgeons of New York. It is to appear on the 1st and 15th of each month, the numbers containing 24 pages of matter. Price $2 per annum in advance.

Dr. Roberts is very favorably known to the profession in the United States by at least two able papers which he published in the N. Y. Journal of Medicine; one entitled "*Pyretological Inquiries*," the other "*On the Pathology of Leuccorrhœa, &c.*"

Judging from the merits of these papers, we should think the author well calculated to conduct a Medical Journal with ability. He says in his Prospectus :—

"The plan of this Journal differs in most respects from that of any which has proceeded it. To a certain extent, it will have the character of a medical *Newspaper*; and great pains will be taken to make it really what it is entitled, a record of Practical Medicine in this City; to exhibit in its pages what occurs in public and private practice, and to chronicle all passing events of professional character which may possess interest."

As a specimen of the high-toned professional sentiment of the Editor, we give the following extract from his introductory address :—

"A lover of peace and concord; a staunch upholder of professional etiquette and proper dignity; a hearty hater of humbug and detester of quackery in every shape and form, and utterly untinctured with belief in any of the new medical heresies which have, of late, inflicted such serious inroads upon the constitution of our science, and have tended so much to its degradation in the public eye, the Editor will hold fellowship only with those who follow the path of Inductive Science, and eschew those shorter cuts to fame and fortune, of untenable hypothesis, of miraculous agencies, as unreal as their impressions are fugitive, and of false systems of treatment, so repugnant to truly scientific practice. Over all of these, time, the great leveller will, it is hoped, ere long throw the mantle of oblivion; they will exist but as facts in the history of the gullibility of man, disgraceful alike to those who practised them and to those by whom they were permitted."

The contents of the present number are entirely *original*, and quite interesting. New York, like our own city, certainly abounds in *materiel* for a Medical Journal; the only difficulty lies in getting the members of the profession to prepare it for the press. But we must confess there is much more literary industry at the North, than we of the South can lay claim to. We wish our new cotemporary all success in his undertaking and will take pleasure in exchanging labours with him.

Part Third.

EXCERPTA.

I.—*Report on the Progress of Human Anatomy and Physiology, in the years* 1844–5. By JAMES PAGET, Lecturer on General and Morbid Anatomy and Physiology, and Warden of the Collegiate Establishment, at St. Bartholemew's Hospital.

[*Continued.*]

RESPIRATION.

Structure of the lungs. Some interesting points in the anatomy of the lungs are determined by the investigations of Mr. Rainey.* The air passages he divides into the bronchial tubes and the intercellular passages continued from them, taking for the line of demarcation between them the parts at which the mucous membrane and ciliary epithelium of the bronchi terminate. These terminate abruptly in bronchi of from 1-50 to 1-30 of an inch in diameter; the mucous membrane retaining to this boundary its longitudinally and circularly fibrous character and its pale colour. Beyond the boundary, the tubular form of the air passages continued from the bronchi is for some distance retained, but it is more and more lost as the passages branch and approach the surface of a lobule, and as the number of air-cells which open into them continually increases. Thus, at last, each minute division of the air-passages becomes quite irregular in its form, air-cells opening into every part of it, and almost constituting its walls, till itself ends, without dilatation, in an air-cell. Beyond the boundary, at which the bronchial mucous membrane abruptly terminates the walls of the air-passages are formed only by a membrane similar to that which walls-in the air-cells; and they have no epithelium. Those air-cells are smallest and most vascular which are situated nearest to the centre of the lung; their size increases and their vascularity diminishes at the parts nearer the circumference—in adaptation, probably, to the larger and more ready supply of fresh air to the former parts. Those cells which open directly into the bronchial tubes and intercellular passages open into them by large circular apertures; they are themselves similarly opened into by other cells, which again are similarly opened into by others, forming thus, for each opening into a tube or passage, a series of communicating cells; each series reaching usually from the passage to the surface of the lobule. The cells which are placed in the angles of the branching of an intercellular passage probably open into both the branches, as well as into one another. The walls of the air-cells, on which the capillaries are placed, are formed by thin and transparent but fibrous membrane, which in the formation of the successive communicating cells is folded, or as if con-

* Medico-Chirurgical Transactions, vol. xxviii, p. 581, 1845.

stricted, so as to form a definite sharp-edged circular opening of communication between each two, and between the first of the series and the passage into which it opens. In the reptiles the corresponding folds bordering the sacculi of their lungs inclose each a double or folded layer of capillary plexus; in the mammal's lung each such fold incloses a single layer of capillaries, so that both sides of all these capillaries are exposed to the air in the adjacent cells. And this plan of arrangement and structure of the air-cells exists in the mature mammalian fœtus just as it does in after-life.*

Nerves of the lungs. In his admirable essay on the physiology of the nerves, Volkmann,† to prove the influence of the pneumogastric nerves on the bronchial tubes, relates an experiment analogous to those performed by Dr. C. J. B. Williams. He tied a glass tube drawn fine at one end into the trachea of a beheaded animal, and when the small end was turned to the flame of a candle he galvanized the pneumogastric trunk; every time he did so the flame was blown, and once it was blown out. The experiment succeeded even when the chest was opened; but the lungs having then collapsed, the efflations were slighter. The movements following the irritation were pulse-like.

Air-changes in respiration. Dr. Marchand,‡ from experiments on frogs, confirms the observation of Valentin and Brunner,‖ that there is always more oxygen absorbed in respiration than is employed in the formation of the carbonic acid expired. The surplus combines with hydrogen, and forms water. When the animals were deprived of food, less carbonic and more water were formed. He found that frogs would not live more than three hours in hydrogen, however pure; they soon went to sleep, and gradually died.

Dr. Vierordt§ has performed on himself a series of 578 experiments to determine, chiefly, the variations produced by different internal and external conditions in the results of the respiratory process. The full evidence for his deductions is given in numerous tables.¶ He shows, in a first section, the influence of variations in these following conditions: 1. *The time of day.* From 9 to 10 a. m. the pulse becomess less frequent; from 10 to 12 it is stationary; from 12 to 2 it increases fast and much; from 2 to 7 p. m. it regularly falls to the frequency of 9 a. m. In the same periods there are nearly corresponding changes in the number of respirations; but at 7 p. m. they are less numerous than at 9 a. m. There is a slight increase in the quantities of air and of carbonic acid expired in a minute between 9 and 10 a. m.; a slight decrease between 10 and 11; a greater decrease between 11 and 12; a very great increase between 12 and 2 p. m.; and a nearly regular and great decrease from 2 to 7 p. m., at which latter time they are much less than at 9 a. m. The increase from 12 to 2 is probably due to the influence of feeding and digestion, for the chief meal was between 12 and 1, and if it were not taken the respirations diminished before 2 p. m. The other changes also are probably due to variations in other internal processes rather than to those of external circumstances. 2. *The effects of dinner* are to produce an increase, *per minute*, in the pulse of 16·3 beats; in the respiration of 1·72 times; in the air expired of 269 cubic inches; in the carbonic acid expired of 19·37 inches. The evening meal produces similar but

* There is a paper on the anatomy of the lungs by Dr. Eichholtz in Muller's Archiv, H. v, 1845, in which he tries to prove that the nuclei of certain cells, like hepatic cells, which he finds in the lungs, are transformed into blood-corpuscles.

† Wagner's Handworterbuch der Physiologie. Art. Nervenphysiologie, p. 586.

‡ Report of the Scientific Meeting at Bremen, in the Medic. Central-Zeitung, October 9, 1845. ‖ See last Report.

§ Physiologie des Athmens; Karlsruhe, 1845, 8vo.

¶ The mode of investigation is also detailed; but, as it was the same in all the experiments, it is unimportant to notice it here. Moreover, I have noticed only the facts obtained by his investigation; but the work contains, besides these, many useful deductions from the labours of others.

slighter effects in all but the frequency of the pulse, which is unaffected by it. 3. *Spirituous drinks*, as Dr. Prout showed, diminish the exhalation of CO_2 by $\frac{1}{2}$ per cent., or $\frac{1}{8}$ the whole quantity previously exhaled in a given time; and they do this especially when taken into an empty stomach. 4. *Bodily exercise*, in moderation, makes the exhalation of carbonic acid about $\frac{1}{3}$ more than it is during rest; and for about an hour after exercise there is an increase of about 118 cubic inches in the air expired per minute; and an increase of 14 per cent. in the proportion of CO_2 contained therein. 5. *Sleep.* Vierordt has not examined the products of respiration during sleep, but he fully confirms Dr. Prout's observation that there is a great, though quickly transitory, increase in the exhalation of CO_2 directly after, or even in the act of waking. 6. *A change of external temperature* equal to 10° F. produces the following changes in the rates per minute: in the number of respirations, ·28; in the quantity of air expired, 33·4 cubic inches; in the carbonic acid expired, 2·114 cubic inches, or ·0183 per cent. These changes are by increase when the external temperature diminishes, and by diminution when it increases. The temperature in which the observations were made ranged between 38·7 F. and 75·7 F. Within this range the pulse is unaffected by the changes. 7. *A rise in the barometer* equal to half an inch increases the rate per minute, of the pulse, 1·3; of the expirations, ·74; of the quantity of air expired, 230 cubic inches; and, as Dr. Prout showed, diminishes the relative proportion of CO_2 expired by ·3 per cent. The barometric pressures in which the observations were made ranged between 29·3 and 30·2 inches. The influence of changes of atmospheric pressure was greater when the temperature at the time was high.

In a second section Dr. Vierordt discusses the influence of *variations in the respiratory movements* on the products of respiration: 1st, *In various frequencies of respiration* the proportionate quantities of CO_2 in the expired air are, with six respirations in the minute, 5·528 per cent; with 12, 4·262; with 24, 3·355; with 48, 2·984; with 96 respirations in the minute, 2·662 per cent.* But the absolute quantities of CO_2 exhaled in a minute in the same circumstances are (since by rapid breathing the whole quantity of air that is breathed is proportionally increased) as follows: with 6 respirations per minute, 171 cubic inches; with 12, 246 cubic inches; with 24, 396 cubic inches; with 48, 696 cubic inches; with 96, 1296 cubic inches. 2. *By increasing the volume or depth of the respiration*, the per centage proportion of CO_2 in the expired air is diminished; in the deepest respiration it is 1·97 per cent. less than in ordinary breathing. But for this proportionate diminution also there is a full compensation in the greater total volume of air which is thus breathed; for equal volumes of expired air, whether breathed slowly or quickly, deeply or not, contain equal quantities of CO_2. 3. It appears, moreover, that if the air in one ordinary expiration contains on an average 4·48 per cent. of carbonic acid, the air expired in the first half of the whole expiration contains ·76 per cent. less than this average, and that expired in the last half contains ·96 per cent. more than the average, proving that the air in the parts of the lungs nearest the great bronchial tubes contains less CO_2 than that in the deeper and more distant parts does. 4th. *In holding the breath*, the †proportion of CO_2 in the air within the lungs increases greatly, but in a diminishing ratio; in one minute's holding it becomes 2·42 per cent., and in 100 seconds 3·08 per cent. more than it is in an ordinary expiration. In this condition also the composition of all the air within the lungs very soon becomes uniform.

Finally, Vierordt shows how wide the range of occasional variation is within which, even in health and perfect bodily rest, the several parts of the respira-

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* The author hence deduces a general rule for calculating the proportions, in which, however, he is probably premature.

† See, on the anatomical adaptation of the lungs to this condition, Mr. Rainey's Observations, at p. 553.

tory functions are discharged. In these conditions the pulse ranged, in occasional instances, between 54 and 101 per minute; the respirations between 9 and 15; the expired air between 1637 and 3676 cubic inches; the carbonic acid therein contained, between 3·358 and 6·22 per cent.; the volume of each expiration between 144·6 and 275 cubic inches.

The changes in the quantity of carbonic acid exhaled in much higher and lower temperatures than Vierordt observed them in have been investigated by M. Letellier.* The experiments were performed on small birds, mice, and guinea pigs, who were made to respire for periods varying from half an hour to six hours. The results are stated in tables which show generally that the quantity of carbonic acid exhaled at a temperature between 82° and 104° was about as much below the average at ordinary temperatures as the quantity exhaled at temperatures about zero were above the same average; and the greatest quantity exhaled at the lower temperatures was about twice as much as the smallest exhaled at the higher temperatures. None of the animals experimented on could live without danger in a temperature equal to that which is natural to their own bodies; and all speedily died when the temperature was increased to 5° higher. At temperatures from 80° to 92° they lived and breathed quite tranquilly.

Hannover,† also, has instituted experiments to determine the quantities of carbonic acids exhaled in certain diseases, and finds, 1. That chlorotic females exhale an absolutely larger quantity of it than healthy ones do; 2. That the quantity is much diminished in all cases of phthisis; and 3. That it is little or not at all changed in bronchitis.

Asphyxia. Many interesting matters connected with the physiology of respiration are well discussed and illustrated by Mr. Erichsen,‡ in his essay on Asphyxia. 1. To show that, although the cessation of the respiratory movements is not the sole cause of the cessation of circulation in asphyxia, yet their continuance enables it to be continued longer than it will after they have ceased, artificial respiratory movements with a small quantity of air (a quantity too small to retard materially the occurrence of asphyxia,) were maintained in a dog till all the chemical respiratory changes had ceased, and the heart was motionless. On examination after death, the difference between the quantities of blood in the two sides of the heart was not nearly so great as in ordinary cases of asphyxia: the left cavities contained nearly as much blood as the right, proving that the respiratory movements, unassisted by the chemical changes, had for a time facilitated the passage of blood through the lungs. 2. To show that the arrest of the blood in the small vessels in asphyxia is not the consequence of the action of venous blood in the nervous centres, three dogs were so arranged that while one of them was being asphyxiated by ligature of the trachea, arterial blood might be propelled through its carotids from the carotids of the other two. Care was also taken to prevent the passage of venous blood through the vertebral arteries of the asphyxiated dog, and to prevent congestion of its nervous centres by opening one of its jugular veins. The result was that the dog was asphyxiated in the ordinary time, and that there was the usual amount of congestion of its vessels, although arterial blood had, throughout the experiment, circulated through its nervous centres.

3. Two experiments are related in evidence that, if the action of the heart be maintained, black blood may be propelled by it through a lung long after the chemical respiratory changes have ceased. Dogs were pithed, and while artificial respiration was being maintained the right bronchus was tied. As long as the heart's action continued (and it continued much longer than in

* Annales de Chimie et de Physique, Avril 1845, p. 478.

† De Quantitate . . . acidi carbonici ab homine sano et ægroto exhalati; Havniæ, 1845, 8vo.

‡ An Experimental Inquiry into the Pathology and Treatment of Asphyxia. Edinburgh, 1845, 8vo.

ordinary cases of asphyxia), there flowed nearly as much black blood through a right pulmonary vein as of red blood through a left one.

4. A description is given of the contraction of the small arteries, and distention of the veins of the mesentery during asphyxia, as observed with the microscope. And this contraction is maintained to be the principal or sole agent in the obstruction to the passage of the blood observed in asphyxia, the black blood being regarded as a stimulant to the contractile coats of the small systemic arteries and pulmonary veins. [But the description does not disagree with the more probable opinion that the small arteries of the mesentery contracted only because less blood was impelled into them.]

Mr. Erichsen's conclusion from these and some other previous experiments* is that the cessation of the circulation in asphyxia depends upon all three of those circumstances, to each of which, by various former writers, it has been exclusively ascribed, viz.: 1. The arrest of the respiratory movements; 2. And more importantly, the weakening of the heart's action in consequence of the lessened quantity and altered quality of the blood which passes into the left ventricle, and the coronary arteries; 3. Obstruction to the blood in its passage through the small vessels; [*i. e.* as he thinks, by the refusal of the minute pulmonary veins and systemic arteries to receive venous blood, but, as others believe, because the capillaries cannot transmit blood charged with carbonic acid.]

The average periods, in minutes and fractions thereof, at which the chief phenomena of asphyxia are observed in dogs are stated thus: voluntary movements cease in $1\frac{3}{4}$; involuntary in $2\frac{1}{2}$; the blood in the arteries becomes as black as that in the veins in $1\frac{3}{4}$, or, occasionally, in $1\frac{1}{4}$; ventricular contractions cease in $9\frac{1}{2}$; (at the earliest in $6\frac{1}{2}$, the latest, in adult animals, in 14); the left auricle ceases to act in 18 minutes, the right in $19\frac{1}{2}$, but once, in an adult animal, the former continued acting for 37, and the latter for 44 minutes; pulsations in the femoral artery continue for $7\frac{1}{2}$ minutes. In puppies four days old the ventricles contracted regularly for 117 minutes, and with irregular movements for three hours; and the auricles continued acting for three hours and twenty-five minutes.

ANIMAL HEAT.

Liebig† has shown the error of the opinion derived from Dulong and Despretz, that the quantity of heat generated by an animal which consumes by respiration a certain quantity of oxygen in a given time, is less than would be produced by the direct combustion of carbon and hydrogen in the same quantity of oxygen. He proves that this opinion is founded on incorrect premises; the combustion-heats of carbon and hydrogen having been reckoned too low by both Dulong and Despretz; and having established the authority of new and more accurate numbers to represent these combustion-heats, he shows that the amounts of heat developed by animals consuming certain proportions of oxygen, are nearly equal to those which are produced by the direct combustion of definite proportions of carbon and hydrogen in the same quantity of oxygen.

For the sake of their relation to the foregoing observations of Vierordt, I place here some of these by Gierse,‡ whose work should have been noticed in the Report for 1842–3. At different periods of the day, he found the temperature under his tongue as follows:—from 11 P. M. to 2 A. M. 98·26° F.; from 6 to 8 A. M. before breakfast, 98·55° F.; from 6 to 8 A. M. on other days, after breakfast, 98·73°; between 9 and 11 A. M., 99°; just before dinner, 98·82°; after dinner, 99·5; between 3 and 6, P. M., 98·61°; between 6 and 10 P. M., 98·86°; from 11 P. M., to 1 A. M., while asleep, 98·15°. All the other observations by this author relate to the temperature of inflamed parts, except a few from which he believes that there is no increased heat in the vagina during men-

* Medical Gazette, 1842.

† Lancet, Feb. 22, 1845, and Annalen der Chemie und Pharmacie, Jan. 1845.

‡ Quænam sit ratio caloris organici, Dissert. inaug.; Halæ, 1842, 4to.

struation of parturition. He found almost constantly that an inflamed part communicated its heat to the thermometer more rapidly than a healthy part of the same temperature did. He found also that, in animals, struggling and excitement commonly raised the temperature more than the inflammation of a part did.

Some of these results also, are confirmed by Dr. John Davy.* He finds the temperature highest in the morning, on rising after sleep; high, but fluctuating, till the evening; and lowest about midnight, ranging from 98·7° to 97·9°. In active exercise, within the limits of exhaustion, the temperature of the body is increased in direct proportion to the exertion; in passive exercise in the cool air it is lowered; during quietude in an atmosphere from 42° to 32°, it may be reduced from 1° to 2°. Sustained mental exertion slightly raises the temperature; a light meal scarcely alters it; a heavy one lowers it.

Sundry observations are recorded by Dr. Dowler and other American surgeons† of considerable increase of temperature of the surface of the body after death from yellow fever and coup de soleil. Within a few minutes after death the temperature is said to rise to 102°, and then to increase steadily to 106° or 108° or even to 113°. Thus it may remain for from four to six hours, being usually higher at the thigh and abdomen than in the heart; then it gradually subsides to the temperature of the surrounding medium. The cases are said to have all occurred in the summer, in a temperature of at least 80°; but they are very imperfectly related. In the paper first referred to in the subjacent notes, another phenomenon is mentioned, which is certainly very rarely if ever observed in this country, namely, the maintenance of the power of forcible contraction in the voluntary muscles for six or seven hours after death. When all the muscles of the fore-arm are dissected bare (the arm being amputated at the shoulder) a blow with the hand will excite powerful contractions in the bared muscles; and twenty minutes or more after death a large hatchet weighing three pounds being tied in a subject's hand, it is said that the fore-arm was bent several times so as to raise the hatchet and strike the trunk as often as the flexor muscles were struck with the hand.

DIGESTION.

STRUCTURE OF THE DIGESTIVE ORGANS; *The Tongue.* Dr. Nuhn‡ has minutely described a pair of glands in the substance of the lower part of the apex of the tongue. Each gland lies about two lines from the median line, just below the ranine artery, on the outer side of the expanding branches of the lingual division of the trifacial nerve, under some longitudinal fibres of the anterior part of the stylo-glossus. It has the structure of a salivary gland, and measures from 7 to 10 lines in length, from 3 to 4½ in breadth, and from 1½ to 2½ in thickness. It has at least five ducts, which open through the mucous membrane over it by small orifices, each surrounded by a slightly elevated ring; it receives many branches from, and sometimes surrounds, the ranine artery. Dr. Nuhn has at present found the gland in man and the orang alone; he has sought it in vain in many other mammalia; he thinks therefore, it may be a mucous gland whose fluid facilitates the movement of the tongue in speaking: [it looks exactly like a salivary gland].

Structure of the Palate.|| Dr. Tourtual§ has detected a new muscle of the posterior nares and palate, and found it regularly in six men as well as in dogs,

* Proceedings of the Royal Society, No. 61, June 19, 1845.

† B. Dowler, (New Orleans) in the Boston Medical Journal, Aug. 6, 1845; in the New York J. of Medicine, Sept. 1845; in the Medical Examiner, Sept. 1845.

‡ Ueber eine his jetzt noch nicht næher beschr. Druse im innern der Zungenspitze; Mannheim, 1845, 4to. I supposed he had discovered these glands, but Schlemm (Muller's Archiv, H. v, 1845) shows that they were discovered by Blandin, and described in his Anatomie Topographique, p. 175; Paris, 1834.

|| On the Structure of the Teeth; see, in a future part, Prof. Owen's account of their development.

§ Muller's Archiv, 1844, Heft v, p. 452

cats, and sheep. It lies between the mucous membrane and the internal pterygoid plate, before the Eustachian tube, and behind the inferior meatus of the nose. Its upper edge or origin extends from below the palatine process of the turbinated bone backwards and upwards, towards the upper margin of the Eustachian tube; in this line the muscle arises from the vertical plate of the palate bone, and from the internal pterygoid plate of the sphenoid, reaching sometimes to the cartilage of the Eustachian tube; hence its fibres straight descend, and are augmented by others arising from the several parts near which they pass; and going just behind the hard palate, they enter the anterior and outer part of the soft palate, in which they expand on an aponeurosis, mingling with that of the circumflexus palati. The posterior margin of the muscle is usually confused with the anterior fibres of the levator palati, and is covered by a short fold of mucous membrane, which forms the anterior margin of the orifice of the Eustachian tube.

The discoverer proposes for this muscle the name of pterygo-palatine, or levator palati mollis anterior seu minor.* It receives a (probably sensitive) filament from the internal palatine branch of the fifth; and has, probably, also motor fibres from the glosso-pharyngeal. The function of the pair of muscles is probably that of elevating and of slightly stretching transversely the *anterior* and lateral parts of the soft palate, for which the larger and posterior levatores palati do not provide. In this tension of the palate they probably prevent the anterior fibres of the circumflexi from drawing the anterior part of the palate backwards. They probably are important in sounding the palatine consonants, and their posterior portions may assist the greater levatores palati in narrowing the Eustachian orifices.

Structure of the Liver. An elaborate anatomy of the liver, with measurements of all its measureable parts, is published by Theile.† He confirms Mr. Kiernan's account of its lobular structure; for though he admits that the investment of each lobule, which is so dense in the pig and some other animals, may be absent or very thin in the human liver, yet he urges, and very rightly, that the latter is, by the arrangement of its vessels, divided into lobules, just like those of the pig and others. For in the human liver, as in theirs, each portion thus bounded by vessels, if not by a distinct capsule, is a miniature liver, having in itself a complete bile-secreting apparatus. He confirms also the account of the hepatic cells being arranged in networks with a radiated plan; but he denies the existence of the vaginal branches and plexuses of the portal vein as described by Mr. Kiernan. These vaginal plexuses, he says, "are derived, not from the portal veins, but from the hepatic arteries, from which they are completely filled when both arteries and veins are at the same time injected; and when they appear to be injected from the portal vein, it is because the injection has passed through those vessels by which the blood of the hepatic artery is carried to the portal vein. The interlobular portal veins are therefore, he says, derived directly from the portal veins; and those which appear to be vaginal branches of the portal vein are its *internal roots*, by which it receives the blood which has served for the nutrition of the hepatic ducts and other vessels of the liver. But that which is most new in his essay is the account of the glands of the biliary ducts. The orifices of these are, as Mr. Kiernan showed, arranged in two opposite rows through the whole extent of the ducts within the liver, as far as they can be dissected; an arrangement peculiar to the human liver. Their number is less in the human than in the other livers which Theile examined, but their form is more complex. They generally consist of an elongated tortuous canal, bearing alternate small sacculi and pedicled bunches of cel-

* This name had better be used; for the former has been already applied to what is probably a part of this muscle, arising from the hamular process of the pterygoid plate, and mingled with fibres of the pterygo-pharyngeus. See Haller, Elem. Physiol. t vi, p. 80, note *t*.

† Wagner's Handwœrterbuch der Physiologie, art. Leber.

lules; reminding one of the Meibomian glands. These canals, moreover, branch, and their branches anastomose with one another, and with those of the adjacent glands. Such a net-like connexion of the gland-canals occurs within the Glisson's capsule, investing the larger and middle-sized hepatic ducts; but it is most remarkably developed in the transverse fissure of the liver. Here, after the hepatic duct has been well injected, red streaks, forming plexuses, can be seen, which are nothing but large examples of these glands.* The canal-shaped glands in this situation are a quarter of a line in diameter, (in other parts they are from one fifth to one tenth); their walls are beset by pouches and bundles of sacculi, some of which are one sixth of a line in diameter, and there open into them at short intervals elongated glandules, just like those found in the prolongations of Glisson's capsule, except in that they do not open directly into the hepatic ducts.

Krause† suspects that these glands of the hepatic ducts, as well as the vasa aberrantia of Weber, are incompletely injected bile-ducts, [or, perhaps, they should rather be considered as such ducts imperfectly developed; for if Krause's account of the acinous structure of the liver be true, no line of essential distinction could well be drawn between them and the more perfect ducts]. He holds still‡ that all the bile-ducts have acini at or near their ends, having confirmed his opinion by recent numerous injections. He agrees generally with Kiernan and Weber's account of the reticular arrangement of the minutest hepatic ducts, having often seen this by injection; and he adds that, on many of those ducts he has injected, and has seen, after dissolving the injection that was in them, regular round and oblong vesicles or acini, from 1-730 to 1-480 of an inch in diameter; many of these acini, grouped upon their several ducts, compose each lobule, each group of acini having one duct, and therefore each lobule having probably several ducts, or roots of the hepatic ducts, which proceeding from it enter into the lobular plexus of ducts. He says also, that the acini may often be discerned entire at the edges of very thin sections of the human liver; they appear within the lobules as regular, round, or oval, yellowish grey and very thin-walled corpuscles or cells, invested by very fine and short fibro-cellular fibrils, and each containing from six to eight hepatic cells, with, in many cases, bile.

M. Natalis-Guillot‖ has also, (if I rightly apprehend his expressions), confirmed by injections the account given in the last Report of the retiform arrangement of the minutest bile-ducts. These, he says, surround, either in a net, or in dense tufts, the whole surface of each lobule, and spread over the surface of each of the "ultimate ramifications of the vena portæ," i. e. of the interlobular portal veins. The ducts themselves, he thinks, are surrounded by minute branches of the hepatic artery.

Properties of the digestive fluids. *Saliva.* Experiments by M. Magendie§ and others on the saliva of the horse have shown a difference between that secreted by the parotid gland and the mixed saliva from all the glands. The parotid saliva obtained from a fistula into the duct is more alkaline than the rest [as Mitscherlich also showed]. It contains carbonate [?] of potass which the rest does not contain, and a much larger proportion of ptyaline and albumen, the latter constituting one-fifth of the whole solid mass. The parotid saliva also has no influence on starch-paste or raw starch, at a temperature of 104° to 167°; but the mixed saliva speedily transforms the starch-paste into sugar

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* E. H. Weber has described these as a part of the vasa aberrantia of the hepatic ducts, similar to those in the left lateral ligament and some other situations. See last Report, p. 29.

† Müller's Archiv, Heft v, 1846.

‡ His former paper is in Müller's Archiv, 1837; and the same account is given in his Handbuch der Anatomie.

‖ Comptes Rendus, 18 Novembre, 1844.

§ Archives Gén. de la Médecine, Nov. 1845, from the Acad. des Sciences, 20 Oct.

at 104°, and at the same temperature acts slowly but evidently on raw starch and coagulated albumen. This mixed saliva, obtained from food, masticated and swallowed into the œsophagus, is not limpid and clear like that from the parotid, but yellowish-gray, easily becoming turbid. It is weakly alkaline, and contains no carbonate, but abundant alkaline chlorides. There is a great "analogy" between the different kinds of saliva poured into the mouth of man, and those just described as secreted in the horse.

Gastric Fluid. The researches of M. Blondlot,* from which he deduced the presence of an acid phosphate of lime in the gastric fluid, are partly confirmed and partly opposed by those of Dr. R. D. Thomson.† Thus, like Blondlot, he has never found a volatile acid in the stomach if digesting animal food alone; and he disproves the presence of hydrochloric acid in the gastric fluid during the digestion of either kind of food. But, contrary to Blondlot, as well as to the experiments next mentioned, he says, that the acid of the stomach can be saturated with chalk; and that therefore it cannot be an acid phosphate of lime. What the acid is, his experiments leave uncertain; but in the digestion of vegetable substances, he finds a volatile acid always present in very minute quantity, and a fixed acid which, he says, resembles the lactic more nearly than any other.

MM. Bernard and Barreswil hold that the gastric acid is the lactic. Their experiments,‡ like those of M. Blondlot, show that it cannot be either the acetic or hydrochloric—the first cannot be distilled from it; and, though the second can, yet it is only at the last, when the chloride salts are decomposed by the acid really present. Moreover, a minimum of oxalic acid produces a precipitate of oxalate of lime, which it could not do if even 1-2000 of free hydrochloric acid were present. But they show that M. Blondlot failed to observe effervescence on adding carbonate of lime to the gastric fluid, because of the state of dilution of the fluid; when it is concentrated by evaporation distinct effervescence ensues. This effervescence proves the existence of some acid besides the phosphoric; yet, since the addition of an excess of carbonate of lime cannot completely neutralize the acid, it is highly probable that a small proportion of phosphoric acid free, or in an acid salt, is present in the gastric fluid and gives it part of its acid reaction. The other acid with which the carbonate of lime effervesces is, the authors believe, the lactic; and they show that in every essential respect the conduct of the gastric fluid with various tests is similar to that of water acidulated with lactic acid, and with the addition of a little chloride of sodium.

The experiments also of M. Melsens|| confirm some of these, proving that both marble and other carbonates of lime lose weight when immersed in gastric fluid, and that there must therefore be a free acid in it.

Bile. The most careful examinations of the urine and blood of a patient with intense jaundice did not enable Scherer§ to detect in either of them a trace of any constituent of bile except the colouring matter and cholestearine. In evidence of the speedy transformation which the biline would probably undergo in the blood, he mentions that in a large quantity of green fluid vomited, and containing abundant biliary colouring matter, he could not detect a trace of the biline which it must previously have contained. In the same essay he gives an accurate account of his analysis of the biliary colouring matter which he collected from the patient's urine.

The conclusion respecting the non-existence of the essential principles of the bile in the fæces is confirmed by the delicate test for bile invented by Petten-

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* See last Report, p. 21.

† On the digestion of vegetable albumen, &c. Philos. Mag., May, 1845; and Lancet, May, 17, 1845.

‡ Comptes Rendus, 9 Decembre, 1841.

|| Comptes Rendus, 9 Decembre, p. 1289.

§ Annalen der Chemie und Pharmacie, Mærz, 1815; and Lancet, May 24, 1815.

kofer.* To the fluid supposed to contain bile ⅔ of its volume of sulphuric acid are added by drops, that the temperature may not rise above 140 ° F., and then from two to five drops of a solution of sugar (one to four parts of water.) Presently a reddish violet colour appears, intense in direct proportion to the quantity of bilic acid. By this test no bile (except the colouring matter) could be found in healthy fæces; but the fæces of diarrhœa and those discharged after purgatives contain complete bile. So also, by this test, bile could always be found in the urine of the pneumonic.

Dr. Redtenbacher† has found that tarine contains 26 per cent. of sulphur, and his discovery is confirmed by Dr. Gregory.‡

Pancreatic fluid. Twenty-eight grains of the pancreatic fluid of an elephant, collected eight days after death, yielded to Professor Bergmann,|| of Bonn, 92.77 per cent. of water and 7.33 per cent. of albumen, caseine, and a minute quantity of chloride of sodium and carbonate of soda.

Digestive Principles. MM. Barreswil and Bernard§ also maintain that the active organic digestive principle (presently again to be mentioned) is the same in the saliva, the gastric fluid, and the pancreatic secretion; and that the special action which it appears to exhibit in these several fluids is due only to its being combined with an acid in the gastric fluid, and with an alkali in the others. They say that from whichever of these three sources this principle is obtained it will, if an acid be added to it, digest meat, gluten, and other azotised compounds, and act like artificial gastric fluid; but if it be made alkaline it will only be capable of digesting the amylaceous principles, and thus will be an artificial saliva or pancreatic fluid. So also, by making gastric fluid alkaline, it will act like saliva or pancreatic fluid; and by making either of the latter acid it will act like gastric fluid.

Process of Digestion. The principal researches of the year to be placed under this head have had reference to the digestion of the saccharine and amylaceous principles of food. The most considerable are those of MM. Bouchardat and Sandras¶ whose results are as follows:

When cane-sugar is given to dogs, it is found either unchanged or converted into *sucre interverti* or lactic acid, in the whole length of the digestive canal. After it has been given for several days it may be found in the urine, and in the bile, blood, and chyle. When cane-sugar is introduced directly into the blood it passes into the urine; but when the same quantity of *sucre interverti* or lactic acid was so introduced, none was detected in the urine. In order that cane-sugar may be destroyed in the blood (i. e. ultimately reduced to carbonic acid and water), it must first be transformed into *sucre interverti*, or lactic acid in the digestive canal.

Raw starch is very imperfectly digested by men and carnivora. The greater part of it may be found unaltered in the excrements. A greater effect is produced on it by the digestive organs of herbivorous rodents. It undergoes no change in their stomachs; but in the contents of their small intestines (which are always alkaline, except sometimes at the pyloric part of the duodenum) there are found, together with entire starch granules, others that are cracked, eroded, or almost wholly destroyed; and, also, dextrine with traces of grape-sugar. The cæcum contains an acid paste, in which there are some entire

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* Annalen der Chemie u. Pharmacie, No. iii, p. 90.

† Annuaire de Chimie, p. 724, from the Comptes Rendus, t. xx, 1845.

‡ Outlines of Chemistry, Part ii, p. 566.

|| Oesterr. Med. Wochenschr., Dec. 14, 1844, from the Med. Corresp. Blatt Rhein. u. Westphal. Aerzete, No. 17, 1844. Much more concerning the nature of the pancreatic fluid will be found in the followieg paragraphs:

§ Gaz. Med., 12 Jul. 1845, from the Report of the Acad. des Sciences, 7 Jul.; Comptes Reudus, 25 Jul.

¶ Report from the Académie des Sciences, 20 Jan. 1845, in the Gazette Médicale, 25 Jan. 1845.

starch granules, dextrine, grape-sugar, and lactic acid. The same materials are found in the rectum. The dextrine, grape sugar, and lactic acid may also be detected in the blood and the bile, but not in the urine of these animals ; and the blood of their vena portæ contained more water and more of these three substances than their arterial blood did—making it sure that they are absorbed by the blood-vessels, not by the lacteals.

Graminivorous birds digest raw starch more completely than mammalia do. It undergoes no change in their crops ; and in their gizzards, though there are traces of dextrine and grape-sugar, yet nearly all the starch-granules appear unchanged. In the small intestines the starch-granules are gradually more and more destroyed, and ultimately they undergo the same changes as in the intestines of the herbivorous rodents, but more completely. In both, the authors consider the changes to be due to the high temperature, the alkaline reaction, and the presence of a secreted principle which acts like diastase, only with less energy : all which conditions exist in greater force in the birds than in the mammalia.

When the starch-granules have been burst by cooking they are digestible by men and carnivorous animals. Their solution commences in the stomach, and is slowly continued through the intestinal canal. The products are dextrine, grape-sugar, and lactic acid; which are found mixed with some starch remaining unchanged.

If more than a certain proportion (one gramme, at the most, for an adult dog) of a feculent or saccharine principle be mingled at once with the blood, sugar is always eliminated by the kidneys. Hence the slow introduction into the blood of the products derived from the digestion of these principles appears to be an essential condition for their due disposal ; and this condition is secured by two chief means, namely, the slowness of their solution, and the chief manner of their absorption. They are formed iuto soluble compounds in the intestines ; and these being absorbed by the blood-vessels, are carried to the liver, where, if combustible matters are in excess in the blood, the greater part of them are secreted and discharged with the bile into the intestines, whence some of them may be absorbed with the other soluble constituents of the bile. "Thus then is established a limited circulation of the combustible matter, which, by this admirable artifice, is only gradually carried into the general circulation."

These conclusions concerning the transformations which starch undergoes in digestion are, to some extent, confirmed by Dr. R. D. Thomson,* whose experiments show that dextrine and soluble starch exist in the stomachs of pigs fed on farinaceous food during and for some time after digestion, and that sugar exists in the blood of the same animals in the proportion of from 2·57 to 8·05 grains in 1000 grains of the serum.

In a subsequent memoir† MM. Bouchardat and Sandras state that the principle which, as above-mentioned, appears to act like diastase in the transformations of starch, is secreted chiefly by the pancreas. They find the pancreatic fluid of birds transparent, viscid, slightly alkaline, and capable of liquefying starch-paste, and of transforming it into dextrine and grape-sugar. Portions of pancreas cleared of blood and large vessels possess the same power in a very high degree ; and no other organ besides the pancreas, and in a slighter degree the salivary glands, possesses such a power.‡ It is, moreover, wholly destroyed both in the pancreatic fluid and in the pancreas itself by such influences as destroy the like property in diastase, such as a temperature of 212°, tannin, mineral acids, metallic salts, &c.

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* Philos. Mag., May 1845 ; and Lancet, May 17, 1845.

† Arch. Gén. de Médecine, Mai 1845 ; Report from the Academie des Sciences, 14 Avril.

‡ See in connexion with this subject a paper by M. Bouchardat, "Sur la fermentation saccharine ou glcosique," in the Annales de Chimie et de Physique, Mai 1845, t. 89, p. 61.

The same influence which these authors ascribe to the pancreatic secretion is ascribed by M. Mialhe* to the saliva, from which he gives directions for obtaining the digestive principle, *animal* or *salivary diastase*,—by filtering it and then treating it with five or six times its weight of absolute alcohol. The diastase being insoluble in alcohol is thus precipitated in white flocculi. He describes the aqueous solution of this substance as insipid and neutral, not precipitable by subacetate of lead, and when left to itself undergoing a transformation into butyric or some similar acid. With raw starch this salivary diastase requires several days for the production of dextrine and sugar of starch; but with starch-powder the change is quickly effected; and with starch-paste it is very speedily completed if aided by a temperature of about 160°.†

The experiments of M. Lassaigne‡ confirm those of MM. Bouchardat and Sandras as to the properties of the pancreas and its fluid; and, at least in great measure,|| those of M. Mialhe on the properties of the saliva. He shows that at the natural temperature of the body, saliva has no effect on whole starch, and that mastication does not change the form in which it naturally exists in cereal grains; that horse's saliva does not act on starch even when its grains are broken; but that human saliva, though it does not affect raw and whole starch at a temperature of 100°, can even at a temperature of from 64° to 68° convert powdered starch partly in dextrine and partly into sugar of starch; the envelopes of the granules preserving at the same time their property of becoming violet when touched with iodine.

Influence of the bile in digestion. Dr. Platner§ has made experiments to find how the bile contributes to digestion. He has confirmed, what Simon and others showed, that the fæces contain none of the bile except its colouring matter [and some of its fat?]; and what Purkinje showed, that the bile will put a stop to or prevent the artificial digestion of coagulated albumen. On mixing pure artificial digestive fluid, neutralized by carbonate of soda, with bile, no change took place; but on adding hydrochloric acid to the mixture it became very turbid. The same happened when bile was mixed with digestive fluid not neutralized; but hydrochloric acid added to bile alone produced no precipitate. The precipitate consisted of bilic acid united with some organic body, perhaps pepsin, explaining probably the fact quoted above from Purkinje.

When bile was added to a solution of albumen in acetic acid, a precipitate was formed which was insoluble in all acids, but soluble in alkalies. When bile and albumen was mixed and acetic acid added, a precipitate like coagulated fibrine was formed; and a similar precipitate was formed by the agency of even carbonic acid; showing that although the bilate of soda (i. e. the pure principle of bile) retains its composition under the action of either acids or alcalies alone, yet it is decomposed easily by combinations of acids with organic substances. When bile was added to a solution of albumen or gelatine obtained by artificial digestion, precipitates were formed which were soluble in acetic acid and consisted of bilic acid united with the organic substance. Sugar and gum in like circumstances appeared to unite with the fatty matters of the bile.

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* Ibid., 5 Avril 1845; Report for the 31st of March.

† According to Dr. R. D. Thompson (l. c.) the transformation of starch into dextrine is effected to some extent by boiling it for half an hour in pure distilled water.

‡ Arch. Gén. de Médecine, Mai 1845, from the Report of the Acad. des Sciences, 7 Avril.

|| Ibid., Juillet 1845, from the Report of the Acad. des Sciences, 2 Juin. In the earlier paper his results were opposed to those of M. Mialhe.

§ Muller's Archiv, 1845, Heft iv. He has since published a special work, "Ueber die Natur und den Nutzen der Galle," Heidelberg, 1845; but I have not yet received it. Another work, relating to the physiology of the bile is by H. Meckel, De genesi Adipis; Halis. 1845, 8vo.

Fæces. The usual microscopic constituents of human fæces are thus enumerated by Dr. Gobee.* 1. A large quantity of vegetable cellular tissue, with or without epidermis and hairs. 2. Vegetable hairs. 3. Vegetable spiral vessels. 4. Elongated quadrangular plates of light yellow colour in great abundance, of uncertain nature; they are not affected by acetic acid, and are insoluble in cold ether, but iodine displays transverse striæ on them. [Probably they are portions of muscular fibre. I have found such, tinged pale yellow by the bile, in the fluid discharged through an artificial anus.] 5. Large quantities of crystals of phosphate of ammonia and magnesia. 6. Fat-globules or cells in various quantity. 7. A great quantity of granules. 8. Few epithelium- and mucus-cells. 9. Much of the brown-colouring matter of the bile.

ABSORPTION.

Structure of the Lymphatics. The subject of one of Mr. Goodsir's excellent essays† is the structure of the lymphatic glands. At the points of connexion between the extra- and intra-glandular lymphatic vessels, the coats of the former (whether afferent or efferent) separate. The outer coat is continued into the external capsule of the gland, from which processes pass inwards, binding together the substance of the gland, and supporting the vessels within it. The middle, or fibrous coat, is usually nearly lost, as the vessels pass towards the centre of the gland. But the internal coat becomes thicker and more opaque in the intra-glandular lymphatics; and when, in any of these thickened, dilated, and oft anastomosing vessels, it is broken up, it appears composed of two substances; namely, first, a thin transparent membrane, in which ovoidal bodies, containing one or more minute vesicles, are imbedded, as "germinal spots,"‡ at regular distanses; and secondly, thick layers of close-packed spherical nucleated particles about 1·5000th of an inch in diameter, which make the vessel appear opaque, and leave only a narrow and irregular canal along its axis, the walls of which canal appear formed by them, not by any membrane lining them. The capillaries in the lymphatic glands ramify in contact with (not in the substance of) the external layer of their coats; as they do on the ultimate ducts of the true secreting glands; and they form as fine a net-work.

The general result of these observations is plainly favorable to the opinion of an intimate analogy between the lymphatic and the true secreting glands; of which some account was given in the last Report, and more will presently be said in speaking of the glands without ducts.

Process of Absorption. Some observations by E. H. Weber|| are said to prove that the chyle is first absorbed into the epithelium-cells covering the villi, which, at a certain period, are found full of chyle-globules, and from which it is transferred into the proper cells of the villi, to be by them conveyed to the lacteal vessels. And among the cells of the villi, it is said that two peculiarly large ones are often seen in man during digestion, which touch each other, and of which one contains an opaque-white liquid, and the other a clear fatty matter.

Experiments on absorption have been performed by Mr. Fenwick,§ and their results are very like those obtained by Herbst. 1. He relates two experiments to show that indigo will pass into the lacteals. 2. He relates many to prove that the lacteals do not absorb strychnia, or milk, or other food, from any part of the digestive canal in which the blood is not circulating. 3. He shows, as

* Kliniek; Tijdschrift voor wethensch. Geneeskunde; voor G. C. Gobée, 1844, St. 4. † Anat. and Pathol. Observations, No. viii, p. 44.

‡ The expression has reference to the author's general theory of nutrition; the bodies he describes appear to me identical in aspect with the nuclei or cytoblasts of many secreting gland-ducts.

|| Archives d'Anatomie Gén. et de Physiologie, Jan. 1846.

§ Lancet, Jan. 11, 18, 25, Feb. 1, 1845.

Herbst does, the passage of liquor sanguinis and blood into the lacteals when the adjacent blood-vessels are much congested. 4. In other experiments, oily matters (?) and prussiate of potash injected into the pleura of a rabbit were shortly after found in the lacteals. 5. Others again show that the action of the lacteals and lymphatics is independent of nervous influence. 6. And others confirm the fact already known, that they continue to propel their contents after apparent death. 7. From his experiments, from analogy, and from many ingenious, but I think insufficient, arguments, Mr. F. concludes that these vessels obtain their fluid neither by absorption, nor by secretion from the blood-vessels adjacent to them, but by parts of the contents of the blood-capillaries, according to the degree of congestion, being directly and mechanically effused into them.

Propulsion of Lymph. An attempt has been made by Dr. Bidder* to determine the average quantity of lymph and chyle which flow through the thoracic duct in a given time. The measurements were made by collecting what flowed from the thoracic duct immediately after death. In five cats, the fluid continued to flow from one to six minutes, and, judging by the quantity collected in this time, the average quantity which would have flowed in an hour, was 373 grains (the extremes being 276 and 480 grains); and the average proportion between the weight of the cat and the weight of the chyle and lymph, which at the same rate would have flowed in twenty-four hours, was as 5·34 to 1 (the extreme proportions being as 6·8 : 1, and as 5·1 : 1). In two dogs the average rate of efflux (similarly calculated) was 3858 grains in the hour; and the average weight of the dogs was, to that of the chyle and lymph which would have flowed in twenty-four hours, as 6·66 : 1. Now, the average weight of blood in cats is, to the weight of their bodies, as 1 : 5·7; and of dogs, as 1 : 4·5; hence the quantity of fluid daily traversing the thoracic duct of a cat, is about equal to the whole quantity of blood in it; and the quantity of the same in a dog is equal to two-thirds of its blood.

Lymphatic hearts. Volkmann† has proved that the rhythmical movements of the lymphatic hearts of frogs depend on the direct influence of portions of the spinal cord. They cease on the instant of destroying the cord, though those of the blood-heart continue for many hours. But repeated experiments showed that the contractions of the anterior hearts would continue long while they retained a nervous connexion with the cord about the third vertebra; and those of the posterior hearts as long, if their connexion with the cord at the eigth vertebra was uninjured. The movements thus continued in the lymphatic hearts though the whole of the cord, except these portions, were destroyed; and on the instant of destroying either of these portions, though all the rest of the cord were intact, the movements of the corresponding hearts ceased. Removal of the brain had no influence. The movements continued also after the division of the posterior spinal roots (they were therefore not reflex), but they ceased directly on the division of the anterior roots.

(*To be continued.*)

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* Muller's Archiv, 1845, Heft i.

† Ibid. 1844, Heft iv, and Wagner's Handworterbuch, art. Nervenphysiologie, page 489. Valentin, in the body of his Physiologie (vol. ii, pag. 767), denied the truth of these experiments, but in a later appendix confirms them. For other papers on the lymphatics, see Oesterlen, as presently quoted; H. Nasse, an elaborate article on the lymph in Wagner's Handworterbuch; and papers on some unusual arrangements of the lymphatics, by Svitzer and Patruban, in Muller's Archiv, H. ii. 1845.

Part Fourth.

MEDICAL INTELLIGENCE.

FOREIGN.

1.—*On Amorphous Quinine, as it exists in the substance known in commerce as Quinoidine, A new discovery published exclusively in the Lancet.* By Baron Liebig.

In the preparation of sulphate of quinine, after all the crystals which can be obtained are separated, a dark-coloured mother-liquor remains, having an extremely bitter taste. On the addition of an alkaline carbonate, this liquid loses its colour and bitter taste, depositing, at the same time, a yellowish-white, or brownish precipitate, which, after being rinsed with water, and exposed to a gentle heat, agglutinates into a coherent mass, exhibiting the appearance of resin.

From the experiments of Sertuerner, Thiele, Bucholtz jun., Koch, and other chemists, it has been long known that this resinous substance possesses the properties of a base, that it neutralizes acids perfectly; but the salts which are formed by these combinations with acids, have baffled all attempts at crystallization.

Sertuerner, who was the first chemist to separate this resinous substance from the mother-liquor of sulphate of quinine, considered it to be a distinct and peculiar organic base, existing in yellow and red cinchona barks, associated with quinine and cinchonine. He assigned to this, as he supposed, new substance, the name quinoidine, and greatly extolled its medicinal efficacy, in which he declared it was in all respects equal to quinine. In his journal, (Ueber die neuesten Fortschritte in der Chemie, Physik und Heilkunde, Bd. iii., No. 2, page 269,) he terms it "a true fever-destroyer."

Subsequently, this substance, under the term quinoidine, has been employed medicinally in many places, and even introduced into the lists of commercial articles or price currents of many of the druggists of Germany.

In certain mother-liquors of quinine left in the preparation of the sulphate, which were analyzed by Henry and Delondre, and also a sample of quinoidine examined by Geiger, these able chemists discovered an amount of quinine and cinchonine, accompanied by a resinous substance which they considered impeded the crystallization of the sulphates of the two bases, and which in their experiments they failed to separate. The results obtained by these chemists, and the inferences obviously deducible from these results, rendered it indubitable that the medicinal efficacy of quinoidine must vary according to the greater or less proportion of quinine it may happen to contain. Now, there cannot be a doubt but that this uncertainty with respect to the relative amount of quinine in commercial quinoidine, has prevented many physicians from prescribing the latter as a remedy, notwithstanding the testimony borne to its efficacy.

Having occasion, some time since, to pass through Coblentz, I procured from Messrs. Jobst and Co., of that town, a sample of quinoidine, for the purpose of employing it for the preparation of quinoleine—a substance discovered by Gerhardt to result from the transformation of quinine, and to which much scientific interest attaches, in consequence of the recent discovery of Professor A. W. Hofmann, that quinoleine is identical with leucol, a body which is one of the components of the essential oil of tar, prepared from anthracite coal. It then occurred to me, that if the sample of quinoidine which I had procured contained quinine, it must yield a corresponding amount of quinoleine, and that, consequently, a very simple method of testing quinoidine for the amount of quinine it may contain might be based upon this property of quinine to be transformed into quinoleine.

On subjecting the sample I had obtained (which amounted to several ounces) to distillation with strong potash ley, I confess I was surprised at the large amount of quinoleine produced, which proved the presence of a far larger proportion of quinine than could have been anticipated. This unexpected result induced me to subject quinoidine to a stricter examination; and in order to avoid being misled by accidental circumstances, I procured, beside the Coblentz sample, specimens from Messrs. Hess, Leissler, and Fiedler, of Mayence, and from Messrs. Mettenheimer and Simon, at Frankfort, and also from a druggist at Hamburgh.

These various samples of quinoidine I received partly in irregular-shaped masses, and partly as square cakes of a darker or lighter brown colour, which, by the warmth of the hand, became soft and flexible, but were readily pulverizable in the cold. The operation of powdering it imparted to it an extraordinary degree of elasticity. All these samples were completely insoluble in cold water, but scantily soluble in hot water, imparting to the latter a strongly bitter taste. I may here however observe, in passing, that some commercial specimens which I have since seen are soluble in cold water, arising from a considerable admixture of other substances; differing also, from the same cause, in many of the following properties :—

All the first samples I speak of dissolved in alcohol, in the proportion of one part to two of the menstruum; and from this alcoholic solution, water precipitates copious, yellowish-white, resinous flakes, which cohere into a mass like the original quinoidine. Dilute mineral acids, as well as most of the organic acids, dissolved my samples entirely, and by adding a sufficient amount of the substance, became completely neutralized. From these solutions in acids, ammonia and alkaline carbonates precipitated resinous flakes. On agitating the fluid containing these flakes and the flocculent precipitate with an equal volume of ether, the precipitate dissolves in the ether, *with the exception of a dark-brown residue.* On evaporating the ether, a resinous mass is obtained, having all the properties of an organic alkaloid.

Its salts are precipitated by tannic acid. Chloride of platinum produces in its solution in hydrochloric acid a yellow precipitate. Moreover, it dissolves completely in a solution of sulphate of copper with the separation of oxide of copper. Now there exists no resin, nor, indeed, any other substance similar to resin, which possesses this peculiar property.

These observations can leave no doubt whatever as to the chemical character of a considerable proportion of the residue to which the term quinoidine has been applied—namely, that it is a true organic base.

On subjecting the purified substance to elementary analysis, the following were the results :—

1st.—From the quinoidine of Mayence, 0.490 grammes yielded 1.3204 grammes of carbonic acid, and 0.3395 grammes of water.

2nd.—From the quinoidine of Frankfort, 0.618 grammes yielded 1.6575 grammes of carbonic acid, and 0.4250 grammes of water.

3rd.—From the quinoidine of Coblentz, 0.3475 grammes yielded 0.9475 grammes of carbonic acid, and 0.2375 grammes of water.

According to these analyses, this substance contains

	i.	ii.	iii.
Carbon, . . .	73.49 . . .	73.14 . . .	74.33*
Hydrogen, . .	7.69 . . .	7.64 . . .	7.57

The determination of the nitrogen, by the method of VERENTRAPP and WILL, yielded the following results:—

0.515 afforded 0.289 of platinum.
0.617 " 0.401 "

And, consequently, the substance under examination contains, according to the first analysis, 8.04 of nitrogen; according to the second, 9.54 of nitrogen, —the medium of the two analyses giving us as its amount of nitrogen, 8.79.

ANALYSIS OF THE CHLORIDE OF PLATINUM AND THE BASE FROM QUINOIDINE, (AMORPHOUS QUININE.)

I.—0.6663 grammes of the double salt yielded 0.1755 of platinum; 0.8700 grammes of the double salt yielded 1.349 carbonic acid, and 0.303 of water.

II.—0.881 grammes of double salt yielded 0.224 of platinum.

III.—1.0668 grammes of double salt yielded 0.2715 of platinum.

From these analyses, therefore, the following are the proportions of carbon, hydrogen, and platinum, which exist in one hundred parts of the chloride of platinum, and the substance derived from quinoidine:—

	i.	ii.	iii.
Carbon, . . .	32.44		
Hydrogen, . .	3.86		
Platinum, . .	36.23 . . .	26.32 . . .	26.45

Now, if we compare the proportion of carbon, hydrogen, and platinum, existing in the chloride of platinum and this base, derived from quinoidine, with the amount of the same elements present in the corresponding chloride of platinum and quinine; and, further, the amount of carbon, hydrogen, and nitrogen contained in the substance under examination, with the proportion of the same elements as they exist in quinine; we perceived at once that the two substances have identically the same composition.

Quinine according to the formula,

$$C_{20}\ H_{12}\ NO_2$$

contains—

Carbon, 74.33; hydrogen, 7.75; nitrogen, 8.62.

Chloride of platinum and quinine, according to the formula,

$$Cl\ H\ C_{20}\ H_{12}\ NO_2 + Cl\ Pt$$

contains—

Carbon, 32.38; hydrogen, 3.53; platinum, 26.83.

The inference from these experiments, then, is irresistible: the unchrystalline substance derived from quinoidine bears exactly the same relation to ordinary quinine that unchrystalline sugar (barley-sugar) bears to chrystalline (sugar-candy.) Both yield the same products of decomposition; both have the same atomic weight, and identically the same composition; they differ only in form; in one word, the one is chrystalline, the other, *amorphous*.

I deem this to be an important discovery, when we consider the high price of quinine, the possibility of a check to the supply of cinchona bark from the countries producing it, and the amount of the crude quinoidine which has accumulated since the manufacture of sulphate of quinine was commenced. Quinine, indeed, seems to be absolutely indispensable for the treatment of diseases; the progress of civilization in modern times has depended, far more than has been

* Carbon=75, according to Prout and Dumas.

conceived, upon the discovery of a remedy for the fevers which prevail where tillage is imperfect, and in new and unbroken soils.

This chemical investigation has thrown an interesting light upon the testimonies borne to the efficacy of quinoidine in the treatment of fever, and the highest encomiums have been passed upon it; but the commercial specimens have differed very much in value; while some have consisted nearly altogether of amorphous quinine, others have contained only a small per centage.

It is necessary that the amorphous quinine should be separated from all admixtures and impurities, and prescribed in its pure state. There can be no doubt but the same substance will produce the same effect on the animal organism, whether exhibited in a chrystalline or an amorphous state. The system, as we may say, makes no difference in such a case. As I have already observed, the mystery about quinoidine is completely solved by the discovery, that it usually contains a very large per centage of pure quinine in an amorphous state.

In a commercial point of view, it is certainly a matter of great importance that we should be able to judge by the mere external appearance of a remedy, of its purity; and consequently, how far we may rely upon its efficacy. This is thought to be the case with the chrystalline sulphate of quinine, whilst the non-chrystalline form of quinoidine has probably led to a disregard of the evidence for its usefulness, even more than the fact of its being, as usually sold, an admixture of various substances. But with respect to the mere amorphous form, when the quinine is separated from all its adhering impurities, it is in the same case with opium, castor, and many more of the most efficient remedies which we possess, particularly with the extracts of our pharmacopœias. It is necessary to be assured of their purity before we employ them, but their amorphous form does not prevent their use. In many of these cases, indeed, having no direct or ready way of testing them, we rely solely upon the honorable character of the merchant and dealer; but we have a completely satisfactory test for the purity of amorphous quinine. Few medicinal agents afford so ready a means of distinguishing them, and detecting admixtures, as the organic alkaloids; but if these tests are not employed, it is easy to be deceived in purchasing chrystalline sulphate of quinine, as the amorphous.

Amorphous quinine is completely soluble in dilute sulphuric acid, and in alcohol, as I have said above; it is also completely soluble in a solution of sulphate of copper, with separation of oxide of copper. And if its solution in a dilute acid yields, upon precipitation by means of ammonia, exactly the same amount of precipitates as the weight of the substance originally dissolved in the acid, there can be no doubt remaining as to the perfect purity of the sample under examination.

It only remains for me to observe, that no dependence should be placed upon the ordinary quinoidine of commerce. As I have already stated, some samples which I have seen, dissolve incompletely in water, forming a dark-brown muddy fluid; these have been probably produced by simply evaporating the mother-liquors of sulphate of quinine to dryness. They are therefore uncertain mixtures of various substances with sulphate of amorphous quinine, with or without excess of acid, so that in purchasing such specimens, the buyer is paying the price of an organic alkaloid for sulphuric acid, &c. The pure amorphous quinine should be separated, and it would then form a most valuable remedial agent; but the prescriber must be assured of its purity, and the test I have given will suffice for this purpose.

2.—*Monograph on the Action of Animal Charcoal, on Organic and Inorganic Substances*, by F. C. Calvert.

In consequence of the daily increasing importance acquired by animal charcoal in the arts, I have thought it might be interesting to give a summary of recent researches on the subject, and to preface it by a short notice of those heretofore published.

It is to Kilhs that we owe the discovery of the decolourizing property of animal charcoal, (*Journal de Physique*, 1793.) But the fine researches of M. M. Payen, Bussy, and Desfosses, were those which gave us exact notions respecting the action of animal charcoal, and it was M. Payen especially, who called the attention of chemists to the absorbing property of charcoal, by showing that this substance removed lime from water, and even from the salts which are formed with lime as a base. M. Lassayne afterwards showed that charcoal removed iodine, even to the last traces, from liquids which dissolve it.

M. Graham has discovered that charcoal possesses the property of completely eliminating a very large number of basic metallic salts dissolved in pure water, or water containing ammonia, but that the neutral salts and arsenious acid are exempt from its action.

Finally, M. Chevalier has published an interesting essay on the subject of the action exerted by animal and vegetable charcoal on the soluble salts of lead, that is to say on the nitrates and acetates; he stated that the charcoal removes the oxide of lead from these salts, leaving the acid free. The author calls the attention of chemists to the difficulties which may arise when in cases of poisoning the liquids are decoloured by means of animal charcoal. The fact, cited in this memoir support the remarks of M. Chevalier.

The new researches of which we are about to give an account, are due to MM. Warrington, Wapen, and Gorrod, who have examined only the action of purified* animal charcoal on organic and inorganic substances. By using this charcoal they have been able to explain certain anomalies which have been observed, and to give a great number of new facts.

M. Warrington remarks that by using purified animal charcoal, it acts contrary to the statements of Mr. Graham, on certain neutral salts, such as the sulphates of soda and magnesia, and the chloride of barium.

M. Wapen has proved this action of charcoal by experimenting with the following salts, among which are some neutral salts.

Sulphate of copper,
" of zinc,
" of protoxide of iron,
" " of chrome,
Bichloride of mercury,
Acetate of the protoxide of iron,
Neutral acetate of lead,
Tartrate of antimony and potash,
Nitrate of cobalt,
" of silver,
" of the protoxide of mercury,
" of the deutoxide of "

It generally requires 30 parts of purified animal charcoal for one of the salt held in solution; but the author remarks that he has not been able to remove the last traces of certain salts whatever, by any excess whatever of the charcoal he may have employed. Thus the protoxide of iron is decomposed into an insoluble sub-salt, and a soluble acid which remains in solution. The saline compounds which present this reaction are incompletely precipitable by animal charcoal, and consequently the acid salts should be ranged in the same category.

Not only metallic oxides are precipitated by the charcoal from their solution, but some metallic acids also, such as antimonic, tungstic, and plumbic

* The purified animal charcoal was prepared by boiling animal charcoal several times in chloro-hydric acid, then with water, and finally calcining it in a close vessel in an obscure red heat.

acids. The arsenites, the arseniates are exceptions, as also some other salts, such for example, as alum. The chromate of potassa, and chromate acid, are not only reduced by the charcoal, but the chromate of potassa passes to the state of a carbonate. Finally, a solution of the ioduretted iodide of potassium is decoloured by purified animal charcoal.

M. Wapen refutes the observation of M. Mulder, who admits that charcoal reduces to a metallic state the oxide of lead contained in the neutral acetate.

The action of charcoal on organic substances presents some remarkable facts. M. Warrington has shown that animal charcoal removed, in a cold state, only some particular bitter principles from their solutions, and excited no action on others; thus, the bitter taste of porter disappeared quickly when filtered through charcoal, whilst the extracts of cinchona, of opium, of nux vomica, and the salts yielded to these substances undergo no changes in their degree of bitterness. M. Warrington correctly observes that these diversities of the action of purified charcoal may be employed so as to enable us to determine whether any of these substances have been added to malt liquors, as is done in England, to augment their bitterness. For this end, we must avoid warming the liquids with the charcoal, for it has been observed that with the aid of heat the charcoal possesses the property, when in proportion of 30 parts to one of the matter, of removing not only the above mentioned bitter principles, but also all those submitted by him to the experiment. The manufacture of the sulphate of quinine being now an important branch of commerce, M. Warrington sought to determine the degree which charcoal possesses of absorbing this compound. With this view he dissolved 0,1 grain of bisulphate of quinine in two ounces of water, to the liquid heated added charcoal until the bitterness of the solution disappeared; in order to effect this it was necessary to employ about 9 grains of the charcoal.

These interesting results had already been anticipated by Dr. Holpff; but M. Warrington has generalized and defined the action of animal charcoal upon the bitter principles; and his researches have acquired a new value since their confirmation by M. Wapen, in a memoir of which I will only give a summary in a tabular form.

The first column indicates the proportion of the bitter matters operated upon; the second, the names of these substances; and the third, the weight of charcoal employed in the absorption of each respecting.

BITTER PRINCIPLES.

Weight of each Substance.		*Weight of Charcoal.*	
10 grains dissolved in 60 grains of water	Colocynth,	10	
	Columbo, (radix)	30	
	Gentian, (id)	20	
	Quassia,	30	
	Cascarilia, (cortex)	30	
2 grains dissolved in 60 grains of water	Menyanthes trifolia,	30	
	Aloës, ...	40	
RESINOUS PRINCIPLES.			
1 drachm of the tincture diluted with its volume of alcohol.	Resen of Guayac, .	13	so as scarcely to precipitate with water.
	" of Jalap, . .	26	
ASTRINGENT PRINCIPLES.			
1 gr. of the extract dissolved in 60 grs. of water.	Nut salts, ...	20	
	Tannen, ...	20	
10 grs. dissolved in 60 grains of water.	Rhatania, (radix) .	20	so as not to give a red colour with the salts of iron.
	Cinchona, (cortex) .	20	

I will conclude with a summary of the results, obtained by M. Gorrod, respecting the property, possessed by perfectly pure animal charcoal, of acting as an antidote to certain poisons.

1st. Charcoal added in sufficient quantity should possess not only the property of removing the active animal and vegetable principles, but might even remove these principles from a solution analogous to the gastric juice, maintained at the temperature of the stomach.

2nd. Charcoal should likewise form insoluble compounds with arsenious acid, and different metallic salts; whence the author concludes that charcoal is an excellent antidote against the poisonous effects of arsenious acid; he even affirms that it is as efficient in this case as the hydrated peroxide of iron.

3rd. The insoluble compounds formed by animal charcoal with poisonous substances, having no action upon the animal economy, we may employ this agent successfully in cases in which the poison has not yet been absorbed by the organism.

4th. This substance is especially applicable as an antidote against the effects of those substances which produce their effects in very small doses, such as belladona, stramonium, nux vomica, aconite, and opium. The quantity given should vary with the nature of the poison; thus, 15 grains of charcoal is sufficient for 20 grains of nux vomica, and the same quantity for one grain of strychnine; but it is always better to employ an excess of the charcoal.

Finally, it is of the first importance to employ animal charcoal which is completely deprived of calcarious salts; for the common animal charcoal possess no action as an antidote, which last circumstance accounts for the fact, that M. Gorrod obtained the absorption of arsenious acid by the charcoal, whilst other experimentors have published contrary results.

—*Journal de Phamarcie et de Chimie.*

AMERICAN MEDICAL INTELLIGENCE.

The Southern Journal of Medicine and Pharmacy.

In the September number of this Journal, we are twitted with making mistakes in our medical symbols; —— "the sign of the drachm being substituted in many places for that of the ounce, in the New Orleans Journal."

The following letter will show that if there be mistakes in the matter it is not on our side: Edrs.

"Mobile, Sept. 15th, 1846.

I have just received your's of the 10th, and hasten to reply.

The Editors of the *Southern Journal of Medicine and Pharmacy* have certainly made some mistake, when, in allusion to my article on the *Stillingia*, they thus write of me—"*He gives the following formula as the one most commonly used by him:*"

℞. Stillingia root, } each 3 *ounces.*
Sarsaparilla " }
Senna leaves, - 2 "
Water, - - - 32 " M. &c.

[P. 578, No. 5.]

Now this is unquestionably an error, at variance with the letter of Dr. Simons, now lying before me, with my communication to your

Journal, and with my case book, into which the formula, as published by you, is frequently transcribed.

The letter received from Dr. Simons, while I practised in Georgetown, South Carolina, dated April 5, 1834, contains this prescription :

Rad.	Stillingiæ Sylvat.	ʒ iv	(drachms.)
Rad.	Sarsapar. -	ʒ iij	"
Fol.	Sennæ. -	ʒ ij	"
Aq.	Fontan. ℥ xxii	——	&c.

In using it I varied it only by making the proportions of the first two ingredients equal, but I speak from the record before me (*there are no ounces in Dr. Simons'*) *formula as written to me*, except in specifying the quantity of water. At page 42 of your July number, you have copied correctly from my manuscript.

Very truly your's, &c., A. LOPEZ.

NEW ORLEANS, NOVEMBER 1, 1846.

On account of the unexpectedly great extent of our first and second parts in this number, we again find ourselves completely cramped in our notice of Medical Intelligence, both Foreign and American ; but especially the latter. Some reviews and editorials are unavoidably excluded, and we are reduced to the narrowest limits for what we have to say in regard to the health of the city and country.

HEALTH OF THE CITY.

When our last number was issued only one or two cases of supposed yellow fever had occurred, which being of a somewhat doubtful character, we deemed improper to notice amongst the diseases of the day ; more especially as it might excite unnecessary alarm. Since then this disease has increased considerably, and *sporadic* cases have occurred in all parts of the city from one extremity to the other. We have heard of no attempt to trace the disease to any *central focus* from which it radiated, nor has there appeared to be the least connection between the cases occurring in different parts of the city. First we had a case on Esplanade street—then one on Rampart street—then on the verge of Lafayette—then in the lower part of the Third municipality—then about the lower market—then in the back part of the city, and so on without any connection or communication whatever. It has prevailed to the greatest extent in the Third municipality or lower part of the city, where we understand one physician has attended between forty and fifty cases in private practice. In the Second municipality hardly any physician has attended more than half a dozen cases. The disease has been confined almost entirely to the lower class of society, chiefly Irish and German labourers ; consequently the cases have been mostly congregated at the Charity Hospital, where twenty or thirty have been seen at a time, and five or six deaths in a day. At no time has there been enough of the disease to be entitled to the name of an *epidemic.*

The general character of the disease has certainly been quite different from that of ordinary epidemics occurring earlier in the year. It has been less violent and inflammatory, required less sanguineous depletion, and was much slower in its progress, often terminating in black vomit from the 9th to the 15th day. The general tendency to jaundice remarked in the fevers and dysenteries of the summer and early part of autumn, and the common features of this disease, induced some experienced physicians to deny that it was genuine yellow fever. They were rather inclined to pronounce it a malignant *gastro-duodenitis*. Yet many cases have been known to be attacked in the usual way, with chilliness, pains in the head and back, followed by high fever which terminated fatally with black vomit on the fifth day. We knew one case to take this course, the subject a vigorous and healthy young man having been in the city but twenty-four hours when he was attacked. But we will say no more about it at present, as we expect to give a full account of the origin, progress, character and treatment of the disease in our next number. With this exception there has been no disease worthy of special notice, the state of health has been most excellent. The citizens who have spent the summer abroad have returned unusally early, and the influx of strangers is very great for the season.

The weather has been for the most part dry and pleasant. On the night of the 19th October, it was nearly cold enough for frost. (See Mr. Lillie's Meteorological Observations.)

The river is 12½ feet below high water mark.

Bill of Mortality.—As we have not been able to give regular reports of the mortality during this year, we shall now let it alone till the end of the year, when we will give a full annual report together with a comparison with other large American cities. We may remark, however, that from published reports so far, our city will compare favourably with any other.

HEALTH OF THE COUNTRY.

As far as we have been able to ascertain, the general health of the South-western country has been unusually good. We have heard of more sickness about Nashville, Tennessee, than any where else. We should be pleased to receive an account of it from some of our friends there. We return thanks for the following letters, and welcome, with gladness, our new correspondent at Grand Côteau—hope he will continue his favours. The letter from Pensacola will be read with interest.

U. S. Naval Hospital, Pensacola, Oct. 12th, 1846.

"I have to announce to you, Messrs. Editors, that a disease has appeared here this season similar to that which was noticed for the first time during the summer and autumn of 1844, a description of which I subsequently gave in conversation to Dr. Fenner. I designated it on that occasion as *Febris Intermittens Perniciosa*, and said that I believed it to be the same which had been encountered by our army surgeons in South Florida, described by them as malignant intermittent;—previously by Ramazini, and Torti, of Italy, and by Dr. Gouraud of the South of France, and more recently by the military surgeons of France, serving in Algeria.

Its chief features are, that it attacks sometimes with a distinct chill, sometimes with sensation of heat and cold, sometimes with fever without sensation of chilliness. In light cases it intermits after six or twelve hours, sometimes there are only slight remissions, and the fever only disappears after three or four days. The pain in the head, along the spine and extremities, is intense—not unfrequently vomiting—pains in the globes of the eyes, intolerance of light, cramps of the legs. The grand characteristic is extreme prostration, even when the fever subsides in a few hours—it is on account of this symptom, and the speedy termination in death where remedies do not act, that I have regarded the disease malignant.

The tongue is swollen and indented with the impression of the teeth, not coated in most cases, and only in a few instances is there great thirst. No *black vomit*, even when the termination is fatal.

The remedies used here are cups to nucha and spine, sinapisms to extremities, after pediluvia, and calomel with quinine, in doses of a scruple of each, frequently the first dose only ten grains of each. These are given in all stages of the disease, apparently with equal benefit.—The bowels are to be opened by following the dose with castor oil or injections, or both—bilious evacuations are easily obtained, first of a greenish, then of brownish coloured matter, sometimes almost black and subsequently of fæces mingled with healthy coloured bile.

The postration continues for a day or two after the subsidence of the fever, and then the patient begins to walk about, having a ravenous appetite. He is subject to relapses—we have had six attacks in the same subject—the treatment each time is the same. The sulphate of quinine is the grand remedy, and of that we have this season expended nearly fifty ounces in this hospital.

During the quarter, ending the 30th of September last, we have admitted 218 patients into this hospital, all of diseases entirely foreign to the one in question, and of these I have reported upwards of 200 attacks from this disease, and not one death. It visited my house and took one of my little ones and a servant. At the military encampment within 3 or 400 yards distant from me west it has proved very fatal among persons who came since last Spring from as far North as Holton, Maine.

It extended quite late in the season, to the Navy Yard, a mile east of me, and has carried off the worthy chaplain of the yard, and a young lady. The cases in the Navy Yard have been more severe than those of the hospital. It has scarcely reached the city of Pensacola, but it is still raging in my vicinity, but with less virulence. I have been a subject of it again this year, as I was in 1844, but recovered this time without being long absent from my duties." J. H.

Columbus, Miss., October 15th, 1846.

"We had a remarkably healthy winter and spring. Bowel affections and pneumonia, which heretofore gave us much trouble, were remarkably light. The sickly season here is generally confined to August, September, and the first of October; but this year, marked cases of bilious fever commenced early in July. The first cases gave us some uneasiness, but they soon yielded to very mild treatment. But although we have to record the history of a long unusually sickly season, it is gratifying to remark the mildness of the diseases we have had to combat.—We have had scarcely any fatality. The forms have been remittent and intermittent fever. The cases generally have yielded to quinine alone—purgatives and emetics have not been necessary.

We have had a great deal of rain the past summer. Fourteen days of rain in August—merchants much injured by the goods moulding—farmers complaining of great injury done their crops of cotton. Seven days rain in September—the first of the month warm. The middle cold, which made the disease more formidable if it had no effect in increasing the number of cases. The latter part of this month the worm in the cotton committed great depredation. Every leaf in the largest farms was eaten up clean. The cotton fields presented the appearance of winter. The effluvia from the cotton field was sickening, and almost intolerable.

The first of this month, (October,) has been cool and very dry—not a drop of rain yet—we still have a good deal of sickness—cases more obstinate. Since the middle of September, I have seen four cases of congestive fever, and that is all I have seen this year." L. & L.

Grand Côteau, St. Landry Parish, La., Oct. 5, 1846.

"The health of this region, within the immediate sphere of my professional occupations, has been a continued blessing to the inhabitants. Throughout the whole season, from June to October, an extraordinary exemption from disease has unintermittingly prevailed. I have witnessed none of the usual severe remittents or congestive fevers which have been heretofore our annual visitors; and the endemic intermittents have been characterized by the mildest tertian or double-tertian types. The measles are, however, still among us,—but so mild in attack, as hardly to require the attention of the profession.

The College of St. Charles, and the Sacred Heart Academy, numbering in the aggregate nearly 200 elèves of both sexes, and together with the *Religeuses*, professors, attendants and servants, not less than three hundred souls, serve as an index or thermometer to the healthy temperature of the prairie. For the last three months, but three cases of any importance have occurred in either institution. In the former, a severe *gastro-enterite*, with much functional, bilious derangement, terminated favorably after 12 days' duration; and a fatal case of congestion of the lungs upon an hereditary strumous diathesis; the other at the Convent, a stuborn quotidian upon a lymphatic nervous ideosyncracy which may perhaps, under a changed type, yet "linger in the lap of May." * * * * * * *

Notwithstanding the extraordinary exemption from disease, in this vicinity, there would have seemed to exist all the materials for the production of our usual severest Autumnal epedemics. With continued showers, and occasionally perfect tempests of rain, the thermometer has varied but little during the months of July and August, from the degrees of 85 to 96, in the shade; and our population have been consequently subjected to "heat and moisture" sufficient it would appear to engender the most noxious malaria. But the ordeal is now passing, as we experience the refreshing North winds—the forerunners of health. We are grateful for the past, as well as the prospect before us."

E. M. M.

MONTGOMERY, Ala., Sept. 15, 1846.

"The summer, here, though the most rainy one since 1836, has been the most healthy we have ever known, with the exception of that of 1845, which was the dryest within the recollection of our oldest citizens. Thus our dryest and wetest summers, for the last ten years at least, have been the two most healthy.

We have at present, principally, the various forms of remittent fever to treat—with now and then an intermittent, a case of colic, cholera morbus or diarrhœa.

I regret that the want of variety in the character of our diseases, for some time past, to which may be added a great improvement (not to be regretted) in the health of the place, necessarily render my communications brief, and of little interest." W. M. B.

PARISH OF ST. MARY, La., Oct. 15, 1846.

"At the date of my last communication, a heavy rain commenced falling, which continued for three days. The whole country was flooded; many small streams were overflowed, and in the upper part of the Attakapas, and in Opelousas, much damage was sustained upon plantations. Since then but little rain has fallen. The last week in September was the hottest of the season—the thermometer in the shade at 95 and 98 at mid-day. For the last month no rain of consequence has fallen, and the planters are now complaining for want of it. The weather has now changed; the North wind blows, and the nights are cool;—as a consequence, some cases of chills have occurred; but the few cases that occur are mild, and yield readily to treatment. I have not seen a case of congestive fever this season.

I have heard of some neighborhoods in which considerable sickness has existed, but have learned no particulars relative to it." J. B. D.

ABSTRACT OF A METEOROLOGICAL JOURNAL FOR 1846.

BY D. T. LILLIE, AT THE CITY OF NEW ORLEANS.

Latitude, 29 deg. 57 min.; Longitude, 90 deg. 07 min. west of Greenwich.

WEEKLY. — 1846.	THERMOMETER.			BAROMETER.			COURSE OF WIND.	FORCE OF WIND, Ratio 1 to 10.	Rainy Days.	Quantity of Rain. — *Inches.*
	Max.	Min.	Range.	Max.	Min.	Range.				
Aug. - - 29	89.5	72.5	17.0	30.15	30.01	0.14	S.E.	3	3	0.590
Sep. - - 5	86.5	74.0	12.5	30.10	29.95	0·15	N.E.	3	5	1.725
" - 12	88.0	73.0	15.0	30.20	29.88	0.32	S.E.	3	6	3.152
" - 19	90.5	73.2	17.3	30.14	29.96	0.18	N.	$2\frac{1}{4}$	0	0.000
" - 26	84.5	71.0	13.5	30.17	29.77	0.40	E.	3	4	0.821
Oct. - 3	81.0	65.0	15.0	30.11	29.88	0.22	N.	3	0	0.000
" - 10	83.5	70.0	13.0	30.28	30.09	0.19	E.	3	0	0.000
" - 17	81.0	57.0	14.0	30.17	30.03	0.14	N.E.	.3	2	0.020
" - 24	73.0	49.5	23.5	30.33	30.16	0.17	N.	$2\frac{1}{2}$	0	00.00

During the above nine weeks we have had rain on 20 days, though only to the moderate amount of six inches and 268 thousandths. For the last six weeks we have not had so much as *one inch of rain.*

THE

NEW ORLEANS

MEDICAL AND SURGICAL JOURNAL,

DEVOTED TO MEDICINE

AND

THE COLLATERAL SCIENCES.

EDITED BY

W. M. CARPENTER, M. D.
E. D. FENNER, M. D.
J. HARRISON, M. D.
A. HESTER, M. D.

"Summum bonum Medicinæ, sanitas."—Galen.

NEW-ORLEANS CHARITY HOSPITAL.

JANUARY, 1847.

NEW-ORLEANS.
PUBLISHED BY S. WOODALL, 49, CAMP STREET.
1847.

TO READERS AND CORRESPONDENTS.

We have received a communication from Dr. A. R. Kilpatrick, of Woodville Mississippi.

The following books and pamphlets have come to hand and will be noticed in future:

The Writings of Hippocrates, on Galen. Epitomised from the original Latin Translations. By JOHN REDMAN COXE, M. D. Philadelphia, Lindsay and Blakiston, 1846. 8vo. pp. 681.

An Anatomical Description of the Organs of Circulation and Respiration. By C. E. HASSE, M. D. Philadelphia, Lea & Blanchard, 1846. pp. 377.

Lectures on Natural and Difficult Parturition. By EDWARD W. MURPHY, A. M. M. D. New York, S. S. & W. Wood, 1846. pp. 281.

The Water Cure, or Hydropathy. From the British Foreign and Medical Review. By JOHN FORBES, M. D. F. R. S. Philadelphia, Lindsay & Blakiston, 1847.

Address to the Class of the Medical College of Georgia, at the opening of the Session of 1846–7. Containing a Sketch of the Improvements in Medicine during the present Century. By L. A. DUGAS, M. D. Professor of Physiology and Pathological Anatomy. Published by the Class.

We have received the *Dublin Quarterly Journal of Medical Science*, from the Editors—and our regular American exchanges.

Gross's Pathological Anatomy, and Draper's Chemistry will be noticed in our next.

ERRATUM.

Page 469, line 6 from the bottom, read "*by* the Board of Health," instead of "*and* the Board of Health."

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THE NEW ORLEANS

MEDICAL AND SURGICAL JOURNAL.

JANUARY, 1847.

Part First.

ORIGINAL COMMUNICATIONS.

I.—*Remarks on Periodicity or Intermittence as an Element of Disease.* By W. M. CARPENTER, A. M. M. D. Professor of Materia Medica and Therapeutics, in the Medical College of Louisiana.

The subject of intermittence is one surrounded by difficulties which appear almost insurmountable in the present state of science. Its intimate relations with the incomprehensible and occult functions of the nervous system seem, indeed, to render it almost impossible for us to attain to a complete and positive knowledge of it, until we are fully enlightened upon the physiology of that system. In writing on this difficult subject I am by no means so presumptuous as to imagine that questions, which have so long employed the greatest intellects in our profession, are to be settled by the limited materials at my disposal; and my only desire is to throw out some suggestions, and give such reasons as I possess in their support. My view is, that, on vexed questions, any one who forms, and expresses an opinion, right or wrong, may give an impulse to investigation, and consequently to the general progress of knowledge.

Neither do I pretend to originality in the ideas advanced, for though, at the time of arriving at my conclusions respecting the relations of intermittent diseases, I was not acquainted with the somewhat similar views entertained by some distinguished writers on this subject, they are familiar to me now. Boerhaave, Stoll, Hildenbrand, and Guerin for example, referred intermittent fevers to impressions modifying the functions of the nervous centres, and Reil* attributed the intermittence of diseases to the oscillations of *organic action* in general, and especially to that of nutrition. Roche,† too, entertained much the same opinions. But some of the recent researches in physiology seem to give additional

* Pathologie generale. † Roche et Sanson Pathologie, v. 1, p. 43 et seq.

confirmation to these opinions; and as I have heard of no attempt to bring them to bear upon the subject, it appeared to me proper to call the attention of the profession generally to the subject, hoping that those, who are better prepared to do it justice, will hereafter favour us with their views.

No phenomenon seems to be so universally prevalent in the course of the vital processes as a certain tendency to oscillation, which seems generally to obey something like definite laws, as regards the intervals of time which separate the movements one from another. Nearly all the functions of the economy are more or less decidedly intermittent in their course, and in many cases this intermittence seems to be an absolute requisite to the act of the organ, and may be regarded as an element of the function in its normal state. Thus, the contraction of a muscle is known to consist of the aggregate of a multitude of little alternate and oscillatory contractions of its constituent fibres; the heart's action is alternate; the secretory functions are intermittent in intensity;* the animal temperature has its diurnal oscillations,† and it is well known, that the nervous system has its periods of energy and repose, which are mutually dependent, and Liebig has shown the reasons, why, in this system periods of repose must alternate with those of activity. Indeed, intermittence seems to be an element essential to the existence of the normal action of the economy. "We may," says Bichat,‡ "exempt the organs of animal life, for a certain time, from the law of intermittence, by multiplying around them the causes of excitement; but they must at last submit to it, and after a certain period nothing can suspend its influence. Exhausted by prolonged vigilance, the soldier sleeps under the cannon's mouth, and the slave under the lash, and the criminal in the midst of the tortures of the rack."

It is not, however, only in the progress or intensity of the functions, that the intermittent tendency is shown; but in the processes of secretion, we find the products of the organs varying in kind, to some extent, periodically. Thus Milon and Laveran§ have shown by their experiments on the "*elimination* of *metallic substances introduced into the animal economy*," that some of these substances disappear from the urine, and again appear after a certain time; and in fact, "*follow a true intermittence in their elimination.*" Dr. Wright‖ has likewise obtained similar results respecting the elimination of alcohol from the system by the kidneys. He instituted the following experiment on a man whose ureters opened externally. Three ounces of whiskey was administered and the urine collected, by applying a test tube to each ureter. The tubes were corked and replaced every two minutes for the space of half an hour.

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* Experiments of Milon and Laveran on the elimination of substances from the animal economy. Journal de Chemie Medicale, December, 1845; also Wright's experiments, in Simon's Animal Chemistry, p. 552.

† Chossat's experiments.

‡ Recherches physiologiques sur la vie et la mort.

§ Journal de Chimie Medicale, Dec., 1845, p. 656.

‖ Quoted in Simon's Animal Chemistry, p. 552.

The following table represents the amount of urine in each of the tubes respectively:

1st,	*two minutes.*	½ *drachm.*
2nd,	" "	2 "
*3rd,	" "	5 "
4th,	" "	1 "
*5th,	" "	6 "
*6th,	" "	2 "
7th,	" "	½ "
8th,	" "	½ "
9th,	" "	3 "
*10th,	" "	6 "
11th,	" "	4 "
12th,	" "	8 "
*13th,	" "	7 "
*14th,	" "	6 "
*15th,	" "	4 "

The presence of spirit was only detected in those tubes marked with an asterisk.

It is true, that we are yet in want of well devised and systematic experiments, to establish the intermittence in the action of some of the secreting organs; but, as far as experiments have been made, they would certainly lead to the inference, that they are subject to a kind of intermittence in their action similar to that which is perceived in the more obvious operations of the economy. Indeed, we perceive a marked oscillation in some of those general results or functions, in the production of which the functions of all the organs participate, to a greater or less extent, and the oscillation of which, necessarily implies a similar variation in the intensity of the functions, on which the general function is dependent. The calorigenic function of the economy, for example, undergoes marked oscillations, at regular intervals, and we must infer that the functions upon which this power depends must likewise vary in like manner in intensity. But the calorigenic function may in fact be regarded as dependent upon, and as the product of the functions of all the organs of the economy, and its oscillations imply like variations in the constituent functions. Normal vitality, if this view be correct, is an ever varying resultant of all the functions of the economy; but its oscillations are governed by the same admirable system of adaptations and compensations which are observed to regulate the movements of the heavenly bodies, and all the other oscillatory movements of nature, and thus, these oscillations are perpetuated indefinitely without disturbing the harmony of the whole; and seem even to form essential conditions to the continuance of the healthy state.

But we are surrounded on all sides by physical agencies which may give rise to perturbations in the periodicity of health, and the derangement being an modification of its intensity, may obey any of the periods which mark and measure the oscillations of the functions of the healthy economy, though most generally the periods and intervals are multiples of the diurnal periods, which is that of the oscillations of animal temperature.

The theories which have prevailed respecting the pathology and causes of intermittents.

These theories have generally been so vague and unsatisfactory, that it would be worse than useless, to go through with a tedious enumeration of them. It may, however, be as well to give a general statement of the points which seem to characterize those which are most worthy of notice.

Broussais, Bouisseau, and others, have regarded intermittents, as resulting, essentially, from inflammatory irritation of some organ or tissue, but generally of the gastro-intestinal mucous membrane.

Mongellaz* takes much the same view, regarding inflammatory irritation as an essential to intermittent diseases; but he is more specific in his language, regarding "each access as the return of an irritation," and as independent of the ones which precede and follow it. "Is it not," he asks, "preferable, in order to avoid all hypothesis, to consider the paroxysms as independent of each other, or rather, to see between them no other dependence than that which results from habit, from the periodic influence of certain causes, from the intermittence of the organic functions?" His view is, therefore, that in each paroxysm an irritation first arises in some organ, and from this irritation, the general symptoms take their origin. He viewed the gastro-intestinal mucous membrane as the almost exclusive seat of the *intermittent irritation*, which he thinks is a type of irritation "which exists in nature."

To the above mentioned theories we would answer, that in fatal cases of intermittent disease, dissection often gives no indication of pre-existing irritations; and if, as Mongellaz supposes, irritations or congestions arise at the period of the paroxysms, would it not be more rational in the present state of our knowledge, to regard them in the light of effects, rather than as causes of the paroxysms?

Hildenbrand regarded intermittent affections as resulting from impressions, made by some morbific agents on the ganglionic system of nerves; but he seems to have overlooked the probability, that impressions made on this system might produce continued as well as intermittent affections; for he seems to take it for granted, that he has satisfactorily accounted for the intermittent affections, by referring them to these causes. The opinions of some other writers have already been adverted to.

Piorry, who is followed by some others, attributes the paroxysms entirely to lesions of the spleen, or to impressions made through this organ upon the splenic plexus of nerves. To this supposition we may answer, that in many intermittent affections there are no symptoms whatever referable to the spleen, and even in cases, in which this organ is engorged, it may be rather regarded as an effect, than as the cause of the general perturbation.

The same remarks will apply equally to the theory of those who regard intermittent fever as dependent upon congestions of the capillary system, as this in common with most other structural lesions, observed to occur during the progress of intermittent affections, is obviously of a

* Mongellaz, Monographie des irritations intermittentes, v 1, p. 3 et seq.

secondary nature, and cannot be regarded as the cause of the affections.

Tweedie ventures no opinion, but comes to the conclusion that "it is much to be regretted that no satisfactory explanation of phenomena so remarkable has been given. It therefore appears that the nature of intermittents, as well as the laws which govern their periodicity or intermittence, are involved in the greatest obscurity."

In my remarks on periodicity, it is not my intention to investigate fully the subject of the etiology of intermittent affections, but will confine my remarks mainly to the proximate causes and relations of intermittence in disease.

Proximate cause of periodicity in disease.

In a state of health, the most important functions of the economy are decidedly intermittent in their course, and their periodicity is governed by a power, inherent in the system, and is only in a secondary manner dependent upon the physical influences around. This principle, I will endeavour to illustrate and sustain, respecting some of the most prominent of these functions. This principle, once established, it becomes an easy matter to account for the intermittence of disease, by referring it to the persistent and controlling influence of these physiological oscillations, whose periods and intervals continue to mark and measure its stages and paroxysms. The matter of surprise and inquiry will then become, not why some diseases are intermittent in their course, but, rather, why it is, that all of them are not so. It will then be regarded as a curious circumstance in the history of medical philosophy, that, while continued diseases are really the exceptions to the general law of physiological and of pathological catenation, the principal researches should have been directed to account for intermittence, which is more in harmony with the ordinary course of vital phenomena. It is, however, easy to account for the fact, that intermittent affections have been regarded as greater deviations, from the physiological state, than those, the course of which, is more uniform and regular ; for the physiological oscillations of the vital processes require nice observations for their detection, and the functions, in a healthy state, appear to pursue the even tenor of their way, without any indication of the oscillations, to which they are really subject. Continued affections seem to pursue the same uniform course, that ordinary observation would assign to the vital processes ; while the abrupt and well marked phenomena, observable in the course of intermittent diseases, would seem to stand in a strong contrast to this supposed uniformity in the tenor of the physiological state.

In this place, I will merely throw out the suggestion, that, when any special organic function suffers under a lesion of an intermittent type, the intermittance will conform to the oscillations of the function in its physiological state, or the period of intermission will be governed by multiples of the periods of its normal oscillations. In accordance with this view, lesions of the radical forces of the economy, that is, of innervation and general nutrition, will probably incline to assume an intermittent type, the periodicity of which will conform to some multiple of the oscillations of general vitality in its normal state.

It is to the oscillations of innervation and general nutrition, or of general vitality, that I would principally call attention, as these are the most important, or even the paramount elements, which control and characterize most forms of intermittent diseases. Most of the special functions are so influenced by them, that they can scarcely be regarded as having any independent mode of periodicity; and even when they have modes of oscillation peculiar to them, they are still subjected to the influence of the oscillations of the general vitality. The uterine and ovarian functions may serve as examples of this complication, for though these functions go on with reference to their peculiar periods, still, the influence of the diurnal periodicity is obvious in so many familiar circumstances, connected with these functions and their lesions, that it will scarcely be necessary to enumerate them here. A single familiar example may serve for all; the female bird is governed, like other females, by the general law of periodicity, as to the times, when the ovarium furnishes its crop of ova; they are fecundated, and then the diurnal oscillation in her vitality, exercises its influence in determining the period at which each egg is matured, and at which the forces for its expulsion are called into action.

But, it may be asked, how can we know that the general vitality undergoes such oscillations? and how are its periods to be determined? We would answer, that there are certain phenomena, which must be regarded as having a direct relation to, and dependance upon, the general vitality, and the degree or intensity, with which these phenomena are exhibited, may be taken as a measure of the general vital energy. Take for example the calorigenic function; or the quantity of carbonic acid gas thrown off from an individual at different periods, and either of these may be regarded as an exponent of the vital energy of the individual for the time being. The carbonic acid depending for its production on the metamorphosis of the tissues, its quantity will be an approximative measure of the activity of this process in the general economy; and as this metamorphosis of tissues depends, in turn, as to amount upon the activity of general vitality, of which, in fact, it must be regarded as an important element, the amount of carbonic acid gas, discharged by an individual, may be regarded as a comparative measure of his vitality at different periods. The very same remarks will apply to the relative amount of heat produced by an individual, as this is closely related to the same train of events.

It has long since been shown by Prout, that the quantity of carbonic acid gas, evolved by an individual, is subject to regular variations, which take place with direct reference to diurnal periods: that the quantity increases towards midday, and inclines from that time to midnight, to rise again, gradually, as the succeeding day advances. In other words, there are marked and regular oscillations.

In reference to the diurnal oscillations of animal temperature, the results of Chossat's researches* are fully conclusive, and I hope to be excused for giving them here at considerable length.

M. Chossat has shown, that the animal temperature, instead of being

* Recherches experimentales sur l'inanition; deuxième partie. Des effets de l'inanition sur la chaleur animale. Annales des Sc. Nat., 2nd series. tom. 20, Zoologie, p. 293 et seq.

uniform, in the warm-blooded animals, as has been supposed, experiences a diurnal oscillation, which consists of an augmentation of temperature, commencing at midnight and rising until noon ; and a depression of temperature, commencing at noon and progressing until midnight. His experiments show conclusively, that this variation was independent of variations in the temperature of the air, and often in an opposite sense to these changes. They are shown to be equally independent of season, though this oscillation was rather more considerable in summer than in winter. The variations in the respiration were analogous to those of the temperature.

It might however be asked, if this oscillation of temperature was not dependent upon the fact, that living beings are more active in their movements in the daytime ; and likewise, that this is the period in which they take their food, and in which, consequently, the nutritive functions should be more energetic. To these suppositions, the experiments of Chossat give a decided negative. He placed the animals in dark situations, limited their motions, and deprived them of food, and some, both of food and water. So far from this privation of food diminishing the oscillations, they became greater and greater from the first day of inanition to the last, when death supervened, from inability of the system to react under the nocturnal depression. This nocturnal depression he regards as the cause of death, and as determining its epoch. By dividing the twenty-four hours into two periods, of twelve hours each, one from midday to midnight, the other from midnight to midday, and arranging deaths of the animals in one and the other category, he found, that the great majority of the deaths occurred in the period from midday to midnight; that is to say, the period of maximum mortality corresponded to the period of the decline of temperature.

These experiments not only establish the fact, that the animal temperature is subject to regular diurnal oscillations, which are independent of atmospheric vicissitudes, and variations in the quantity of aliment, but enable us to detect the fact, that these oscillations, under the influence of disturbing causes, undergo a transition from physiological oscillations, to those which must be regarded as elements, or at least exponents of a disordered condition of the economy. In fact, as inanition progresses, we see these oscillations becoming more and more decided, until they become the determining cause of death.

Every physical influence surrounding these animals, was investigated, to ascertain whether the diurnal oscillations were influenced by such agencies or not, but no relation, whatever, could be detected, and still, as these influences were varied, the oscillations continued unaffected. Chossat was forced into the conclusion, that they depended exclusively upon forces inherent in the economy, and were only affected, in a remote and secondary manner, by any known external influence.

Another deduction may be derived from these results, which is of importance in studying the causes of disease. This is, that, as the alimentation of the system was diminished, or in other words, in proportion, as the power of vital resistance became less, the oscillations of vitality were augmented.

It appears, therefore, from the researches already instituted, that some of the most important functions of the economy do undergo important variations in their intensity, at particular and definite periods; and so general are the relations of those functions, whose variations have been proved, that we cannot withhold assent from the conclusion, that the general vitality must undergo corresponding oscillations. Now the researches of medical philosophy are often brought to a close, when a pathological condition can be ascertained to be a modification of some particular physiological condition. Having, therefore, these physiological oscillations, so strikingly analogous to the phenomena of periodicity in disease, and seeing too, that these physiological oscillations are susceptible under the influence of certain conditions, of changing to oscillations of greater intensity, and approximating to those, which mark the course of intermittent diseases, I think it will be fair to conclude, that the periodicity of disease may be regarded as only a modification of that which marks the course of the functions of the economy in its healthy state.

Keeping in view the periods of this physiological oscillation, we may see some relations between its stages and the periods of recurrence of paroxysms, though these relations may not be definite and absolutely fixed. If we divide the twenty-four hours into four parts, beginning at midnight, we find the following conditions of vitality, as indicated by the oscillations of animal heat. During the first two periods, of six hours each, we have the vitality exalted, and we may infer, that its intensity is about the mean at 6 o'clock, A. M. From noon, the vital intensity declines until it reaches the minimum again at midnight, and we may also suppose, that the vital intensity in declining, arrives at its mean again at 6 o'clock P. M. Now it is certain, that the time between 6 A. M. and 6 P. M., is that, during which the paroxysms of most intermittent fevers begin, and it is more common for them, to begin in the time between 6 A. M. and noon. In these diseases then, the paroxysms may be regarded as almost invariably commencing during the time that the animal vitality is habitually above the mean, and more generally during the time, when the vitality is rising from the mean at 6 A. M., to the maximum of intensity at noon. In other diseases we find different laws to govern the time of occurrence of the paroxysms. The paroxysms of some peculiar forms of fever, of Asthma, of Rheumatism, and some other diseases, are equally constant, in occuring during the interval between 6 P. M. and 6 A.M., that is to say, during the time when the vital intensity is below the mean; and more especially during that period, when the vital intensity is declining from the mean at 6 P. M. to the minimum at midnight.

In view of the above stated facts, I would venture the inference, that, while the intervals between the paroxysms of diseases, are measured in multiples of the intervals of the oscillations of the healthy functions; the period of the day at which the paroxysms come on, is determined by the peculiar nature of the disease, and by the functions implicated.

We may next enter upon the inquiry, relative to the influences which give rise to the oscillations in the vital energy in the healthy condition of the system; to those which produce intermittent affections; and we will, at the same time, endeavour to ascertain the portion of the system, upon which the primary impressions of these agents are felt.

By taking a general view of the oscillations of function in the healthy tate, it will be seen that they may, with some degree of propriety, be ranged under the three following divisions, to wit: 1st,—Those oscillations which occur at somewhat definite periods, but which are obviously dependent upon the recurrence at those periods, of certain physical influences. Among these, we may mention the annual excitement of the reproductive functions of some animals and plants, which is evidently dependent on the recurrence of genial temperature and conditions favorable to the general nutrition of the species. 2nd,—Those oscillations, which, though measured by definite periods, seem to have no dependence upon any known external physical influence; and consequently occurring, in each individual, at periods which are independent of the concurrence of any determinate external physical influences. Among these may be mentioned the oscillations in the functions of the generative organs in woman, the periods of which, though determinate for the individual, occur in different individuals at periods, which are arbitrary as to time, at least so far, as to appear independent of any of those general periodical changes, which take place in the physical world around. These oscillations seem to depend, like the next, exclusively upon forces or influences, inherent in the economy. 3d,—Those oscillations, which, though synchronous with the diurnal revolution, have not yet been ascertained to depend upon any of the changes which take place in the surrounding physical influences, and which, as far as we yet know, are caused by modifications in forces or influences inherent in the economy. As far as we yet know, it is only the two last varieties of oscillations which are liable to such modifications, as to assume the form of intermittent affections; those oscillations, in fact, which are dependent upon some force inherent in the economy. There is every reason to believe, that this force is the nervous influence, as modifications in this are sufficient to account for all the oscillations observed in the other systems, or in the general vitality, as it presides over, and directs all the functions of the economy—moreover, in all intermittent affections, the symptoms, at least the initiatory ones, are referable to this system.

It cannot yet be satisfactorily determined, why it is, that morbific influences give rise to intermittent forms of fever at one time and to continued fevers at another. But it seems to some extent probable, that as intermittents arise entirely from impressions felt by the nervous system, the fevers will retain that type so long as the lesion is principally limited to the function of that system; but whenever it is accompanied by either primary or secondary lesions of the mucous membranes of the alimentary canal, air passages &c., the resulting fever will tend to assume the continued type. Thus, we often see intermittents assuming the continued form, when during their course acute gastro-enteritis supervenes. But it may be said, that there is some gastro-enteritis in all intermittent fevers. This is the opinion of many physicians, but it cannot be said to be strictly true; for though, as they maintain, there is some hyperæmia of the mucous membrane, it is rather of a passive or congestive character than of an acute or inflammatory kind.

Intermittent affections of a local character, as neuralgias &c., seem to be attributable to local lesions, in the nerves of the parts which are their seats. These lesions are complicated by a general intermittent condition of the system, which excites the periodical paroxysms in the part.

In regard to the etiology of intermittent diseases, we really know but little. It seems likely, however, that the morbific impression is made upon the nervous system; but the nature of the impression, and the agents seem to be equally beyond our knowledge. It may be, that some of the agents produce their effects by coming into direct relation with the nervous system, while others reach it secondarily, that is, by modifying some other element of the economy, which directly influences the nervous functions. The experiments of Chossat show for example, that as inanition progressed, the oscillations became greater, and we may reasonably suppose, that in these cases the first modification was in the quantity and quality of the blood, which in turn, induced modifications in innervation. We have frequent occasions to observe something of the same kind, in the condition of the economy, in persons affected by intermittent disorders, who are frequently anæmic; and it is often difficult to say, whether this anæmic condition is the cause or the effect of the intermittent affection. Certain it is, that anæmic persons are more subject to intermittent affections than others; and indeed, this pale and sallow appearance is often the only very obvious indication of an intermittent condition, which may be detected by more attentive examination. Such cases are common in many districts of this country, and when the physician is called to examine them, and struck with the peculiar physiognomy, he questions them, he will sometimes have the patients to insist most positively, that they never had any intermittent affection in their lives. But a watchful examination will frequently reveal, not only a marked paroxysmal character in the symptoms, but will frequently detect those conditions of the spleen and other organs, which are attributable to the frequent recurrence of the congestion, which may be regarded as among the most important of the phenomena, connected with intermittents, as regards their general effects upon health. Now, this anæmic condition being a frequent accompaniment of intermittents and analogous conditions having been shown, by the experiments of Chossat, to be capable of giving rise to a kind of intermittence, resembling that of disease, it is reasonable to suppose, that some intermittent diseases may be referable to such a modified condition in the blood. The influence of changes in the quality and quantity of the blood on the nervous functions has been admitted since the earliest times, and is impressively stated by Hippocrates, in his observation, "*Sanguis moderator nervorum.*" At all events, the diminution, either of the quantity or nutritive quality of the blood, if not immediately a cause of this kind of affections, certainly renders the nervous system more susceptible to impressions from morbific influences, whatever be their nature.

Most writers have concurred in attributing intermittents either to a humid condition of the atmosphere in particular localities, or to paludal emanations. We have now abundant proof that intermittent fevers are of common occurrence in districts, where no marshes, swamps, or considerable collections of water are found; and likewise, that many of the most marshy and humid districts in the world are almost exempt from them. In our own State we have examples of all such varieties of conditions. Sometimes, for months together, we have the dew point only a few degrees below the atmospheric temperature, as has been the case during almost the whole of the present year, until the first of August;

and, notwithstanding, this condition, we have during such years no extraordinary prevalence of intermittents.

As to the idea of marsh miasms, our examples are equally conclusive. On some of the bayous of our delta, Lafourche and Terrebonne for example, the habitable land is limited to narrow stripes of from a few hundred feet to a mile in width, which form the banks of the streams, and follow their windings, and which are surrounded, on all sides, by swamps and marshes, in some places wooded, and in others, open and exposed; yet, notwithstanding, these regions are more exempt from intermittents than almost any other portion of the State. On the other hand, there are many regions of country, which are high, rolling and dry, in which the inhabitants are very subject to these fevers.

East Feliciana and the parishes lying east of it, may be taken as examples of this. These parishes consist of high lands, which constitute a portion of the bluff formation of the south, and have an elevation of from one hundred to two hundred and fifty feet above the level of the Mississippi river at that point. The lands are generally thin and covered either with open forests of the long-leaf pine (*pinus australis*), or with those of oak, beech and other trees, intermixed with a growth of the loblolly pine (*pinus tæda*), and other species. This extensive region is traversed by two or three streams of considerable size, which are generally bordered on each side by a narrow stripe of low land. The other streams, which are very small, run in narrow valleys, and are rarely bordered by swamps or marshes, and when their waters dry up in the summer, they leave their beds generally well washed and clean, or covered by a deposit of sand. In this region, the climate is obviously dryer than in the low lands of the delta, as is shown by hygrometrical tables, as well as by the fact, that here the spanish moss (*Tillandsia usneoides*) is scarcely met with, while in the lowlands it covers every tree, and the growth of this plant is a good hygrometrical index. Notwithstanding the favorable aspect of these regions, as respects health, the inhabitants are very subject to ague and fever, and that too, is not only the case with those living near the streams, but is equally so with those residing in the highest, most rolling and arid portions.

But similar observations have been made by almost every medical man, who has observed these things with much attention. Mr. Charles Darwin,* in speaking of the elevated, dry and almost arid coast of Peru, and of the great prevalence of ague there, says, "in all seasons, both inhabitants and foreigners, suffer from severe attacks of ague.—The attacks of illness, which arise from miasma, never fail to appear most mysterious. So difficult is it to judge from the aspect of a country, whether or not it is healthy, that if a person had been told to choose within the tropics a situation appearing favorable to health, very probably he would have named this coast."

There are however instances, in which the presence of stagnant waters certainly do produce decided effects upon the health of limited regions. The prairie region of western Louisiana is composed of high lands, like the districts just described. But the surface of the country is bare and is less broken, having only slight undulations. In some

* Voyage of a Naturalist.—v 2 p. 129.

places there are extensive flat or level spaces, which in rainy weather are covered with water, and in some of these the water, running off very slowly, forms long pond-like and marshy drains, which become dry in the summer. The people living near these places are frequently liable to intermittents, and this is much more the case in very rainy seasons.

Theoretical views of the action of antiperiodic medicines.

Various opinions have prevailed, respecting the mode of action of this class of remedies. Some have supposed that they produced their effects, by increasing or modifying the secretions generally, but more especially the perspiration. To these we may answer, that some of the antiperiodic remedies are not known to produce any effect upon secretions of any kind. Others view them as exercising a sedative, and others again a stimulant action. But the same answer will apply to them, as these remedies do not generally produce any obvious excitation or depression.

If we take the three most important of the antiperiodic medicines and compare their general effects, it will be found that they agree in this alone; that they are endowed with the property of checking paroxysms of intermittent diseases generally.

According to the opinion of the most rational observers, and especially of the most able of the physicians here, the action of these substances is of a *specific* kind, and this opinion is demonstrably correct. But it is after all a mere statement of the fact that it possesses such power, without going any further.

If the doctrines advanced in this paper be true, they enable us to go back a step further in tracing the effects of this class of remedies.

Take a case of intermittent disease in a resident of a miasmatic district, who is subject to regular paroxysms, and administer quinine. In many such cases no obvious effect is produced, excepting that the paroxysms cease, and will not return, so long as the influence of the remedy is kept up, although the person is still surrounded and acted upon by the same influences as before. It would appear, therefore, that the influence of the medicine was exerted upon something within the system, and in view of everything that has been said already, it would be rational to regard its impression as being made upon that force, inherent in the economy, which has been more than once referred to, as the power which presides over general vitality, in other words the controlling function of the nervous system of organic life.

The influence of these agents is no doubt principally exerted on the functions of the nervous system, and their effect seems to consist in increasing its controlling or resisting power, thus restraining its oscillations, and that of the dependent functions within the normal limits of the physiological state, and enabling it to resist the influence of morbific agents, which might give rise to perturbations, either in the time or in the intensity of the regular oscillations of health.

Hippocrates seems to have taken the view, that medicines, which relieved intermittent fevers, acted in a specific manner, by increasing, in the economy, the power of resisting some kind of oscillation; for he says*: "Harum autem febrium (tertiana et quartana) medicamenta

* Hippocrates, de adfectionibus, chap. iv.

hanc habent facultatem, ut his epotis, corpus in loco sit ; hoc est in consueta caliditate juxta locum consistat, neque præter naturam incalescat, neque refrigeretur." Barthez almost follows him in his definition of true tonics, for he says : "I give the title of true tonics to remedies, (such as cinchona and the martial preparations) the specific action of which is to establish in the whole system of forces, what I call the *stability of energy.* "*

Trousseau entertains the same opinion, and says, "the radical or specific tonics act purely and simply by increasing the vital resistance of the organism ;"† and, that, in checking the course of intermittents, they produce no change in the secretions, but their effect is exerted directly upon the nervous function, enabling it to sustain its course free from undue fluctuations.

These views of the action of the specific tonics are in precise harmony with the views advanced in this paper, respecting the nature of intermittence in disease ; and these views will receive further confirmation in what is to follow, respecting the relation of intermittence in some particular acute and chronic diseases.

It is very common to see acute and chronic inflammations, and engorgements arise under the influence of intermittent conditions of the economy ; and to see such induced affections persevere in an unfavorable course with the utmost obstinacy, until means are taken to stop the intermittent, however slight this might be.

It has been long known, that during the paroxysms of intermittents, congestions of certain viscera, commonly, if not invariably, occur ; and so constant is this the case with the spleen, that many have been inclined to regard the lesion of this organ as the cause, instead of the effect of the intermittent. Some other organs are almost as prone as the spleen to be engorged, hypertrophied, and permanently diseased, by the continuation of an intermittent condition. Of this, the uterus is a striking example ; and many of the chronic engorgements and affections of this organ are, as I have stated in another article, referable to this cause.‡ There is no doubt, in fact, that many chronic affections of the viscera take their origin in the same way, beginning in intermittent paroxysms, scarcely perceptible to the individual, or perhaps entirely escaping his observation, unless his attention be called to the subject by the physician. At each paroxysm the organs are congested, and this frequently repeated, necessarily induces structural lesions, chronic engorgements, or morbid deposits. I am sure, that cases have come under my own observation, of chronic engorgements of nearly all the viscera, in which they were entirely referable to intermittent fevers, during the course of which the symptoms of organic disease first began to be exhibited. In some cases of phthisis, intermittents have appeared as forerunners of the tuberculous developement, and if not the exciting cause of their first formation, evidently hastened their developement. The same remark apply to diseases of the spleen, liver, kidneys, and in fact of any organ, liable to this peculiar kind of engorgement.

* Barthez Nouveaux Elemens de la Science de l'homme.

† Trousseau & Pidoux. Therapeutique. vol, 2, p. 411.

‡ Remarks on the periodical formation and discharge of ova : New Orleans Med. & Surg. Journal ; v. 2.

This is however the case, not with chronic diseases alone ; but likewise acute inflammations arise and are sustained by the influence of intermittents. In the treatment of any inflammation or irritation of any of the tissues, we are frequently foiled in our attempts to procure prompt relief, by the supervention of an intermittent, how slight soever its paroxysms may be. I have frequently had occasion to observe, when intermittents occur, in cases under treatment, for irritation of any kind, the influence of the paroxysms is seen to be reflected upon any part, which may be the seat of even trifling irritation. This is observable in external irritations, as well as those of internal organs. Eruptions, or ulcers of the skin, or superficial tissues, ulcers of the shin, chancres, syphilitic ulcers &c., are all unfavorably affected by them. Inflammations of the inguinal glands, whether of a specific character or not, are often attended with a slight periodic febrile movement, and whether this is the cause or effect, it is true, that, even though an active antiphlogistic course be pursued, the inflammation will often run on to suppuration, unless the intermittent be stopped. Swelled testicle is a common occurrence in cases of gonorrhæa, when even slight intermittent febrile excitements occur ; and cases of this kind are common here, in which the swelling yields, readily, only to treatment, which relieves the intermittent condition. Ophthalmias, and all other acute inflammations, are generally aggravated by the supervention of intermittents.

In the puerpural state, especially, I have frequently observed the existence of periodical oscillations, so obvious and decided, as to assume the aspect of marked intermittence, and have seen the acute inflammations, which are incident to child bed, developed under this influence. It is not unusual to see women attacked, a few days after confinement, by a chill ; fever comes on ; and there begins to be perceived some pain or tenderness over the abdomen. The fever and pain perhaps continue with but little abatement, until the next or second day after, when slight rigors are again felt, to be followed by increased fever and pain, and so it runs on, without exciting in the minds of the patients, or their friends, any serious apprehension, until they are no longer able to endure the weight of the bed-clothes, and the disease, unless relieved, would probably run a severe, if not a fatal course. Five or six such cases have come under my treatment, and the affection has been in every case successfully combatted by the free use of quinine.

The same remarks will apply to inflammation of the breasts, in puerpural and nursing women. The inflammation commences with more or less decided rigors, and febrile excitement, which is more marked in the succeeding days, and unless relieved, the inflammation progresses on to suppuration. This has been the uniform course of all the cases, that have come under my observation. By the use of quinine and the topical application of stimulating frictions, to be immediately followed by emollient and anodyne cataplasms, they are uniformly relieved ; and since I adopted this mode of treatment, have not had a single case to terminate in suppuration.

But if intermittent conditions of the system are capable of giving rise to, and sustaining, other lesions of the economy, there are likewise some diseased conditions of the economy, which seem to give rise or predispose to a kind of intermittent condition. Thus during the progress of phthisis, of various cachectic, and other affections, which weaken the power of vital resistance, we have a species of periodical febrile excitement or fluctuation, which bears a striking analogy to the abnormal oscillations, induced in the experiments of Chossat, by the diminution of vital resistance, in the course of inanition. Indeed, the hectic fever which accompanies various chronic diseases, seems to be, to some extent, intermediate in character between the physiological oscillations and ordinary intermittent fevers. They may often be relieved or mitigated by the use of those remedies, which increase the power of vital resistance; and the course of the chronic affection may thus be, to a greater or less extent, checked.

Being aware of the existence of the oscillations which occur during health, and likewise of the circumstance, that perturbations of these oscillations take place in many diseases, either as causes or effects; and being also aware of the influence which these morbid oscillations or paroxysms may exert upon the course of diseases, it becomes important, that we should always be on the lookout for them, and endeavour to obviate their effects.

Intermittence may then constitute an element of diseased conditions, either as cause or effect. If it causes organic lesions, these lesions may become the cause of the continuance of the intermittence; and if intermittence arises during the progress of organic lesions, it may, in turn, cause their continuance, and aggravate their nature.

Intermittence seems to have some striking points of analogy with some other morbid conditions; if indeed it may not be regarded, only, as one of an order of conditions, which may depend on some modifications of the same forces, one lesion of which constitutes intermittence. Pernicious intermittents, congestive fever, and Cholera, are certainly attributable to functional lesions in the nervous system of organic life, or, in other words, in the power of vital resistance. Yellow fever and plague likewise, though the analogies are less obvious and striking, have some important points of resemblance; and in the latter diseases, the force of vital resistance seems to be modified, in a peculiar manner, by the action of some specific agents. And here we may recall the observation of Barthez,* that "the resolution of the radical forces seem to me to constitute malignant diseases."

My learned colleague, Dr. Harrison, seems to entertain much the same opinion, respecting the nature of yellow fever; as he speaks of the impression of *morbific agents*, *sui generis*, being made on the nervous system; and of "the sudden accession of the fever, the rapid and complete prostration of the powers of life &c."†

If these views be correct, and our opinions respecting the modus operandi of quinine and other specific tonics be true, new and important applications of these agents will suggest themselves.

In treating an intermittent, quinine seems to act in a special manner upon the nervous system of organic life, giving its functions energy and stability, which restrain its fluctuations within the physiological limits;

* Nouveaux Elemens de la Science de l'homme.

† Remarks on Yellow fever. By John Harrison, M. D.; Professor of Physiology & Pathology, in the Med. College of Louisiana. (New Orleans Medical & Surgical Journal; v. 2. p. 129 et seq.)

acting, in fact, merely as a prophylactic against the subsequent paroxysms. It is well known that quinine is a complete prophylactic against ordinary intermittents. If so, and, if, in curing intermittents, it is by protecting the system against individual paroxysms, why should it not be efficient as a prophylactic against yellow fever and other cognate affections, by sustaining the "*stability of energy*" in the radical forces, giving greater stability and uniformity to their course, and giving increased power of resistance against causes of perturbation. It appears to me to be at least worthy of trial.

Art. II.—*A Case of immobility of the Lower Jaw, successfully treated by a very simple contrivance.* By J. Marion Sims, M. D., of Montgomery, Ala.

The American Journal of the Medical Sciences contains some very interesting cases of immobility, or rigidity, of the lower jaw, reported successively by Prof. Mott,* Dr. Mighels,† Prof. Müller,‡ and Prof. Brainard,§ in all of which the separation of the jaws was effected by peculiar modifications of the "screw and lever." The various instruments of the different authors referred to, cannot always be obtained by the country surgeon, and it would be well if he could supply himself with a substitute easily had, and at the same time quite as efficient.

All will acknowledge, that some mechanical contrivance is absolutely necessary to prize the jaws asunder, even after the knife has done its best. I do not know that any instrument yet recommended answers this purpose better, than the simple little lever, that I use.

A case will illustrate its application.

George Ringstoff, aged 17, called on me in June 1844, and stated, that when but 7 years old, he had a violent attack of bilious fever, for which he was very badly salivated. It was followed by an anchylosis of the jaws. After a while, the Doctor cut it loose and put a bit of cork between the cheek and jaw, to prevent their reunion, but the pain was so great, that he could not bear it; so he threw the cork away. The parts grew together again and left him in a worse condition, than he was before the operation.

The jaws are now closely clamped, the crowns of the lower incisors passing up behind those of the upper jaw. The inferior maxilla can be moved downwards a little, but not sufficiently to bring the edges of the lower incisors from behind the upper; thus showing, that its downward mobility is very slight indeed. It has also a trifling lateral motion. From their close approximation he has more than once been in imminent danger of strangling to death, during the effort of vomiting, which is an invariable attendant of remittent fever in him. The dread of this, as much as the consideration of any ulterior benefits of operation, has exercised a powerful influence in driving him to seek surgical assistance. He is

* Am. Journal; Nov. '29 & '31.
† Ibid. Nov. '31.
‡ Ibid. May '40.
§ Ibid. Oct. '43.

forced to live entirely on slops, but can introduce solid food into the mouth, by pushing and working it up between the front teeth with the ball of the thumb. His jaw is twisted rather to the right of the mesial line. Right cheek is straight. Left is full above, depressed below, just at the corner of the mouth, where there is an old cicatrix, the result of an abcess formed 2 or 3 years ago, which discharged externally, having broke on the inside of the mouth five times previously. He had not lost all of his deciduous teeth when he was salivated. With them were occasionly discharged small fragments of bone.

The bond of union between the bones is confined to the left side, and extends from the cuspidati back to the ramus. It is very firm and inelastic, and as in all similar cases, of cartilaginous hardness. It extends not only vertically between the bones, but seems to form a perfect cement (as it were) between the cheek and gums. In fact, it appears to be a degenerate cicatrix of inodular tissue, the result of extensive granulation.

The operation was performed on the 3d of July 1844, with the assistance of Dr. Berney, and in the presence of other medical friends. I ran a narrow curved tenotome flatwise between this indurated tissue and the proper substance of the cheek back for an inch and a half, then turning its cutting edge towards the band, divided it with a sawing motion into the mouth. I now pushed the point of a common case knife between the bicuspids on the right side, making of it a lever of the 2nd kind, the bicuspids above acting as the fulcrum; and thus by prizing *and cutting at the same time* whatever appeared most tense, I succeeded in separating the jaws a little, enough however to introduce the lever necessary for the final accomplishment of the operation.

The lever that I used was nothing more than a straight piece of wood, 10 or 12 inches long, about an inch in diameter, but cut down to a wedge shape, at one end being near an inch wide and not more than one eighth of an inch thick right at the point.

After the jaws were opened a little by the first cutting and prizing with the case knife, I introduced the wedge-shaped end of the stick between the bicuspids on the right, and then by rotating it on its own axis, I could use any warrantable degree of force; indeed, I had the jaws perfectly at my command, and while I held this powerful instrument in my left hand, forcing the jaw asunder, slowly but surely, with the right I used the scalpel or tenotome, the one to separate the inodular substance from the bony structure, and the other for the division of any tense bridles on the inside of the cheek.

It will be seen in a moment, how perfectly this simple instrument fulfills the indications of the case. The rotation of half a circle on its axis would convert the space between the jaws from $\frac{1}{8}$ of an inch to that of an inch or more, according to the width of the wedge-shaped end. The process too may be just as slow as the judgement of the operator would decide to be essential to the comfort and safety of the patient, and just as rapid as the division and dissection of the tense bonds of indurated tissue will permit.

Thus, instead of cutting some and then breaking up the remaining adhesions with a screw and lever, (which must be most shockingly painful, if not dangerous,) I prefer making the knife and lever render a *mutual* assistance, each being essential and equal in importance. The knife can do no good without the lever, and the latter could be unsafe without the former.

The particular kind of wood, of which the lever is to be made, is a matter of some importance.

Accident directed me to the Bass wood, (Tilia Glabra.) It is the material used so extensively at the North, for the manufacture of broom handles, and it was one of these broom sticks that I used. A softer wood might not be firm enough for the purpose, while a harder one might inflict some injury on the teeth. The Bass wood lever will yield and even break, but then it is very easy to whittle it down to the wedge shape, while perhaps the patient would be glad of the chance to rest a moment; or, as they cost nothing, we might have several ready made for the occasion.

In the manner described, the operation was finished, and a block of soft wood, an inch in diameter in one direction, less in the other, was introduced between the molars on the well (right) side, which was worn night and day for a month, taking it out at mealtime, and occasionally during the day for the purpose of exercising the jaw. During the progress of cicatrization I had to touch now and then with the nitrate of silver, a small indolent ulcer on the inside of the cheek, which had been kept up by the pressure of the last molar against the raw surface.

A month after the operation, young Ringstoff returned home with the space of an inch and a quarter between the incissors, when the mouth was open; but for fear of the remotest possibility of a gradual closure of the jaws, he insisted on wearing the block between his teeth for a month longer, which was of course readily consented to.

Mastication was performed with the greatest facility, and his articulation was very much improved.

I met him in the streets five months after the operation, and his appearance was so changed, that I hardly recognised him as the same poor, pale, puny, sickly looking boy. A good wholesome diet, instead of his slops, had made him look quite ruddy and vigorous.

A few days ago, I had an opportunity of examining his mouth, and found him still able to open it to the extent of an inch and a quarter.

III.—*Some Remarks on Camp Diseases in Southern Climates.* By Samuel A. Cartwright, M. D., of Natchez.

War and Medicine are more closely allied than is generally supposed. Many of the greatest armies have been ruined in a few days by diseases that might have been avoided. Unfortunately it is too common for commanders of armies to look upon physicians as mere prescribers of pills and powders, and to take for granted that they have nothing to answer on the great question connected with extensive military operations. Although many of them have not, yet it by no means follows that the science of Medicine is limited by the bounds of their knowledge The first step in the science, is to learn to cure such diseases as are curable; but a higher to learn the secret of preventing them. Health is indispensible to the efficiency of an army, particulary a large one. Without health, "numbers effect nothing, but multiply distress." The case is worse if the army be far from home, in an enemy's country, and strangers to the climate and its influences. In such a case, the secret of preventing disease, can seldom be found by the commander of the army alone, unless he is aided by the light of medical science. Captain Cook, who sailed round the world so often, and kept his crew healthy, at a time that scurvy was destroying whole fleets, had learned the secret, so important for every commander to know, of preventing disease. He knew nothing about curing diseases, i. e., he was no doctor, in the common understanding of the term, but he was a great physician in regard to preventing disease. He borrowed light from Medicine, and reflected light on the science he borrowed from. Physicians studied Capt. Cook like a medical author. His discipline, his dietetics, hours of rest, and every thing that passed on board his ship, was studied. Aided by his book, physicians soon banished scurvy from the navy. The camp dysentery is as much the plague of large armies on the land, as the scurvy ever was to mariners. King Henry V. invaded France with fifty thousand men, in August. In September, the camp dysentery swept off nearly half of them, and so disabled the balance, that a disgraceful retreat was the consequence of King Henry's not having learned the art of preventing disease. In 1792, the hundred thousand Austrian and Prussian soldiers, who invaded France, were, in one short month, so much disabled by the dysentery, as to be unable to do any thing but retreat. The dysentery has been an attendant on all military operations, on a large scale, in every instance where the commander has not learned the secret of preventing it. That disease alone, the last century, has destroyed more soldiers than all the weapons of war that have ever been invented. That science which is too poor to cure a single case of organic disease, an ulcer in the lungs, for instance, or any disorder of structure in a part essential to life, is rich in materials for preventing diseases among armies, and curing them promptly when they happen. The diseases of armies are nearly all functional, not structural, as they often are in private practice. Those who have any organic or structural derangement are not received as soldiers. The young and vigorous fill up armies. Their diseases are nearly all owing to derangement of function, not structure, and are mostly curable when they happen, and by proper management can generally be prevented from happening. Too low an estimate will be put on the value of the science of Medicine to armies, to judge of it simply by the curable and incurable cases in private practice. Thus, a physician in private practice must, from necessity, lose every case of confirmed consumption, and every case of dysentery, or other disease connected with any organic derangement of structure essential to life—but the same physician could cure every case of cough, and every case of dysentery, occurring among soldiers, because he has no organic derangement to contend with, their diseases being all functional; that is, some organ has become deranged from some temporary cause; it is whole and entire, but cannot perform its functions from some temporary obstruction. In private practice such cases are readily relieved under almost any kind of treatment, or will generally get well, after so long a time, by the efforts of nature, under good nursing and the comforts of home. Not so in the army. They are deadly without the timely and judicious aid of medicine. At home, under ordinary circumstances, the measles are not commonly fatal with or without treatment. In the army it is deadly without treatment—and deadly with it unless directed by a physician acquainted with its new laws under the new circumstances. The same may be said of dysentery, and bilious fever. If these, and similar diseases, occuring in the army, are not promptly cured, the fault does not lie in the science of Medicine, but in the physician not having studied the science thoroughly, or in the commanding general not aiding and abetting him in the treatment. The rapidity with which disease, particularly dysentery, spreads through a camp, in southern latitudes, often strikes terror into the stoutest hearts. Far from home, and deprived of all the comforts of home, as soon as the soldier finds himself helpless by disease, he loses the man and becomes a child; a feeling of despondency comes over him, unless he has some one to look up to as a protector in his weakness. Fear or despondency will baffle the best directed treatment of the physician, unless the commanding officer drives fear from his camp by winning the affections of his soldiers. If the soldier finds that his officer takes a deep interest in his welfare, he neither fears or desponds. General Jackson walking and a sick soldier riding his horse dispelled fear and despondency, not only from the favoured sick man and all others on the sick list, but also acted as a preventive against disease in those who were well. One great secret of preventing disease among soldiers, is "to keep the vital and animal powers in uniform confederacy." Fear, like a bad diet, want of comfortable clothing, or over exertion, lowers the vital energies, and opens wide the door to diseases of all kinds—particularly dysenteries and fevers. When bad diet, want of tents, and clothing, and over exertion, have been encountered, and no sickness followed, physicians have only to read attentively the history of the commanding general, on those occasions, to read Medicine. Napoleon had the power of preventing disease, in an eminent degree, by infusing his spirit into his soldiers, and making them feel as he felt, and thus enabling them to resist the natural causes of disease—fatigue, hunger, and exposure,—by supplying them with a mental stimulus to make up for the want of the usual animal stimuli, as food, drink, sleep, and rest. If, under great privations, without tents, clothing, or a regular supply of food, and under forced marches, in inclement weather, disease can be prevented by the commanding general, how much more can it be prevented in the ordinary circumstances of camp life? But it is chiefly in what might be called camp luxury that sickness is most rife. Good tents, a plenty to eat, and nothing to do, is sure to bring on disease unless the commander adopt measures to counteract it, by giving his whole attention to the subject of preserving the health of his soldiers. When an army is on the march to meet the enemy, the *amor patriæ* closes up the avenues to disease, but when encamped, the love of country is not so strongly felt, and hence one powerful brace against morbid influences becomes relaxed. Besides, the malaria, or effluvia, arising from a great number of men, stationary, particularly in warm weather, become a cause of pestilence. Those commanders who will not open their eyes to the recorded medical proofs of this source of disease, will see their men suffer much unnecessary evil, which could easily, in the most of cases, have been avoided, by moving the camp half a mile, or mile, before the air around it had time to become pestilential. Pestilential hospitals are also the prolific causes of unnecessary sacrifice of human life in an army—and want of sufficient nursing another. If a sick man has a friend, (and most of them have,) that friend had better be permitted to attend to him, if his services can be dispensed with; not so much as a drudge nurse, as to see that the disabled soldier has every comfort his situation will admit of. Measures to find out the particular friend of each individual on the sick list, could be taken without putting the commanding officer to much trouble. Men recover much quicker from sickness in booths or sheds with only something to keep the sun and rain off than they do in large hospitals, or where many are crowded together. The success in treating diseases in the large European hospitals, is very indifferent. The most bungling country surgeon in America has better success with operations, than the most skilful European operator. The foul air of the hospitals, and the pure air of our American cabins, makes the difference. There is such a thing as dodging a disease. The history of armies affords many instances of a short removal, only a few hundred yards, being sufficient to cut short the pestilence, whether cholera, fever, or dysentery. The want of sufficient attendants to prevent the sick from making personal exertion, is a great evil. The sick soldier should be made to husband his strength, and not waste it by getting up, or making unnecessary exertion. It would be all the better if he were not to leave his bed from the time he is taken sick until the disease comes to a crisis. In private practice, in acute diseases, *absolute* rest is very important, but in hospital practice much more so. Many lives are lost by the soldiers resisting the disease, and not giving up in time. I found when I was in the army against Great Britain, last war, that the majority of the soldiers had a holy horror of a hospital, and would hold their places in the ranks when they ought to have been abed. When they did, at length, yield, their chance of recovery was much diminished, the disease being so firmly fixed on them. I think the usual course, of sending every sick man to the hospital or calling a doctor to see him, a very injudicious one. In the army, diseases, for the time being, are all generally alike, requiring the same treatment. I mean when there is much sickness prevailing. The physician could make known what was the first medicine to begin with. When a soldier complains, he should take the first dose on the spot, and lie down in his tent without going to the hospital. If he did not get well, it would be time enough to go to the hospital when it was ascertained that the disease was about to prove obstinate. I found, by experience, that soldiers often secretly attempted to cure themselves by taking medicine of their own, and not reporting themselves, so fearful were they of the hospital. If a soldier knew that he would not have to go to the hospital until a trial to cure him in his tent had failed, he would not be so backward in letting his indisposition be known in time. If also the formality of being examined by a physician, could be dispensed with, there still would be less reluctance to give up in time. The importance of yielding or giving up to disease at its first onset should be duly impressed on the mind of the troops in order that recovery might be speedy. The diseases of autumn are marked by more debility than in any other season and they generally do not bear bleeding or evacuating remedies well. A serious and hard spell of sickness, disables a soldier too much to be of any service in the campaign—so far from being useful, he is in the way. How necessary, therefore, to prevent long spells of sickness, by attacking the disease before it has got too firm a hold. It would be well, also, for the commanding officer to bear in mind, that new troops, or any other kind of troops, cannot endure as much exertion in a warm as in a cold climate. Discipline, with new troops, should not exceed the proportion of exercise necessary to health. All over exertion or excessive fatigue should be avoided, or reserved for an important object. All drudgery should be avoided as much as possible, or be performed by negroes, or the natives of the climate. The wisdom of such a measure, has been purchased by dear experience in the British campaigns in warm climates. It is cheaper to give a native two prices to perform drudgery work in a hot climate, than have the same work done for nothing by the soldiers. Unless sickness be prevented no army will make any figure that will gratify its officers. Hence the commanding general will not find his time badly employed in studying the secret of not exposing his men to the causes of disease. Too *little* exercise as well as too *much*, should be avoided. The time of day for parading is not unimportant. The dress should be less for show than comfort. The soldier is sometimes troublesome and refractory, yet when he sees his officer interested in his welfare and believes that he will not be exposed to unnecessary perils, or have unnecessary burthens imposed on him, he becomes enthusiastic and will face any danger, endure any fatigue, and is ready to make superhuman exertions, at the bidding of his officer. But to come to the main point of preventing disease. Enthusiasm in military matters, like enthusiasm in religion, can only be kept up to a certain degree of intensity for comparatively a short time—when, like the passions, it requires repose—when too intense, and kept up for too long a time, it is not easy to wake when it falls into repose. Enthusiasm is a thing which has *its* fits and starts. It is a powerful weapon in the hands of a commander who studies how to use it—one who does not ride it to death when there is no occasion to use it, but keeps it in reserve for high occasions, and then knows how to command it to come forth at his bidding and give him aid. When a soldier has plenty to eat and drink, he does not require to be fed on glory, if nothing is to be done—no enemy about—no fortress to storm, his enthusiasm should be suffered to repose. If permitted to take rest, until the occasion comes when it may be needed, there are a thousand ways of waking it up. When gaunt famine stalked through Jackson's camp and pestilence threatened to cut off all his men, that great commander found a substitute for meat and bread in awakening a spirit of enthusiasm among his men, which also banished pestilence from his camp, by giving the body power to resist disease. He awoke the spirit of enthusiasm in one remarkable instance in a very simple way—by merely offering to divide a few acorns with an exhausted soldier. This sinple incident soon spread through the camp, and the spirit of chivalry began to swell every bosom; making his troops forget that they were without food or the prospect of food in a distant wilderness; and nerved them to renewed exertion. The men, who a short time before, were ready to sink to the earth exhausted, and to fall a prey to a pestilential fever, forgot they were hungry on finding themselves heroes. Napoleon always fed his men on glory when meat was scarce, or some great fatigue was to be endured, or whenever he wanted to drive disease out of his camp. High enthusiasm, and epidemic diseases are incompatible. The secret of great military commanders in preventing disease seems mainly to consist in awakening a spirit of enthusiasm on perilous occasions. Some degree of enthusiasm is at all times a healthful stimulant against morbid influences and which it is always well to encourage, but the higher or more intense exaltation of mental excitement not being durable, should only be called up on great occasions. If it would always come on being bid, it would be an easy matter to banish a pestilence from an army by that agency alone, but as it is a fire which cannot always be kindled at will, none of the natural causes, giving rise to disease, should be permitted to haunt the camp when they can be driven out. The British owe many of their large possessions in warm climates to Moseley's discovery of treating camp dysentery successfully. Before a successful plan of treating the disease and banishing it from camp was discovered, Great Britain was in a fair way of losing her southern possessions by having no men to defend them; the dysentery killing them as fast as they could be sent out. The same mode of treatment did not succeed in the cold latitudes of Europe. The wars of the British, French, and Spaniards, during the latter part of the last century, in the West Indies, brought it forth. Our northern medical schools and northern writers, having no occasion for it, have neglected to teach it. It is a treatment adapted to the dysentery of camps in hot climates. For more than twenty years I have found it successful in plantation practice in the south. In communicating it I am giving nothing new, and I claim no credit but that of calling attention to a very successful practice that is almost forgotten.

The one dose cure for Camp Dysentery.

From twenty to forty grains of Ipecacuanha, and fifty drops of Laudanum mixed together in a little sugar and water, molasses, or toddy, and taken at one dose—immediately the dose is taken, the soldier should lie down and be covered over with his blanket, to keep the air off. He should continue thus covered up with his blanket for 24 hours, drinking hot mint tea, or hot sage tea, or any other agreeable aromatic tea, as balm, sassafras, or orange-leaf tea. The drink should be taken as hot as table tea is commonly used. The object is to promote perspiration and to turn the fluxion of blood from the bowels and liver to the skin. No drinks should be taken, for an hour or two, unless the patient vomits. After each spell of vomiting he should drink the *hot* tea. The medicine has the best effect when it neither vomits nor purges. It commonly vomits once or twice, and the next day purges once or twice. It cures the disease in half an hour, if given early in the complaint. That is, it stops the bloody stools on the spot, relieves the pain and tormina of the bowels, determines the blood from within, to without—causes perspiration and carries off fever. And if the perspiration be kept gently up, for a sufficient length of time, the disease will never return. It is important after taking this medicine, that the patient be kept for some days out of a draft of air, and the first day or two be covered over with a blanket; though not to be kept too hot, or in too close an atmosphere. When the pain in the bowels is severe, the quantity of laudanum should be a little more, say a teaspoonful. A full dose to allay spasm and pain is better than a small dose. The Sydenham laudanum is better than the common laudanum; if that is used, 30 drops will be sufficient, as it is stronger than the common laudanum. If the ipecac is very good and fresh, and has never been exposed to the air or light, 20 or 30 grains will be sufficient, and may be made into pills with the laudanum and taken all at once. If the patient lies on his back, and holds a tablespoonful of water in his throat, and will open his mouth, seven large pills can be swallowed at one swallow, if they be pitched in Indian file, down to the root of the tongue, the patient at that instant swallowing them with the water previously held in his throat. But the patient must lie flat on his back to swallow so many pills at one swallow. The dose taken in the pill form, is less apt to vomit. Should sweat not occur in an hour, the complaint may require the loss of a little blood from the arm, or another half dose in the pill form; but in nine cases out of ten, nothing more is required than the hot aromatic tea, above mentioned. It is essential for the success of the *one dose cure*, that it be commenced at an early period of the disease. Different physicians, seeing dysentery under different circumstances, are too apt to conclude that all other plans of treatment are wrong but their own.

Thus, in private practice, in mountainous districts, bleeding is generally necessary. In marshy districts, bloodletting is not well borne. When connected with much bile, the purging or calomel plan succeeds after so long a time. In some epidemics the patient is so suddenly reduced by the first onset of the disease, as to fall into cold sweats, faintings, laborious breathing, etc., requiring instantly the strongest aromatic stimulants to keep him from falling at once into a state of collapse and death. But generally speaking, the one dose cure, for the dysentery in camps, is the best, safest, and quickest. If it fails to cure in an hour, no harm is done: the purging, or any other plan may be adopted. The misfortune of the purging plan is, that the soldier who is cured by it, is seldom worth any thing in the campaign. Half the fluids in his body have to be evacuated by purging, before the plethora of blood in the viscera, causing the dysentery, can be removed. Whereas, by the one dose cure, the fluids are saved—instead of being purged out, they are thrown from the viscera to the surface, kept in the circulation and saved. All, or nearly all the camp dysenteries, have a single cause, a retreat of the blood from the surface *without*, to the viscera *within*. In private practice, the stomach or liver or some other organ may be at fault and constitute the first links in the morbid action; but among soldiers, it is almost always the skin. The scorbutic dysentery is an exception, where acids, lemon juice, and sour vegetables become the best remedies; and in dysenteries connected with remittent and intermittent fevers, the quinine is an important adjunct. After the patient has taken the one dose cure, the next day, if any remnant of the disease still remains, from a teaspoonful to a tablespoonful of the following mixture, should be taken without being mixed with any thing whatever. To be taken on an empty stomach, viz: white vitriol 90 grains, common alum 60 grains, cochineal (the coccinella of Mexico) 3 grains, boiling water half a pint. When the solution is cold, strain through paper. From a tea-spoonful to a table-spoonful of this mixture every morning, fasting, or three or four times a day, will of itself cure mild cases and is one of the best remedies for the chronic form of the complaint. It can be made to operate on the bowels by lessening the quantityof alum in the solution, or to bind the bowels when the discharges are too frequent, by increasing the quantity of alum in the solution. In table-spoonful doses it nauseates and has all the advantages of nauseating medicines—and in smaller doses than a table-spoonful, it is tonic and astringent and aperient at the same time. Partaking of the character of small doses of rhubarb.

The author of the above practice is Benjamin Mosely who published it in the West Indies in 1789. The practice was so successful, that his treatise on the dysentery went through many English editions and was re-published in Latin. After a long experience in Southern diseases I am confident that it is more successful in the great majority of dysenteries than any other method of treatment before or since adopted. Dr. Mosely gave the ipecac first and the laudanum afterwards. According to my xe-perience it is best to give them together, making one dose of it. I have tried the medicine in broken or divided doses, but find it not to succeed as well as the full dose. The aromatic teas to keep up the determination to the skin are important. To follow up the medicine by strong purgatives would defeat the object in view, of determining to the surface. Aromatic waters, or the odour from burning aromatic substances, are better than vinegar or the chloride of lime to correct the atmosphere about dysenteric patients.

IV.—*Remarks on the use of the terms "Congestion" and "Congestive."* By Thomas C. Brown, M. D., of Woodville, Miss.

The term *Congestive Fever* has become so familiar in the South and West, that the young and inexperienced are led to look for a distinctly marked and peculiar fever, and are sadly perplexed in their investigations. At one time it is described as "algid intermittent fever," at another as "malignant or pernicious remittent fever." They find it supervening on intermittent, remittent and continued fevers—on gastritis and gastro-enteritis—on typhoid pneumonia—on epidemic dysentery; and in fact, as complicated with most of our summer and autumnal diseases, and sometimes with our winter epidemics.

Would it not be well for the medical profession, if our journalists and teachers of medicine, would teach that "there is not, properly, any fever characterized by *congestion* in contrast with another which is without it;" but that *congestion* is common to nearly all fevers, whether intermittent, remittent, or continued; and that it is not limited to any one organ; the brain, the spinal column, the lungs, the liver, the spleen, the kidnies and abdominal viscera, may be the seats of congestion, either separately or several of them at the same time; and these as they are separately or severally affected, will characterize the symptoms according to the organ or the several organs which may be involved?

The term *Congestion* comes from the Latin *congero*, to amass, and in a medical sense conveys the idea of turgescence or fulness of blood in a particular part, and implies simple engorgement and over distension of the blood vessels. It seems rather the result of irritation than of inflammatory action, and when it passes into inflammation, it would seem to be more dependent on the general state of the constitution, on the plethoric condition of the blood vessels, with the tendency towards inflammatory excitement, than on the primary irritation which occasioned it. The presence of internal congestions must, of course, be infered from the symptoms which indicate them, and to connect the symptoms with the structural lesions which they denote, so as to furnish practical guidance in the treatment of them, is the proper object of medical research.

Their treatment must depend in part on the nature of their site, but principally on the cause which occasions them. If congestion result from a plethoric habit, mere local treatment can never give permanent and effectual relief; on the contrary, if the congestion be owing to irritation only, then general depletion is unnecessary, and would be injurious.

Congestion generally preceeds, but may follow inflammation, by the irritation set up in neighboring organs, or be transmitted to those more distant by nervous influence. Strong innervation, in which the nervous centres are much excited as under various emotions, will produce congestion, at one time of the brain, at another of the lungs or liver. Certain nervous affections, as hysteria, will give rise to irregular determination and retardation of blood, producing congestion in some organ, which often entirely disappears after the removal of the nervous disturbance. Congestion dependent on mere nervous disorder, will generally be irregular or periodical in its appearance, but if there be a permanent disorder of function in an organ, the congestion depends upon permanent irritation. In the commencement of fevers the congestion is of an irregular or periodic character, but when occuring in the latter stages of fever, it is usually of a more permanent and dangerous character.

Congestion, coming on in the progress of intermittent and remittent fevers, is of frequent occurrence, and when supervening in the latter stages of these diseases, it generally indicates great danger; but when it ushers in an intermittent fever, and the paroxysm is followed by very feeble reaction, it indicates great danger.

The symptoms of the congestion of a particular organ, or of several organs, combining with the sympathy of a particular fever or disease, upon which it supervenes must, necessarily, give rise to great diversity of symptoms, and will require a variety of treatment as dictated by the existing circumstances. Hence the confusion which results in an attempt to study the symptoms of congestive fever, as a distinct species of fever. If we are to have a *nosology*, in which *congestion* is to be the cognomen of a particular species, we must adopt a division of fever somewhat like the arrangement of Armstrong, and dispense with the distinctions of fever into intermittent, remittent and continued.

Woodville, Oct. 14th, 1846.

V.—*An Account of the Yellow Fever that prevailed in New Orleans in the year* 1846. By E. D. FENNER, M. D.

In accordance with one of the main objects of this journal, which is, to keep a faithful record of the more important diseases that prevail from time to time in this region, I shall here attempt to give some account of the yellow fever as it appeared in the city of New Orleans, during the latter part of summer and the autumn of the year 1846.—The type of the disease has certainly been somewhat different from that which usually prevails here; and this fact, on the first appearance of the fever, gave rise to much discussion as to its true nature; some pronouncing the complaint *acute jaundice*, others *pernicious fever*, and others *typhoid fever*. But that we have had more or less genuine yellow fever, now no longer admits of doubt or difference of opinion.—The true value of diagnostic symptoms consists in the certainty with which they enable us to distinguish and identify any particular disease *during life*—which being present, the disease is present, and being absent, the disease is incomplete. Now the diagnosis of yellow fever in its mildest forms, and with the earliest cases, is by no means an easy task. It is generally necessary to witness those symptoms which usually attend the *fatal* termination. Nor is it always easy, with the first one or two deaths, to settle the question satisfactorily even with these aids. A larger number of deaths may be required to be seen before the fever assumes an unquestionable character. Such was the progress of observation and discussion in regard to the fever that recently prevailed here. The first cases were few and scattering, and it was difficult to find two physicians who would agree as to the diagnosis of a case that strongly resembled yellow fever. This was the case even after public attention was drawn to the subject: when the Board of Health was required to report weekly on the progress of the disease, and something of a panic existed, there were still one or two influential physicians, who openly expressed their doubts as to there being any genuine cases of yellow fever. The progress of events, however, cleared away all doubt upon this point, and the only question that remained, related to the extent or amount of the disease. Many cases that recovered were diagnosed yellow fever, which might well admit of doubt; but as the season advanced, the fatal cases were unquestionable; and these were numerous in proportion to the whole. The cases which I shall report in the course of this paper, will, I trust, satisfy the reader that I am describing the origin, symptoms, and progress of *true yellow fever*. A few remarks on the weather, the condition of the city, and the general state of health, may properly precede my account of the fever.

The preceding winter was an unusually cold one—the spring was very late and cool—the summer was not near so hot as the last, but it was remakable for the extraordinary quantity of rain that fell. The weather continued warm and wet until about the 20th of September, when the rains ceased, and the remainder of the autumn was remarkably dry.

The following Meteorological observations are taken from Mr. Lillie's abstracts, furnished regularly to this journal.

WEEKLY. — 1846.	THERMOMETER.			BAROMETER.			COURSE OF WIND.	FORCE OF WIND, Ratio 1 to 10.	Rainy Days.	Quantity of Rain — Inches.
	Max.	Min.	Range.	Max.	Min.	Range.				
June, - 6	88.2	66.5	21.7	30.09	29.97	0.12	S.E.	3½	6	3.940
" - 13	82.5	66.0	16.5	30.07	29.91	0·16	N.W.	2¾	3	1.057
" - 20	90.0	75.5	14.5	30.10	30.01	0.09	S.W.	3	4	1.987
" - 27	89.0	73.0	16.0	30.15	30.04	0.11	S.E.	3	3	0.995
July, - - 4	89.7	72.5	17.2	30.07	29.83	0.24	S.W.	2¾	3	8.900
" - 11	89.7	77.0	12.7	30.17	29.97	0.20	W.	3	2	0.210
" - 18	88.0	69.5	19.5	30.17	29.91	0.26	E.	3¼	5	2.710
" - 25	89.5	74.0	15.5	30.14	29.97	0.17	S.E.	3	1	1.180
Aug. - - 1	90.0	75.0	15.0	30.17	30.03	0.14	W.	2½	5	0.473
" - 8	88.5	76.0	12.5	30.20	30.03	0.17	W.	2½	2	0.725
" - 15	89.0	70.0	19.0	30.14	30.00	0.14	S.E.	2¾	4	4.516
" - 23	89.5	72.5	17.0	30.13	30.01	0.12	S.W.	2½	4	0.658
" - 29	86.5	72.5	17.0	30.15	30.01	0.14	S.E.	3	3	0.590
Sep. - 5	86.5	74.0	12.5	30.10	29.95	0.15	N.E.	3	5	1.725
" - 12	88.0	73.0	16.0	30.20	29.88	0.32	S.E.	3	6	3.152
" - 19	90.5	73.2	17.3	30.14	29.96	0.18	N.	2¼	0	0.000
" - 26	84.5	71.0	13.5	30.17	29.77	0.40	E.	3	4	0.821
Oct. - 3	81.0	65.0	15.0	30.11	29.88	0.22	N.	3	0	0.000
" - 10	83.5	70.0	13.0	30.28	30.09	0.19	E.	3	0	0.000
" - 17	81.0	57.0	14.0	30.17	30.03	0.14	N.E.	3	2	0.020
" - 24	73.0	49.5	23.5	30.33	30.16	0.17	N.	2½	0	00.00
" - 31	76.2	54.5	21.7	30.22	29.97	0.25	N.E.	2¾	1	1.120

It will appear from this table that the number of rainy days was in

June, . .	16, .	quanity,	7.979	inches and thousandths.	
July, . .	11, .	"	13.000	"	"
August, .	18, .	"	6.962	"	"
September,	15, .	"	5.698	"	"
October, .	3, .	"	1.040	"	"
			34.679		

Showing an extraordinary quantity of rain during the hot months.

The condition of the streets and gutters has been notoriously worse than has been known for a number of years past; the scavangers having generally done but little more than drag out the sediment from the gutters and pile it in heaps, where it remained till it returned to its former position, instead of being carried out of the city.

By reference to the May number of this Journal, it will be seen that in the month of April last, there was an extraordinary inundation of the swamp and back part of the city, extending in some places, (Canal street for instance,) as far as Burgundy street. This was produced by the conjunction of an immense fall of rain, and the prevalence for several days of a strong east wind that drove in the waters of the Gulf.—The inundation only lasted a few days.

The river did not rise near so high this year as either of the two preceding, and has been very low during the autumn, leaving exposed to the sun an extensive bank or batture, which had long been covered with water. This bank became very offensive in some places; particularly opposite the lower market, where much filth is thrown into the river.

New Orleans being the great thoroughfare and depot for the army of invasion on the Rio Grande, caused a greater number of unacclimated persons to be in the city throughout the summer than was ever known before. They have been passing to and from the seat of war all the time. In the month of July about *six thousand Louisiana volunteers*, (two thirds of whom, perhaps had never had yellow fever,) were discharged from the service at one time, and returned to the city. A large number of these remained in the city the rest of the summer; which they propably would not have done under other circumstances.

From accounts published in the newspapers, it appears that yellow fever prevailed to a considerable extent in the West Indies, and Vera Cruz, during the summer.

The following table, which I have compiled from the books of the Custom House, will show the comparative number of vessels that arrived at the port of New Orleans, from the principal places in the West Indies, for the years and months stated.

List of Arrivals by Sea at the Port of New Orleans, for the last six years,
From Vera Cruz and the principal West India Ports, from April 1st to Octb'r 1st.

	April.	*May.*	*June.*	*July.*	*August.*	*Septb'r.*	*Total.*	*Ann. Tot'l.*	*Sickness.*
1841.									
Havana,	10	8	8	13	5	5	49		
Kingston, Jam.	3	2	1	1	0	0	7		Severe Epidemic.
Vera Cruz,	0	0	0	0	1	1	2		
St. Thomas.	1	3	0	0	0	0	4	62	
1842.									
Havana,	7	6	2	3	5	3	26		
Kingston,	2	2	1	2	0	0	7		Mild Epidemic.
Vera Cruz,	1	0	1	0	0	2	4		
St. Thomas.	0	0	1	0	0	0	1	38	
1843.									
Havana,	6	9	4	5	4	5	33		
Kingston,	6	4	8	0	1	1	20		Mild Epidemic.
Vera Cruz,	2	5	0	3	3	2	15		
St. Thomas.	9	1	5	1	0	0	16	84	
1844.									
Havana,	14	7	6	8	7	6	48		
Kingston,	3	4	5	2	0	0	14		Mild Epidemic.
Vera Cruz,	2	4	3	4	2	1	16		
St. Thomas.	3	0	0	0	1	0	4	82	
1845.									
Havana,	9	5	6	0	2	4	26		
Kingston,	4	3	3	0	1	0	11		
Vera Cruz,	4	2	0	2	2	1	11		None.
St. Thomas.	1	0	0	0	0	0	1	49	
1846.									
Havana,	5	9	4	4	8	3	33		
Kingston,	4	3	6	8	4	1	26		
Vera Cruz,	3	4	3	0	0	1	11		Little Yellow Fever.
St. Thomas.	2	0	2	0	0	0	4	74	

The arrivals from other ports in the West Indies, have been so few as to be unworthy of notice. The trade between this place and Vera Cruz, has been extremely limited ever since the Texan revolution, in 1836. I have thus glanced at all the facts and circumstances of the year, which I conceive to have any bearing on the origin of Yellow Fever. Notwithstanding all that has been written on the subject, much yet remains unknown, and much difference of opinion still exists. I have no preconceived theory to sustain, but shall endeavour to give a plain statement of facts with all the candour and impartiality I can command.

I shall first give a brief account of the *first cases* of yellow fever that occurred, showing the part of the city in which they originated, and the connection, if any between them—secondly, report a few cases to illustrate the character or type of the disease—and lastly, speak of its extent and mortality, and the conclusions to be drawn from the whole. The facts stated are drawn either from personal observation or directly from those physicians who witnessed the disease.

It may be proper to premise that although there has not been much sickness at any time during the year, the principal diseases that prevailed during the months of May, June and July, were diarrhœa, dysentery and intermittent fever. There was more dysentery at the Charity Hospital than has been witnessed for several years past. The same may be said of acute jaundice or gastro-duodenitis. Many of these latter cases resembled a yellow fever so closely as to give rise to much discussion in regard to the diagnosis. However, the fatal cases, for the most part wanted those decided symptoms, such as black vomit, hemorrhages, and suppression of urine, which generally attend the last stage of yellow fever. But as has been intimated before, some physicians expressed doubts even when these fatal symptoms were witnessed; nor did they yield their assent to the existence of genuine yellow fever, until a number of cases had died with the most unequivocal symptoms.

As far as I have been able to ascertain, the following is the order of occurrence of the first *reported* cases of yellow fever. Some of them were considered questionable at the time; but as they were pronounced yellow fever by the intelligent physicians who attended them, I give the memoranda furnished me, and the reader may form his own judgment.

Case 1st.—(Notes furnished by Dr. A. Mercier.) G. M., an Irish Drayman, aged about 23 years, lived in New Orleans five months, residence at the corner Canal and Trémé streets, Second Municipality. Was taken with headache about 1 o'clock on the 20th July, whilst at work with his dray on the levee. Went on for an hour, when he drank a glass of brandy and water, and sat down in a shady place, his clothes being wet from perspiration. In half an hour he was seized with a chill, accompanied by violent pains in the head and back, and great lassitude of the legs. He went home, took his bed, and sent for Dr. Mercier, who arrived between 7 and 8 o'clock P. M. Dr. Mercier found him with the same distressing pains; skin hot and dry—eyes bright—pulse about 104, full and strong—tongue natural—gums very red. *Treatment,* V. S. *ad deliquium animi; hot mustard footbath—cold applications to head —purgative enema.*

22nd.—Symptoms much the same. Treatment, *Cups over epigast. to ℥ xxiv. Repeat foot baths and cold to head.*

23d.—Morning, pulse 120—vomiting bilious matters—pain in the region of the stomach &c.

1 o'clock, P. M.—Epigastric pain increased—vomiting continues—slight delirium—urine scarce—pulse 120—skin and eyes slightly yellow.

5 o'clock, P. M.—Urine suppressed—breathing embarrassed—epigastric pain very great—constant vomiting—delirium increased. He continued to get worse until 12½ o'clock at night, when he died. He continued to throw up yellow and greenish matters until just before death when they became *black*. They were shown to Dr. M. He says they were *black, but not flocculent*.

At 8 o'clock the following morning Dr. M. saw the corpse. The eyes and inner part of the thighs were slightly yellow—the rest of the body was of a dark livid hue—the face was swollen—bloody froth issued from the mouth. Dr. M. was accompanied by Dr. Luzenburg at this visit, hoping to make an *autopsy*, but the friends of the deceased refused permission.

A day or two afterwards one of the city news-papers (the La. Courier) announced the appearance of yellow fever in the city and cited two cases, the one here reported and another said to be under the care of Dr. Beugnot. On the following day the "Bee" contained cards from Drs. Luzenburg & Beugnot, the former dissenting altogether from Dr. Mercier in regard to his case being one of genuine yellow fever, and the latter denying positively that he had seen recently any thing resembling yellow fever.

Case 2nd.—(Dr. Hunt gave me the following memoranda of this case.)

Mrs. H., aged about sixty years—a very intelligent lady—in New Orleans since last winter. She was taken sick about the 24th of July and was attended by Dr. L. who pronounced the case *fièvre pernicieuse*, though he suspected yellow fever. Dr. H. took charge of the case on the 28th. He found her without fever—skin very yellow—great irritability of stomach—matters ejected of light colour, but flocculent—had no stool for four days—complete suppression of urine—numerous petechial blotches on the skin, &c. Dr. H. pronounced it yellow fever, as also did Dr. Harrison whom he invited to see it. She died on the 2nd of August, without black vomit but with hemorrhage from the bowels.

This lady had been in the habit of going frequently to the markets early in the morning, and walking up and down the levee. She resided at the corner of St. Claude and Esplanade Sts., back part of the 1st. Municipality. Nineteen years ago this lady was going with her husband by sea from this city to New York, and thence to Europe. Whilst passing the West Indies her husband died with black vomit, and she was said to have had yellow fever.

Case 3d.—(At the Charity Hospital—Ward 12—service of Dr. Turpin, who furnished me the following notes.)

V. H., a German, aged forty years—a resident at Lafayette 5 years—plethoric habit—had been sick with fever seven days when he came to the Hospital on the evening of the 29th August. Dr. T. saw him first on the morning of the 30th, when he presented the following symptoms: decubitus dorsal—icterus deep and general—the tongue dry, indented and black—the gums and teeth covered with coagulated blood—but little thirst—frequent disposition to vomit—ejecta sanguenolent—abdomen painful—no stools—suppression of urine—pulse small and soft—skin cool—intellect dull—responses incoherent—jactitation—at night he was delirious—the general irritability became so exalted that he could not bare to be touched without complaining—about noon the vomiting was more frequent and the matters thrown up presented the character designated under the name of *coffee grounds.*

On the following morning, Sept. 1st, he was dead. There was no *autopsy.* This was the first case of black vomit that occurred in the city.

Case 4th.—The notes of this case were kindly furnished by Dr .S. W. Dalton, and I cannot do better than give his report in full, in his own words.

New Orleans, Nov. 6. 1846.

Dear Sir: Agreeably to your particular request, I have hastily attempted to sketch the prominent features and symptoms of the following case of fever which came under my observation and treatment in August last, and to which I invited the attention of one of the members of the Board of Health, immediately after having been called to see it, in order that he might determine the specific character of the disease. And after having visited and carefully examined the case for three days he pronounced it to be a case of acclimating or yellow fever. Here it is, judge ye.

August 24th.—F. S. aged 25 years—a shoemaker by trade and *name*—9 months last has resided in this place, on Circus St., near the New Basin—is a native of Germany, of phlegmatic bilious temperament—was suddenly seized with violent pains in his head, small of his back and legs—so severe in the latter as to render him incapable of sustaining an erect position. These prominent symptoms were preceded by a slight chilliness which accompanied them for some hours at short intervals—disappearing upon being closely covered up and recurring again on the least exposure to the air. He remained in this condition —most of the time covered up with a feather-bed—for about twenty-four hours, under the treatment of some quack, before I saw him: when I called I found him still laboring under violent pain in the front part of his head, back and legs—tongue large and white, except at its edges which were defined with a deep red margin its whole extent—pulse 110, full, but soft and easily compressed, eyes red, dull and heavy expression of countenance—mind incoherent.

Ordered cups freely to the back of the neck and temples, which abstracted from 14 to 16 oz. blood—gave him Dalton's Compound effervescing Saline Cathartic—moved his bowels briskly, which together with the bleeding reduced his pulse to about 100 and very much relieved the other symptoms, particularly the violent pain in his head. Skin became soft and somewhat cool, which suggested, notwithstanding his pulse, the active use of quinine, which I gave in v gr. doses repeated every hour until he had taken ℥ ss.

25th.—Found him much improved in his general appearance and condition, though there was slight hemorrhage from the gums, the quinine having fulfilled its indication. On seeing me he immediately said: "Dr. I feel perfectly well"—and wanted to get up, but inquiringly said to Dr. J——s, "do you suppose he won't let me?" This single fact, is in my humble opinion, almost sufficient of itself to establish the character of the fever with which he was attacked—it is a peculiarity of this disease, in which there always exists a loss of balance between the physical and metaphysical man, disqualifying the mind to determine the capabilities of the body, for whenever the effort is made to get up—and sometimes it is with fatal success to the patient—prompted by sensorial impression, that he has power to do so, the body fails to respond to the call, and the patient falls back on his pillow with a sad expression of disappointment depicted on his facee, and with a tremulous voice, says: "Well, I thought I was stronger." Continued the quinine in the same quantity at each dose as before but increased the quantity of time between doses to four hours, and gave him grs. xvj. during the day, and flaxseed-tea lemonade freely—at night gave him ℈j. calomel, his bowels not having been moved during the day.

26th.—Still improving, the calomel acted well and produced a most favorable effect, having brought away several copious black and very offensive bilious stools. Tongue better, eyes cleared and lively, healthy expression of voice and countenance, with some appetite. Ordered chicken broth and lemonade with a small *dash* of maderia wine.

27th.—Convalescent. Continued the same regimen.

28th.—Sitting up, and in the course of the day "took the responsibility" of swallowing down two quarts of soup: "He was so hungry," and relapsed; from which however he recovered without much difficulty, but it protracted his final restoration to health for several weeks.

Case 5th.—(At the Charity Hospital—ward 18.)

G. D. aged 32—in New Orleans six years—entered the Hospital on the 25th August; then sick 3 days—died on the 3d September. I saw this man among a number of others who visited him during his illness. He was very yellow—ejected every thing taken into the stomach, but did not throw up *black vomit.* The attending physician diagnosed it *gastro-duodenitis*, but several who saw it thought it was yellow fever.

Case 6th.—(At the Charity Hospital—ward 5.)—M. M., an Irish woman, aged 25, in New Orleans 3 months, said she had been sick with a fever 15 days when she came to the hospital, on the 9th September—she died with *black vomit* on the 12th.

Remarks.—It is not probable this woman had yellow fever all this time. She must have had some other complaint which run into yellow fever.

Case 7th.—(Notes furnished by Dr. J. H. Lewis, Esplanade st.)—An English labourer, aged about 30 years, living on Frenchmen's st., Third Municipality, three squares from the river—had lived but a short time in the city—had been sick with fever two or three days when Dr. L. was called to see him, on the 6th September. Found him yellow—buccal membrane much engorged, but not bleeding—stomach irritable—

ejecting whatever he drank, but no black vomit. He recovered in a few days. Dr. Lewis pronounced the case yellow fever.

Case 8th.—At the Charity Hospital—ward ....—H. D., aged 24 years, in New Orleans 10 months, entered hospital Sept. 11th, then sick 8 days—died on the 4th October. Residence unknown.

Case 9th.—W. M., aged 24, in New Orleans 12 months, entered hospital 13th Sept., then sick 3 days—died Sept. 15th. Residence unknown.

Case 10th.—(Notes given by Dr. J. H. Lewis.)—M. N., a girl aged 16 years, but a short time in New Orleans, lived on Marigny, between Victory and Moreau streets, a square and a half from the river, was attacked with fever on the 16th Sept. Dr. L. attended her from the first, and she was convalescent on the 23d. Dr. L. thought it was yellow fever at the time, but subsequent events confirmed his opinion, for on the 9th October following, the mother of this girl was seized with fever, and died with black vomit on the 13th. Two other cases occurred in the same family, but recovered.

Case 11th.—(Notes furnished by Dr. O. Carey.)—M. T., a German servant girl, aged 19 years, in New Orleans six months, residence No. 31 Front Levee street, near the Orleans Cotton Press, upper part of the Second Municipality—was attacked on the night of the 16th Sept. with fever accompanied by severe pains in the head, back and limbs. Dr. C. was called to see her on the evening of the 17th. Found her with high fever, skin hot and dry, pain in head and back, eyes red, restless, nausea and vomiting of bilious matters, great thirst. Gave some purgative medicine, ordered foot baths, enemas, diluent drinks, &c.

Sept. 18th.—Fever somewhat abated though still high, bowels freely purged, delirious, still vomiting. Repeat enemas, foot baths and drinks.

Sept. 19.—Fever subsided very much, still vomiting every thing she drinks, delirious, very restless, &c. These symptoms continued two days longer, with a hemorrhagic condition of the gums. Her bowels were kept open, she was carefully nursed and recovered. After convalescence was established, her eyes became quite yellow, some petechial blotches on the skin, and a sloughing sore upon one of the hands.

N. B. Dr. Carey attended two other cases in the same house shortly afterwards, one of which died with black vomit, the other recovered after a desperate illness.

Case 12th.—(Notes furnished by Dr. Mercier.)—On the 18th Sept. Dr. M. was called to Marigny street, between Moreau and Casacalvo, to see a German girl aged 17 years, of sanguineous temperament, very rosey and plump, thin skin and light hair. She had been in the city 4 months, and had been in the habit of standing in the sun washing clothes. A few hours before Dr. M. saw her, she had been seized with a slight chill, soon followed by burning fever, headache, pain in the thighs, &c. Dr. M. found her with the same pains, skin hot and dry, cheeks and eyes highly injected, tongue natural, white coat upon the gums, pulse 112, full and strong. *Treatment*, V. S. ℥ xviii, full warm bath, flaxseed enema, drink flaxseed tea.

Sept. 19th.—No improvement. Ordered cups to abdomen ℥ xvi, lemonade, repeat enema and drink.

Sept. 20th.—Symptoms same, with the addition of nausea, pulse weak and irregular, pain in the epigastrium.

Sept. 21st.—Skin pale and cool, vomiting every thing she drinks, slight delirium, urine scanty, no sleep, epigastric pain increased, breathing somewhat embarrassed, tongue still natural. Repeat enemas and mucilaginous drinks.

Sept. 22nd.—Symptoms all increased until 8 P. M., when dark floculi appeared in the ejecta from the stomach. She continued to throw up this floculent matter until 5 o'clock the next morning, when she died. Dr. M. saw the corpse at 10 o'clock, A. M., it was perfectly yellow. No *autopsy* was allowed.

N. B. This girl belonged to a family of five, four of whom died; the other three with *black vomit.*

Case 13*th.*—(At the Charity Hospital.)—W. F., aged 24, in New Orleans 12 months, entered Charity Hospital Sept. 19th, then sick one day—died Sept. 27th. Residence in city unknown.

Case 14*th.*—(Notes furnished by Dr. J. E. Kennedy.)—Captain N., of ship ——, had arrived from Boston but a few days when he was seized with yellow fever, on the 19th Sept. He was attended by Dr. J. E. Ker, at a house on Moreau, near Mandeville street, Third Municipality. Dr. Kennedy was called in consultation on the 23d of Sept. He had then been sick four days, and his condition was perfectly hopeless. He was throwing up flaky matters which became genuine *black vomit* in four hours, and he died the same night.

N. B. Dr. Kennedy relates the following cases in connection with this, which I will here record for the benefit of those who believe that the disease may be communicated from one person to another.

1. Mr. C. an assistant stevedore who had visited Capt. N. frequently during his illness—resided in the vicinity—had lived in New Orleans most of the time for the last 9 years, but never had yellow fever—was attacked the very night after Capt. N. died. Dr. K. thinks his was a well marked case of yellow fever. He recovered.

2. Mrs. B., an Irish woman who nursed Capt. N. during his illness, had been in N. O. but a short time, and never had yellow fever. She was somewhat unwell from the time of Capt. N's. death, doubtless caused by fatigue and sitting up, but on the 28th, five days afterwards, she had a plain and severe attack of yellow fever. She was now at the house of Mr. L. on Royal, near Esplanade st. Dr. Kennedy was called to see her on the 29th—had her cupped and gently purged—ordered repeated warm foot baths &c. which produced a remission in the fever. He then gave her quinine freely and she was convalescent on the 5th day.

3. At this time, October 2nd, Mr. L. at whose house the last case laid sick—a stevedore, who had lived in N. O. a number of years, but never had yellow fever—had nursed both Capt. N. and Mrs. B. during their illness—was now seized with all the symptoms of yellow

fever—such as burning fever, severe pain in the head and back, red eyes, irritability of the stomach &c. He had taken a dose of castor oil, but rejected it before Dr. K. arrived. Dr. K. ordered him to be cupped over the epigastrium and nucha to ℥ xx—purgative enema, hot mustard foot baths and diluent drinks. On the following day there was a distinct remission in the fever and he was sweating freely. Quinine was given liberally and the patient was convalescent on the 3rd day. By similar facts occurring in other places some persons have become convinced that yellow fever may be communicated from one person to another. But to show the error of such a conclusion in this instance, it will be necessary to recollect that at the same time a few sporadic cases were occurring in nearly all parts of the city, from one extremity to the other.

Case 15*th*.—(Notes furnished by Dr. Mercier.)

On the 20th Sept. Dr. M. was called to see B. L., a Jew, aged about 35 years—bilious temperament and very robust. Residence on Marigny st.—in N. O., most of the time the last 3 years, but never spent the summer here. He had been attacked the night previous with chilliness, followed by high fever—pains in the head, back and limbs &c. Dr. M. found him with same pains—skin hot and dry—pulse 120, full and strong—tongue red at the tip—white coat upon the gums. *Treatment.*—Ordered V. S. baths—diluent drinks &c. The large amount of 3lbs. of blood was taken without causing syncope.

Evening. Symptoms the same. *Treatment.*—Cups over abdomen to 2lbs. (which did not cause fainting,)—purgative enema—mucilaginous drinks &c.

Sept. 21st.—All the symptoms better—pulse 96. Ordered full warm bath—enema—same drinks.

22nd.—Same as yesterday—much improved. He now heard of the death of the girl reported in case 11, same neighborhood, became very much frightened and soon got much worse.

23.—Worse in every respect—skin engorged and dark. Ordered stimulating frictions of quinine mixed in whiskey over the whole body—gave an infusion of serpentaria and valeriam with camphor.

24th.—Much worse—very delirious—pulse weak and irregular—tongue red and pointed—vomiting every thing he drank—complete suppression of urine—breathing embarrassed—intense pain in the epigastrium, could not bear the slightest pressure—discovered ulceration on the sacrum and hips. Continued treatment.

25th.—Still worse, rejects every thing taken into the stomach, but presents nothing like black vomit. Died soon after midnight.

The body was very much reduced and became dark yellow—the penis and scrotum were in a gangrenous state.

This list of cases includes the first that were thought to be yellow fever in private practice and the first fatal cases at the Charity Hospital. The fatal cases are given in preference because their termination left less doubt as to the nature of the complaint.

The following recapitulation will show the date of the attack and the part of the city where they presided.

Case 1.	Attacked	July 20,	Corner Canal and Treme sts. 2nd M.
Case 2.	"	July 21,	Corner St. Claude and Esplanade, 1st M.
Case 3.	"	Aug. 21,	Lafayette.
Case 4.	"	Aug. 22.	Circus st. near New Basin, 2nd M.
Case 5.	"	Aug. 23.	Unknown. *
Case 6.	"	Aug. 25.	"
Case 7.	"	Sept. 3.	Frenchmen st. 3d M.
Case 8.	"	Sept. 3.	Unkown.
Case 9.	"	Sept. 10.	"
Case 10.	"	Sept. 16.	Marigny st. 3d M.
Case 11.	"	Sept. 16.	Upper part of 2nd M.
Case 12.	"	Sept. 18.	Marigny st. 3d M.
Case 13.	"	Sept. 18.	Unknown
Case 14.	"	Sept. 19.	Moreau st. 3d M.
Case 15.	"	Sept. 20.	Marigny st. 3d M.

In regard to the first cases that were discharged *cured* from the Charity Hospital, the following abstract from the records will be sufficient.

Cases.	Age.	Res. in N. Orleans.	Sick before admission.	Time of admission	Discharg-ed.
Case 1.	16 years.	15 days.	1 day.	20th Sept.	28th Sept
" 2.	9 "		6 days.	25th "	30th "
" 3.	40 "	10 mo's.	14 "	26th "	4th Oct.
" 4.	25 "	7 "	2 "	24th "	4th "
" 5.	37 "	8 wk's.	8 "	11th "	5th "
" 6.	23 "	15 days.	4 "	22nd "	7th "
" 7.	40 "	4 mo's.	10 "	24th "	8th "
" 8.	27 "	6 wk's.	7 "	26th "	9th "
" 9.	22 "	2 yr's.	7 "	25th "	10th "
" 10.	21 "	15 days.	10 hours.	30th "	10th "

As this list was made out from the book of *discharges* it will be observed that the order of date is according to the term of discharge, instead of admission or attack.

It will also be observed that the date of some of these cases is earlier than some of those given in the preceding list. I believe that both taken together will represent pretty fairly the first cases that occured. At least this is the result of the most careful inquiry I could make.

As to the relation that existed between the first cases, the probable communication of the disease from one to the other, or its commencement and gradual radiation from any particular focus or locality, a single glance at the foregoing facts and statistics ought to be sufficient to satisfy the mind. The fever certainly prevailed to the greatest extent in the 3d Municipality or lower part of the city, though cases were seen from one extremity to the other. The parts that were more exempt than any other, were the upper portion of the *first* and the lower portion of the *second* municipality, composing the heart of the city.

*The cases marked *unknown* were taken from the books of the Charity Hospital, where the special residence is not recorded.

I will give some further memoranda, obligingly furnished me by Dr. J. H. Lewis, who resides on Esplanade street, opposite the Mint, and has an extensive practice in the Third Municipality. Dr. L. thinks he has known of at least 50 cases in this region, of which more than 20 died. On Marigny street, which is just below the Ponchartrain Railroad, there were ten cases, of which 5 died.

Within a circumference of 4 squares there were 17 cases, of which 7 died.

Dr. L. saw 8 cases at the lower extremity of the Third Municipality—just above the Tobacco warehouse, and one square from the river; one died with black vomit. This warehouse is at least three miles from Canal street.

Dr. L. saw three cases on the Mexican Gulf Railroad, seven squares from the river—two of them in the same house, the other half a mile off—two died, the other recovered.

Dr. L. saw a man on Cotton Press street, who *had black vomit and suppression of urine;* yet he recovered. He saw a similar case at the corner of Casacalvo and St. Ferdinand streets.

In his treatment Dr. L. gave no mercurials, nor indeed any thing to move the bowels, save enemata. He generally had his patients bled or cupped—then gave the sulph. quinine in 4 gr. doses every hour, until what he terms *intoxication* was produced—it was then omitted until this subsided, when it was resumed and continued until convalescence was confirmed.

He says that none of the cases to which he was called within the first 24 hours, turned yellow during convalescence—the fatal cases generally terminated with black vomit and suppression of urine.

Dr. Lewis saw intermittents, remittents, congestive, and yellow fever, also dysentery prevailing at the same time, and in the same neighborhood.

Intermittent fever prevailed to an unusual extent in his neighborhood throughout the summer, which led him to place so much reliance upon quinine.

Dr. John Ker, who resides on Casacalvo, just below Esplanade street, thinks he saw 30 or 40 cases of fever during the summer and autumn, but would not pronounce them all *yellow fever.* These two physicians I presume saw more of the disease than any six others in the city. The physicians of the Second Municipality saw very little of the disease in private practice. Indeed it was confined almost exclusively to the lower or labouring class of the community, most of whom reside in the lower part of the city, on account of the cheapness of rent.

I shall now report a few cases from my own note book, taken whilst acting as visiting physician to the Charity Hospital, for the purpose of showing the character of the fever.

Case 1.—A. L., a large German labourer, aged 27—came to New Orleans eleven months since—went to the Rio Grande in the spring, as a volunteer from Louisiana—returned in July—had intermittent fever, and was discharged cured from this hospital on the 1st September. He remained out till the 26th September, when he again entered the hospital. He then said he had had a chill and fever regularly every day for the last six days. He looked yellow and livid—eyes yellow and injected—severe pain in head and back—gums red and spongy as if disposed to bleed—tongue rather red, but not much furred—stomach somewhat irritable—bowels confined—urine free.

Treatment.—Had him cupped freely over the epigastrium and loins —gave him at once sulph. quinine, and mass. hydrarg. aa. grs. x.— followed by lemonade with a little sulph. magnes. dissolved in it—warm poultice over epigastrium.

The case lost all the characters of an intermittent, and no one hesitated to pronounce it yellow fever. I shall not give the notes from day to day—suffice it to say, he had but little fever after he was cupped and took the quinine and blue mass.—he became very yellow, and had hemorrhage from the fauces for three or four days. His bowels and kidneys acted very freely. He was blistered over the stomach—sinapisms and dry mustard were applied freely to his extremities, and he was permitted to drink freely of iced porteree. He came very near dying, but finally rallied and was discharged cured on the 9th of October.

N.B . Here is a plain intermittent, running into yellow fever.

Case 2.—T. A., aged 23—a rather slender young man; native of Boston—in New Orleans 15 days, when he was seized with a violent fever, pains in the head, back, &c., which he suffered for four days without treatment, when he came to the Charity Hospital on the 22nd of September.

Appearance on admission—his skin was about the natural temperature, but had a livid hue, inclined to yellow about the neck—capillary circulation very sluggish—pulse scarcely 80, soft and regular—his eyes had a muddy yellow cast—gums engorged and disposed to bleed— tongue dark red, and pretty clean—considerable thirst—some pain in the head and back—bowels easy—urine free—some irritability of stomach—he was extremely weak.

Treatment.—Blister to epigast.—hot mustard pediluvia and friction— iced porteree—lumps of ice—light nourishment.

He became extremely debilitated—hemorrhage appeared in the mouth —the yellowness increased, and extended over the face, neck, and breast —on the extremities the skin was more livid; the capillary circulation very languid. He became very restless, had nausea, and appeared to be threatened heavily with black vomit. However, by means of the foregoing remedies, an occasional anodyne, and as much nourishment as his stomach would bear, he slowly recuperated, and was discharged cured on the 7th of October.

Remarks.—When this young man entered the Hospital he was in the usual condition on the 4th day of yellow fever, viz:—great prostration of the physical powers, skin turning yellow about the neck and face—pulse slow and soft, hemorrhagic tendency of the gums, &c. He appeared to be on the verge of black vomit for several days.

Case 3d.—V. B., a German, aged 36—in New Orleans since 28th of May—entered the Hospital on the 2nd of October. Says he has been suffering under dysentery with frequent bloody stools, pain and tenesmus for two or three weeks past—about four days ago this gave place to high fever, accompanied with pains in the head and back, &c.

Present state—very much debilitated—skin livid, rather cool and dry —conjunctivæ engorged and beginning to turn yellow—tongue very red and dry—gums and lips dark red—bowels loose—has no pain—pulse very slow and weak—slight nausea—appears to be listless and indifferent. *Treatment*—large blister over epigastrium—iced gum water and porteree—sinapisms to extremities—sponge surface with hot vinegar and mustard—arrow root.

This course of treatment with slight variation was continued until the 7th of October, when the patient appeared to be considerably improved. His skin was now moist, turning yellow about the neck and face, and not so livid on the extremities—bowels quiet—urine free—pulse 60, and still very weak—sleeps pretty well—has but little appetite. *Treatment*—continue sponging with warm vinegar—porteree and light nourishment.

October 8th.—Seems much better—rested pretty well—skin and eyes quite yellow—sweating gently—less thirst—tongue moist—bowels easy—urine free. Ordered porter and chicken soup.

October 9th.—Found patient much worse, from some cause—he is very restless, constantly moving from one part of the bed to another—frequently attempting to rise up—had a bad night—does not complain of any pain—no thirst—tongue moist and clean—pulse very slow and soft—skin cool and moist on the forehead, now very yellow—showed me a large purple spot on the left arm that looked like a *bruise*, though he says he has had no blow or fall to cause it—the sinapised places on his legs were likewise purple—eyes very yellow—bowels open—urine free.

Treatment.—℞—Sod. Bi-Carb. ℈ ii.
Morph. Sulph. gr. i.
Aqu. Font. ℥ ii.

Give a tablespoonful every two hours. Repeat stimulating, frictions, &c.

Evening.—Found him much relieved—had slept 2 or 3 hours, and was quite easy. Continue treatment.

October 10th.—Rested pretty well, and is better in all respects. Allowed light nourishment.

October 13th.—Patient has steadily improved since last date. This morning, found him sitting at the window with his clothes on—still feeble—appetite improving—skin clearing up—pulse 60. Ordered him to bed immediately—to have porter and light nourishment.

October 14th.—Rested well—is convalescent—asks for his discharge. I advised him to remain a few days longer, but he insisted on going out. Discharged.

N. B. Here we see a plain case of dysentery running into yellow fever. The purple blotches on the skin spoken of, were of the nature of echymoses or petechiæ, and gradually cleared up.

Case 4.—P. D., Irishman, aged 24—been in New Orleans one year—entered the Charity Hospital November 1st., said he had then been ill with a fever 4 days, and had taken no medicine. He was ordered a foot bath, and lemonade by the house surgeon.

November 2nd.—I saw him first at my morning visit. *Existing state.*—I found him with his clothes on walking about the ward—he did not appear to be exactly in his right mind—complained of no pain—skin cool and beginning to turn yellow—eyes yellow—pulse 80, soft and regular—tongue rather clean—considerable thirst—bowels open. *Treatment.*—Ordered to get into bed and remain there—to have hot mustard foot bath, and infusion of sage.

Evening.—Found him still walking about—apparently much worse—mind deranged—skin quite cool, &c. *Treatment.*—Ordered to be kept in bed—large blister over stomach—blister to nucha—repeat hot foot bath—to have calomel and quinine each grs. x.

November 3rd.—Spent a wretched night—had to be bound hand and foot—delirious—sleepless—making constant efforts to rise. My term of service at the hospital now expired, and Dr. Pequet took charge of the ward. Dr. P. prescribed sulph. quinine ʒ i, in 8 doses—one every hour—repeat foot bath, frictions, &c.

November 4th.—Extremely ill—still confined—breathing hurried—pulse upwards of 100, soft and gaseous—clothes wet with urine—skin cool, and of a greenish yellow hue—no vomit. Died in the evening without vomiting.

We have here a specimen of what have been termed "*walking cases.*" His condition was altogether hopeless when he entered.

Case 5.—J. B., an athletic German, aged 30—in New Orleans 18 months. Says he was attacked on the 1st of October with slight chilliness, soon followed by burning fever, with violent pains in the head, back, &c. On the 2nd, he took of his own accord an emetic of antimonial wine, which vomited him very severely, and continued to operate till the 5th of October, when he entered the hospital.

Present state—has but little fever—skin yellow about the face and neck, livid on the extremities—eyes very yellow—great thirst—tongue red and dry—still vomits—tenderness over the epigastrium—gums and lips dark red, as if disposed to hemorrhage—pulse slow and soft.—*Treatment.*—Sinapism over epigastrium—hot mustard foot bath—sponge the body with hot vinegar—iced gum water and lumps of ice.

October 6th.—Somewhat better, but still very thirsty, and disposed to vomit. *Treatment.*—Blister over stomach—continue cold gum water—repeat hot foot baths and sponging.

Evening.—Had considerable hemorrhage from posterior fauces—otherwise better.

October 7th.—Clear of fever—pulse hardly 60 to the minute—skin cool, with a livid yellow cast—hemorrhage continues from fauces—gums and lips dark red—still disposed to vomit. *Treatment.*—Repeat frictions with hot vinegar—drink porteree.

October 8th.—Rested pretty well—feels better—skin moist and warm—tongue natural—pulse very slow and soft—less hemorrhage from the mouth—less thirst. *Treatment.*—Infus. prunus virgin.—chicken soup.

October 9th.—Found patient much worse—spent a restless night—vomiting a glairy fluid, with dark bloody specks in it, probably from the fauces—skin cool and moist—pulse slow and soft—eyes as yellow as gold—but little thirst—no pain—bowels open—urine free. *Treatment.*—Re-apply blister to epigastrium—frictions with hot vinegar and mustard—drink porter iced.

Evening.—Feels better, but still vomits at long intervals—hemorrhage nearly ceased. *Continue treatment.*

October 10th.—Rested well, and is better in every respect. *Take porter and soup.*

October 13th.—Patient has continued to improve since last note, yet he still vomits his food, and drinks occasionally—eyes and skin very yellow—has some appetite—convalescent.

October 15th.—Improving—vomited some green bile early this morning, but took his breakfast with a relish.

October 17th.—Asked to be discharged, which was granted. He had a good appetite, and his skin was assuming a healthy hue.

Case 6th.—(Attack within 24 hours after entering the city. This case is interesting, as it shows the natural progress of fatal yellow fever undisturbed by treatment.)

H. R., aged 24—native of New York, and very robust—first came to New Orleans in the spring of 1844—drove a dray the rest of the year, and had no sickness. In the spring of 1845 returned to N. Y., spent the summer, and returned in the fall. Worked here until June last, when he again went back on a visit to N. York—returned to N. Orleans by the western route, and arrived in vigorous health on the 8th of October—put up at a boarding-house at the corner of Esplanade and Old Levée sts. On the following day, 9th Oct., he was taken with a chill, accompanied with aching in the head, back and limbs, and soon followed by high fever. Says he drank a pitcher of ice water, and soon broke into a profuse sweat which lasted all night, but without taking off the fever. Took no medicine. Had high fever with great thirst—bowels very loose—urinated freely.

Oct. 10th.—Fever, thirst and pains continued—continued to sweat, purge and urinate freely, but without any relief. Took no medicine. Oct. 11th—Rested badly—says he "sweated tremendously"—pains and thirst the same. About 9 o'clock in the morning the fever and pains began to abate, and he began to vomit a light coloured, acid fluid—vomited frequently during the day. About 5 o'clock, P. M., he was brought to the Hospital. The House Surgeon prescribed lemonade, and ordered body to be sponged.

October 12th—Morning—I saw patient for the first time—now the fourth day of his illness—his intellect was perfectly clear, and he gave me the foregoing history of his case. *Existing state.*—Habit plethoric—skin very red and hot—conjunctivæ highly injected—pulse 86, full and soft—tongue covered with a white fur, edges red—great thirst—frequent thin stools—urine free—complains of great debility—has no pain—disposed to sleep, and says he dozes sweetly—vomits frequently a brownish, ropy, acid fluid containing flocculi which settle at the bottom of the vessel, aptly compared to *beef-steak gravy*, by Dr. Snowden, who was present.

Treatment.—As the case had progressed thus far without medical aid, and all the indications except blood-letting, had been fulfilled by *the efforts of nature*, I thought it best to interfere as little as possible. He was now at the beginning of *black vomit*, and his only hope of salvation depended upon the *vis medicatrix naturæ*. Ordered a little lime water every hour—whole surface to be sponged frequently with warm vinegar—to have porter iced, and lumps of ice *ad lib.*

——— Evening.—Says he feels better—has only vomited twice since my last visit—ejecta getting darker, sediment like bloody mucus—has had two stools—sweats and urinates freely—skin cooler, but still red from capillary engorgement—no pain—eyes very red—pulse 84—less thirst—sleeps easy and well. *Continue treatment.*

Oct. 13.—Rested pretty well—vomited only once in the night—this morning throws up *genuine black vomit*—purges thick black matter—skin cooler and paler—less thirst—no pain—sleepy—intellect clear.

Treatment same—With the addition of mustard to the hot vinegar, and barley water to his drinks.

——— Evening.—Patient worse. (Dr. Gordon, of Thibodauxville, and Dr. Dashiell, of Lake Providence, saw him with me.) He threw up 6 or 8 ounces of black vomit in our presence—said it now had a sweetish taste—debility increasing—pulse 80, soft and gaseous—skin cool and clammy—no pain—but little thirst—bowels open—urine free—appears to be perfectly calm and indifferent about his situation.

It was now very evident that the patient must soon sink without aid, and although I had but little hope, I determined to resort to blisters, stimulants, and astringents.

Treatment.—Blisters to epigastrium and legs—pure tannin grs vi, every 4 hours----to drink mint julep or claret, as he preferred.

Oct. 14th,—Found patient *in articulo mortis*—the nurse said he had vomited but twice after taking the tannin—that he had been delirious and muttering for the last 6 hours. He died at 9 A. M., almost without a struggle.

As the poor fellow had a *chère amie* who came to see him daily, and seemed much distressed at his death, no autopsy was made, out of respect to her feelings. Indeed none was necessary.

Remarks.—This case is exceedingly interesting—1st, as showing how soon a person in full health may sometimes contract yellow fever upon getting into the infected atmosphere—2nd, as displaying the natural history or progress of the disease, undisturbed by medical treatment—and 3rdly, as to what may be expected from the unassisted efforts of Nature. I have, however, seen a few cases in which the disease went through all the earlier stages without medicine, and finally recovered with but little medical aid. In 1844 I was called to see a boy on board a ship just from Kingston, Jamaica, where yellow fever was prevailing. A few days after getting out to sea he was attacked with a violent fever which continued until the fourth day----no medicine whatever was given, I saw him about the fifth day—he then had no fever—skin turning yellow—had no evacuation from his bowels since he was taken—belly somewhat tumid—great prostration. I simply ordered him a cathartic enema—a warm poultice over the abdomen—a little porter and light nourishment. He recovered rapidly. The following case may also be considered as a spontaneous cure:

Case 7th.—M. B., aged 40, a fine looking and very intelligent Irishman, recently from the State of Ohio. Arrived in N. O. about the 10th af October last, and put up at a boarding-house on the levee, just above the lower market. Says he was attacked with a cholera morbus on the first night after his arrival. He took no medicine, and continued to puke and purge for at least three days—had intense thirst during this time, and drank a great deal of water. The vomiting and purging now stopped, and he was seized with a violent fever, which lasted three or four days and then declined without sweat. Took no medicine during all this time—says he had no money to buy it with, and would not *beg* even transportation to the hospital. He then found himself very weak, having taken no nourishment since his attack. On the 19th Oct. he, with difficulty, made his way on foot to the Charity Hospital—says he was about three hours performing it. I saw him first in the evening. *Existing state*—skin and eyes very yellow—surface cool—pulse 70—tongue clean.–no pain—bowels costive—urine free—no thirst—some appetite.

Treatment.—Infus. Prun. Virgin.—purg. enema—light food.

Oct. 20th.—Rested well—bowels open—urine free and high coloured—tongue clean—pulse 60—no pain—slight hemorrhage from the nose. *Treat.* Infus. prun. virg.—porter—chicken soup.

Oct. 21st.—Did not rest well—had a pain in his hip, which on examination I found swelled—appetite improving—bowels open—pulse 66—hemorrhage continues. *Contin. treatment.*

Oct. 22d.—Rested badly on account of pain in the hip—nose still bleeds a little—skin cool—pulse 70—seems more feeble—has less appetite—bowels confined. *Treat.* Ordered pills containing quinine, blue mass and rhubarb—porter and chicken soup—vol. Linament to hip.

Oct. 23d.—Something better—no stool—other symptoms same. *Treat.* Purg. enema—soup &c.

Oct. 25th.—Much better—hemorrhage ceased—yellowness general and intense, extending even to the tongue—no pain—good appetite—convalescent. He continued to improve from this time and was discharged cured.

Case 8th.—(This case will also show the natural course of the fever, undisturbed by medicine in the first stages, excepting two doses of salts, which were rejected from the stomach without purging.)

E. C., an Irish Baker, aged 34—residence in the upper part of the 2d Municipality—lived in N. O. for the last four years, but never sick here before—having had a press of business and worked *all day and night* of the 18th October, went into the yard about day-break on the morning of the 19th and strained himself lifting at a piece of wood—soon afterwards he was seized with a chilliness followed by hot fever, accompanied by severe pains in the head, back &c. Of his own accord he took a dose of epsom salts which he soon vomited up together with some bilious matters. Bowels costive. In the evening of the same day he took another dose of salts, which was likewise thrown up without moving the bowels. He took no more medicine, but continued to vomit from day to day—fever, pains and thirst continued. Says he drank a great deal of cold water—voided much urine, but never sweated nor had any evacuation from the bowels On Sunday morning the 25th Oct. (7th day of sickness,) says he began to turn yellow. On Monday the 26th, he came to the Hospital, after my regular visit—the house surgeon ordered him to be sponged and gave him lemonade—he had one hard stool after he entered, the first since he was attacked.

Oct. 27th.—I saw him the first time. *Existing state.* Eyes and skin intensely yellow—skin of natural temperature and inclined to sweat—tongue very red, but not foul or dry—great thirst—still vomits whatever he drinks—slight pain in the back—very restless—pulse 80 and soft. *Treatment.* Blister 6 by 8 inches to epigast—purg. enema—sponge extremetics with hot vinegar and mustard—gum water iced—porteree iced.

Oct. 28th.—Feels better—has not vomited since he felt the blister—had no stool—(enema neglected,) urine free—no thirst—tongue clean and moist—pulse 86, very small—skin and eyes very yellow—soreness in the muscles of the legs and arms—some appetite. *Treatment.* Purg. enema—dress blister with quinine ointment ℈ i to the ℥ i.—infus. prun. Virgin.—chicken soup.

Oct. 29th.—Much improved—still costive. Ordered a dose of castor oil.

Oct. 30th.—Better in every respect—bowels freely purged—rested well—has an appetite. From this time he continued to improve and was discharged at his own request on the 2d of November.

Case 9th.—J. G., a Scotchman, aged 22—lived about N. O. the last eight years—does not recollect ever to have been sick in his life—employed as a hand on a steamboat that runs to Attakapas. Having been in the city (3d Municipality) three days, he was attacked on Sunday night, 25th October, with chilliness, soon followed by hot fever, violent pains in the head, back &c.—Took no medicine. 26th, fever very high and pains continued—took a dose of salts, which both puked and purged him freely. 27th—says the symptoms continued—had no more stools—no sweat—no urine—great thirst—28th, fever began to abate towards evening—other symptoms same.

Oct. 29th.—entered the Hospital—then had but little fever—had a very sore throat—was very restless. The House Surgeon gave him a gargle and some lemonade.

Oct. 30th.—I saw him for the first time—his intellect was perfectly clear, and he gave me the above account of himself. *Existing state.* He had black vomit and hemorrhage from the nose and bowels—skin cool and of a muddy yellow colour—eyes yellow—pulse 80, soft and gaseous—complete suppression of urine—tongue brown, but moist—but little thirst—no pain except in his throat, which he says is very sore—breath very fœtid—says he will die, but seems quite indifferent about it—says that one of his comrades on the same boat died on her last trip to Attakapas, in the same manner. *Treatment.* Ordered porter and ice *ad lib.* He continued to throw up black vomit, and to discharge blood from the bowels—died at 2, P. M.

Case 10th.—T. McC., aged 24—been in N. O. 2 months—entered Hospital October 19th—says he has been sick 15 days—taken no medicine—commenced throwing up black vomit soon after he entered. *Existing state.* I found him very yellow—skin moist and of natural temperature—pulse 80, and soft—tongue clean—no pain—considerable thirst—bowels confined—urine free—disposed to sleep—intellect clear—throwing up black vomit. *Treatment.* Had no hopes of him, but ordered whole surface to be rubbed with hot vinegar and mustard—to have porter and ice *ad lib.*

Evening.—Comatose—throws up great quantities of black vomit—

pulse weaker and more frequent. Ordered sinapisms to epigast. and extremities—continue porter and ice.

Oct. 20th.—Extremely low—insensible—black vomit has ceased—skin cool and clammy; of a greenish yellow colour. Died in the evening.

These cases are deemed sufficient to show the character of the fever. They have been selected for that object, and not to illustrate any plan of treatment. I have chosen from my notes chiefly such cases as had been *least modified by treatment.* I have given no autopsies. Many were made, but nothing extraordinary was discovered. The post mortem appearances of yellow fever have been so frequently and so fully described that I deem it useless to cumber this paper with them. As the season advanced the fever assumed more of a typhoid type, frequently terminating in hemorrhages and black vomit from the 11th to the 15th day or later. In this there was a resemblance to the yellow fever which occurred in Scotland in 1844, described by Dr. Cormack, at Woodville, Mississippi, described by Drs. Stone & Kilpatrick, and on board H. B. M. ship "Ecclair" in 1845, described by Dr. Kellogg. There were other points of resemblance to the Woodville fever; among which I may mention the good effects I witnessed from calomel given liberally in several desperate cases.

I have reported cases in which dysentery and intermittent terminated in yellow fever. There were other cases which did the same. I saw an instance in Dr. Henderson's Wards, were a man had been attacked with intermittent fever at Vicksburg, 400 miles above New Orleans—he came down to the city, having a regular Tertian—entered the Charity Hospital and died in a few days with *black vomit.* Intermittents were plentiful at the Charity Hospital throughout the summer and autumn; and cases of remittent, bilious and congestive fever were not unfrequent. At one time I had within the walls of a single Ward, one or more specimens of intermittent, bilious, congestive, typhoid and yellow fever at the same time.

The disease did not become common, until after the 1st of October, and a few cases were to be seen, both at the Charity Hospital and in private practice, as late as the last week in November.

The following is a synopsis of the last cases I observed at the Charity Hospital.

Case 1st.—(Ward 13.) M. A., a German, aged 23—been in the city 6 weeks—entered Hospital November 24th—then sick three days. When he came in, the fever had subsided, but he had great irritability of stomach—Nov. 26th, commenced throwing up black vomit and purging the same in great quantities—Nov. 27th, continued to puke and purge black vomit—Urine free. Died on the 29th.

Case 2d.—(Ward 11.) O. C., aged 22—Entered Hospital Nov. 11th—said he had been in the city but three days—staid in the 3d Municipality—was attacked with fever, attended with great irritability of stomach, on the 9th. After entering Hospital he was cupped over stomach—hemorrhage continued from scarifications, also from the gums for several days.—Nov. 27th, I saw him—found him very yellow in all parts—no hemorrhage—no vomit—urine free—very weak. Dec. 1st saw him again—appearance much the same—Extremely low, but has a prospect of recovery.

Case 3d.—(Ward 11,) J. H., aged 34—been in the City two months—entered Hospital Nov. 11th, said he had then been ill with *a disentery* for a week—on the 2nd day after admission yellow fever made its appearance and the disentery ceased. He was cupped over the stomach—hemorrhage continued from the scarifications and from the gums for several days. Nov. 27th, I saw him first—he was convalescent—skin and eyes slightly yellow. Dec. 1st, walking about—

Case 4th.—(Ward 19.) P. O., aged 19. Entered Hospital Nov. 25th—was very ill and could not give a good account of himself. Nov. 27th—I saw him—he was then comatose and beginning to throw up black vomit—died on the 27th.

Case 5th.—(Ward 24.) J. M., a Frenchman—lived on Hospital street, near the river, the last 8 months—Entered the Hospital Nov. 28th—had been sick 8 days. Dec. 1st. I saw him—he was convalescent—his skin and eyes were slightly yellow—no fever.

There was a heavy white frost on the morning of the 26th November—lighter ones had been seen a little earlier.

Very few cases of the fever were admitted in the Marine Hospital or either of the private Infirmaries. I heard of no cases of yellow fever having been imported into the city from the West Indies or elsewhere. By reference to the last number of this Journal, under the head "Health of the Country," it will be seen, that D. Hulse, U. S. Naval surgeon, gives an account of a fatal fever prevailing at Pensacola; but he does not pronounce it yellow fever.

The following is a monthly report of deaths from Yellow Fever, in the city of New Orleans, obtained from the Board of Health:

August - - -	No. of deaths - -	1	
September - -	" " - -	8	
October - -	" " -	118	
November - -	" " -	33	total 160.

The following deaths occurred at the Charity Hospital—included in the above report.

August - - -	No. of deaths - -	1
September - -	" " - -	8
October - -	" " - -	50
November - -	" " - -	30.

I said in the beginning of this paper, that the type of the fever was different from that, which has usually prevailed here before. The following are the distinctions which I deem worthy of notice.—1., the attack was generally milder and more insidious; 2., the fever and pains were not so violent, nor did they demand such prompt and free depletion; 3., the progress of the fever was more tardy, frequently not terminating either fatally or favorably, until from the 11th to the 15th day; sometimes even later; 4., I do not think, that suppression of urine was so generally present in the fatal cases.

I saw no case recover after throwing up *black vomit*, though I had two or three to do so, after appearing to get to the very verge; ejecting a flocculent, ropy fluid, with a sediment at the bottom of the vessel. Some cases recovered, after having had various hemorrhages for many days. The cases at the Charity Hospital generally turned yellow after the crisis, though there were exceptions.

In regard to the extent of the disease, I have before remarked, that it prevailed chiefly in the 3d Municipality, and amongst the lower order of the people. The most of the cases admitted in the Charity Hospital, came from that part of the city, but there certainly were cases from the upper part of Lafayette, down to the Tobacco warehouses.

The mortality at the Charity Hospital will appear to be great for the number of cases, but allowance must be made for the fact, that many recovered, which were not pronounced yellow fever. Besides, most of the decided cases had generally passed the stage suitable for active treatment, when they entered—some were *in articulo mortis*.

From all the facts here stated, I think the following conclusions may be fairly deduced; viz:

1.—That the fever, which I have attempted to describe, is genuine yellow fever.

2.—That it originated spontaneously in this city.

3.—That whatever be the true cause of our summer fevers, the amount of sickness is not uniformly in proportion to the combined amounts of heat, moisture and putrid matters.

4.—That there must exist a close affinity between all the summer and autumnal fevers, that belong to this locality, inasmuch as we see them arise together—prevail together—the milder frequently running into the more severe—and all declining together on the appearance of frost.

Whether all the fevers that prevail here during summer and autumn, be merely *different forms of the same disease*, arising from *the same remote causes or elements*—or *separate and distinct* diseases, dependent upon *separate causes* existing together, forms an interesting problem for the medical enquirer. May not a striking analogy be found in what are termed *isomeric substances*, in chemistry?—substances, whose sensible properties are as different as spirits turpentine and oil of lemons, carburetted hydrogen gas and otto of roses, aldehyde and acetic ether, and many others, which are composed of the *same elements and in the very same proportions*. In conversation with a professional friend upon the subject a few days since, he threw out this suggestion, and I think it has much plausibility.

I will here close my task. It has been a somewhat laborious one, and I fear, will not prove very satisfactory to my readers, but I crave their iudulgence towards my humble effort. When I commenced this paper, I was not aware, that any thing else would appear upon the subject, and this was my only reason for undertaking it. After nearly finishing it, I was pleased to learn, that the Board of Health had appointed a Committee, to report on the sanitary condition of the city, &c. We give both papers, and trust, that neither will be considered superfluous.

VI.—*Report of the Board of Health, on the Sanitary Condition of the City of New Orleans during the year* 1846, *and the means of improving it.* By W. P. Hort, M. D., Chairman of the Committee.

The Committee appointed by the Board of Health, to report on the sanitary condition of the city, during the present year, beg leave to submit the following remarks.

The complete exemption of the city of New Orleans from yellow fever during the year 1845, and its sensible abatement for several years previous, had induced many of our most intelligent citizens to flatter themselves that it had disappeared for ever from this section of country.

From this opinion there probably resulted an undue confidence, and a relaxation of those police measures and regulations which must be energetically and fully carried out, before we can reasonably hope to escape from the visitation of yellow, or any other malignant fever.

This inference appears to be natural when we consider the unusually filthy condition of the city, since the spring of the year, a fact which has been noticed repeatedly by the public press, and has been an almost daily subject of conversation amongst the citizens in all parts of the city.

No attention has been paid to the banks of the river, since the water began to fall early in the summer; and filth and garbage of every description, both animal and vegetable, have been deposited on the newly exposed alluvial bank to undergo a putrid fermentation beneath the burning rays of the sun, at a temperature ranging between 130 and 140 degrees, (Fahrenheit.)

In the latter part of the month of July, a member of this committee was informed by the Harbor master that several vessels had to be moved from the wharves between Hospital and Barracks streets, to the Second Municipality, on account of the insufferable stench arising from the bank of the river. In the latter part of the month of September, and the beginning of October, five or six captains and mates were taken sick on board ships lying at those wharves, most all of whom died.

Complaints of stagnant and putrid water in vacant lots, and even under houses in some instances, in the back streets of the city, were made early in the season; and the stagnant and putrid condition of the water in the gutters, and the accumulation of filth at the crossings of the streets, particularly in the Faubourg Tréme, during the drought which commenced in September, and continued throughout the whole months of October, was a subject of general remark. The stench arising from this putrid filth was in many instances intolerable and overwhelming. (See also New Orleans Medical and Surgical Journal for Sept., 1846, p. 275.)

When these circumstances are duly considered, and that moreover, disbanded volunteers already debilitated by disease on the Rio Grande, were returning every week during the summer and fall; that most of them were unacclimated men from the western States, and compelled to remain for some time in the city, it is only surprising that New Orleans has suffered comparatively so little this last season with malignant and and yellow fevers. It has been estimated that more than twenty thousand Europeans have settled in this city in the course of the four or five last years, none of whom have been acclimated by yellow fever, and they would necessarily have been exposed to its attacks, and amongst the first victims, had the fever been *epidemic* in its character. Every fact tends to prove that it was local in its origin, and confined to the locality where it originated. Its form, therefore, was *sporadic*. (See again art. "Health of the City" in the New Orleans Medical and Surgical Journal, September and November, 1846.)

It is probable that this city would have been as healthy and exempt from yellow fever in 1846 as it was in 1845, if the authorities of the city had acted energetically on the suggestions contained in a report, which was unanimously adopted by the Physico-Medical Society, on the 15th of February, 1845, printed copies of which were presented to the Mayor and Councils of the three Municipalities.

The committee would take this occasion to renew those suggestions of the Physico-Medical Society.

It was recommended in that report,

1. That the Commissaries be required to look into back yards and lots; and be authorized to cause every thing offensive to be removed.

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

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

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

The committee would here remark, that as by far the greater, and certainly the most offensive part of the offal is accumulated after dinner, it would appear to be better to have the same removed by the scavengers in the evening, rather than in the morning; in which case there would be very little accumulation before the dinner hour, whereas at present, the offal is left in the streets all night, and often half the next day exposed to the hot sun.

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

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

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

7. That instead of depositing the filth and offal collected in the streets by the scavengers, in empty lots, or in the rear of the city, it is recommended to the city authorities to have all such filth and offal thrown into the current of the river."

It is difficult to decide which cause is the most productive of disease, vitiated atmosphere, or bad water. In the back part of the city, the inhabitants, who are chiefly of the labouring class, are much distressed for the want of water, when there is a long continued drought. Cisterns here and there, which may occasionally be found, are soon exhausted; purchasing river water which is hauled perhaps a mile from the bank of the river, is often beyond their means; and what then remains for them to use but the impure stagnant water of the wells: the committee would therefore recommend that public hydrants or pumps be established in the back parts of the city, at convenient places and suitable distances, that the working classes and the poor may have at all times an abundant supply of pure water.

Public baths as recommended by Dr. Ely in an article in a late number of the Commercial Review, are also deemed of great importance by the Committee. Besides the healthy tonic influence of cold water on the surface of the body, there is nothing more conducive to health, particularly in a warm climate, than personal cleanliness.

The members of the Board of Health and the Commissaries may faithfully discharge the duties required of them by law; but it is out of their power to abate all the nuisances that have been spoken of and to carry into effect all the measures recommended.

Damaged flour, condemned fish and oysters, fruit, hay, hemp, rotten hides, potatoes, grain, dead animals, besides the garbage and filth from the vegetable and fish markets and the crowded boarding houses near the Levee, are thrown on the bank of the river some time during the night, and to prevent this injurious practice from being continued it is necessary that a sufficient number of persons should be employed by the Councils, to watch the Levee by night whenever the river begins to fall in summer, and one man should be employed in a skiff to shove out into the middle of the river dead animals that float down and lodge on the bank.

A member of this Committee early in the summer over his own signature called the attention of the city authorities to this disgusting condition of the Levee opposite Hospital street, and for some distance above and below. It received no attention, nor did the remonstrances which the Harbor Master repeatedly made in behalf of the Captains of ships in that neighborhood. Since then a communication on the same subject was addressed to his Honor the Mayor and the Board of Health; but the Committee cannot see that any measure has been adopted to remedy the evil.

The evil arising from stagnant and fœtid water in the gutters in the back part of the city, must be remedied by draining operations under the control of the city authorities. The furnishing hydrants or pumps in certain parts of the city, and a bountiful supply of the river water to run through the gutters in the streets, is also the business of the Municipal Councils.

The mere announcement of the existence of a dangerous disease to the public, is the least important duty of a Board of Health; their paramount duty is, and their great aim should be, to detect all causes of disease, and to recommend those measures by which the former may be removed and the latter averted.

The present Board of Health was not established until early in the month of August, a period too late for them to be of much service, beyond the annonncement of the existence of disease.

It was frequently remarked by members at the regular meetings of the Board, that a large proportion of the fever cases admitted into the Charity Hospital, were of an intermitting character and comparatively unimportant at the time of admission, but that the type changed to some malignant or yellow fever in the course of three, four, five or more days.

As the greater part of the cases of yellow fever occurred in the Charity Hospital, it would sêem to be probable that there existed some peculiarity in the atmosphere unfavorable to the condition of the fever patients, and which possibly might have been corrected by the use of disinfecting agents.

It is a fact observed long ago in Europe, that the atmosphere occasionally becomes very much vitiated in crowded Jails and Hospitals, generating a malignant and fatal fever which received the name of Jail or Hospital fever: at the same time the army surgeons noticed that under certain circumstances, all the wounds in the Hospitals became gangrenous and fatal. (See Huxham, Ferriar, Sir John Pringle, Hennen &c. on this subject.)

To avert so great an evil, the British Government offered a large sum of money for the discovery of an efficient dis-infecting agent. It was awarded to Dr. Carmichael Smyth, who recommended "fumigations with nitre and concentrated sulphuric acid to be used thriee daily in all the wards," and which, according to Ferriar, (vol iii. p. 327, Med. Hist.) "was employed with such success in his Majesty's militiary and naval Hospitals." This agent has in a great measure been since superceded by the introduction of the chlorides of lime and soda, and latterly the sulphate of iron in solution has been introduced into the dissecting rooms in Paris with beneficial effect.

The Committee cannot but think that this is a subject at all times worthy the consideration of the physicians who attend on the sick at the Charity or any other Hospital. In connection with this subject it is a fact well worthy of consideration, that while during the latter part of September and the month of October the cases and deaths of yellow fever were on the *increase* in the Charity Hospital, they were as much on the *decrease* out of the Hospital in private practice, although our citizens who had been absent during the summer either at the north or at the watering places or the lakes, (together with strangers who visited the city for the first time,) were returning by thousands every week.

Much doubt has been expressed as to the authority of the General Council to confer sufficient police power on the members of the Board of Health, so as to enable them to render efficient services to the city.

The committee have taken some pains to investigate this subject, and think it may be expedient to introduce all the documents connected with it in the present report.

They invite attentin first to "an ordinance for the establishment of a Board of Health for the city of New Orleans, and defining its powers." Sitting of Thursday 16th July 1846.

Art. 1. Be it ordained by the General Councily of the city of New Orleans in council convened, That from and after the passge of this ordinance, there shall be established in the city of New Orleans, a body to be known as a Board of Health.

Art. 2. Said Board shall be composed of twelve practising physicians, four from each Municipality. That at the next sitting of the Council after the passage of this ordinance for this year, and in the month of May each and every subsequent year, they shall be elected *viva voce* by the General Council.

Art. 3. Seven persons shall constitute a quorum for the transaction of business, and said Board shall at each sitting elect, *viva voce*, one of their body as President.

Art. 4. The Board of Health shall appoint a suitable person to be Secretary of said Board, who shall receive a salary of fifty dollars per month, payable *pro rata* by each Municipality.

Art. 5. That such Secretary shall keep an office in such place as the majority of the Board may determine; the rent of said office shall not exceed three hundred dollars per annum, payable *pro rata* by each Municipality on the warrant of the Mayor. Said office shall be kept open from the 1st of June to the 31st of October, from 9 o'clock A. M. to 3 o'clock P. M. and trom 5 o'clock P. M. until 8 P. M., and from the first of November to the 31st of May, from 12 o'clock noon until 3 o'clock P. M.

Art. 6. That it shall be the duty of the Secretary of the Board of Health to keep a correct record of the proceedings of said Board and all the reports made by any member thereof, to make full and complete meteorological notes and remarks during the entire year, noting the temperature of the atmosphere at three different periods of the day, at least as indicated by the thermometer; the daily changes of the barometer; the quantity of rain as shown by the pluviometer; the amount of sensible electricity as shown by the electrometer; the course and strength of the winds; the daily appearance of the clouds, and all other such phenómena as may be directed by the Council or Board of Health to the Observer and Register, and to receive all reports of the keepers or sextons of cemeteries, and record the same in a book which he shall keep for that purpose, which register shall at all times be open to the inspection of the citizens.

Art. 7. That it is hereby made the duty of each Commissary acting under the authority of either of the Municipal Councils of this city, to cause all orders emenating from the Board of Health relative to the cleansing and keeping in good order any and every street, lot or yard within the limits of their respective districts to be promptly obeyed.

Art. 8. Each and every person offending against the provisions of the foregoing ordinance, shall be punished by fine not exceeding fifty nor less than twenty-five dollars, recoverable before any court of competent jurisdiction. That in addition to the above, said Board of

Health shall enjoy and exercise all the rites and priviledges, and perform all the duties now vested in the Board of Health, by the ordinance approved June 8, 1841, creating a Board of Health; and all subsequent ordinances amendatory thereto, not contrary to the provisions of this ordinance.

Art. 9. All ordinances contrary to the provisions of the foregoing be and the same are hereby repealed."

2nd. To the ordinance of 1841.

To abbreviate this report, the committee will give the general outlines of the ordinance of 1841, as found in the "ANALYTIC TABLE," leaving out all that is repealed by the ordinance of 1846.

"BOARD OF HEALTH----Has the entire sanitary condition of the city under its supervision and control. Elects its Secretary, who receives the reports of the cemeteries, and has the same published with meteorological statements.

Sextons or Keepers of Cemeteries shall report to the Board, through its Secretary, under a penalty. Have the right of exacting from every person or persons bringing a corpse to the cemetery a certificate embracing certain particulars. It shall be the duty of said sextons to file said certificates, and record them in a book to be delivered to the Mayor at the end of each year.

The Board of Health shall publish a weekly statement of deaths.

Police officers and Commissaries shall report to the Board on nuisances and on all matters concerning the sanitary condition of the city. Duties imposed on the Board of Health in relation to the inspection of Cemeteries; shall report to the Mayor any thing having a tendency to impair the public health.

Physicians shall report to the Board of Health the existence of any infectious or contagious malady. The Board may take such steps to protect the community, as to them may seem proper.

The Board shall give early notice of the existence of epidemic disease.

Municipal Councils invited to establish each a Vaccine Institution. Municipal Councils to endow said Institutions, and to vest in the Board full and adequate authority.

Certificate of death to be furnished by the attending Physician under penalty of a fine, should he refuse to give the same.

Undertakers of Hearses who convey a corpse to the Cemetery without a license, shall be liable to a fine. Bound to keep a copy of the certificate of death of any person carried by them to the grave, and to deliver the same to the Secretary of the Board of Health monthly. None shall act as such, unless provided with a license from the Mayor. Burying out of the cemeteries forbidden under a penalty.

Annual report to be published by the Board of Health in January of each year.

Sittings of the Board of Health to be held at least once a month."

The Committee have in one or two instances changed the phraseology, to make the meaning of the ordinance more apparent.

Nothing has been done in relation to the Vaccine Institutions spoken of above. It is very difficult as well as expensive to procure a regular and abundant supply of fresh matter, yet the subject is so important that we would earnestly call the attention of the municipal authorities to it. The Small Pox, it is true, has not hitherto done much mischief in this city, but there is no telling what ravages it may some day commit under peculiar circumstances.

No meteorological reports have been as yet made by the Secretary, because he has not been furnished with the appropriate instruments.

Many persons have doubted the practicability of determining the amount of sensible electricity as shown by the electrometer (art. VI, ord. 1846) but to show that such an instrument for the said purpose has been successfully employed in meteorological observations and reports, the Committee give the authority of Volta, Saussure, Peltier, Armstrong, Ferraday, Schübler, Arago, Becquerel, Breschit, Gay-Lussac and Biot. (see art. "atmospheric electricity, part IX. Popular lectures, by Dr. Lardner."

There is one more meteorological instrument which the Committee thinks should be added to those already enumerated in art. VI of the ordinance of 1846, establishing a Board of Health. It is the Hygrometer, designed to show the dew point, and the difference between the temperature and the dew point. The ingenious and simple instrument of Daniels, they would recommend as the best calculated for that purpose.

Having ascertained what powers have been conferred on the Board of Health by the ordinances of the General Council of 1841 and 1846, it remains to be seen by what authority the General Council granted such powers.

On the division of the city in 1836 into three municipalities, the City Council was formed from the three Municipal Councils; amongst other powers conferred by the Legislature on the City Council, as it was then called, we find " 6th, to enact all police regulations of a general nature, the operation of which shall be uniform in all parts of the city." (Act of 1836, p. 329, sec. 2101, Greiner's Digest.)

It appears that this City Council was not changed into the present General Council until 1840, when we read: " The General Council of the city of New Orleans, shall in future be composed of twelve members, four of whom to be elected by general ticket by the qualified voters of each municipality at the time of the election of the aldermen of said municipalities, and the General Council so constituted shall enjoy all the rights and privileges now vested in the City Council, &c." (p. 333, act of 1840, Greiner's Digest.)

The Committee are of opinion that the establishment of a Board of Health is fairly included in the general expression, "*all police regulations of a general nature which shall be uniform in all parts of the city,*" and that therefore the General Council has full authority to grant those powers to the Board of Health, with which its members have been invested.

The Committee avail themselves of the Meteorological Journal and reports of Mr. D. T. Lillie, as published by him in this city, to institute a comparison between the temperature and other peculiar circumstances of the atmosphere in 1845 and 1846.

	THERMOMETER. 1845.			THERMOMETER. 1846.			BAROMETER. 1845.			BAROMETER. 1846.		
	Max.	Min.	Range	Max	Min.	Range	Max.	Min.	Range	Max.	Min.	Range
Jan.,	71.5	41.5	30.0	70.0	32.0	38.0	30.57	29.67	0.90	30.54	29.51	1.03
Feb.,	78.0	37.5	41.5	68.5	40.0	28.5	30.46	29.94	0.52	30.44	29.81	0.63
M'ch,	80.5	42.0	38.5	74.0	46.0	28.0	30.50	29.94	0.56	30.48	29.72	0.76
April,	88.0	52.5	36.5	82.7	46.0	36.7	30.32	30.00	0.32	30.25	29.90	0.35
May,	89.5	64.0	25.5	88.5	61.0	27.5	30.25	29.98	0.27	30.18	29.80	0.38
June,	90.0	70.0	20.0	90.0	66.0	24.0	30.27	30.01	0.26	30.15	29.91	0.24
July,	92.7	73.0	19.7	89.7	69.5	20.2	30.26	30.15	0.11	30.17	29.83	0.34
Aug.,	92.5	72.0	20.5	90.0	70.0	20.0	30.22	29.96	0.26	30.20	30.00	0.20
Sept.,	87.5	63.0	24 5	90.5	71.0	19.5	30.16	29.90	0.26	30.20	29.77	0.43
Oct.,	83.7	51.5	32.2				30.37	29.93	0.44			
Nov.,	77.0	32.5	44.5				30.33	29.95	0.38			
Dec.	69.0	25.0	44				30.44	29.99	0.45			

	1845.				1846.			
MONTHS.	PREVAILING COURSE OF WIND.	FORCE OF WIND, Ratio 1 to 10.	RAINY DAYS.	QUANTITY OF RAIN. *Inches.*	PREVAILING COURSE OF WIND.	FORCE OF WIND, Ratio 1 to 10.	RAINY DAYS.	QUANTITY OF RAIN. *Inches*
Jan'ry,	N.W.	1.9	7	3.097	N W.	$2\frac{3}{4}$	6	2.195
Feb'ry,	N.W.	2.8	3	1.498	W.	$2\frac{7}{8}$	9	2 649
March,	S.S.W.	2.4	10	4.671	E.	$2\frac{3}{4}$	7	8.410
April,	S.	2.4	6	1.416	S.E.	$3\frac{1}{8}$	14	12.557
May,	S.E.	$2\frac{1}{4}$	15	4.959	N. & S.W.	3	10	11.010
June,	S.	2	7	2.795	S.W. S.E.	3	16	7.979
July,	S.S.W.	$2\frac{1}{8}$	10	1.592	S.W. S.E.	3	11	13.000
August,	S.W.	$2\frac{1}{2}$	12	1.838	S.W. S.E.	$3\frac{1}{4}$	13	6.962
Sept.,	N.W. N.E.	$2\frac{1}{8}$	9	1.280	N.E. S.E.	2 4-5	15	5,698
Oct.,	N.E.	$2\frac{1}{3}$	10	1.145				
Nov.,	N.E.	$2\frac{1}{3}$	6	2.760				
Dec.,	N.W.	$2\frac{2}{3}$	13	1.877				

Remark.—Rain Guage graduated to the one thousandth of an inch.

It will be seen by the foregoing table, that the maximum degree of heat as indicated by Fahrenheit's thermometer, was greater each month in 1845, from the 1st January to the 1st Oct., than it was in 1846, excepting in two instances; the maximum for the month of June in both years is 90°, and the maximum of September, 1846, exceeds that of 1845 by 3°. The Committee are of opinion that no inference affecting the origin of yellow, or any other form of malignant fever, can be deduced from a consideration of the comparative temperature during the two years, or from the slight variations observable in the barometer.

From January to Oct., in 1845, there were seventy-nine rainy days; and the quantity of water that fell as indicated by a rain guage, graduated to the $\frac{1}{1000}$ part of an inch, was 23 inches 143 dec. or 1 foot 928 dec., while during the same period in 1846, there have been one hundred and one rainy days, and 70 inches 460 dec. or 5.87 feet of water have fallen—rather more than three times as much as fell in 1845. If any inference can be drawn from this fact, the committee are inclined to consider it as favorable to the health of the city in 1846. The experience, however, of former years, would lead us to conclude that more or less rain, or a greater or less degree of heat, has very little to do with the production of yellow fever; for that disease has been known to prevail here alike in dry or wet seasons, and without regard to the variations of temperature in the summer months. Many deaths have occurred amongst the captains and mates of the packet ships, and other large vessels which arrived here from the North, while the Committee are not aware of a single case having originated at that part of the levee where brigs, schooners, and other small vessels trading to Havana and different West India ports, are located.

The Committee are of opinion, that the number of cases of yellow fever has been greatly overrated. The members of the Board of Health took pains, whenever practicable, to investigate the reports of yellow fever, which were at different times in circulation.

It was confidently affirmed, that the Captain of the Arab steamer, which arrived here from Havanna, via Vera Cruz, on the 13th September, had reported that his crew had suffered, and were still suffering from yellow fever. On investigating this case, it was ascertained, that some of the crew had been sick on the passage, and that the mate had died at sea, but no sickness had occurred on board since her arrival in the river. The Arab too, was at that time anchored off the point, on the other side of the river.

A vessel from Jamaica, that moored on the wharf opposite the Pontchartrain rail road, on Sunday night, September 13th, was reported to have all hands on board sick with the yellow fever. On examining the vessel, it was found that two or three of the crew had been attacked with chills and fevers on the passage, but at the time she arrived here, there was no sick person on board—many other similar or analogous facts might be cited, but it would swell this report to too great a length; besides, the Committee consider that the question of the importation or domestic origin of yellow fever, has been permanently settled in this part of the world.

Difficult as it is to prove, what is actually the cause of disease in the malignant fevers of the South, the experience of thousands of patient observers in all parts of the world for many centuries, has traced a connection between certain *local* causes and *certain* diseases.—In the absence then of *all* the knowledge on this subject, which we desire to possess, we should not be deterred from doing that which ample experience has demonstrated to be eminently beneficial.—We repeat—let all known causes of disease be removed, and all such measures, as have been recommended, rigidly enforced; and to accomplish this, we invoke the aid and co-operation of the Mayor and the three City Councils, feeling assured, especially after the experience of the last season, that New Orleans may thus be rendered as healthy as any city on this Continent from January to December.

CHARITY HOSPITAL.

Admission		Date of Death			Discharge
August	29	Sept.	1	"	
Sept.	3	Sept.	6	"	
"	"	"	7	"	
"	4	"	"	"	
"	6	"	12	"	
"	7	"	10	"	
"	"	"	"	"	
"	8	"	"	"	
"	"	"	11	"	
"	"	"	13	"	
"	9	"	10	"	
"	9	Sept.	12	"	
"	9	"	14	"	
"	10	"	12	"	
"	11	"	14	"	
"	"	"	"	"	
"	"	Oct.	5		"
"	13	Sept.	15	"	
"	13	"	13	"	
"	19	"	27	"	
"	20	"	21	"	
"	20	"	28		"
"	21	"	26	"	
"	22	Oct.	7		"
"	24	"	4		"
"	"	"	8		"
"	"	"	13		"
"	25	Sept.	27	"	
"	"	"	28	"	
"	"	"	30	"	
"	"	Oct.	10		"
"	"	"	2	"	
"	26	Sept.	26	"	
"	"	"	29	"	
"	"	"	30	"	
"	"	Oct.	9		"
"	27	Sept.	29	"	
"	"	Nov.	3		"
"	28	Sept.	30	"	
"	29	Oct.	4	"	
"	"	Nov.	2		"
"	30	Oct.	27	"	"
"	"	"	10		"
"	"	"	1	"	
"	"	"	10	"	
Oct.	1	"	2	"	
"	1	"	2	"	
"	2	"	2	"	
"	2	"	3	"	
"	3	"	20		"
"	"	"	15	"	
"	4	"	14	"	
"	5	"	17		"
"	7	"	26		"
"	"	"	14		"

Admission		Date of Death			Discharge
Oct.	10	Oct.	15	"	
"	"	"	19	"	
"	"	"	20		"
"	"	"	11		" ?
"	"	"	17		"
"	"	"	25		"
"	11	"	15	"	
"	12	"	14	"	
"	"	"	17	"	
"	"	"	24		"
"	"	"	28		"
"	13	"	27		"
"	"	"	16	"	
"	"	"	16	"	
"	"	"	16	"	
"	14	"	24		"
"	"	"	23		"
"	"	"	15	"	
"	"	"	16	"	
"	"	"	16	"	
"	"	"	16	"	
"	"	"	17	"	
"	15	"	27		"
"	"	"	20		"
"	"	"	18	"	
"	"	"	21	"	
"	16	"	27		"
"	"	"	25		"
"	"	"	20	"	
"	17	"	25		"
"	"	"	24		"
"	18	"	25		"
"	"	"	18	"	
"	19	"	25		"
"	"	Nov.	3		"
"	"	Oct.	20	"	
"	"	"	"	"	
"	"	"	26	"	
"	20	Nov.	4		"
"	"	Oct.	24	"	
"	"	"	28	"	
"	22	Nov.	2		"
"	23	Oct.	23	"	
"	"	"	26	"	
"	"	"	28	"	
"	24	Nov.	3		" ?
"	"	Oct.	24	"	
"	25	"	27	"	
"	26	Nov.	3	"	
"	"	"	1	"	
"	"	"	2		"
"	29	"	7		"
"	"	Oct.	30	"	
"	30	Nov.	14		"
"	"	"	2	"	

Admission		Date of Death		Discharge
Oct. 31	Oct.	3	"	
" "	"	6	"	
Nov. 1	"	2	"	
" "	"	3	"	
" "	"	4	"	
" "	"	4	"	
" 2	"	5	"	
" 4	"	5	"	
" 5	"	8	"	
" "	"	12	"	
" "	"	15	"	
" 7	"	21		"
" 9	"	10	"	
" "	"	12	"	
" 11	"	12	"	
" 13	"	16	"	
" "	"	17	"	

Admission		Date of Death		Discharge
Nov. 15	Nov.	16		"
" 18	"	19	"	
" 21	"	21	"	
Total		64 deaths,		42 cured.

Admission		Date of Death		Discharge
MAISON DE SANTE				
Oct. 8	Oct.	13		"
MARINE HOSPITAL.				
" 1	"	14		"
" 6	"	16	"	
" 9	"	15	"	
" 12	"	16	"	
		3 deaths,		1 cured

From the foregoing statement, taken from the books at the Charity Hospital, it appears, that the yellow fever appeared there on the 29th August, that from that time to the 18th November, 105 cases, including 2 marked doubtful, occurred, of whom 64, or 65.40 pct. died, and 42 recovered.

That at Dr. Stone's "Maison de Santé" there occurred but one case, admitted on the 8th October and discharged cured on the 13th—at the Marine Hospital, four cases, of whom 3 died and one recovered—On the first of Aug. Mrs. Holley, residing in the faubourg Tremé, was reported to the Physico-Medical Society as a case of yellow fever in "articulo mortis."

In 1839, there were admitted into the Charity Hospital 1086 cases of yellow fever, of whom 452 died, being 41.62 pCt. In 1841, the admissions amounted to 1113, of whom 690 or 62 pCt. died. In 1842, there were 410 cases, of whom 211 or 51.46 pCt. died, and in 1843, 890, of whom 438 or 49.21 pCt. died. Since 1839, the disease has been decreasing in private practice, and has been confined chiefly to the Charity Hospital, showing it to be the result of some *special* and not of a *general* cause.

List of Deaths and Interments of New Orleans, in 1846,
As furnished to, and recorded by the present Board of Health.

From	23 to 30 August - -	68 deaths,	no yellow fever reported			
"	30 Aug. to 6 Sept. -	70 do	1 case of yellow fever			
"	6 to 12 Sept. - -	74 do	no yellow fever reported			
"	12 to 19 Sept. - -	93 do	3 cases of yellow fever			
"	19 to 26 Sept. - -	68 do	5 cases		do	
"	26 Sept. to 3 Oct. -	94 do	21*	do	do	
"	3 Oct. to 10th - -	88 do	12	do	do	
"	10 to 17 Oct. - -	107 do	39†	do	do	
"	17 to 24 Oct. - - -	98 do	22	do	do	nearly all from the Charity Hospital,
"	24 to 31 do - - -	91 do	14	do	do	
"	31 Oct. to 7 Nov. -	84 do	12	do	do	
"	7 to 14 Nov. - -	82 do	8	do	do	

The Committee regret, that they are unable at present to compare the above, with similar reports from New York, Baltimore, Philadel-

* 19 from the Charity Hospital. † 21 from the Charity Hospital.

phia and Boston. They have no fear of the result. From the middle of September citizens and strangers were pouring into the City by thousands, yet the increase of deaths was in no ratio with this fact, but only such as invariably occurs at that peculiar season of the year.

W. P. HORT,
LE MONNIER, } *Committee.*
VANDERGRIFF.

P. S. The Committee, at the time their report was written, were not aware that the secretary had, through the politeness of Mr. D. T. Lillie, furnished for the use of the Board of Health, a record of meteorological observations.

A. H.

Remarks on Strangulated Umbilical Hernia, with a Case, by A. J. Wedderburn, M. D. Professor of Anatomy in the Medical College of Louisiana.

Strangulated umbilical hernia, being an affection of very rare occurrence in the adult male, it is deemed proper to report the following case for which a successful operation has been performed.

A negro man of 300 pounds weight, aged 31 years, the property of Dr. Slone, residing about two miles below New Orleans, states that he has always had an umbilical hernia, easily reducible, and about the size of a hen's egg—was attacked with a pain in the abdomen,—found the tumour enlarged,—attempted its reduction, but failed. I saw this case about 12 hours after the strangulation occurred, and found the tumour about the size of a child's head, very tense and elastic. About three hours before I saw the case Dr. Slone had used the various means recommended for reduction in such cases, and on my visiting the case with him, we further endeavored for the space of an hour, to effect it by taxis, and to aid in the attempt we administered tartar emetic, tobacco injections, large injections of cold water with a hydrostatic injecting tube, and applied ice and sulphuric æther to the tumour. These efforts having failed, we determined upon an operation as the only means, calculated to afford the patient a chance for his life. A short time previous to the operation, whilst making forcible taxis, a considerable portion of gas contained in the incarcerated bowel escaped with a gurgling sound, and the tumour seemed to subside so rapidly under my hand, that I was induced to believe that I had succeeded in the reduction, but in a few minutes was convinced that the intestine could not be returned, or even the escape of more of the gas effected. By this effort the tumour was reduced so much by the escape of air from the intestine, and the tension consequently so much relieved, as to lessen the danger of wounding the intestine in dividing the integuments. We informed the patient of the necessity of an immediate operation, to which he somewhat objected, and only consented after we had allowed him to make an effort at reduction himself, which he continued for something like ten minutes, using all the while considerable, and well directed force, for he had returned the intestine so often himself when the hernia was smaller and reducible, that he had acquired quite a degree of dexterity in the matter.

Operation.—An incision was made upon the top of the tumour three or four inches in length through the skin in a vertical direction, so as to expose the superficial fascia, which was considerably condensed. A small opening was made through the fascia at the lower part of the wound, by light and careful touches with a scalpel, into which a director was introduced, and carried to the upper part of the first incision, and the fascia divided to correspond with the same, with a probe pointed bistoury. I then introduced my finger between the peritoneal sac and the superficial fascia, and carrying it in every direction, easily effected the separation of the two membranes. Finding there was not room to work in, the wound was enlarged by extending it about an inch above, and more than inch below,—this enabled me to turn aside the integuments, and obtain a good view of the sac, which contained floating in a nearly transparent fluid, a large portion of the omentum, and as well as I could determine, about fifteen inches of the small intestinal tube, which appeared to be perfectly black. A small opening was next made in the lower part of the sac, by seizing up a small portion of it with a pair of artery forceps, and carefully cutting in a horizontal direction.—After the escape of about four or five ounces of fluid, a director was introduced, and the sac divided upon it about four inches. In a few moments the intestine which presented the dark colour before mentioned, began to assume a red appearance, from the action of the air acting upon the blood contained in its vessels, through the coats of the intestine; from which circumstance, and also from a close examination, I was convinced that the gut retained its integrity, and at once endeavored to return it by an attempt to kneed in a small portion at a time;—but in my efforts I not only failed, but additional portions of the omentum and intestine were forcing themselves from the abdomen. In order then, that the stricture could be arrived at without endangering the gut, it became necessary to extend the incisions, not only in the integuments nearly to the base of the tumour, but also in the peritoneal sac. The finger was then forced into the umbilical ring, and a probe pointed bistoury passed by its side, with which the stricture was divided at the lower part about the fourth of an inch. An attempt was again made to return the bowel, but without success, when a division of the ring was made at the upper part, less than a fourth of an inch in extent, by which means I was enabled to return the bowel by pushing in a small portion at a time. After returning the intestine, the omentum was returned, and spread out in front of the small intestines, as well as it could be done by the introduction of my finger into the cavity of the abdomen for this purpose.

After returning the contents of the sack, the blood was sponged from the wound, then leaving it open for the space of about 15 minutes, in order that the oozing from the divided vessels might be entirely arrested, the edges of the wound were brought in contact, and three interrupted sutures applied. The skin, which was very loose, was gathered up like a bag, with the sack contained within, but not included in the ligatures, and a graduated compress applied, with the hope that the mass would, if healed, be a barier to a further protrusion, and effect a radical cure.

The operation was performed on the 23d of September last, and the patient recovered without a bad symptom. Small doses of calomel and opium were administered during four or five days. In ten days after the operation the patient walked across the room and is now entirely well, a radical cure having been effected.

As the operation just described differs somewhat from those recommended by different surgeons, I have thought proper to give below a few extracts, with some general remarks on the subject of exomphalos.

In Cooper's Surgical Dictionary we find the following: "In consequence of the great fatality of the usual operation for the exomphalos, I think the plan suggested and successfully practiced by Sir A. Cooper in two instances, should always be adopted when the tumour is large and free from gangrene; a plan that has also received the high sanction of that distinguished anatomist and surgeon, Professor Scarpa. (*Traité des Hernies, p.* 362.) Perhaps I might safely add, that when the parts admit of being reduced, without laying open the sack, this method should always be preferred. It consists in making an incision just sufficient to divide the stricture, without opening the sack at all, or, at all events no more of it than is inevitable."

In umbilical hernia, of not a large size, Sir A. Cooper recommends the following plan of operating: "As the opening into the abdomen is placed towards the upper part of the tumour, I began the incision a little below it, that is, at the middle of the swelling and extended it to its lowest part. I then made a second incision at the upper part of the first, and at right angles with it, so that the double incision was in the form of the letter T, the top of which crossed the middle of the tumour. The integuments being thus divided, the angels of the incission were turned down, which exposed a considerable portion of the hernial sack. This being then carefully opened, the finger was passed below the intestines to the orifice of the sack at the umbilicus, and the probe-pointed bistoury being introduced upon it, I directed it into the opening at the navel, and divided the linea alba downwards to the requisite degree, instead of upwards as in the former operation. When the omentum and intestine are returned, the portion of integument and sack which is left, falls over the opening at the umbilicus, covers it, and unites to its edge, and thus lessens the risk of peritoneal inflammation, by more readily closing the wound.

In Gibson, page 128, we find the following: "Strangulated umbilical hernia very frequently proves fatal, as much from disorder of the intestinal function as from the strangulation. When the usual remedies fails, an operation should be resorted to. This may be done in the following way. An incision, several inches long, is made very cautiously through the integuments and superficial fascia, when the sack, if not absorbed, as it often is, will appear. Into this a small opening should be made, from which fluid in considerable quantity generally issues. The opening may then be enlarged, and a finger carried upwards between the omentum and intestine as high as the umbilical ring. Upon the finger a bistoury is next to be carried through the linea alba, to the extent of an inch, which, in most cases will relieve the stricture sufficiently to enable the operator without much difficulty to restore the parts to their former situation.

"Dr. Physick has proposed, in strangulated umbilical hernia, to make a crucial incision through the integuments, as far as the neck of the sack, then open the sack at its upper part to an extent sufficient to enable the operator to examine its contents, and reduce them, if possible, without dilating the umbilical ring. Should the latter expedient, however, become necessary, the stricture must be divided on the outside of the sack. After the omentum and the intestine are restored to the abdomen, a ligature should be drawn around the neck of the sack, with a view of closing the cavity and obviating peritoneal inflammation. The late Dr. Wistar once performed the operation with success. In the case of a Mrs. N., a very respectable Jewish lady, I performed a similar operation about fifteen years ago. The tumour, however, was as large as a child's head, and had been strangulated several days before I saw the patient, and, on this account, the operation did not succeed. The patient, too, was advanced in years, extremely corpulent, and had long suffered from derangement of the functions of the stomach and intestines. Under these circumstances, no operation, probably, would have answered the purpose, even if performed in the very commencement of strangulation."

In Lawrence, on ruptures, we find the following:

"The greatest practical writers have strongly represented the frequent fatality of the operation for strangulated exomphalos; and the results of my own experience coincide entirely with their statements. I have, indeed, operated successfully on a large intestinal exomphalos, containing several convolutions of small intestine, of a bright red colour, without any omentum, in a fat woman advanced in years; but the majority of cases, in which I have either operated myself, or seen the operation done by others, have ended fatally.

Perhaps this fatality may be in some degree explained by considering that the exomphalos is most frequently in fat gross subjects, unfavorable for operations; that general intestinal disorder either exists with rupture, or is speedily produced by it; and that irritation and inflammation are readily propogated to the stomach, which is close to the umbilicus."

In the same work we find the following: "The great fatality of the ordinary operation for exomphalos makes it advisable that we should employ every precaution calculated to diminish subsequent irritation and inflammation. Hence it would be proper to adopt, especially if the tumour exceed a moderate size, the mode of operating which is applicable to large inguinal herniæ; in which the tendon is divided without opening the sack; or the latter part is only cut sufficiently to allow the division of the structure. This will permit the return of the parts if they are not adherent; and if adhesion should have formed, the immediate cause of danger, the strangulation is removed. The approximation of the sides of the wound by sutures, or adhesive plaster, will prevent the occurrence of inflammation in the tumour. The practicability of this mode of operating in umbilical ruptures is fully proved by two cases recorded in the work of Sir A. Cooper, and the successful termination of both instances proved the judgment and sagacity which had suggested that peculiar treatment."

From the concluding remark of the foregoing paragraph it will be perceived, that great credit is awarded Sir A. Cooper for his success in operating without dividing the peritoneal sack.

Too much importance is no doubt attributed to wounding the peritoneum, and this opinion is well established by the recorded cases in Churchill, of Cæsarean operations, in which mothers were saved. We must look to the anatomy of the umbilical ring for the true explanation of the fatality attending operations for exomphalos; and I think we will learn, that to be more successful, we should apply our different means for reduction in quick succession, so that failing in all but the knife, we may resort to it the sooner.—If we introduce a finger into the umbilical ring, through which a protrusion which is reducible, has taken place, we will at once be convinced from the firmness and unyielding nature of its walls, that a strangulation occurring at this point, would necessarily result in a disorganization of the tissues much more rapidly, than when it takes place at the internal inguinal ring, which is the mouth of a sac (the tunica vaginalis communis) continuous with the fascia transversalis, a thin elastic membrane, capable of considerable distension under any circumstances.—Lawrence relates a case of a lady forty-eight years of age, terminating fatally in seventeen hours from the commencement of the strangulation.

Different methods have been given for the division of the stricture—such as dividing the linea alba downwards, to the extent of an inch; its division upwards has also been recommended, and Velpeau recommends, that several slight nicks be made at different points of the ring. Dividing the ring downwards to the extent of an inch, would make so large an opening as to present an obstacle in the way of a radical cure. In the case related in the first part of this paper, the ring was divided about a quarter of an inch at the lower part, and this division not being sufficient, a mere nick at the upper part relieved the stricture. In cases where much cutting would be necessary to relieve the stricture, the plan proposed by Velpeau, enables us to relieve the stricture, without augmenting much the size of the ring, as effectually, as by a single long incision in any direction.

The only cases, that I have been enabled to find recorded, in which this operatiou has been successfully performed, are two cases by Sir A. Cooper, one by Mr. Lawrence, one by Mr. Walker of Hurtsperpont, one by Velpeau.—All the operations mentioned, were upon females.—I have not been able to learn, whether the successful operation mentioned before, by Dr. Wistar, was upon a male or female.

Part Second.

REVIEWS AND NOTICES OF NEW WORKS.

1.—*Elements of Physiology, including Physiological Anatomy, for the use of the Medical Student.* By WILLIAM B. CARPENTER, M. D., F.R.S., &c., with 180 illustrations. Philadelphia: Lea & Blanchard. pp. 566, 1846.

In this book there is so much to approve of, so much to object to, that it is impossible to do it justice within the limits to which we are restricted. We shall, therefore, confine ourselves to certain topics suggested during its perusal.

In the first place we feel bound to compliment the author upon the general scope of his work, but particularly upon his chapters on the vital stimuli. The four sections of his second chapter on Light, Heat, Electricity and Moisture, in their operations upon organic bodies, are valuable contributions to the profession. We know not where these topics have been handled so elaborately and so well.

"The fundamental idea of *Life*," according to Dr. Carpenter, "is that of a state of constant change or action; this change being manifested in at least two sets of operations; the continual withdrawal of certain elements from the inorganic world; and the incorporation of these with the peculiar structures termed organized, or the production from them of the germs that are hereafter to accomplish this. As the *conditions* of this continual change, we recognize the necessity of an *organized structure* on the one hand, or of a germ which is capable of becoming so; whilst we also perceive the necessity of a supply of certain kinds of matter from the inorganic world, capable of being combined into the materials of that structure, which may be designated as the *alimentary substances;* and, further, we see that the organism can exert no influence upon these, except with the assistance of certain other agencies, such as light, heat, &c., which are termed *vital stimuli.* This expression includes all the essential phenomena of *vegetative* or *organic* life; whether as witnessed in the lowest or highest members of the Vegetable kingdom; or as displayed in a large proportion of the structures composing the Animal fabric.

"But just as we find among Inorganic bodies, that various kinds are to be distinguished by their different properties, whilst all agree in the general or essential properties of matter, so do we find that living organized substances are distinguished by a variety of properties inherent in themselves, whilst they all agree in the foregoing general or essential characters. In many instances, the difference of their properties is as *obviously* coincident with differences in their structure and composition, as it usually is among the bodies of the Mineral world: thus we always find the property of Contractility on the application of a stimulus, restricted to a certain form of organized tissue, the Muscular; and we find that the property by which that stimulus is capable of being generated and conveyed to a distance, is restricted to another kind of tissue, the Nervous. In a great number of cases, however, very obvious differences in properties manifest themselves, when no perceptible variations exist, either in structure or composition; thus it would be impossible to distinguish the germ-cell of a Zoophyte from that of Man, by any difference in its aspect or composition; yet neither can be developed into any other form than that of the parent species, and they must be regarded, therefore, as essentially different in properties. In the same manner we shall find that, in the same organized fabric, there are very great varieties in the actions of its component cells, which indicate a similar variety in their properties; and yet they are to all appearance identical. But there can be no reasonable doubt, that differences really exist in such cases; though our means of observation are not such as to enable us to take cognizance of these, by the direct impressions they make upon our senses. Analogous instances are not wanting in the Mineral world; for the Chemist is familiar with a class of compounds designated *isomorphous*, in which, with perfect similarity in external form and physical properties, there is a difference, more or less complete, in chemical composition.

"Whatever may be the *peculiar* vital properties possessed by an organized tissue, we find that they are always dependent upon the maintenance of its characteristic structure and composition, by the nutritive operations of which we have spoken; and that they thus form a part, as it were, of the more general phenomena of its Life. They manifest themselves with the first complete development of the tissue; they are retained and exhibited so long as active nutritive changes are taking place in it; their manifestation is weakened or suspended if the nutritive operations be feebly exerted; and they depart altogether, whenever, by the cessation of those actions, and the uncompensated influence of ordinary Chemical forces, the structure begins to lose that normal composition and arrangement of parts, which constitutes its state of *organization*. Hence we may regard these peculiar properties as conformable, in all the essential conditions of their existence, with those more general properties, which have been previously dwelt upon as characterizing a living organized structure."

With regard to the hypothesis of a "*vital principle*" as *something superadded* to matter, we are pleased to see that the author has taken decisive ground "against it. The whole section, on the connection between vitality and organization," is most ably written, but we have only room for a short extract.

"Thus, then, we are at no loss to discover examples, in the Inorganic world, of an interchange of the sensible properties, both Chemical and Physical, of the bodies composing it, by a change in the conditions in which they are placed. And it may be stated as a general fact, that we never witness the manifestation of new properties in a substance, unless it has undergone some change in its own condition, of which altered state these properties are the necessary attendants. We have no right, therefore, to speak of any *property* as *distinct* from the matter which exhibits it; or as capable of being *superadded* to it, or *subtracted* from it. On the other hand we are led to the conception of *properties* as either *dormant* or *latent*, on the one hand; or as *active* or *sensible* on the other; the difference being entirely due to the condition of the substance. Thus, oxygen and hydrogen have a latent or dormant affinity for each other; this does not manifest itself in either of them, so long as they are separate; nor does it manifest itself at ordinary temperatures, when they are mingled together.

But if through such a mixture we transmit an electric spark, or if we raise the temperature of the smallest part of it by the contact of a heated body, or if we simply introduce into it a portion of platinum in a state of minute division, the requisite stimulus or excitation is given to these affinities, and chemical union of the two substances is the result.

"Now if we apply these views to the phenomena of Life and Organization, we see that they enable us to regard these phenomena as *analogous* in character to those of the Inorganic world, though not *identical* with them; and they lead to a simplification of our ideas of them, which more clearly marks out the path to be pursued in their investigation. We find that the essential materials of Animal and Vegetable structures are the four elements, Oxygen, Hydrogen, Carbon, and Nitrogen; these are distinguished by the extraordinary number and variety of the combinations into which they will enter,—so much so, indeed as to constitute, in this respect, a group quite distinct from all the other elementary substances. Now we are perfectly justified by what we elsewhere see, in attributing to these elements the property or dormant capability of exhibiting *vital* actions (in addition to the ordinary chemical ones with which we are familiar,) so soon as they are placed in the requisite conditions; in other words, as soon as they are made a part of the living system by the process of Organization.—It is only the peculiarity of the conditions required to manifest this capability, which prevents us from recognizing it as an ordinary property of matter, or at least of those forms of it, which we know by experience to be capable of entering into organized structures."

We are the more pleased that these views of life have been taken by so able an author, as we have been long convinced that if Physiology is to make any real progress, we must begin by driving out from the temple this mere phantom of the mind—this "old phlogiston of the chemists now dressed up in medical rags," to use the language of Liebig. What possible light can chemistry throw upon the functions of digestion, respiration, &c., or of the higher functions of nutrition and secretion, if all these acts be controlled by a mysterious power entirely distinct from, and superadded to the inherent properties of matter! To be sure this would be no good reason to reject a belief in the existence of such a power, were there any facts proving such an existence. But there are no such facts, and none of the arguments that we have seen urged in its behalf, will stand a logical examination. Most of them are *argumenta ab ignorantia.*

But one of the strangest delusions that has ever entered the mind of men, is, the opinion so frequently expressed by medical writers, that there can be a compromise in this question. "*In medias res tutissimus ibis*;"—a good precept in the practical affairs of life, but totally out of place in questions concerning natural science. A vital principle exists, or it does not exist. If it exist; then, as it is a power, it must exert a control over the molecular actions which occur in organic bodies, and these actions are no longer chemical actions, since the molecules are controlled by a higher power. If it do not exist, then the vital processes go on by virtue of the inherent powers wherewith the substances engaged in those actions are endowed. There is no escape from the dilemma.

One more extract, and we shall conclude our notice of this interesting work.

"All sciences have their "ultimate facts;" that is, facts for which no other cause can be assigned than the Will of the Creator. Thus, in physics, we cannot ascend above the fact of Attraction (which operates according to a simple and universal law) between all masses of matter; and in chemistry, we cannot rise beyond the fact of Affinity (limited by certain conditions which are not yet well understood) between the particles of different kinds of matter.—When we say that we have *explained* any phenomenon, we merely imply that we have traced its origin to these properties, and shown that it is a necessary result of the laws according to which they operate. For the existence of the properties, and the determination of the conditions, we can give no other reason than that the Creator *willed* them so to be ; and, in looking at the vast variety of phenomena to which they give rise, we cannot avoid being struck with the general harmony that exists amongst them, and the mutual dependence and adaptation that may be traced between them, when they are considered as portions of the general economy of Nature. There is no difference in this respect between Physiology and other sciences ; except that the number of these (apparently) ultimate facts is at present greater in physiology, than it is in other sciences, because we are not at present able to include them under any more general expression. Thus we find a certain peculiar endowment existing in one form of structure ; and another endowment, equally peculiar, inherent in another ; but we can give no reason why the structure called muscular, should possess contractility, and why the structure called nervous should be capable of generating and conveying the force which excites that contractility to action. Each of these facts, therefore, is for the present the limit to our knowledge ; we can ascertain the conditions, according to which the muscular contractility, and the exciting power of the nerve, are called into operation, and can form some estimate of the amount of the forces which they generate ; but we cannot see clearly that they are necessarily connected by any common tie, such as that which binds together the planetary masses, at the same time that it weighs down the bodies on the surface of the earth towards its centre."

In the above remarks there is a common but a very important and radical error. It consists in supposing that all unexplained phenomena are "ultimate facts." Many facts are inexplicable in the present state of science, but they are far from being "ultimate facts." "An ultimate fact" is one in which we see all, the circumstances attending it or, at least, suppose we do. When oxygen unites with hydrogen—when a stone falls to the earth after being thrown in the air—when a globule of mercury is drawn towards another globule and coalesces with it—we see and record all the circumstances of the phenomena, but cannot give a reason why. Explanation is out of place ; for explanation can only apply to compound, not simple phenomena. But muscular contractility, and the propagation of nervous force, are not simple phenomena, or in other words "ultimate facts"—though to the present time they may be unexplained facts. There are many motions going on to produce the grand result which, from their nature, escape our observation. This must be evident to any one who reflects that when he wills to move his hand or his foot, observation and experiment have proved that the molecular actions commence in the encephalon—that they are propagated along the nerves, for if the nerves be divided, no motion of the muscles will take place through volition. It is obvious, then, *that all the circumstances* in the case are *not* known, and that the muscular contraction is a compound phenomenon, and not a simple one. It might, therefore, had we the intermediate knowledge, be explained—for explanations of natural phenomena consist merely in the reduction of compound phenomena to simple ones in a connected order. Simple phenomena or "ultimate facts" cannot be explained.

In conclusion we may observe that this work is "for the use of the Medical Student," and to all who are really *students*----that is, who possess something more than the mere name, we recommend it. J. H.

II.—*A Practical Treatise on the Disease of Children.* By James Milman Coley, Member of the Royal College of Physicians in London, etc. etc. Philadelphia Barrington & Haswell—pp. 414, 1846.

Perhaps we are not wide of the truth when we assert that nearly all attempts to give a complete and satisfactory history of the diseases peculiar to children and the treatment they require, have signally failed; not only because writers have uniformly aimed to establish distinctions when there is in reality but little or no difference, but because disease, regarded as an entity, is essentially the same at every epoch of human life. Is not tubercle, pleuritis, gastritis, and the entire catalogue of *itises*, pathologically the same in the girl of ten as in the matron of forty years of age. It can not be denied that we have a small class of affections *peculiar* to the infantile state; but even these require for their treatment only a slight modification of those well established principles in therepeutics which are applicable to the diseases of mature age. Do not inflammations of the serous and mucous tissue call for the same treatment both in the infant and adult? The application of five or ten leeches in the first case may produce the same salutary results as four times that number in the instance of the second. But shall we on that account contend that the disease in the two instances cited, demands two separate chapters to explain its symptoms, pathology and treatment? Let it be remembered that the dose must always be graduated to the age &c. of the subject and the violence of the disease. To us, it would appear more rational and in accordance with the truth, to specify *only* those diseases peculiar to childhood; to inform us what organs are most liable to certain affections;—to signalize those morbid products peculiar to certain inflammations in the child. We know, for instance, that in the early period of life, inflammation of the air-passages is exceedingly prone to end in the production of a false membrane—arachnitis rapidly terminates in the effusion of serum—irritation of the gastro-enteric surfaces is rapidly reflected upon the cerebro-spinal centres, producing convulsions, spasms and other morbid phenomena.

Such, seems to us, should be the spirit, the scope and the aim of works designed to elucidate the diseases of infants.

From the greater relative size of the liver in early youth, it has been supposed that the preparations of mercury are much better borne by, and are far less injurious to the infant than the adult. It is certain that the former are with much more difficulty brought under the specific influence of mercury than the latter. But can this be ascribed to the cause above mentioned? May we not find a more satisfactory explanation in the greater activity of the capillary system of vessels, thus enabling the economy to expel from the system, through the kidneys, bowels and skin, the particles of mercury introduced into the blood.

Such are some of the most interesting subjects connected with the diseases of early youth; and to such and other topics of a kindred nature, writers on the diseases of children should restrict their remarks. On the diseases connected with dentition, much has been said by writers, and yet the subject has not been fairly set before the profession. It should be borne in mind that this is a physiological, and not a pathological process; to accomplish which, nature in her wisdom establishes a fluxuary movement of the fluids about the gums and neighboring glands. The consequence of which is heat, tension, redness, and considerable pain of the parts. But to prevent any serious consequences from this state of congestion, a free secretion from the glands and buccal membrane is soon established, which serves at once to deplete from the vascular system and soften the tissues covering the teeth. The physician should, therefore, be careful not to interfere with this physiological process, unless alarming complications, such as cerebral congestion, convulsions, etc. should supervene.

The too common practice of administering opiates and other cordial draughts to quiet that feverish and irritable state of the system attendant upon dentition, should be condemned in every instance. The application of a few leeches along the base of the jaw, or to the mastoid, or even to the alveolæ itself will remove the tension and relieve the pain much better than "Godfrey's Cordial" or other pectoral syrops. Mild cathartics, and a warm bath, the first to give a downward tendency to the fluids, and the second to sooth irritability and promote diaphoresis, will prove most excellent adjuvants.

We have thus far spoken without reference to any thing advanced by our author. He seems to have embraced in his work the entire list of diseases peculiar to children; yet we have been unable to find any thing that may be considered new; nor is this all: he has repeated much which has been better told again and again in other books on the same subject. Many of the so-called facts and speculations advanced by Dr. Coley might be shown to be incorrect. He contends, for example, that all tubercular deposit is the result of inflammation—a doctrine denied by some of the ablest pathologists of the day.

The book is readable, after all. A. H.

III.—1st. *United States Dissector, or Lessons on Practical Anatomy.* By B. W. E. Horner, M. D., Professor &c. 1846.

2d. *A Manual of Examinations upon the different Branches of Medicine.* By J. L. Ludlow, A. M. M. D. Second Edition, 1846.

Anything, emanating from the pen of Professor Horner on anatomical science, is sure to be at once clear and correct. For more than 20 years this valuable little work has been before the profession, and during that period, sustained the high reputation of its author as a great practical anatomist.—It is at the same time concise and simple in its arrangement, beginning with the alphabet of the science, and gradually leading the student onward to the more ample part of the study. Every student, who wishes to acquire a correct knowledge of Anatomy, should have a copy of this book spread before him on the dissecting table.

We are sorry, that we cannot speak so favourably of the 2nd work, the "*Manual of Examinations*," by Dr. Ludlow; it is a big book, though full of emptiness, and is not worth the paper on which it is printed, judging from the partial examination, which we have given to the work. By this, we do not mean to say, that the book does not contain some truth; but it is so garbled and put together, not giving the rationale of a single fact, or an explanation of any of the symptoms of disease, that we think its perusal will beget in the student's mind, a belief, that he has mastered the entire circle of medical science, whereas, in truth, he has but gathered a few pebbles of truth, gleaned a few isolated facts, which will only serve to inflate his vanity, without adding anything solid or substantial to his attainments. Perhaps the book is about as good as most works of the kind, but we are yet to learn, that the profession has gained anything by such publications; they lead the unprofessional public to suppose, that we have only to memorize a few questions and answers to become learned Doctors and skilful practitioners. Than such a supposition, nothing could be wider of the truth; still the interest of the profession would be promoted by disabusing the mind of the uninitiated of errors, so likely to fix themselves upon it.

Whence comes it, that Philadelphia is so prolific of such works? as if *she* alone enjoyed a monopoly of science,—as if *she* held the prescriptive right of transcribing medical truths from the Esculapian tablets and issuing them to the world, in the form of "Manuals," commentaries, appendixes, and annotations! That there is skill, learning, and a large share of that *esprit du corps*, so necessary to every profession, in our sister city, Philadelphia, we are ready to admit; but why will not the profession of that and other northern cities allow English and other foreign medical works to pass through their hands without disfiguring them with brackets, or encumbering their pages with trite and unimportant notes? Do they think, that the works of such men, as Sir B. Brodie, Williams, Watson, Liston, Stokes, M. M. Barth, Roget and others, require the inscription of their names, to give them currency and popularity in the United States! These authors may well exclaim against these modern Bathylluses—*hos nos libros fecimus, tulit alter honores;* they rear the monument, on which others seek to inscribe their names; they achieve all the labor, and others gather the honors. Truly, we are anxious to see the medical men of this country, begin to put forth their efforts in the right direction, and strive to add something to our science, worthy themselves and their country. We have looked to the old world, for improvements in medicine, until we have forgotten or overlooked the means in our hands, which, if properly employed, might enable us to shed much light upon the path of medical sciences.

We must apologize to Dr. Ludlow for this digression, and in doing so, advise him to quit his occupation of bookmaking, which, if profitable to himself, is the very worst book that can be placed in the hands of a student, and therefore as a Manual should be condemned.

A. H.

IV.—*Adulteration of various Substances used in Medicine and the Arts, with means of detecting them; intended as a Manual for the Physician, &c.* By Lewis C. Beck, M. D. Professor of Chemistry in Rutgers Colege, &c. New York; Samuel S. and William Wood, 1846. 12 mo. pp. 333.

The appearance of this work is really a most important event to the Medical profession in this country. In times past the physician could trust, to some extent at least, the safety of his patient, and his own reputation, to the apothecary from whom he bought his medicines or who put up his prescriptions. Things have, however, now undergone an important change; adulterations of Medicinal substances, are now practised to such an extent as to deprive therapeutics of much of that certainty which it promises when the medicinal articles employed are of the best quality. Hardly any article of the Materia Medica of much commercial value is now sold which may not be said to be generally adulterated. We would not charge apothecaries, as a class, with dishonesty, but there are obvious defects in the business of apothecaries and druggists, as well as in the laws regulating it, which must be remedied if physicians would retain the confidence of the public.

Every physician, in order to be sure of the genuineness and purity of the articles employed by him, in medicine, should possess the means of submitting them to those examinations and tests which alone can give him that assurance. He should be sure that he or his patients are not imposed upon by apothecaries; for he becomes, at least in a secondary manner, responsible for all such frauds, as it will be in vain for him to assert that any failure of success in his treatment is attributable to defection or bad qualities in the Medicines used, though he may possess proofs of the fact which are satisfactory to himself.

This work is full of evidence respecting the importance of some measures on the part of physicians to counteract the unfavorable influence which this extensive adulteration of Medicines must be destined to exert upon practice. The following example is given without selection, being the first which met our eye on opening the book at random, but it will answer well to illustrate the subject. All physicians are aware that hydrocyanic acid, though an important article in the Materia Medica, has been almost discarded from use in consequence of its variable strength, and in consequence of the welcome discovery of a substance (the cyanide of potassium) which was regarded as an equivalent and which had the advantage of being uniform in its strength. But it seems that in this case, as in all others, the value of the remedy is to be sacrificed or at least rendered uncertain by the frauds which are practised, and the amount of impurities which the article in commerce is discovered often to contain. The average of these impurities is 35 per cent, but it is often as high as 50 per cent.

The method proposed for determining the per centage of cyanide of potassium in any sample, is as follows: Dissolve a given quantity of the sample in distilled water, and then add a solution of nitrate of silver cautiously, until the precipitate formed ceases to be dissolved. This solution is evaporated to dryness, treated with muriatic acid, again evaporated to dryness and fused, to determine the quantity of silver. The amount of cyanide of potassium may then be known by the following calculation: As 108 is to 132, (two equivalents of cyanide of potassium,) so is the weight of silver obtained multiplied by 4, to the per centage of cyanide of potassium in the sample.

We will however give another example. It was only the other day that the scientific world hailed the announcement of Liebig, that quinoidine was only an uncrystallizable form of quinine, a fact of paramount importance now that this important substance is becoming scarce in trade. But hardly was the announcement made, that this substance was of any considerable commercial value, before adulterations were practised on an extensive scale, and the greatest uncertainty thrown over the value of the article as found in trade. We will quote the author's remarks respecting this substance.

QUINOIDINE.

"The substance known by this name, of which it is said a considerable quantity has accumulated in the process for procuring sulphate of quinine, has recently been shown by Liebig to be quinine in an amorphous state. It bears the same relation to ordinary quinine that uncrystallizable sugar (barley sugar) bears to crystalline sugar. It may therefore, when pure, be used in the same cases as quinine.

The commercial samples of quinoidine are often largely adulterated, and of course differ greatly in their value. The following tests of its purity are given by Liebig: Amorphous quinine is completely soluble in dilute sulphuric acid and in alcohol. It is also completely soluble in a solution of sulphate of copper with separation of oxide of copper. And if its solution in dilute acid yields, upon precipitation by means of ammonia, exactly the same amount of precipitate as the weight of the substance originally dissolved in the acid, there can be no doubt of the perfect purity of the sample under examination.—(*The Lancet, May*, 1846.)

To such physicians as desire to make such examinations readily and satisfactorily this work will de a great desideratum. To apothecaries it may be considered as an almost absolute requisite.

A full analysis of this volume would be of little value in a journal like ours, and would much exceed our limits. The eminent abilities of the author will, every where, be a sufficient guarantee of its merits, which are fully commensurate with the high expectations we had formed of the work before having fully examined it.

Each article of the Materia Medica, susceptible of adulteration is examined, the various means of adulteration enumerated, and the most approved methods for their detection given;—at the end of the work in the appendix, directions are given for the preparation and mode of using the re-agents and tests, together with the results derivable thereby.

W. M. C.

V.—*The Dublin Quarterly Journal of Medical Science;* consisting of Original Communications, Reviews, Retrospects, and Reports, including the latest discoveries in Medicine, Surgery, and the Collateral Sciences. New series. Vol. 1, Dublin: Hodges & Smith, 1846. p.p. 582.

It is with great pleasure that we acknowledge the receipt of this handsome volume, direct from the publishers, through the hands of Mr. J. B. Steel, bookseller of this city. It contains the February and May numbers, and is gotten up in a style worthy of its valuable contents.—In addition to this, we had the pleasure of receiving from the editor, through the same channel, the number for August. In these numbers we find valuable communications from the editor, (whose name, however, does not appear in any part of the journal,) Sir Philip Crampton, Drs. Graves, Stokes, Neligan, and others, who are well known to the profession. The Dublin Quarterly is one of the most valuable periodicals in the world—one that we would strongly recommend to those who wish to subscribe for a Foreign Medical Journal.

We take this occasion to say that we have found great difficulty in establishing an exchange with our contemporaries abroad. We have sent our Journal upon divers occasions to the leading Journals of London, Dublin, Edinburgh, and Paris, and have seen it noticed in several of them, yet they have not come to us regularly in exchange.

We had begun to think that our offer was declined, and therefore ceased sending. We are aware, however, that it is more difficult to forward from this place to Europe than from there here, and therefore hope we may yet succeed in establishing a regular exchange. If our foreign contemporaries are disposed to exchange with us, they will please indicate the best method of getting our Journal to them. Messrs. Wylie & Putnam, of New York, will take charge of any thing sent to us.

The editors of the Dublin Quarterly would confer a great favour upon us by a brief allusion to the subject. EDS.

VI.—*The Medical Formulary:* Being a Collection of Prescriptions, derived from the writings and practice of many of the most eminent physicians in Europe and America. To which is added an appendix, containing the usual dietetic preparations, and antidotes for poisons. The whole accompanied with a few brief Pharmaceutic and Medical Observations. By BENJAMIN ELLIS, M. D., late Professor of Materia Medica and Pharmacy in the Philadelphia College of Pharmacy. Eighth edition, with numerous additions. By SAMUEL GEORGE MORTON, M. D. Philadelphia: Lea & Blanchard, 1846, p.p. 272.

The title alone of this work so fully indicates the nature of its contents, that it is only necessary to announce the appearance of a new edition; and the number of editions that have been called for, affords ample evidence of the favour it has obtained with the medical profession. It is, indeed, an elegant formulary of such medicines as are in common use, and very convenient, especially for the young practitioner. The addition of Dr. Morton's name must give increased currency to the volume. We would advise all young practitioners to have Ellis's Formulary on their tables.

F.

VII.—*Corpus Luteum: Its value as evidence of Conception, and its relation to Legal Medicine, &c.*, By SAMUEL S. PURPLE, M. D. New York; J. & H. Langley, 1846.

The functions of the ovaria have excited a good deal of speculation of late years, and some new and peculiar views have been suggested in relation to their physiology. Among the most important of these is the notion, that at each monthly period an ovum matures and is expelled from the ovarium, leaving behind it a well defined yellow body, bearing a strong resemblance to the corpus luteum of pregnancy; yet differing from it in some striking particulars.

This difference it is of importance to establish not only as regards the interests of physiology but that of legal medicine—and Dr. Purple of New

York in the above pamphlet, has approximated closely toward this desirable end.

He gives us not only an admirable account of these yellow bodies, and the opinions of distinguished authority in relation to them — but also institutes a comparison between the *true* and *false*, in which their prominent points of difference are clearly exhibited.

They are, as follows: in the true corpus luteum the result of conception;—we find:

1. As to *external form*, an increase in the size of the ovary, when compared to its opposite, sometimes amounting to nearly one half, which seems like the addition of a portion to the substance of the ovary.

The prominence of a dark colour, varying from a deep red to a dark blue or purple—upon the summit on excavation, or depression, leading to an opening in a cavity below (this last is the appearance of the early stages,) and running over the surface a number of red and tortuous lines.

2. As to *size* and *form*. When cut into, varying according to the age from conception, generally from five to eight lines in length, being in form irregular, round or oval, and possessing a cavity or a stellated line or cicatrix.

3. As to *colour*, varying also according to the period examined, if previous to the third or fourth month, frequently of a reddish brown or light yellow—the centre lining the cavity of a dark red. If examined towards the close of gestation, the colour approaches a light or greyish yellow, having generally a white radiated or stelliform line of a pearl colour in the place of the cavity. We have noticed in one case at the full period a large cavity filled with a dark fluid, the cavity surrounded by the yellow matter about an eigth of an inch in thickness.

4. As to *structure*, which appears to be glandular, resembling somewhat a section of the human kidney, or that portion of the brain called by anatomists, *centrum ovale*. It is vascular, and capable of receiving a fine injection when thrown into the spermatic arteries.

These may be said to be the leading features or characteristics of a *true corpus luteum.*

Those of the false corpora lutea are;

1. The want of any elevation or enlargement of the ovary.
2. *The size*, which may vary, being from that of a pea down to the size of a millet seed, when it appears like a spot in the substance of the ovary.
3. *The colour*, which is often of a dirty, at other times of a light yellow or grey color. It wants the glandular appearance and the central cavity lined by smooth membrane.
4. In the fact of its not being capable of receiving an injection. Dr. Montgomery has given, as one of the characteristics, the absence of the external cicatrix. "The external cicatrix," says he, "is almost always wanting." This is the case in those which arise from an enlarged Graafian vesicle, and by the subsequent absorption of the fluid, producing a puckered or shrivelled appearance; but is not the case in those which arise from a ruptured Graafian vesicle during menstruation, *which is by far the most common form of false corpora lutea.*

Thus we see by the above quotation, that the essential points of difference between true and false *corpora lutea* are plainly established—yet as the subject is of the greatest importance and interest, particularly in a medico-legal point of view, it merits, we think, further investigation, which should be made with all that precision and good faith, which its importance demand. A. C.

VIII.—*Human Physiology*, with 368 illustrations, by Robley Dunglison, M. D., Professor of the Institute of Medicine, in Jefferson Medical College, Philadelphia, &c. Sixth Edition, greatly improved. In two volumes. Philadelphia, Lea & Blanchard; 1846.

We are sincerely glad, to see the interest taken in the study of Physiology of late years. It is the foundation of medical science, and that an increasing interest in this branch is taking place in the minds of the Medical Profession; the present publication is evidence enough. The first edition of this valuable work was published in 1832—the fifth in 1844, and in two years we have a sixth edition. A review of such a well known work would be out of place at the present time. We have looked over it, and find, what we knew would be the case, that Dr. Dunglison has kept pace with the science to which he has devoted so much study, and of which he is one of the living ornaments. We recommend the work to the medical student as a valuable text book, and to all inquirers into Natural Science, as one, which will well and delightfully repay perusal. J. H.

IX.—*Medical Education in the United States: An Address delivered to the Students of the Philadelphia Association for Medical Instruction, at the close of the Session of* 1846. By Alfred Stille, M. D. Lecturer on pathology and the practice of Medicine. (Pamphlet.)

This is a very elegant and able address, and one which we would like to see disseminated throughout our country. Dr. Stillé first shows the importance and necessity of "summer schools," which he says "were originally instituted for the very purpose of supplying the defects of the winter courses, the number and variety of which delivered in the short period of four months, were long ago found to embarrass and confuse the most attentive and intelligent students." He draws a forcible comparison, or rather, contrast between the standards of medical acquirement in Europe and America. He has the boldness and independence to hold the mirror up to truth, and to point out the defects and disabilities under which we labour; instead of flattering his countrymen, with sycophantic spirit, into the notion that *American physicians are superior to all others*, as was done by a distinguished professor in New York. That the medical profession in the United States, as a body, is sadly deficient in scientific attainment, and that its progressive improvement bears no sort of proportion to the rapid increase of its followers, are, we think, indisputable facts. If we cannot, or *will not* see our defects, how can we hope for amendment? The thing is preposterous! No one denies or doubts the fine native talent of our countrymen, but scientific knowledge is not to be obtained by *intuition.* It is the fruit of long years of study, and these we have not devoted to it. As Dr. Stillé justly remarks—"if a tree is to be known by its fruits, in the name of justice, nay, of common sense, what have we done to deserve the greatness which is thrust upon us?" Where are the evidences of our superiority? Dr. S. shows plainly enough that they are not to be found, and it is vain to flatter ourselves with the belief that we possess them. He points out some of the reforms called for by the present state of the profession in this country, and looks forward to the meeting of the National Medical Convention for their accomplishment. We sincerely hope he may not be disappointed.

Dr. Stillé maintains the following "propositions," which he says he should have considered self-evident but for the implied opposition they have received, viz: "that medical science, like every other branch of natural science, is a unit; that there cannot be one science of medicine for Europe, and another for America; that there is everywhere the same need for accomplished physicians; and that the same methods are requisite for producing them here, as experience has shown to be most effectual elsewhere." We assent to all these propositions, and believe they are generally admitted. Dr. S. thinks the Atlantic States should establish a standard of medical education for themselves, and that the Western States should do the same for themselves. We do hope that the whole country, north, south, east, and west, will be represented in the National Convention, which is to meet at Philadelphia next Spring, and that a general elevated standard will be agreed upon. Let those institutions which do not subscribe to it, be put down upon their proper level. We regret that our limits will not allow us to say more upon the subject of Dr. Stillé's address. It does credit to the author, who is a young professor of high promise.

E. D. F.

X.—*Introductory Lecture, read at the commencement of the Course.* By S. Henry Dickson, M. D., Professor of the Institutes and Practice of Medicine in the Medical College of the State of South Carolina, on the 5th of November, 1846. Published by the Class.

We thought we had completed our labours on this part of the present number, and had given all into the hands of the publisher, when we received by mail the Introductory Lecture of Professor Dickson of Charleston. We sat down to read it, and arose from the perusal with mingled admiration and delight. Never did we read an Introductory Address with so much satisfaction as we have that of Professor Dickson. We feel proud of him as a member of our profession, and more especially as a Southern teacher of medicine. He surely must have a mind that is bold, liberal, philosophical and well stored with experience and medical lore. Since reading this address, we feel an irrepressible desire to be in his presence, and hear lessons of wisdom from his lips. Our limits will only allow a few extracts.

After a brief salutation, he opens in the following eloquent style.

"We live in a day of active movement. All art and every science labour for prompt and definite results. Men are enthusiastically bent on making progress; and the recent successes attained in their eager efforts, are almost miraculous. Within the last half century steam has been tamed and harnessed to the car, and chained like a galley-slave to the oar; and the thunder-cloud not only disarmed of its dreaded bolt, but compelled to become our messenger and newsman. Time and space are thus literally annihilated, and the powers of mind rendered completely triumphant over the obstacles presented by grosser matter. The glowing sunbeam is arrested in its rapid flight; and shadows—proverbially transient—disposed into living pictures, images of inanimate nature and of the human face divine,—fixed and rendered more permanent than life and nature themselves. The decay of frail mortality is prevented, and the delicate structures of which our bodies are constituted, by some marvellous process of petrifaction reversing the fable of Pygmalion, hardened and converted into stone. Nor is it only in the physical world that such trophies as these are gathered. Right or wrong—for weal or for woe—the desire of improvement runs hastily forward into the experiment of change and substitution, and *reform* is the universal watchword of the masses, however divided or bound together. New laws, new governments, new constitutions are tried; new theories of instruction, of subsistence, and of punishment, are becoming prevalent; and in this ferment the conservative is confounded with the stationary laggard, and like him despised and trampled on.

When all is thus in motion around us, it is difficult, if not impossible, that we should stand still if we were inclined; and our profession, as you know, has been from the earliest times given to the desire of change and the hope of improvement. Medical reform is the order of the day; and those who know best the condition of things in medicine, whether considered as an art or a science, are most thoroughly convinced of the reasonableness of the demand, and the call for prompt compliance with it—the inevitable necessity of vigorous and earnest action. This will appear clearly, whether we regard our actual position internally and absolutely, or externally in our social relations to the community, and comparatively with other departments of learning."

Dr. D. mentions the respect and veneration, which the medical profession has commanded from remote antiquity, and speaks in feeling terms of the sad depreciation into which it has fallen in modern times. Like Dr. Stillé, he gives a true picture of its condition in our beloved country, and points out the defects under which it labours. We are highly pleased to find, that he gives his full approbation and support to the great objects sought to be accomplished by the National Medical Convention. In connection with this subject, he makes the following remarks respecting the comparative requirements from those, who devote themselves to the different professions and pursuits.

"In all other professions besides ours, the absolute necessity for protracted, careful and diligent training, is universally acknowledged. To every mechanical trade, an apprenticeship must be served; book-keeping does not come by inspiration; a formal examination, ordered by the Judge from the bench, and conducted by grave associates of the long robe, must precede admission to the bar; a pilot cannot attain without serious labour and assiduity, a regular branch; an engineer must rise by accumulating proofs of theoretical qualification and practical competency; the pulpit of every sect is guarded by custom, conventional arrangement and positive regulation. In compensation for these restraints, carefully defined by ordinance or universally recognized, society, by law, or by customs having all the force of law, confers on those who comply with the conditions thus imposed, certain privileges of specific value and established importance. An intruder is not permitted to deliver his drowsy common-places in the church; may not annoy his Honor in Court with unskilful and irrelelevant pleadings; or blow up a steam-boat, or locomotive, or run an unlucky vessel on a bar: these immunities are reserved for the formally initiated. But neither by law nor custom among us is any one prohibited or prevented from undertaking "to minister to the mind diseased," or the body imbecile, or in pain, or threatened with dissolution. "*Hic patet ingeniis campus*:" this field is open to all adventurers without restraint of time, or teaching, or character; and the result is as obvious in fact, as it was easily and clearly foreseen to be inevitable. Education and diligence, scholarship and learning, enjoying no privilege, are at a discount; they are rather impediments than advantages. They interfere with the intolerant and arrogant pride of republican equality, and are therefore regarded with suspicion and dislike. I say it with pain and shame, with the profoundest mortification and the keenest regret, the influence of these circumstances upon our noble but depressed profession is but too visible, and it is to be most seriously feared that the medical character, already impaired in some of these United States, is deteriorating in all. We are loudly called on for an effort to restore and maintain it. Conventions and conferences, and general assemblies may depose an unworthy brother from the clerical dignity—the bar may expel a discreditable member by a public and official act of great force and weight—but we have no such resource. Our local societies are destitute of power, because those who are fearful of their control keep aloof from them. A National Medical Association, however, may, if well organized, well officered, and well conducted, through its branches, or affiliated societies diffused over our wide and still expanding territory, exert a most beneficial influence, inevitable and irresistible. I trust that the organization of such a body may be effected at the next meeting of the Convention, and would fain hope that the countenance and aid of our government may, in some constitutional mode, be extended to it; perhaps through a recognized connection with the National Smithsonian Institute."

The following is the conclusion of this excellent Address.

"I am destined soon to pass away from the professional arena: my labors with my life are almost at an end. Whatever of honor and distinction was attainable, I have struggled for with all my energies; but I have always felt, that if I had in my hopes and aspirations, nothing more lofty or satisfying than the gratification of ambition, which Canning has pronounced to be "the only desirable thing in the world," the choice of my profession would have been an infinite mistake. We may win the personal respect, the cordial esteem and friendship, and the sincere gratitude of our patients, and the circle to which we may belong. These constitute the principal solace, the delightful reward of our unequalled sacrifices, our constant toil. Far from detracting from their value, I am not willing to allow that there exists a single individual to whose happiness they have more largely or essentially contributed. But I speak now of public consideration, of social position, of recognized weight, of distinction openly conferred. Nothing of this sort, in these United States, beckons forward or cheers the heart of the physician; there are for him no promises, no prospects of ultimate reward from the Commonwealth. Upon all her other children the republic smiles; for him alone she has not one single glance of official recognition or approval.

The soldier, the sailor, the legislator, the inventor, the architect, the sculptor, the painter, each obtains the civic honors which he deserves; and let him ever wear them without envy, or jealousy, or detraction. But no wreath is twined for the brow of the physician; his silent labors go unheeded and unrecorded; confounded by the neglect of the law with the empiric and the vender of nostrums, neither honor nor advancement in any shape are attainable by him, nor place, nor pension, render less wretched the decline of life or the incapacity of old age.

I know how unpalatable the opinion that government should take charge of matters of science, and am well aware of the violent predominance in many portions of our republic, if not throughout all its borders, of the absurd and paradoxical notion, that "the best government is that which governs least;" but I have no fear or reluctance at being in a minority, and as long as I can speak at all, will denounce error wherever I meet with it. Contrast the course of the more enlightened nations of Europe in this matter with ours, and look at the results, and we can hardly fail to blush for the difference presented in the two pictures. In those great centres of civilization, an honorable place in the body politic is assigned us. Our institutions are specially cared for, and protected by all such ordinances as our expert and aged brethren indicate to the constituted authorities. This is the fact every where in deep thinking Germany and intellectual Italy. In France—and where do science and art flourish as in France? —every thing is done to aid, to favor and to elevate us. A Medical Congress was lately held in that kingdom, at whose meetings the minister of public instruction—would to God that such a functionary had a place in our cabinet!—was a frequent attendant and an intelligent hearer of the discussions, debates and suggestions. All honor to M. Salvandy for the pure and lofty sentiments to which he gave utterance, while pledging himself that his government would exert its utmost ability to foster the divine art, whose advancement was the object of their assembling together.

Where but among Frenchmen could the following scene have occurred, most forcibly and graphically described by Pariset, in his Eloge de Larrey, lately read before the Academie Royale de Medicine de Paris. During the retreat from Moscow, as is well known, whole regiments, entire battalions of the wretched remains of the army sunk exhausted, blackening with their corpses the glittering surface of the desolate plains of snow, which they traversed. In this disorganized crowd of living spectres, all soldierly discipline was lost. Pressed upon by a cruel, revengeful, and remorseless foe, and perishing with cold, hunger, terror and disease, they approach a river. Two bridges are thrown across it, but afford very inadequate passage to the immense throng urged forward by the cannon, lances, swords and bayonets of the enemy. In the midst of the tumult, afar on the advancing wave of human bodies, they distinguish Larrey! and even in this desperate struggle, a thousand cries are heard. "Save him who has saved us! Let him come! Let him approach!" The crowd opens, Larrey is passed over the bridge by the soldiery from hand to hand; he is saved, and almost immediately the overburdened structures yield, and are crushed under the weight which loads them, carrying down in their ruin, men, women, children, soldiers, horses, and "all pomp and circumstance of war."

In England also, there is at this time, as in France and here, much agitation and great anxiety for the improvement of the Medical Institutions, and two or three different bills for the better regulation and general elevation of the profession are before Parliament. And there also the ministry are under promise to carry into effect, any arrangements that may be pointed out and agreed on by the professional masses of the kingdom, as likely to exert that tendency.

In France, the Peerage is adorned by its bestowal upon Physicians and Surgeons of eminence, and portraits of these, her great men, decorate her national galleries with those of her statesmen, her warriors, and her men of learning, thus proving her claim to the true glory of the highest civilization, by rewarding alike the acts of peace, and benevolence, and preservation, with those of force, devastation, and destruction. In England, the aristocratic sentiment of her haughty nobility yields with far less readiness a place to science; yet, title and rewards are sometimes conferred upon a favorite. What is vastly more important, laudable, and impressive, however, some of the niches in the magnificent Parliament house, now rising from the ashes of old St. Stephens, are reserved for the statues of Harvey, Hunter, and Jenner.

Let such names and examples as these, gentlemen, always excite and stimulate you; and although I cannot promise you titles, places, public honors or distinctions as the rewards of your future exertions; yet, if these are wanting in our humble sphere, we may well content ourselves with the far higher gratifications derived from the consciousness of duties fulfilled, of good actions performed from benevolent motives; with the recollections of a well spent life, the filial trust in an approving God, and the confident hope of happiness beyond the grave.

This is the kind of men to raise the Medical Profession from its present deteriorated condition in our country; and now is the time to put their shoulders to the wheel. The *ball is in motion*, it matters not how started; and it must not be suffered to roll backwards.

E. D. F.

Part Third

EXCERPTA.

1.—*Rapport a l'Academie Royale de Medécine sur la Peste et les Quarantaines, fait, au nom d'une Commission*, par M. le Dr. *Prus;* accompagné de Pieces et Documents, et suivi de la Discussion dans le Sein de l'Academie.—I. et II. Parties. 8vo. pp. 663. Paris, 1846. Bailliere.

2.—*Correspondence respecting the Quarantine Laws, since the Correspondence last presented to Parliament.* Presented by Command to the House of Commons, in pursuance of their Address of May 19, 1846. Folio, pp. 48.

[From the Medico-Chirurgical Review.]

It must surely be quite unnecessary to say a single word in the way of soliciting our readers' patient and most attentive consideration of the facts and reasonings, which we are about to bring under their notice. The subject of the Quarantine Laws is one of public and very general interest. All persons are more or less immediately concerned in their operation and effects; for whatever interferes with the free and unrestrained intercourse of one nation with another, cannot fail to affect the common welfare. To the medical man the subject is, as a matter of course, doubly and trebly interesting; some of the most curious and important questions, connected with the natural history of epidemic diseases, are involved in its right adjustment. It is to medical doctrines and to medical opinions that we owe the present system of prohibitory restrictions, which so seriously interfere with the social comforts and commercial success of numerous countries; and therefore for this reason alone, if there was no other, it well becomes the members of our profession to be foremost in making a calm and candid examination of those doctrines and opinions from which such grave consequences have followed. Now, every one who has made himself acquainted with the subject, be he physician or merchant, traveller or statesman, has of late years, without exception, come to the decided conviction that it is high time for a thorough revision and a very material modification of the quarantine laws, such as they now exist, to take place. The absurdly foolish and most ridiculous principles which they embody, the vexatious and oppressive restrictions which they impose, the wretchedness and suffering which they almost necessarily give rise to, and the great increase of mortality which, we have reason to believe, they often occasion, are surely sufficient grounds for the scrutinizing investigation that is so generally demanded. For some years past, the British Government and that of France have been using their best exertions to effect a change, and have been trying to get the other Continental powers to co-operate with them in their good work. With this view, they have proposed that a Congress of Delegates from the different States of Europe should be held in Vienna, or elsewhere, for the purpose of agreeing upon some general and uniform system of Quarantine regulations to be adopted in the ports of the Mediterranean. Many difficulties, we regret to say, have been thrown in the way of this most equitable proposal by Prince

Metternich, on the part of Austria, and by the representatives of some of the minor powers.* Will it be believed that, in several of the Italian States, the quarantine boards of health are actually independent of the government?—they possess a sort of patent vested right in the profligate exaction of their fees of office! Still, we must not be discouraged, nor diverted from the reform that is so loudly called for. Truth will assuredly prevail in the long run, despite the opposition of ignorant bigotry on the one hand, and of the basest mercenary intolerance on the other. We have only to keep the subject prominently and steadily before the public mind, by collecting accurate and well-authenticated facts from every quarter, and by not ceasing to expose the enormous fallacies and absurdities which prevail, and we may be confident that, ere long, the system which has so long existed to the serious detriment of commerce and to the disgrace of common sense, not to talk of science and humanity, will be made to undergo such changes as the present state of general enlightenment and sound knowledge requires.

It will be seen, from the following pages, that neither the French nor the English government has at all relaxed in their efforts for this most desirable end. Both have been accumulating materials for information, and collecting the opinions of competent authorities, in order that they may be ready to meet the objections or overcome the prejudices of their opponents.

In August 1844, the Royal Academy of Medicine in France appointed a Commission to examine all the varied questions connected with the Plague and with Quarantines. This commission was composed of the following members—men, we may remark, of the highest professional and scientific attainments—MM. Adelon, Begin, Dubois (d'Amiens,) Dupuy, Ferrus, Londe, Melier, Pariset, Poiseuille, Prus, and Royer-Collard. M. Ferrus was named the president, and M. Prus the secretary and reporter. The Commissioners were engaged in their deliberations for upwards of twelve months, and had every facility granted them by the French government, to render their enquiry as complete and as accurate as possible. At length, the report was drawn up and read at the sittings of the Academy, on the 5th, 10th, 17th, and 24th of March and the 5th of May of the present year. It is certainly a very elaborate and instructive work, replete with most valuable facts and data, which cannot fail to be truly acceptable to every enquirer upon the great questions under consideration, whether he admits the soundness of the conclusions adopted by the majority of the Commission or not. It is for this reason that we have thought it right to bring before the attention of our readers, with as little delay as possible, a faithful summary of its contents, in order that they may be able to judge for themselves of the value of the original.

Although the Plague has so often ravaged the world and there has been no lack, as a matter of course, of books and memoirs published at different times upon the subject, it must be confessed that the number of instructive and really accurate narratives of well-observed *facts* is by no means very considerable.—The epidemics, of which we have the most trustworthy histories, are the following:—that of Nimeguen in 1635, described by Diemerbrock; that of London in 1665, described by Sydenham and Hodges; that of Marseilles in 1720, by Chicoyneau, Verney, Deidier and Bertrand; that of Transylvania in 1755, by Chenot; that of Moscow in 1771, by Mertens, Orrœus, and Samoilowitz; and those of Egypt in 1798, 1799 and 1800, which have been so well described in the writings of Desgenettes, Larrey, and Louis Frank.

But, however valuable the records of the epidemics now mentioned may be,

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* It is only doing justice to the Austrian government to state that it has already made some very useful practical reforms in its quarantine regulations. While France has been talking and planning, Austria, in imitation of the example set by this country, has been acting. Passengers from Alexandria can now reach Paris, *via* Trieste or Southampton, considerably sooner than by going to Marseilles, in consequence of their detention in the lazaretto there!

it must be admitted, we think, by all who have attentively studied the history of the plague, that it is only within the last ten or twelve years that we have anything like a positive and truly scientific acquaintance with the disease.—Dr. Aubert-Roche was the first to display that brave and generous devotion to humanity and science, which has since been followed by so many of his professional brethren, when he brought himself in direct contact with his friend Dr. Fourcade, who died of the plague at Cairo on the 20th of February, 1835.*

Shortly afterwards, numerous plague patients were received into the hospital of Esbekiè, at Cairo. Clot-Bey, anxious to give the most complete authenticity to the observations which might be made of these cases, proposed to MM. Gaetani, Lacheze, and Bulard to join with him in forming a committee or board for the purpose of attending together upon all the patients in the successive stages of the disease, and of making *post-mortem* examinations. These four gentlemen carried through this task with the greatest zeal and devotedness.—The infected were waited upon like other patients; they were freely touched whenever there was occasion to do anything for their relief, or for the investigation of their symptoms. The bodies of those who died were taken to the dissecting amphitheatre, and every organ was most attentively inspected. The results of each visit in common were carefully reported in a register, and each report was regularly signed by all four. The register, (which was submitted to the perusal of the government Commission) is the chief basis of the works, which have been published by Clot-Bey† and Bulard.

Subsequently to these researches, the professors of the medical schools at Abouzabel (about four leagues from Cairo) personally attended upon 140 plague patients, of whom 38 died. Professor Perron has communicated a report of the observations and *post-mortem* examinations then made, in a memoir which he addressed to the Academy.

Drs. Aubert-Roche and Rigaud, attached to the great hospital at Alexandria, displayed no less courage and disinterestedness in their enquiries. The latter gentleman died of the plague, leaving behind him an account of 68 dissections which he had made of fatal cases.‡ The former has published an account of his observations, collected either by himself or in conjunction with his lamented colleague.

The conduct of M. Lesseps, the French consul-general at Alexandria, has been the theme of universal admiration. By his own example, he powerfully contributed to dissipate the exaggerated apprehensions of visiting and even touching plague-patients. His conduct towards Dr. Rigaud, up to the last moment of his friend's life, was a memorable instance of noble generosity.

Since 1835, the medical men resident in Egypt have continued their efforts to render our knowledge of the plague more and more complete. In 1837, an epidemic broke out at Adana, in the corps of the Egyptian army that then occupied Syria. In 1841, Damietta, Cairo, and a number of the towns or villages in the Delta were visited by the pestilence. It is also to be remembered that not a year has passed since the great epidemic of 1835, without a greater or less number of sporadic cases occurring every now and then in different parts of Lower Egypt.

But the plague has been studied of recent years in other countries besides

* De la peste et du typhus d'Orient. Paris 1840, p. 90.

† This indefatigable person has sent no fewer than 50 memoirs, at different times, on the subject of the plague to the French Academy. Many of these memoirs, written by able men who have had ample opportunities of studying the disease, well deserve to be published.

‡ In the very valuable pamphlet on Oriental Plague and Quarantines, published by Dr. Bowring in 1838, it is stated that Dr. Rigaud, after having been, during the most fearful crisis of the pestilence (1835,) constantly engaged in visiting and assisting the living or in dissecting the dead, at length fell a sacrifice "just as the plague was ceasing, when its violence appeared wholly exhausted, and the season of its disappearance was about to arrive."

Egypt. To confine our notice to modern works only, we may mention Dr. Brayer's *Neuf années à Constantinople;* Dr. Gosse's account of the plague in Greece during 1828 and 1829; and the reports of Dr. Morea on the plague of Noja in 1817, and of M. Hemso on that of Morocco in 1818.

M. de Segur du Peyron, although not a physician, has rendered great services to medicine by the publication of the three reports which he addressed, in the years 1834, 1839, and 1846, to the minister of commerce, and which contain a great mass of observations collected by him in the principal ports of the Mediterranean.

Lastly, the Academy has received a memoir on the plague and quarantine, published in 1845 by Dr. Moulon, physician of the lazaretto at Trieste; and also a printed report on the transmission of the plague and the yellow fever, that was drawn up by a committee of the medical society of Marseilles, and unanimously approved of and adopted in August 1845.

Besides the published works above enumerated, a number of very valuable manuscript documents have been submitted to the examination of the Commissioners.

Among these, we may mention the original papers respecting all the cases of plague that have occurred in the lazaretto of Marseilles since 1720, along with a letter and memoir from Dr. Robert, one of the physicians of this lazaretto;—the register kept in Egypt and Syria, during the years 1828, 1829 and 1830, by the plague commission, of which M. Pariset was the president;—the report addressed in 1842 to the minister of commerce, by Dr. Delaporte of his mission to Constantinople, Smyrna, and Alexandria, for the purpose of studying the plague in these places;—a statistical statement of 506 epidemics of the plague drawn up by Dr. Rossi of Cairo, who, like Dr. Delaporte, nearly fell a sacrifice to an attack of the pestilence;—the statistic report of all the cases of plague observed in the lazaretto of Alexandria since 1835, by Dr. Grassi, who has been physician of that establishment since 1831;—a memoir on the plague in Persia by Dr. Lacheze;—one on the plague in Algeria, from the year 1552 down to 1819, by M. Berbrugger, corresponding member of the Institute, and conservator of the library and museum of Algiers;—a memoir on the contagiousness of the plague by MM. Pezzoni, Leval, and Marchand, members of the council of health of the Ottoman empire, dated June, 1842;—and lastly, a memoir on the antiquity and endemicity of the plague in the East, and especially in Egypt, by Dr. Daremberg, the learned librarian of the French Academy.

In addition to these numerous sources of information, the Minister of Foreign Affairs granted to M. Prus the privilege of consulting the dispatches of the French Ambassadors and consuls in the Levant on all topics connected with his enquiries. The dispatches of M. Lesseps, (to whom we have already alluded,) during the frightful epidemic of 1835 in Egypt, were found to be especially valuable. The Minister of Marine also put all the official documents under his control at the free disposal of the Commissioners.

With the view of rendering their enquiry as complete and comprehensive as possible, the Commissioners invited to their meetings the attendance of medical men and others, who might feel inclined to give any verbal communication. In this way, they received much valuable and interesting matter.

The Report is divided into four parts or sections.

In the *first*, the following points are examined and determined:—the countries were the plague has been observed to become spontaneously developed;—the cause of spontaneous plague;—the disappearance of the plague, whenever these causes have ceased to exist;—the countries where the persistance of these causes renders the plague endemic, or at least makes the return of the spontaneous disease to be apprehended—and, lastly, the measures that are really and truly prophylactic against spontaneous plague.

In the *second* part, the three following questions are answered:—1. Has the plague always exhibited the characteristic features of epidemic diseases, whenever it has raged in Africa, Asia, and Europe? 2. What are the distinctive characters between epidemic and sporadic plague? 3. Does the plague spread after the manner of epidemic diseases; *i. e.* by the migration of certain atmospheric influences, and independently of the agency of those persons who are infected by it?

In the *third* part, the important question as to the transmissibility of the plague from one individual to another is examined. Is the disease transmissible by inoculation? Is it transmissible away from, as well as in, epidemic *foci* by immediate contact with the sick? by the contact of clothes, furniture, or merchandise? or by miasms exhaled from the bodies of the sick, and diffused through the atmosphere? This part closes with an examination of the following three questions. 1. Can persons affected with sporadic plague occasion *foci* of infection sufficiently active for the transmission of the disease? 2. Is the plague more or less readily transmissible, in proportion to the intensity of the epidemic; according as the disease is in its first, its second, or its third period; and, lastly, according to the oaganic susceptibilities of those who are exposed to the action of the pestilential miasm? 3. If the plague be transmissible away from epidemic *foci*, are there any grounds to apprehend that the importation of a few cases into France might occasion a pestilential epidemic?

In the *fourth* and last part, the question as to the ordinary or exceptional duration of the incubation of the plague is discussed. The general conclusions of the Report, and the application of these conclusions to the important subject of Quarantine are appended to this part.

First Part.

Chap. 1.—*What is the country, or what are the countries, where the Plague has been observed to arise spontaneously?*

In attempting to trace back the history of the plague, with the view of throwing some light upon this question, it would be little profitable to carry our researches beyond the sixth century of the Christian æra, as there is too good reason to believe that the terms λοιμος and *pestis* were previously used in a generic sense, to denote all epidemic diseases which caused great mortality. The "boils breaking forth with blains upon man and beast," recorded by Moses, need scarcely to be alluded to. The famous plague of Athens, so graphically described by Thucydides, is supposed by the best authorities to have been a malignant form of typhus, complicated with a peculiar eruption and with gangrenous eschars. The Greek historian says that it was believed in this day that the pestilence had been imported from Egypt into the Piræus.

Whether we are to admit the genuineness of the passage from Rufus of Ephesus, a celebrated physician in the time of Trajan—discovered by Cardinal Angelo Mai, at Rome, in 1831, in the writings of Oribasius, who lived in the time of the Emperor Julian—is doubted by some learned enquirers; by M. Pariset, the accomplished secretary of the Academy, among the number. The passage indeed contains a remarkably accurate description of the characteristic symptoms of the plague,* and the writer refers to epidemics of the disease in Egypt, Syria, and Libya, mentioned by Dioscrides, Posidonius, and Dionysius, who (are supposed to have) lived two or three centuries before the Christian æra. It may be worthy of notice here that the writings of Cicero, Strabo,

* The following is one of several paragraphs that might be quoted:—

"A pestilential carbuncle is that which is accompanied with a severe inflammation, with acute pain, and delirium. In many of those who were affected with it, there occur also hard and painful bubos, and the patients soon die of these carbuncles. This is the case more especially with those who live in the neighborhood of marshes."

and Pliny afford evidence that Egypt was regarded, in their time, as a country that was fertile in the plague. There are allusions too in the works of Galen and Aretæus, not to mention Hippocrates, that would seem to indicate their acquaintance with malignant fevers accompanied with bubos and carbuncles.

But, without dwelling longer on the uncertain history of the plague, we shall at once come down to the year 542 of the Christian æra, when that terrible epidemic, the description of which by Procopious and Evagrius can leave no doubt as to the true nature of the disease, ravaged the city of Constantinople. From this period, the appellation has been very generally restricted to that form of fever that is accompanied with bubos, carbuncles, and petechiæ. If we are to believe that from the 6th to the 16th century the term "plague" has been properly applied, we are surely justified in assuming that, from the beginning of the 16th century—that is to say, subsequently to the establishment of lazarettos in Europe—this word has only been employed in its right acceptation.

In the 16th century, there was (as far as we know) but one epidemic of plague in Egypt, and we find no mention of any in Turkey (in Asia,) or in Syria; whereas, in the course of this century, there were no fewer than fourteen invasions of the pestilence in France, twelve in Germany, eleven in Italy, nine in Dalmatia, six in Turkey (in Europe,) five in England, five in Spain, two in Portugal, two in Poland, two in Belgium, and one in Switzerland.

In the 17th century, we have the account of but two invasions in Egypt, and of not one in Turkey (in Asia) or in Syria; whereas there were nineteen in Germany, eleven in Italy, eleven in France, six in England, five in Russia, four in Turkey (in Europe,) three in Spain, two in Holland, two in Switzerland, two in Denmark, one in Sweden and one in Poland.

It seems, therefore, impossible that any one, who will take the trouble of comparing these figures (always supposing that they can be depended upon—*Rev.*) should not be struck with this remarkable circumstance; to wit, that the plague has repeatedly and most destructively made its appearance in many points or localities in the world, more especially in Europe, at various epochs when either it did not exist, or was only very rare, in Egypt. If such has been the case, we are surely bound to admit that the disease has often arisen spontaneously in other countries, besides in Egypt, Turkey, and Syria. This is the view which M. Littré has taken; for he observes (article *Peste* in the Dictionarie de Medecine) that the "plague was very frequent in Europe during the 16th and 17th centuries. Italy, France, England, Holland and Germany were attacked by this pestilence; and Paris and London witnessed it spring up in the midst of them just as Cairo and Constantinople now do."

In the 18th century, epidemic plague occurred nineteen times in Egypt, seven times in Turkey (in Europe,) four times in Dalmatia, four times in Germany, thrice in Russia, thrice in Spain, twice in Poland, twice in Greece, once in Italy, once in Sweden, and once in France, viz. when Provence and Marseilles suffered so severely in the years 1720 and 1721.

In the course of the present century, the epidemic plague has broken out eight times in Egypt, six times in Turkey (in Europe,) thrice in Greece, twice in Syria, twice in Italy, twice in Russia, once in Turkey (in Asia,) once in Germany, once in Dalmatia, and once in Morocco.

The statistical data which we have given, more especially those which have reference to the 16th and 17th centuries, appear to prove most incontrovertibly that the plague has arisen spontaneously, at certain periods, in very many of the countries of Europe and Asia. Our convictions upon this point will be much strengthened, if they are found to be supported by facts observed in our own day.

Dr. Lacheze, in the account of his recent travels through Persia, informs us that the plague had been repeatedly observed to arise spontaneously in several of the cities of Asia Minor, and particularly at Erzeroum, situated near the northern source of the Euphrates and about five days' journey from Trebisond ; a statement which has been amply confirmed by the report of the Turkish council of health, that was established in the year 1838. The same remark may be made respecting Aleppo.

There are numerous facts also that seem to prove that the plague is apt to appear spontaneously upon the banks of the Danube, as it does on those of the Nile and the Euphrates.

The Russian army in 1828, while engaged in war with the Turks in Moldavia, Wallachia, and Bulgaria, was attacked with a very malignant fever that was accompanied with bubos in the groins and axillæ. Dr. Witt, principal physician of this army, while he has acknowledged that the fever resembled in every respect the true plague, gave it, however, as his opinion that it must be distinguished from this disease, because it arose on the banks of the Danube, independently of any importation from abroad! Dr. Schlegel, who had been sent by the Russian government, before the arrival of Dr. Witt in Wallachi, to determine the nature of the epidemic admitted that, although it showed great affinity to the plague, it differed from the latter in being attributable in that country to putrid emanations containing mephitic gas! On the other hand, Professor Seidlitz of Petersburg did not hesitate to regard the fever as genuine oriential plague.* If it was really so—and surely there is no good reason to think otherwise—we have the authority of both Dr. Witt and Dr. Schlegel that the disease was truly endemic and of spontaneous origin in the localities were it prevailed.

Although the plague may therefore arise spontaneously in a number of different localities, it is no doubt true that, in recent times, Egypt, Syria, and Constantinople—more especially the first—have been the principle *foci* of the disease. It is right here to mention that, since the year 1839, it appears that there has been no case of plague observed in Constantinople. The board of health of that city attributes this exemption altogether to the quarantine measures, that have been adopted of late years. May such be the truth ! but let it not be forgotten that, before the terrible epidemic of 1812, not one case of the disease had occurred in that immense city for eight years ;—a fact that is proved by the registers of the French embassy at the Sublime Porte.

Syria also appears, according to the testimony of Mr. Lander, the English Consul at the Dardanelles, and of M. Beclard, the French Consul at Smyrna, to have been completely exempt from the plague since the same period (1839.) Dr. Lasperanza, attached to the Constantinople board of health, informs us that, in consequence of various sanitary improvements that have of late years been introduced, the disease has ceased to be endemic in Jaffa, as well as in other parts of Syria. In the present day, it is almost exclusively from Egypt that the importation of the plague may be apprehended.

The general *conclusion* from all that has now been stated is that—

"The plague has been observed to arise spontaneously, not only in Egypt, Syria, and Turkey, but also in many other countries of Africa, Asia and Europe."

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* This gentleman has shown that, whenever in times past the Russians have carried on war against the Turks on the banks of the Danube and on the coast of the Black Sea, their armies have almost invariably suffered from the plague. In consequence of this fact, and other considerations to be afterwards mentioned, he does not hesitate to affirm that, in these cases, the plague is to be viewed as only the worst form of the endemic fever of the country.

CHAP. II.—*In countries where the spontaneous plague has been observed, can the development of the disease be reasonably attributed to any determinate hygienic conditions?*

To solve this question, the Commissioners examined with great care the histories of the various localities in which the plague has arisen spontaneously, within the last fifty years. And first with respect to Egypt. Now the most competent observers assure us that there is nothing in the mere climate of this rich, and, in many respects, highly favoured country that will account for the generation of the pestilence; indeed, travellers have written in the most glowing terms of its beauty and salubrity. The year in Egypt may be divided into three periods or seasons. The *first* commences in August and ends with October; it is the period of the inundation of the Nile. The *second* comprises the next six months, from November to April; it is the season of the winter harvests, the ground being covered with trefoil, wheat, barley, flax, &c. The *third* begins in May, and terminates in August or September; it is the time for the cultivation of cotton, indigo and rice. As we have already said, the natural climate of Egypt is on the whole a very salubrious one. Its drawbacks are but few; the chief being the coolness and humidity of the nights, the frequent and rapid variations of temperature in the day, the rains and fogs of the Delta during the winter months, the great heat and excessive dust in summer, and, lastly, the singular effects of the South wind, the *Kamsin*, upon the living body. Whence then comes the pestiferous atmosphere of some parts of this land? The answer is ready; man himself has given it birth; the inhabitant of the Delta, says M. Hamont,* who long resided in Egypt, has prepared the causes of his own destruction. The destitution, filth and misery of the poor inhabitants are extreme. Their wretched hovels are so horribly disgusting as almost to defy description; they are not only surrounded by, but are actually receptacles of, heaps of ordure and putrid matters. Not unfrequently the dead are buried immediately under the mud floors of these dwellings of the living; and many of the graves in the cemeteries (which are always within the villages), being left open, are continually exhaling a stench that is utterly intolerable to any stranger. Then, again, the food of the Fellah is always of the worst description, and often too of the most scanty supply. Rotten cheese, decayed vegetables, semi-putrid flesh or fish; such are the articles that he lives upon. The very water that he drinks is filthy and impure. And then think of his mental and moral condition; the brutish degradatiou of all his faculties and affections, his hopeless servitude, his blank unmitigated wretchedness.

The hygienic state of the cities and larger towns in Egypt is not much better than that of the villages. Cairo, with its 200,000 inhabitants, is a very hot-bed of the most disgusting and pestiferous impurities. From the canal, which traverses it, there is constantly steaming forth a cloud of intolerable offensiveness; and yet this is the supply of water for the use of its people! There are no fewer than 35 cemeteries, of which 25 are within its walls. In the Copt quarter of the town, the dead are buried under the floors of the houses; and nothing but a few boards separate the living from the putrid bodies of the deceased. From 80 to 90 corpses have been known to be huddled together in these horrible *sub-domal* receptacles. Can we therefore wonder that Cairo should be a generating focus of pestilential disease?

That the circumstances now mentioned must tend to promote the development, and aggravate the intensity of the plague, will be disputed by none; but then the question comes to be, are they sufficient to produce or originate it? This thing is certain, that the disease has never been known to appear spontaneously in Egypt, except in places and seasons when these most pernicious agencies were at work.

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* Destruction de la peste et des quarantaines. (Bulletin de l'Academie Royale de Medicine—Paris, 1844, t. x. p. 40.)

The plague does not arise in Upper Egypt, Nubia, and Abyssinia; nor does it ever extend above the first cataract of the Nile. The good quality of the soil, the ready efflux of the waters, the small number of the inhabitants, and the strong currents and agitations of the atmosphere appear entirely to counteract the morbid influence of the mode of life followed by the inhabitants.*

We are informed by Gaetani Bay†—first physician to Mehemet Ali, and who has resided in Egypt for the last 25 years—that the plague never extends beyond Assuan, in consequence of the difference in the situation, heat, dryness, and nature of the soil; whereas it readily finds its way into the localities where there is much stagnant water. It is for this reason that Bagdad and Bussorah are in the present day subject to invasions of the pestilence, from which they were formerly exempt when effective police regulations were in force in these towns.

The seasons exert a no less marked influence on the development of the plague. The dry heat of what is called in Egypt the second summer, the prevalence of the northerly wind that usually sets in about the summer solstice, and the first dews that commence about this time, change alike the condition of the atmosphere and the organic aptitudes or susceptibilities: the pestilence ceases.

What has been now said respecting the artificial insalubrity of Egypt, arising from man's own negligence and vice, is nearly quite as applicable to Constantinople as it is to Cairo. The filth of some of its environs is altogether intolerable and disgusting. It is usually in the month of July, when the north or tramontane wind ceases and is succeeded by a southerly sirocco, that the pestilence makes its first appearance. As a matter of course, the putrefaction of all organic matters goes on much more actively at that season, in consequence of the high heat on the one hand, and the moist relaxing influence of the wind on the other. The localities that are first attacked, are those which are chiefly occupied by the poor Greeks and Jews: hence the village of San Dimitri is usually the place where the earliest cases are observed.

We need scarcely say that if Constantinople be bad, Erzeroum is much worse, in everything that respects hygienic salubrity. Fortunately the frequent severity of the winter season there, as well as the high winds that prevail in Armenia, tend much to attenuate the existing causes of the plague.

If from the Euphrates we pass to the Danube, we shall find the same causes of endemic insalubrity prevailing in those localities, where the pestilence has been known to arise. The poorer classes in Moldavia and Wallachia live in the greatest misery and filth. After the heats of summer, almost all the prevailing diseases assume a character of marked gravity. Malignant intermittent fevers are always more or less prevalent in autumn; these generally precede the appearance of the plague, which in these countries is usually only sporadic. Professor Seidlitz has endeavoured, as we have already seen, to establish the intimate connection between these two forms of febrile disease.

Dr. Mirolanof, who treated the plague at Achial in 1828, says that "the soldiers and officers who had the intermittent fever, were affected with bubos and carbuncles. In the month of September the plague shewed itself especially in those who were convalescent from agues, and assumed the form of a tertian fever. The bubos appeared after the first or second paroxysm."

Dr. Rinx, who was at Adrianople during the whole course of the epidemic, remarks of the third degree of the epidemic that "the least severe degree of the plague so much resembled an intermittent fever that it was scarcely possible to distinguish the one from the other, before the appearance of the bubos."

From all these various facts, it is abundantly obvious that the hygienic condition of the four distinct localities, in which the plague has of recent years

* Pariset, Causes de la peste.—Paris, 1837.

† Sulla peste che afflisse l'Egitto, l'anno 1835. Napoli 1841.

broke out spontaneously, is very nearly the same. It is a circumstance, too, of no trifling import that wherever the producing causes of the disease are most abundant and concentrated, there it is always most severe and most readily propagable. The form most dreaded is that which appears in Egypt; next comes that of Constantinople, and after this that of Erzeroum; while that of the Danube, which has hitherto been generally regarded as of Constantinople growth, has not yet been sufficiently studied to enable us to decide respecting its relative severity.

Is it not also a remarkable fact that the four geographical points or localities, now mentioned, are all subject to malignant intermittent and other fevers? Are we to believe, with MM. Begin and Boudin, that the plague belongs to the family of marsh fevers? There are many circumstances certainly which seem to militate in favour of this opinion. Without dwelling on the geographical condition of Syria and other plague countries in the present day, we well know how prevalent intermittent fevers were in London during the 17th century, when that city was occasionally visited by the oriental pestilence. The readers of Sydenham are well acquainted wity this fact.

The outbreak of the plague has not unfrequently followed upon wars, famines, and other wasting calamities; and, on the other hand, its ravages have invariably been observed to become less frequent and less desolating in proportion as the condition of the inhabitants of the affected countries, in point of civilization and comfort, has improved. The researches of MM. Papon* and Aubert-Roche† have satisfactorily proved the truth of this.

The general *conclusion* to which we arrive is that,

"In all countries where the spontaneous plague has been observed, its development may be reasonably attributed to certain determiuate conditions acting upon a large portion of the inhabitants. The principal of these conditions are, residence upon marshy alluvial soils near the Mediterranean or near certain rivers, as the Nile, Euphrates, and Danube; the dwellings being low, crowded, and badly ventilated; a warm moist atmosphere; the action of putrescent animal and vegetable matters, unwholesome and insufficient food; and great physical and moral wretchedness."

Chap, III.—*If the preceding statements be correct, the plague must be endemic in Lower Egypt, where all the conditions of insalubrity which we have pointed out are constantly present: Is such the case?*

All the most accurate and enlightened observers agree in answering this question in the affirmative. Not a year passes without the plague shewing itself at Alexandria in a *sporadic* form; generally between the months of November and the following June. This fact cannot be disputed; it is incontrovertibly proved by the reports of the Council of health that was established in that city 12 years ago. The same thing holds true with respect to Cairo, and other places in Lower Egypt; the testimony of Gaetani-Bey is unqualified upon this point. The *epidemic* plague, that which has so fearfully mowed down the Egyptian population, is happily more rare; although incomparably more frequent than in any other country of the world.‡ The number of epidemic invasions of the pestilence in Egypt from the year 1695 to 1834 have been (according to one statement drawn up by an Arab cheik) 19 in all. The mortality caused by some of the invasions has been truly frightful. If we take account of those only which have been very destructive of life, we find that Egypt has been visited with the scourge about once in every ten years.

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* De la peste et des epoques memorables de ce fleau. Paris, an. viii. 2 vol. 8vo.

† De la prophylaxie de la peste, Paris, 1843.

‡ According to the calculations of M. Hamont, the population of Egypt, which was once, it is believed, upwards of ten millions, and was fully three millions at the commencement of the present century, does not now exceed a million and a half.

Conclusion.—" All the producing causes of the plague being found united in Lower Egypt, the disease is endemic in that country, where it is seen every year in the sporadic, and about every tenth year in the epidemic form."

The object of the next Chapter, the IVth, is to shew that Egypt was exempt from pestilential epidemics in ancient times, and until about the commencement of the seventh century of the Christian æra. Certain it is that we have no very authentic account of any wide-spread and destructive invasion of the plague at an earlier period. The cases alluded to by Rufus, appear to have been only *sporadic;* at all events, this writer makes no distinct mention of any epidemic pestilence having ever prevailed in Egypt, as he does of one that ravaged Libya upwards of 300 years before the birth of Christ.

That Egypt was once a remarkably healthy country is expressly attested by Herodotus. The land was rich and very populous, abounding in all the necessaries of life, and the inhabitants were prosperous, enlightened, and happy. The custom of embalming the dead, not human beings only but animals of all sorts, may have had a salutary influence, by withdrawing so much corruptible matter from putrefaction and decay.* This " salutary practice" (of embalming) was abolished in A.D. 356.† Subsequently to this period, the ignorance and fanaticism of the Mussulmen have brought on that frightful state of moral degradation and physical wretchedness of which we have spoken in a preceding chapter. Is this lamentable state of things always to last, to the disgrace of the country and the injury of the world? There cannot be a reasonable doubt but that, if proper sanitary regulations could be established and duly executed in Egypt, the pestilence might be extirpated, and Egypt rendered as healthy as it was in days of yore. Mehemet Ali is well aware of the truth of this. His convictions on this point are so positive, and already he has acted so well in the right course, that Gaetani-Bey—who accompanies him twice a year in his tours of inspection across the Delta, from Alexandria to Cairo—has not hesitated to declare that, if the Viceroy was not thwarted in the execution of his plans, this great and desirable end might be accomplished.‡

The question proposed in Chapter V. is to the effect whether the present condition of Syria, of Turkey in Europe and Asia, and of the Barbary States has become so much changed or ameliorated, since the time when pestilential epidemics have broken out in them, as to justify any rational expectation that such invasions may not recur. The answer, as might be anticipated, is decidedly in the negative. Wherever the Ottoman dominion has prevailed, civilisation and social improvements have retrograded rather than advanced. We have seen, indeed, that the recently instituted board of health at Constantinople has attributed the exemption of that metropolis from an invasion of the plague for some years past exclusively and entirely to the establishment of lazarettos and quarantine restrictions there; but we must not be too ready to yield our unhesitating assent to this opinion.

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* There seem to be some inconsistencies between a few of the statements in this chapter and those that have been already made, respecting the existence of the plague in Egypt in ancient times. We may remark also that the testimony of Herodotus, respecting the salubrity of ancient Egypt, is said to be at variance with that of other authors more worthy of credit. M. Daremberg informs us that Hæser (*Recherches historico-pathologiques sur les maladies epidemiques*) has collected together a number of texts to prove the unhealthiness of Egypt in ancient times.—Compare also Lorinser, *die Pest im Orient.*

† M. Prus suggests, among other hygienic reforms necessary in modern Egypt, the re-establishment of the practice of embalming (!) or of some other equivalent method of counteracting the evils of animal putrefaction in that country.

‡ Sir W. Pym, in a letter addressed by him in Jan. 1845 to the Board of Trade, acquaints us that Mehemet Ali, on being informed that there was a very short quarantine in England against Egypt, replied: " There ought to be no quarantine, it is our own fault. *We must get rid of the plague!*"

It is not necessary to adduce any details to shew that the sanitary condition of such places as Erzeroum and the surrounding villages, of Tunis, Tripoli, &c., has not at all improved of late years, so that they should be less likely to be visited by the pestilenee than they may have been hitherto. With respect to Algeria (Chap. VI.)—which seems to have been less frequently the scene of spontaneous epidemics of the plague than any of the other Barbary States, in consequence probably of most of the towns and villages being built upon the slopes of hills, and neither crowded together nor over-peopled—there is good reason to anticipate that, under its present administration, it may become as seldom the theatre of the pestilence as almost any of the countries of Europe.

The answer to the question, proposed in Chap. VII.—*What are the means that should be employed to prevent the development of spontaneous plague?*—must be sufficiently obvious from what has been already said respecting the causes which promote, if they do not induce, the development of the disease in Egypt and elsewhere. M. Villermé has with great ability discussed the general question as to the origin and diffusion of epidemic diseases, and has very satisfactorily shewn that they invariably become less frequent and less destructive in proportion as countries pass from the miseries and degradation of barbarism to the social comforts of civilized life. Dr. Aubert-Roche also has with much care examined this subject, more especially in reference to the plague; and he comes to the same conclusion. In all times, and in all places, this disease has disappeared before civilisation; it has returned with a country's decline and barbarism. Everywhere the same causes have produced the same effects.

Second Part.

Chap. I.—*Has the plague always exhibited the principal characters of epidemic diseases, when it has raged with violence in Africa, Asia, or Europe?*

The characteristic feature of epidemic diseases are these:—1. They generally manifest in their progress three distinct periods, of commencement, persistance or status, and decline. These periods often display neither the same symptoms, the same lesions, nor the same gravity. 2. During the prevalence of an epidemic, other diseases are less numerous than usual, and they receive the stamp or impression of the prevailing affection. 3. When an epidemic disease prevails, even those persons who retain their health generally feel its morbific influence more or less. 4. Epidemic diseases not unfrequently return and cease at the same season (of the year;) and they have usually about the same duration. 5. An epidemic disease is often preceded by other affections, more or less severe and more or less widely diffused; these seem to be in some way its precursors.

Now the plague exhibits each and all of these features in a striking manner. Its severity or malignancy is usually most intense on its outbreak, and for the first few weeks afterwards. Pugnet says that, towards the end of the epidemic at Cairo in the year 1800, almost every patient recovered notwithstanding the most opposite methods of treatment, whereas very few indeed recovered upon its first outbreak.* Not to accumulate authorities, we may state that Clot-Bey remarks that, "when an epidemic commences, almost all who are attacked with it perish. During the first period, death occurs within 24 or 48 hours after the invasion; in the second, on the 4th or 5th day, or it may be not till the 14th or 20th. There are scarcely any fatal cases in the third period;" the pestilence having by this time lost its malignancy.†

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* *Memoire sur les fievrès de mauvais caractere du Levant et des Antilles, Paris* 1804.

† The following observations of Sydenham may be aptly quoted here:—

Observare insuper est quod, sicuti epidemicorum quilibet in subjecto particulari suas habet periodos (augmenti scilicet, status, et declinationis) ita etiam constitutio generalis quæcunque, quæ huic alterive morbo epidemicæ producendo favet, pro ra-

It must be obvious from this circumstance, how cautious medical men should be in estimating the value of any remedial means in the treatment of such a disease as the plague, and how important it is to pay great attention to the period of the epidemic visitation when these means have been employed. This is a great practical truth, which is far too little attended to in the present day. "At the commencement of the epidemic (1841,") says Dr. Penay, surgeon-major of a cavalry regiment in the Egyptian army, "I lost almost every patient, in spite of my best exertions. Subsequently, several got well without my being able to determine what line of treatment seemed to be of decided benefit. During the decline of the epidemic, nearly all my patients recovered, and the greater number without any other remedy except local applications to the bubos and carbuncles." The following extract from a report of M. Masserano, one of the members of the Egyptian council of health, is highly illustrative of the same subject.

"While the plague was at its height, almost all the persons who were attacked sunk at the end of four and twenty hours; and such was the violence of the epidemy in some of these cases, that the patients died suddenly while engaged in their employments, as if they had been struck with lightning. The pestilential characters in the middle, and towards the end, of the epidemy were much less intense. The acute cerebral congestions and complete state of prostration were no longer observed; and petechiæ were of rare occurrence. The sick were distressed with restlessness and weariness; exhaustion and headache threw them into a state of stupor. They experienced more or less severe glandular pains, shooting uneasiness in those parts where bubos were expected to appear: these bubos passed readily into suppuration. When the epidemy approached its close, I saw many persons attacked with bubos, without discontinuing their occupations. Two of my servants, among others, were attacked with the disease in a mild form; they pursued their employment without saying any thing to me. At the time when the disease was most intense, we remarked that, out of 22 persons attacked, ten died; whereas, towards the end, out of sixty seizures only two proved fatal."

What has now been said will abundantly show how truly the plague exhibits the *first* of the characteristic qualities of Epidemic Diseases. We proceed to examine the *second* one which we enumerated; viz. how far are other diseases, that may exist during the prevalence of the pestilence, influenced and modified by it. Diemerbrock, writing of the plague at Nimeguen in 1635–6, uses these words; *vix ullus morbus peste incomitatus fuit.* Pugnet says that "the plague stamps with its own peculiar character all other co-existing diseases." A proof and, at the same time, an effect of this decided influence of the pestilential constitution upon intercurrent diseases is the circumstance that these resume their own proper physiognomy, (a remark made three hundred years ago by Prosper Alpinus,*) as soon as the pestilence subsides. It would be easy to multiply authorities upon this subject, if it were necessary.†

All medical men, who have had an opportunity of studying the plague in Egypt or elsewhere, have remarked that, during the prevalence of an epidemic, those persons, who have already had an attack of the disease, usually feel pain

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tione temporis quo dominatur, suas etiam periodos habet, quatenus scilicet indies magis et magis epidemice grassatur, donec ακμην *attigerit suam, atque exinde iisdem fere gradibus decrescat, donec tandem penitus exoluerit, alteri constitutioni locum cedens. Symptomatum enim quod attinet vehementiam, atrociora sunt omnia ubi primum se ostendit; quæ quidem paulatim mitescunt, et in constitutionis catastrophe tam sunt benigna atque* ευφορητα *quam patitur morbi natura in quo fundantur.*—Observ. Med., sect. iv.

* *Medicina Ægyptonem.* Lib. 1, cap. 16.

† Sydenham has remarked, in his description of the plague of London, that the ordinary endemic fevers of a country are apt to retain, for a season or two, after a severe attack of the pestilence, some of its peculiar and characteristic features or symptoms; *pestilenti aeris diathesi etiamnum ex parte perseverante, nec dum in aliam salubriorem penitus immutatâ.*

or uneasiness in the scars of their old bubos and carbuncles, without their general health being much affected; and moreover that all those, who have escaped the disease and remain tolerably well, have still nevertheless experienced a certain feeling of *malaise;*—and even a slight degree of tenderness of the lymphatic glands in the groins and axillæ. This is the case equally with those who are in strict quarantine, or who enjoy free pratique. Dr. Delong, in his account of the Epidemic at Cairo in 1841, observes that, it may be fairly said that the entire population had the plague in its first and mildest degree. So impressionable and sensitive are those, who have once had the plague, to a pestiferous condition of the atmosphere that, it has been supposed, they can generally predict the approach of the disease by the shooting pains and uneasiness in their old bubonic and carbuncular cicatrices. It is possible that in this way we may find out if a pestilential constitution (of atmosphere) exists, or is impending.

Does the plague exhibit the *fourth* character assigned to epidemic maladies? viz. that of having in general nearly the same duration in different countries, and of appearing and disappearing at epochs which may be determined beforehand?

M. Levison, the Russian Vice-Consul at Alexandria, has drawn up the following statement from the data supplied to him by the Cheik Ibrahim-Bassi:

"The most intense pestilential epidemics in Egypt are those which, commencing *sourdement* in November, have reached their acme about the end of February or during March. On the other hand, those, which have not displayed great violence, have always made their appearance in the course of this last month. In the month of June, both one and the other have often ceased.

The malignant plagues of Egypt have usually lasted about four months, whereas the milder ones have in general not exceeded two months, or two months and a half."

It is a remark as old as the time of Prosper Alpinus, and one which is amply confirmed by the observations of subsequent writers, that the disease in that country almost invariably ceases in the month of June.

At Constantinople, epidemic plague habitually begins in the first or second week of July—during the great summer heats and the prevalence of southerly winds and thick fogs—after or before the arrival of the convoy of merchant ships from Egypt and other places, and usually ceases towards the end of the year.—The great plague of 1812, which had been mild up to the end of August, became very malignant in September, carrying off in little more than three months no fewer than 160,000 persons. It entirely ceased by the end of December. At Smyrna, the pestilence generally reaches its height in May, and ceases about the middle of August.

The *fifth* characteristic feature of epidemic diseases—that of being usually preceded by certain precursory maladies—belongs without doubt to the plague. Its outbreaks have been repeatedly observed to be preceded by bad forms of intermittent and continued fevers. "During the winter of 1816–17, there prevailed in this place," writes M. Berbrugger, the learned librarian of Algiers, "a very fatal epidemic that was termed in the bills of health a malignant fever. This has been remarked to be a usual precursory sign of an outbreak of the plague itself, when this reappears after a long interval of time." Many analogous circumstances might be quoted. That Typhus not unfrequently precedes, and coexists with, the regular plague is admitted by most of the medical men who have resided for some years in Egypt. Dr. Delong, who lives at Cairo, has made the remark of the epidemic of 1841 that the plague often commenced under an intermittent form, intermittents having been prevalent for some time; and that quinine occasionally seemed to arrest the progress of the malady.

Such being the case respecting the frequent precedence of other epidemic affections to the outbreaks of the plague, it must be obvious that medical men may readily fall into a seeming error, if they happen to be consulted respecting the nature of an existing disease, before the proper pestilence has fairly manifested itself. Hence then the necessity of their giving a guarded opinion, whenever there are grounds to believe that a pestiferous state of the atmosphere is impending. Moreover, at the commencement of an epidemic, there are neither bubos, carbuncles, nor petechiæ in most of the cases. Gaetani-Bey has pointed out a very useful diagnostic sign to direct the medical observer under such circumstances. The lymphatic glands, internal as well as external, should be most attentively examined; and if the patient has died of the plague, one or more of these glands will invariably be found to be enlarged and more vascular than usual. The dissections, made at Abouzabel in 1835 by the medical men who did not then know the opinion of Gaetani-Bey, amply confirmed the justice of his remark.

The facts and observations, adduced in this chapter, lead distinctly to the *conclusion* that "the plague combines in a very marked degree the principal characters of epidemic diseases."

We shall now briefly look at the causes of pestilential plague considered exclusiuely in this point of view. These causes, like those of all epidemic diseases, are of two kinds. The first relate to the soil and the atmosphere; the second to the physical and moral condition of the inhabitants.

When Dupuytren enquired of the young Egyptian students, who had been brought by Clot-Bey to Paris for medical education, what was the opinion of the most enlightened men in Egypt respecting the origin of the Plague, the answer they gave was, "la peste vient de la terre." All that is conveyed by such an expression is merely that a humid and marshy soil, more or less covered with decaying vegetable and animal matters, is a powerful cause of the alteration of the atmosphere, and consequently of the disease. Now nothing can better serve to shew the importance of the conditions of the soil, in reference to the production of the plague, than the comparing together of two localities in the same country, inhabited by the same people, and governed by the same laws and customs, in one of which the disease is endemic, while the other remains entirely exempt from its attacks, even although the infected may die within its walls.

"Fayoum is elevated above the level of the sea: Damietta borders upon the shore. At Damietta, the air is hot and damp; at Fayoum, it is hot, but dry. Fayoum is free from marshes; Damietta is surrounded with ponds of fresh and salt water. While at Damietta the cemeteries are in the town itself; at Fayoum, they are at a distance from the dwellings. Here, the water, although not very pure, may be drunk without inconvenience, owing to the quantity of nitre it contains; at Damietta, the fresh water is either mixed with sea-water, or it is rendered impure by excrementitious products, and by animal and vegetable matter in a state of putrefaction. Fayoum is surrounced by the desert of Lybia; Damietta is enclosed by rice-fields, and situated in front of the pestiferous Delta."

Great atmospheric vicissitudes have also a decided influence on the development and progress of epidemic plague. Larrey, Pugnet, and all other physicians who have seen the disease in Egypt, agree that its attacks are more frequent, and its mortality greater, when the air is warm and moist, and when the weather has been stormy. At Constantinople, the same causes produce the same effects. We shall not enlarge upon this subject; but only add that it has been too generally supposed that it is to some causes, either actually or recently existing, in the condition of the soil, the atmosphere, or the kind of food, that we must refer the morbific effects observed. And yet, as remarked with his accustomed sagacity by Baron A. Humboldt, the most favourable cause for the development of epidemic diseases is to be found in a uniform and long-continued type of meteorological phenomena. For example, in the case of the plague, it is after a lengthened duration of the same temperature and of the same winds that the pestilence, in an epidemic form, has been observed to break out in Egypt, Syria, and at Constantinople. It may be readily believed that, when a population has lived for a length of time in the same conditions of climate, atmosphere, alimentation, &c., the system of each individual becomes profoundly modified in the same manner, and may be disposed to receive, or even to develope spontaneously, the same disease. Perhaps it is in this way that we may account for what has been very positively asserted to be the case by some authors, but denied by others, viz. that persons, who have been long exposed to the same physical influences, may become affected with the same disease at a given period, although they are then far distant from each other.

The action of epidemic diseases is observed to vary much in point of degree or intensity in different races of mankind, when exposed to the same morbific influences. No fact has been more clearly proved than the very peculiar predisposition of negroes to contract the plague. To Dr. Aubert-Roche we are indebted for the following table of the relative mortality in the different races, during the great plague at Alexandria in 1835:

Negroes and Nubians lost . . .	1528	out of 1800	= 84	per cent.
Malays	367	" 600	= 61	"
Arabs, not soldiers	10,936	" 20,000	= 55	"

The Negroes, Nubians, and Arabs were all living in nearly the same hygienic conditions, and were all in free pratique. With respect to the other residents in Alexandria, our conclusions must be more uncertain, in consequence of the very great difference in point of hygienic condition, isolation, &c., enjoyed by different classes of the population. Here, however, are Dr. Roche's calculations:—

Greeks	lost 257	in 1800	= 14	per cent.
Jews, Armenians, and Copts . .	482	" 4000	= 12	"
Turks	678	" 6000	= 11	"
Italians and others from the South of Europe	118	" 1600	= 7	"
French, English, Russians & Germans	52	" 1000	= 5	"

These figures carry with them their own signification. It is scarcely necessary to say that the liability to the attacks of the pestilence among all classes of the population, native or stranger, is almost uniformly observed to be inversely proportionate to their cleanliness, good living, and general comfort.— An instructive illustration of the truth of this is afforded by Dr. Roche.

"On the banks of the canal, which leads from Alexandria to the Nile, lies a property belonging to the Greek consul, M. Tortizza, who received it as a present from the viceroy. The fellahs who work upon this property, being better treated and better fed than the fellahs of the surrounding villages, only lost, during the epidemic of 1835, 12 out of 400; while their neighbours, placed in the same conditions with respect to atmospheric influences and free communications, lost one half of their number."

Having most satisfactorily shewn that the Plague must be placed in the first rank of epidemic diseases, M. Prus makes the following general reflections upon the subject:—

"The epidemicity of the plague is in truth the fundamental fact of its history, that which most merits the attention of the physician, and which can alone make him comprehend a number of points which, without taking it into account, remain in complete obscurity. The certainty that the plague is a disease which is epidemic in a marked degree, will suggest another consideration to the mind of the physician. It will furnish him with the means sometimes of preventing, and always of diminishing, the ravages of the pestilence. If the existence of epidemic *foci* of plague is satisfactorily demonstrated, things will not affect in the same manner those who remain in or who come into the *foci*, as they do those who are placed, or who remove themselves, beyond their influence.

"Every person remainining in an epidemic focus of plague is exposed to contract this disease. Numerous and authentic facts, observed in Egypt during the years 1835 and 1841, have proved that the most complete isolation and the most severe quarantine do not always preserve those who submit to them. The same remark had been made in as positive a manner at Marseilles and at Toulon, at the time of the plague of 1720.

"It requires sometimes but a very short period to be passed in an epidemic focus to become affected with the plague. The professors of the school of medicine at Abouzabel,—which, in 1835, was not attacked by the epidemic influence for more than a month after the capital was ravaged by the disease,—have seen inhabitants of that place, who have only remained a few hours at Cairo, return infected.

"Now, what will happen to persons in health, or already affected with the plague, who shall remove from or be taken beyond the epidemic focus? Before answering this question, we must receive it as a fact recognised by science, that in the most wide-spread and severe pestilential epidemics, experience has shewn that all the localities of the same country have not been subject at the same period to the epidemic influence. It has been stated a hundred times, that by the side of a town ravaged by the plague, other towns in free communication with it continued exempt from the disease. Nay more, plague-patients out of infected towns have come either to die or to be cured in localities where the epidemic influence did not prevail, without the disease having spread. We shall find numerous examples in support of these two propositions, both in the works of modern writers on the plague, and in the documents annexed to this report. Observation has also taught us that it is often tolerably easy to determine the limits of the epidemic focus. This may be circumscribed within the limits of a single town, as Pagnet remarked at Damietta at the time of the plague of 1799, or as was seen at London in 1665, notwithstanding that in both instances the communication with the neighbouring towns remained perfectly free.

"This being established, we may assert, in answer to the question stated above, that, when a population is struck with a pestilential epidemic, persons, whose duties and interests do not require them to remain in the midst of the epidemic focus, will escape the danger by withdrawing from the infected district.

"In 1835, when the epidemic constitution prevailed at Cairo, Gaetani-Bey advised that the 22,000 of the soldiers on active service, composing the garrison, should be sent some leagues from the city, and be encamped under tents in a dry and airy situation, leaving only 2000 invalids for the service of the city.—The plague did not commit any ravages among the active troops, whereas it raged among the 2000 invalids as it did among the rest of the population."

Some time before this, Clot-Bey had given a similar advice respecting the fleet which was in the harbour of Alexandria. Although put and kept in severe quarantine, the pestilence made its appearance on board some of the ships, while they remained exposed to the epidemic influence. Not a single case occurred, when the fleet was withdrawn from the focus.

In 1813, Sir Thomas Maitland, the Governor of Malta, finding it impossible, in spite of the most severe measures, to extinguish the plague which then prevailed at La Valetta, took the resolution to have barracks constructed fairly out of the city, and obliged the population forthwith to occupy them. From that moment the plague entirely ceased.

Dr. Masserano has related a similar instance with respect to his regiment in garrison at Damietta in 1841; removal to a healthy spot at once put a stop to the disease.

The enlightened portion of the inhabitants of Cairo and Alexandria has already begun to discover the inutility of quarantine within their own dwellings, and now trusts for safety only to removal beyond the sphere of infection. The Persians have long acted upon this principle, and have no doubt found its advantage.*

Removal from an epidemic focus has been found to be most useful, not only in preserving the unattacked from the pestilence, but also in promoting the re-

* Lacheze, *Memoire sur la peste en Perse.*

covery of those who have caught it. On this latter point, Dr. Delong observes in reference to the plague of 1841:

"When I had the good fortune to be called at the onset of the disease, I instantly ordered a change of abode; whenever it was possible. I caused my patients to be removed to situations that were elevated, dry and airy. The disease then almost always assumed a more favourable appearance, and the morbid phenomena were found less to resist the combined action of nature and of a sound method of treatment."

A similar remark was made by the medical men at Abouzabel in 1835, and it has been repeated by M. Penay in the history of his patients in 1841. The *conclusion*, to be drawn from the numerous facts and observations that have been adduced in this chapter, is surely that,

"Whenever the plague has raged with violence in Africa, Asia and Europe, it has always exhibited the principal characters of epidemic diseases."

CHAP. II.—*What are the differential characters between Epidemic and Sporadic Plague?*

The medical men residing in Egypt, as well as those at Smyrna and Constantinople, agree, almost without exception, that the sporadic form of the disease is not transmissible;* whereas, with respect to the epidemic form, many of them hold the opposite opinion. This is, indeed, just what might have been expected, for it is in exact accordance with the views entertained by most observers on other analogous diseases (Dysentery for example,) which occur at one time sporadically: and at another time epidemically: in the former case, the malady is not transmissible; in the latter, it is often so in a very high degree.

If we admit the accuracy of this opinion, it will be obvious how important it must often be for a medical man to determine if a case of plague be simply sporadic, or if it be connected with a pestiferous constitution of the atmosphere. As a matter of course, this point cannot be fairly determined by a mere summary or off-hand enquiry; things must be carefully watched for some time, before a decided opinion is given. If the disease be limited to a few isolated cases, and if these occur only in the localities where the pestilence arises spontaneously, there will be reason to believe that its type is merely sporadic. The disease too is usually less malignant in this than in the epidemic form. The sporadic plague does not exhibit in its course the three regular periods of commencement, persistance, and decline; nor is it preceded by other epidemic disorders. Moreover, other maladies are not less numerous than usual, nor do they display anything of the prevailing pestilential stamp or impression; persons in health do not experience the effects of an atmospheric influence acting in an especial manner on the lymphatic system; and lastly, whereas the epidemic form of the disease commences between November and February, and ceases about the end of June, the sporadic form is observed in every month of the year. The *conclusion* is therefore very fair and obvious that "Sporadic differs in some very important respects from Epidemic plague."

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* The testimony of Dr. Laidlaw, who has resided so long in Egypt, and seen so much of the disease, is very strong upon this point:

"I have no hesitation whatever," says he, "in expressing my decided conviction that, unless the state of the atmosphere is favourable to the spread of the disorder, as is undoubtedly the case during the epidemic, there is no danger whatever from these causes, that they were purely accidental, and that it is impossible to produce by them the spread of the disorder. I have never seen a case of plague occurring sporadically where any person about the patient or in contact with him was attacked; and I cannot find any one that has *seen* one, although it is talked of among the Levantines as a common occurrence."—*Dr. Bowring's Observations.*

CHAP. III.—*Does the plague extend, like most epidemic diseases, by the migration of certain atmospheric influences, and independently of the agency of the sick who are affected with it?*

Whoever examines with attention and candour the histories of many epidemics of the plague, can scarcely fail to observe that the disease has often broken out about the same time in a number of different localities, distant from and having no intercourse whatsoever with each other. Always originating in unhealthy spots under the influence of those causes which have been already mentioned, the epidemic pestilence may be either confined within the circuit of a single town or city, although this remains in free communication with the surrounding district, as occurred at Damietta in the year 1799; or else it may become diffused over a number of countries, as in the formidable epidemics of 542 and 1348. It is scarcely necessary to adduce further instances to prove that the localities and districts, immediately adjoining to an affected spot, often remain quite exempt from the disease. Not unfrequenty several towns are attacked about the same time, the intermediate villages remaining quite free; at other times, the pestilence advances in a more regular manner, attacking a number of places "de proche en proche" and in succession.

Clot-Bey and Dr. Roche are of opinion that the plague may traverse seas, and pass from one continent to another—from Alexandria to Marseilles, for example—through the medium of the atmosphere alone. On the other hand, that it may meet with unsurmountable obstructions not far from the spot where it has arisen, would seem to be the case from the well known fact that the plague of Lower Egypt never passed beyond the first Cataract of the Nile. In some malignant epidemics, indeed, the disease spreads into districts that are very generally spared. This has been observed at certain periods in Upper Egypt and the Hedjaz.

It appears that the plague never extends to a great elevation above the level of the sea. There is a village about five leagues from Constantinople, situated on the mountain of Alem-Daghe at an elevation of about 500 *metres*, where the pestilence has never been known to exist: it serves, indeed, as a place of retreat to the inhabitants of the Turkish capital during the prevalence of an epidemic. On the same mountain, but at a less elevation, there is another village that by no means enjoys the exemption of the first. There is a spot in Malta to which the plague has never reached; from this circumstance it has received the name of safi (pure.) According to the testimony of Desgenettes and Clot-Bey, the Citadel of Cairo, which stands upon a lofty eminence, has uniformly escaped during the worst epidemics that ever raged in that city.

A most important question here suggests itself to our consideration; viz: *When a pestilential epidemic prevails in a place, how many cases of the disease are to be attributed to the influence of the epidemic constitution, and how many either to the absorption of the miasms emanating from the sick, or to direct or indirect contact with them?* As may be imagined, the solution of this problem is attended with many difficulties. Dr. Lacheze, when physician of the hospital at Cairo in 1835, endeavoured to determine the point. According to the observations of this gentleman, not more than one person in 400 of those who were entirely isolated or in quarantine was attacked; whereas the pestilence of that year carried off no fewer than one in three of the general population, that remained in free pratique. Without disputing the accuracy of the figures given by this gentleman it is the opinion of many able observers that they should be interpreted very differently from what M. Lacheze has done; at all events, it must never be forgotten that the very persons, who put themselves in quarantine during the prevalence of an epidemic, are precisely those who enjoy the greatest comforts of life and pay most attention to a hygienic regimen. The Commissioners were therefore desirous of obtaining if possible some less objectionable data for comparison, and it occurred to them that the best plan would be to ascertain the results in some large public establishment, either at Cairo or Alexandria, the inmates of which were living in very nearly the same conditions as the rest of the population out of doors. For this purpose, the Arsenal at Alexandria, which always contained 6000 workmen at the least, was selected for observation. Now, what has been the result of their enquiries? The words of the report are these:

"In this establishment, no attack could be attributed to an accumulation of pestilential miasms. Such an acumulation never existed; for, whenever an invalid was discovered to be infected, he was instantly taken into an hospital situated without the arsenal. Neither could contact with those infected be regarded as the cause; since, whether from the invalids having been removed at the beginning of the disease, or from quite a different cause, the neighbors of those who were seized with plague, as well as those who had touched them, were never attacked with the disease. The number of workmen at the arsenal, removed to the hospital on account of plague, gives us then that of the cases attributable to the epidemic, apart from any other agency. Now 300 workmen having been attacked with the disease in a total of nearly 6000, we may fairly believe that the epidemic influence alone acted upon one individual in every twenty. This proportion is without doubt very different from that pointed out by M. Lacheze; but it also varies very considerably from that furnished by the population in free partique; for this at Cairo and Alexandria, as we have already seen, lost one in every three. Are we to believe, with Clot-Bey, that the difference of hygienic condition completely accounts for these facts, and that, if the mortality among the workmen in the hospital did not amount to one in every three of their number, this was solely owing to their having been kept cleaner and better fed than the rest of the working population at Cairo and Alexandria?

"While we readily recognize and publicly admit the very great power of hygiene in preventing and moderating the ravages of the plague, we must at the same time distinctly state that the deductions drawn by Clot-Bey appear to us to excede what the facts warrant.

"Neither can we assent to his final conclusion, that all cases of plague should be attributed to epidemic influence alone. We must reject this conclusion, on the one hand, because it does not appear to us to be based upon positive and satisfactory proofs; and on the other hand, because, if it were rashly received, it would have the serious inconvenience of checking all enquiry into those causes, which secondarily, tend to propagate the plague, and increase its fatality. In science, a false explication is less dangerous in that it extends error, than that it impedes the search after truth.

"From the facts and considerations contained in this chapter, we think we are fairly justified in drawing the following *conclusion:*

"The plague, abstractedly from the influence which the infected may exercise, spreads itself after the manner of most epidemic diseases, viz. by the action of general causes."

[*To be Continued.*]

Part Fourth.

MEDICAL INTELLIGENCE.

FOREIGN.

1.—*The Chemistry of Respiration.* *By* LIEBIG. L. Lancet.

AN ANALYSIS OF THE CHEMICAL ACTIONS PRODUCED IN RESPIRATION, AND THE FORMATION OF FAT IN THE ORGANISM.

All the oxygenous and non-nitrogenous constituents of plants contain carbon and the elements of water; they without exception, contain less oxygen than their carbon and hydrogen would require for their conversion into carbonic acid and water.

Oxalic acid is the only constituent of plants, which, assuming it to be anhydrous, contains no hydrogen. Many volatile oils, caoutchouc, &c., contain no oxygen.

The whole of the carbon of plants being indisputably derived from the carbonic acid of the atmosphere, and the whole of the hydrogen from water, the constituents of plants cannot have been formed in any other way than by the separation and secession of the oxygen of the carbonic acid, and the submission of an equivalent amount of hydrogen for part or the whole of this oxygen.

An organic substance containing in one equivalent 4, 6, 12 equivalents of carbon, has derived these 4, 6, 12 equivalents of carbon, from 4, 6, 12 equivalents of carbonic acid. A substance containing 4, 6, 12 equivalents of hydrogen, has derived these from 4, 6, 12 equivalents of water.

Now, viewing the constitution of carbonic acid as analogous to that of the organic acids, this acid appears to be a combination of a compound organic radical, carbonic oxide, with oxygen. The chemical deportment of carbonic oxide fully supports this view of the constitution of carbonic acid.

The following formula would accordingly express the composition of carbonic acid:

$$\text{Carbonic acid} \ . \ . \ CO_2 = CO + O.$$

C O being assumed as = 1 equivalent of the radical, which we will now designate by R, and thus the formula would be

$$\text{Carbonic acid} \ . \ . \ CO + O = R + O.$$

Now assuming that the carbonic acid has been decomposed in the organism of the plant, $\frac{1}{8}$, $\frac{1}{2}$, $\frac{1}{6}$, $\frac{1}{5}$, $\frac{1}{4}$, $\frac{1}{3}$, $\frac{1}{2}$, of its oxygen having separated, this may be expressed by a formula in which the radical is represented to have increased in corresponding proportion. For instance, if the carbonic acid, for its conversion into oxalic acid, loses one quarter of its oxygen, this may be expressed by the formula—

$$C_2O_3$$

or by $$C_2O_2+O=2\,R+O$$

Assuming, further, that the oxygen of the carbonic acid has been eliminated and replaced by the equivalents of hydrogen, we obtain compounds of different properties, according as it is the oxygen *within* the radical, or that which we assume to exist *without* the radical, that is replaced by hydrogen.

For instance: supposing the oxygen *within* the radical replaced by hydrogen, we have—

$$\begin{array}{lll} 1)\ C\,O+O & \text{Carbonic acid} & =R+O \\ 2)\ C\,H+O & \text{“} & =R+O \end{array}$$

this is, two bodies of analogous constitution, and differing simply in this, that the radical of the second is a carburet of hydrogen, corresponding to carbonic oxide.

If we assume, on the contrary, that the oxygen existing in the carbonic acid *without* the radical is replaced by hydrogen, the properties of the compound thus produced must, of necessity, differ from those of the compound produced in the former instance. If this latter were an acid, the former may be a neutral substance.

$$\begin{array}{ll} C\,O+O=R+O & \text{Carbonic acid.} \\ C\,O+H=R+H & \text{“} \end{array}$$

If we assume the whole of the oxygen, both that belonging to, and that superadded to, the radical, may be replaced by hydrogen, we obtain a carburet of hydrogen analogous in composition with carbonic acid, and such as exists in reality in marsh gas.

$$\begin{array}{ll} C\,O+O=R+O & \text{Carbonic acid.} \\ C\,H+H=R+H & \text{Marsh gas.} \end{array}$$

All organic compounds that contain carbon, and the elements of water, as being formed from carbonic acid, belong to one of the following series:—

$$\left.\begin{array}{lllll} R+O; & 2\,R+2\,O; & 3\,R+3\,O; & 4\,R+4\,O, & \text{\&c.} \\ 2\,R+O; & & & & \\ 3\,R+O; & 3\,R+2\,O; & & & \\ 4\,R+O; & 4\,R+2\,O; & 4\,R+3\,O: & & \\ 5\,R+O; & 5\,R+2\,O; & 5\,R+3\,O; & 5\,R+4\,O, & \text{“} \\ 6\,R+O; & 6\,R+2\,O; & 6\,R+3\,O; & 6\,R+4\,O, & \text{“} \\ 7\,R+O; & 7\,R+2\,O; & 7\,R+3\,O; & 7\,R+4\,O, & \text{“} \\ 8\,R+O; & 8\,R+2\,O; & 8\,R+3\,O; & 8\,R+4\,O, & \text{“} \\ \vdots & \vdots & \vdots & \vdots & \\ 34\,R+O; & 34\,R+2\,O; & 34\,R+3\,O; & 34\,R+4\,O, & \text{“} \end{array}\right\}\ \text{Carbonic acid.}$$

Putting entirely out of the question the views which are entertained respecting the capacity of the organic acids to neutralize bases, and to form salts with them, we have reason to believe, that all these compounds have a similar constitution to that of the organic acids; and the most natural supposition seems to be, that they contain a compound radical, of which hydrogen forms a constituent part; so that the transformation of the carbonic acid into an organic acid has been effected by the substitution of hydrogen for part, or the whole, of the oxygen in the radical. Thus, for instance, formic acid may be regarded as the first link of the (above) second series: it may be looked upon as carbonic acid, in the radical of which one half of the oxygen has been replaced by hydrogen.

The constitution of the most frequently-occuring organic acids, citric acid, tartaric acid, malic acid, and acetic acid, viewed in this way, appear equally simple.

$$\text{Carbonic acid } C_2 \left.\begin{matrix}O\\O\end{matrix}\right\} + O_2 = 2\,R + 2\,O$$

$$\text{Formic acid } C_2 \left.\begin{matrix}H\\O\end{matrix}\right\} + O_2 = 2\,R + 2\,O$$

$$\text{Oxalic acid } C_2 \left.\begin{matrix}O\\O\end{matrix}\right\} + O = 2\,R + O$$

$$\text{Malic acid } C_4 \left.\begin{matrix}H_3\\O_2\end{matrix}\right\} + O_2 = 4\,R + 2\,O$$

$$\text{Acetic acid } C_4 \left.\begin{matrix}H_3\\O\end{matrix}\right\} + O_2 = 4\,R + 2\,O$$

$$\text{Tartaric acid } C_4 \left.\begin{matrix}H_2\\O_2\end{matrix}\right\} + O_3 = 4\,R + 3\,O$$

$$\text{Citric acid } C_{12} \left.\begin{matrix}H_5\\O_7\end{matrix}\right\} + O_4 = 12\,R + 4\,O$$

The formulæ of acetic acid and of malic acid, it will be readily remarked, correspond to oxalic acid; tartaric acid is carbonic acid, in which one half of the oxygen of the radical is replaced by hydrogen, whilst one-fourth of the oxygen without the radical has separated.

By the action of heat, malic acid is converted into fumeric acid and maleic acid; citric acid into aconitic acid.

These conversions are effected simply by the separation of water, or by the secession of hydrogen and oxygen from the elements of the radical of the acid.

Malic acid becomes Maleic acid—

$$C_4 \left.\begin{matrix}H_2\\O_2\end{matrix}\right\} + 2\,O \qquad C_4 \left.\begin{matrix}H\\O\end{matrix}\right\} + 2\,O$$

Citric acid becomes aconitic acid—

$$C^{12} \left.\begin{matrix}H_5\\O_7\end{matrix}\right\} + 4\,O \qquad C_{12} \left.\begin{matrix}H_3\\O_5\end{matrix}\right\} + 4\,O$$

In like manner, the action of heat upon acetic acid causes the separation of one equivalent of carbonic acid from the elements of that acid, and the consequent production of a neutral inflammable fluid, acetone.

$$C_4 \left.\begin{matrix}H_3\\O\end{matrix}\right\} + 2\,O \ \ \begin{matrix}\text{Acetic acid is}\\\text{resolved into}\end{matrix} \ \ C\,O_2 \ \ \begin{matrix}\text{Carbonic acid}\\\text{and}\end{matrix}$$

$$C_3 \left.\begin{matrix}H_3\\O\end{matrix}\right\} \text{Acetone.}$$

In this way, and likewise as the substitution of hydrogen for the oxygen in the carbonic acid does not take place equivalent for equivalent, but in unequal proportion, (when, for instance, upon the separation of the oxygen, both within and without the assumed radical, one equivalent of the oxygen separates without being replaced by hydrogen; or when one equivalent of hydrogen replaces two, three or four equivalents of oxygen, which separate,) new series of compounds are formed, of which the radical differs from carbonic oxide, (the radical of carbonic acid.) Thus, benzoic acid and oil of bitter almonds belong to none of the above given series; they contain a new radical, which has been formed from the radical of carbonic acid, by the separation of one-half of its oxygen, and the substitution of five equivalents of hydrogen for part of the other half.

$$\text{Benzoic acid} \ldots\ldots\ C_{14} \left.\begin{matrix}H_5\\O_2\end{matrix}\right\} + O$$

$$\text{Oil of bitter almonds} \ldots\ C_{14} \left.\begin{matrix}H_5\\O_2\end{matrix}\right\} + H$$

If we examine the formulæ of malic acid and tartaric acid, the two organic acids, which are of most frequent occurrence in the vegetable kingdom, and bear in mind that they have been formed from carbonic acid, by the separation of oxygen, and the accession of a certain amount of hydrogen; if we bear in mind, likewise, that these acids are capable of further modification in the organism of the plant, from the same causes that effected the decomposition of the carbonic acid,—we arrive at the natural result, that these acids, by the simple separation of a fresh amount of oxygen, must gradually lose their characteristic properties, and become converted into neutral substances, containing hydrogen and oxygen in the same proportion as these elements exist in water, and consequently in a similar proportion as they are contained in sugar, gum, starch, and woody fibre.

Two equivalents of oxygen, separating from the elements of malic acid leave

$$C_4 \begin{matrix}H_2\\O_2\end{matrix}$$

three equivalents of oxygen separating from the elements of tartaric acid, leave, likewise,

$$C_2 \begin{matrix}H_4\\O_2\end{matrix}$$

and by the simple accession of $1\frac{1}{3}$ — $1\frac{2}{3}$ equivalents of water, wehave the composition of starch, of gum, or of grape sugar. Most of the above-enumerated organic acids, contain, in their radical, hydrogen and oxygen, and this either in the proportion in which these elements exist in water, or the proportion of the oxygen exceeding that of the hydrogen.

Acetic acid, and the whole of the fatty acids, contain in their radical a larger proportion of hydrogen; from acetic acid upwards, there is none containing a sufficient amount of oxygen to form water with the whole of the hydrogen present. The most frequently occurring of the fatty acids may be looked upon, in their isolated state, (as hydrated acids,) as compounds of oxygen with the radical of carbonic acid, in which the whole of the oxygen is replaced by its equivalent amount of hydrogen. Assumed to exist in the anhydrous state, one equivalent of oxygen would remain unreplaced in each of them.—(DUMAS.)

Table showing the constitution of the most frequently occurring of the fatty acids:—

In the isolated state, as hydrated acids. (R = C H)			In their salts.
Butyric acid	8 R+4 O	$C_8 \begin{matrix}H_7\\O\end{matrix}$	$\Big\}$ +2
Valerianic acid	10 R+4 O	$C_{10} \begin{matrix}H_9\\O\end{matrix}$	$\Big\}$ +2 O
Caproic acid	12 R+4 O	$C_{12} \begin{matrix}H_{11}\\O\end{matrix}$	$\Big\}$ +2 O
Caprylic acid	16 R+4 O	$C_{16} \begin{matrix}H_{15}\\O\end{matrix}$	$\Big\}$ +2 O
Lauric acid / Pichurimic acid	24 R+4 O	$C_{24} \begin{matrix}H_{23}\\O\end{matrix}$	$\Big\}$ +2 O
Carinic acid	26 R+4 O	$C_{26} \begin{matrix}H_{25}\\O\end{matrix}$	$\Big\}$ +2 O
Ethalic acid / Palmic acid	32 R+4 O	$C_{32} \begin{matrix}H_{31}\\O\end{matrix}$	$\Big\}$ +2 O
Margaric acid	34 R+4 O	$C_{34} \begin{matrix}H_{33}\\O\end{matrix}$	$\Big\}$ +2 O
Stearic acid	2 (34R)+7 O	2 ($C_{34} \begin{matrix}H_{33}\\O\end{matrix}$	$\Big\}$ +3 O

It is scarcely necessary for us to state that this table is not intended to express the exact constitution of these acids; my intention here is simply to point out their origin, their similarity of composition, and their mutual relations.

The most cursory reflexion will at once show that an organic substance is to be placed the higher in the scale of organic compounds, the more its composition differs from the original type, represented by carbonic acid.

Among the neutral substances of the vegetable kingdom, dry grape sugar approaches most nearly, in its composition, to carbonic acid, although the manner of the arrangement of its elements must differ, as does its chemical deportment.

Cane sugar, gum, starch, and woody fibre, appear as organic compounds of a higher order.

CANE SUGAR.

Carbonic acid.		Grape sugar.		Gum.		Starch.
$C_{12}\left.\begin{matrix}O_{12}\\O_{13}\end{matrix}\right\}$	...	$C_{12}\left.\begin{matrix}H_{12}\\O_{12}\end{matrix}\right\}$	...	$C_{12}\left.\begin{matrix}H_{11}\\O_{11}\end{matrix}\right\}$	...	$C_{12}\begin{matrix}H_{10}\\O_{10}\end{matrix}$

If, therefore, starch is converted into grape sugar during the process of digestion, the transformation of grape sugar into carbonic acid, in the process of respiration, is effected by the simple substitution of oxygen for the hydrogen present.

In that peculiar process which we designate by the name of fermentation, a certain amount of carbonic acid separates from the elements of the grape sugar; it is obvious that the other product of this process, alcohol, must again form a representative of carbonic acid; and alcohol contains, indeed, the same number of elements as carbonic acid.

Carbonic acid.	Alcohol.
$C_{8}\left.\begin{matrix}O_{8}\\O_{8}\end{matrix}\right\}$	$C_{8}\left.\begin{matrix}H_{12}\\O_{4}\end{matrix}\right\}$

The sugar has been formed in the plant from carbonic acid by the separation of oxygen, and the accession of hydrogen instead. The reverse is the case in the animal organism—viz, the hydrogen is eliminated and replaced by oxygen, and it is in this way alone that the carbon re-assumes its original form. This is, generally speaking, the essential and characteristic mark of the process of decomposition or respiration; it is an indirect process of combustion, taking place at a lower temperature, and with limited access of oxygen. We know of no instance in which the carbon of an organic compound combines, under these circumstances, directly with oxygen into carbonic acid; strictly speaking, no combustion of the carbon takes place, but the hydrogen of the organic compound is oxydized and eliminated in the form of water, whilst one equivalent of oxygen takes the place of the thus eliminated hydrogen. If one of the intermediate compounds formed by the gradual substitution of oxygen for hydrogen happen to possess an individual affinity for oxygen, several equivalents of oxygen will be required in substitution for one equivalent of hydrogen.

The evolution of heat in the process of respiration is, accordingly, not owing to the oxidation of the carbon, but to the conversion of the hydrogen of the substance into water, and the accession of one or several equivalents of oxygen in the place of the thus eliminated hydrogen.

The transformation of alcohol into carbonic acid, one of the most thoroughly studied processes of decomposition, will best serve to illustrate the process of the gradual substitution of oxygen for hydrogen.

Alcohol.		Acetic acid.		Formic acid.		Carbonic acid.
$C_{8}\left.\begin{matrix}H_{12}\\O_{4}\end{matrix}\right\}$	...	$C_{8}\left.\begin{matrix}H_{8}\\O_{8}\end{matrix}\right\}$	...	$C_{8}\left.\begin{matrix}H_{4}\\O_{12}\end{matrix}\right\}$	...	$C_{8}\begin{matrix}O_{4}\\O_{12}\end{matrix}$

If in the conversion of alcohol into acetic acid there happens to be less oxygen present than is required to replace the hydrogen by its equivalent of oxygen, four equivalents of hydrogen are eliminated without being replaced by oxygen, and thus the intermediate compound, aldehyde, is formed, which changes afterwards to acetic acid by the direct assumption of oxygen.

$$\underset{\text{Alcohol.}}{C_8 \left.\begin{matrix} H_{12} \\ O_4 \end{matrix}\right\}} \quad \cdots\cdots\cdots \quad \underset{\text{Aldehyde.}}{C_8 \left.\begin{matrix} H_8 \\ O_4 \end{matrix}\right\}} + 4\ O = \underset{\text{Acetic acid.}}{C_8 \left.\begin{matrix} H_8 \\ O_8 \end{matrix}\right\}}$$

But in presence of a sufficient amount of oxygen, aldehyde passes immediately to the state of formic acid: and in this case the four equivalents of hydrogen, eliminated in the form of water, are replaced, not by four, but by eight, equivalents of oxygen.

Aldehyde plus 12 equiv. of oxygen = 4 equiv. of water + formic acid,

$$C_8 \left.\begin{matrix} H_8 \\ O_4 \end{matrix}\right\} \qquad\qquad\qquad\qquad C_8 \left.\begin{matrix} H_4 \\ O_{12} \end{matrix}\right\}$$

But this is not the only form of the process of organic decomposition: there is still another, and far more remarkable form; and it is this second form which requires particular attention in studying the transformation of sugar into fat.

The fatty substances occurring in plants and animals are looked upon in chemistry as a particular species of salts, consisting of an organic oxide, oxide of glyceryle, and a volatile or fixed acid. They possess the property in common, to suffer decomposition (saponification) by most of the metallic oxides. In this decomposition the metallic oxide takes the place of the organic oxide. Upon removing the metallic oxide by means of an acid, the so called hydrates of the fatty acids are obtained, in which the metallic oxide is replaced by an equivalent of water.

The whole of the fatty substances which contain oxide of glyceryle yield, when heated to decomposition, (in the process of dry distillation, for instance,) acroléine, a product of exceedingly pungent odour, and which irritates the eyes excessively. This product owes its formation to the oxide of glyceryle. Another characteristic property, which the whole of the volatile fat or oils have in common, besides the production of acroléine upon heating, is the formation of a crystallizable substance, soluble in water, and which, in many respects, is similar to benzoic acid; and although differing from the latter in composition, the name of sebacic acid has been assigned to this substance. Its formula is

$$O_{10}H_9O_4$$

Oleic acid changes afterwards into a soluble, crystallizable acid, called elaidic acid.

The composition of oleic acid is not known with certainty. Produced from oils, it attracts oxygen with the greatest eagerness and rapidity. This strong affinity for oxygen renders its production in the pure state exceedingly difficult.

The fact appearing indisputable, that the wax in the body of the bee is produced from sugar, and the fat in the body of the graminivorous animal is produced, in the process of fattening, from starch, or rather, from sugar, (since starch is assimilable only in the form of sugar,) it is perfectly certain, that the transformation of the sugar into fat is possible only in consequence of a simultaneously arising decomposition and fermentation; which means, in other terms, that the transformation of a substance rich in oxygen into one containing less of this element, is effected by the resolution of the sugar into two compounds, of which the one contains the excess of the oxygen.

As has been shown and developed in the preceding remarks, in the processes of decomposition, the oxygen combines with the hydrogen of the substance, and the highly oxygenated compounds which are eliminated in the processes of fermentation are carbonic acid and water.

According to this view, the transformation of sugar into fat takes place in consequence of the oxidation of its hydrogen, and separation of a certain amount of its oxygen, in the form of carbonic acid and water.

Upon leaving a solution of sugar to ferment, at a high temperature, in contact with putrefying cheese, excluding at the same time the oxygen of the atmosphere, a certain amount of carbonic acid and hydrogen separate from the elements of the sugar, and a fatty acid, *butyric acid*, is obtained.

It is obvious that, in the process of the formation of the same acid in the organism of the cow, (which yields butyric acid in butter,) the hydrogen cannot separate in a free state from the elements of the sugar, because in this process the access of oxygen is not precluded, the hydrogen combines here with the oxygen furnished by the blood, and separates them from the elements of the sugar in the form of water. The formation of caproic acid, capric acid, and the rest of the fatty acids, must take place in an analogous manner.

The following formulæ, which are intended to illustrate the formation of a few of the fatty acids from sugar, will show at the first glance that the smallness of the amount of oxygen in the fatty acid stands in direct proportion to the amount of the oxygen eliminated in the form of water and carbonic acid :—

Sugar.

$$C_{12}\left.\begin{matrix}H_{12}\\O_{12}\end{matrix}\right\}+4\,O=\left\{\begin{array}{ll}\text{Butyric acid . .} & C_8H_8O_4\\ \text{Water . . . .} & H_4O_4\\ \text{Carbonic acid . .} & C_4O_8\end{array}\right.$$

Sugar.

$$C_{18}\left.\begin{matrix}H_{18}\\O_{18}\end{matrix}\right\}+4\,O=\left\{\begin{array}{ll}\text{Caproic acid . .} & C_{12}H_{12}O_4\\ \text{Water . . . .} & \phantom{C_{12}}H_8\,O_8\\ \text{Carbonic acid . .} & C_6\phantom{H_{12}}O_{12}\end{array}\right.$$

Sugar.

$$C_{24}\left.\begin{matrix}H_{24}\\O_{24}\end{matrix}\right\}+4\,O=\left\{\begin{array}{ll}\text{Capric acid . .} & C_{16}H_{16}O_4\\ \text{Water . . . .} & \phantom{C_{16}}H_6\,O_6\\ \text{Carbonic acid . .} & C_8\phantom{H_{16}}O_{16}\end{array}\right.$$

Sugar.

$$C_{12}\left.\begin{matrix}H_{12}\\O_{12}\end{matrix}\right\}+3\,O=\left\{\begin{array}{ll}\text{Oxyde of glyceryle} & C_9\,H_6\,O_3\\ \text{Water . . . .} & \,H_6\,O_6\\ \text{Carbonic acid . .} & C_3\,O_6\end{array}\right.$$

It would be an idle display of figures, to pursue these examples still further, since, for the present, we are unable to produce any other fatty acid from sugar besides the butyric acid. But it should not be forgotten, that butyric acid is really producible from sugar, and that the fatty substances are produced from sugar in the animal organism.

The solid crystallizable fatty acids belong, in respect to their constitution, to one and the same type. Analogous causes must have been active in their formation, and the manner of their action must have been similar.

The formation and production af a fatty substance from sugar cannot be imagined without a separation of oxygen in the form of carbonic acid, or a similar resolution of the sugar atom, as takes place in the process of fermentation; it cannot be imagined, without a corresponding diminution of the hydrogen, the elimination of which from the organism, in the form of water, can take place only in consequence of a simultaneously arising process of decomposition.

In this respect, the transformation of sugar into fat exhibits the greatest similarity with the process of decomposition of woody fibre.

2.—*Gun-Cotton.—Xyloidine.* London Medical Gazette for October, 1846.

It is rather more than two months since we inserted a notice of a remarkable chemical discovery reported to have been made by Prof. Schönbein, of Basle.* We allude to the preparation of cotton so as to give it fulminating

* August 21, p. 351.

properties, and to render it a safe, inexpensive, and simple substitute for gunpowder. We then announced it as probable that the Professor would give a full account of his alleged discovery at the meeting of the British Association at Southampton. To the surprise and disappointment of all scientific men, this meeting was converted into an advertising medium for the so-called gun-cotton; and the Professor declined to give the least intimation respecting the preparation of the substance, as it was his intention to take out a patent for it, and thus render it a commercial speculation. After the noble example of Sir H. Davy, who declined to patent his safety-lamp, we should have thought scientific men would have hesitated before resorting to the patent laws for a pecuniary remuneration; and we certainly think that the British Association committed a grave error in allowing the subject to be brought publicly forward, when there was no intention, on the part of the alleged inventor, to describe the process by which the gun-cotton was prepared.

Within the last week, public attention has been much directed to the subject. It is reported that the German Diet has conditionally awarded 100,000 florins as a reward to the inventor. The *Athenæum* informs its readers that a hundred weight of the gun-cotton is now on its way from Basle to Woolwich, having been ordered by our Government with a view of testing its applicability to heavy ordnance.

In the meantime, although it does not appear that Professor Schönbein had divulged his secret, Dr. Otto, Professor of Chemistry in Brunswick, has addressed a letter to the *Hanoverian Gazette*—since published in the *Times*—in which he states that he was led, from the researches of Pelouze, to infer that the cotton was soaked in nitric acid of a certain strength, washed, and dried. Thus the secret of the gun-cotton became at once public. On the 4th of October, Dr. Otto performed certain experiments with his preparation, the results of which satisfied him that it must be identical with the gun-cotton of Schönbein. At a late meeting of the Academy of Sciences, in Paris, M. Arago gave an account of certain experiments performed with prepared cotton by a M. Morel, the results of which satisfactorily showed that it was capable of forming an admirable substitute for gunpowder; and, with all that enthusiasm which characterises our Gallic neighbours, M. Arago pictured an army entering on a campaign, with a few bales of cotton and a few gallons of nitric acid, making their own explosive cotton as they required it! M. Morel, it is stated, has secured a patent for France; and, so far as we can ascertain, he has acquired his knowledge of the subject independently of any communication from M. Schönbein. The latest intelligence is, that the last mentioned gentleman has procured a patent for England and her colonies.

Having thus given a slight history of what has transpired publicly on this subject, we now propose to consider how far M. Schönbein has a claim to be regarded as the inventor of gun-cotton, assuming that he employs nitric acid like Dr. Otto and M. Morel.

About six or seven years since, it became pretty generally known to the chemists of England, from the researches of M. Pelouze, that when woody fibre, whether as paper, sawdust, or linen, was saturated with strong nitric acid, washed, and dried, its properties were considerably altered. A principle called *xyloidine* was produced; and the woody fibre, although all the acid was washed out of it, burnt rapidly, and often with explosive violence. We saw this experiment made about six years since; but, from that time, the subject appears to have received from chemists no particular notice, until the alleged invention of Schönbein recalled the attention of Dr. Otto and others to the researches of Pelouze.

In various chemical works published in 1842–3, the action of nitric acid on woody fibre is especially mentioned. Thus, in Turner's Chemistry (ed. 1843, p. 1222,) it is stated, in reference to woody fibre, "In strong nitric acid sawdust dissolves; and, on the addition of water, a white insoluble powder is deposited, which contains nitric acid, and explodes when heated." In Graham's Chemistry (ed. 1842, p. 295,) the facts are more explicitly stated, as the following extract will show:—"Nitric acid, in its highest state of concentration, exerts no violent action upon certain organic substances, such as lignin or woody fibre and starch, for a short time, but unites with them, and forms singular compounds. A proper acid for such experiments is procured with most certainty by distilling 100 parts of nitric acid with no more than 60 parts of the strongest oil of vitriol. [These are exactly the proportions recommended by Dr. Otto.] If paper is soaked for one minute in such an acid, and afterwards washed with water, it is found to shrivel up a little, and become nearly as tough as parchment, and, when dried, to be remarkably inflammable, catching fire at so low a temperature as 356°, and burning without any nitrous odour (Pelouze.")

Professor Graham here, it will be seen, gives, in 1842, an outline of Pelouze's discovery, and, by the substitution of cotton for paper, it becomes the so-called discovery of another in 1846!

We shall now give an extract from the Traité de Chimie of M. Dumas. At page 12, tome vi. published in 1843, this author says:—"When cloth (either linen or cotton,) or a sheet of paper, is soaked for a few minutes in nitric acid of a specific gravity of 1·4, and afterwards washed in water, the xyloidine formed at the expense of a part of the vegetable tissues remains locked up in the fibre, rendering the paper and the cloth impermeable to water, and much more combustible. These properties suggested to M. Pelouze the idea of employing them in the manufacture of cartridges for artillery."*

M. Schönbein may have known nothing of Pelouze's researches: nevertheless, it appears to us clear that Pelouze was the real and original inventor of the process for preparing gun-cotton. His countryman, M. Dumas, informs us that he even announced the plan of applying these substances to the very purpose for which M. Schönbein has taken out a patent.

With respect to the process for preparing this substance, we have found that the acid best adapted for the purpose is that recommended by Pelouze. It is obtained by distilling ten parts of dry nitre with six of sulphuric acid: and it is strictly speaking the acidum nitrico-nitrosum. The cotton wadding should be thoroughly steeped in this acid for about three minutes, then plunged into water, and washed under a current, until litmus paper is no longer reddened by the washings. The cotton should be well squeezed in a cloth—picked out and gently dried before a fire. It requires some time to dry a mass of it thoroughly; since the porous material is very retentive of water. When at all humid it burns slowly and without explosion. When dry it burns suddenly with a bright yellow flame, a feeble detonation, and leaves no residue. Compared with its bulk its explosive properties do not appear remarkable: but when compared with its weight they are greater than those of gunpowder. The explosion is less rapid than that of the fulminating compounds of the metals; but more rapid than that of gunpowder. When well prepared it explodes at a very moderate heat (about 420°,) gives off scarcely any visible smoke, and leaves no residue. This substance has a great advantage over gunpowder in the fact that when well prepared the whole of it is dissipated in gas—carbonic acid and nitrogen. With gunpowder there is always a residue of carbon and sulphuret of potassium,—the latter tending to corrode the metal. Experiment only can determine the relative gas-producing powers of the two substances. The gun cotton may be inflamed over gunpowder without igniting it. There is nothing extraordinary in this: hydrogen may be inflamed in contact with gunpowder without kindling it, and a small quantity of alcohol may be burnt over it with

* Lorsqu'on trempe pendant quelques minutes un morceau de toile ou une feuille de papier dans l'acide azotique à 1·4 de densité, puis ensuite dans l'eau, la xyloidine formée aux dépens d'une partie des membranes végétales, reste interposée et rend le papier et la toile imperméable à l'eau, et beaucoup plus combustibles, propriétés qui ont suggéré à M. Pelouze l'idée d'appliquer ces enveloppes à la confection des gargousses pour l'artillerie.

a like negative result. Gunpowder requires a red heat for its ignition, and unless one particle of the mass reaches this temperature it does not explode.—There can be no doubt that the gun cotton explodes at a much lower temperature than gunpowder, and as percussion will produce the same effect as heat, it should be cautiously handled when thoroughly dry.*

It is altogether a remarkable substance; and may, upon trial, entirely supersede gunpowder. Should this be the case, it may have the effect of rendering nitric acid cheaper and more abundant, since the employment of nitre for the manufacture of gunpowder would be no longer necessary.

We have prepared this substance with the strongest nitric acid 1·52—with the acid at 1.4, distilled as above stated, and with a mixture of nitric and sulphuric acids; but, according to our experiments, the most certain and satisfactory process is that above described. The cotton is undoubtedly highly oxygenized, and it may be worth inquiring, whether in a highly dried condition it may not be liable, in masses, to spontaneous combustion. It might be hazardous to keep a large store of it completely dried. The presence of a very small quantity of moisture is sufficient to counteract its explosive properties; and as cotton from its porousness is very hygrometric, it will be proper to consider how far this property may interfere with its employment as a substitute for gunpowder. If it be true, as it is reported, that the government have ordered a hundred weight of this substance from Basle, in Switzerland, the cost of transport will far outweigh the cost of the materials. This quantity might have been made at Woolwich under the superintendence of M. Schönbein, and rendered fit for use at a small expense in the course of a few hours.

In conclusion we shall observe that although we think the merit of the discovery is due to M. Pelouze, yet M. Schönbein deserves credit for having at least called public attention to the subject. He has, however, been the involuntary means of making the practical value of M. Pelouze's researches well known. His secret has transpired in spite of his attempt to conceal it. *Suum cuique.*

3—*Animal Chemistry.—Can a substance be obtained from albumen, &c., free from sulphur? is there such a substance as proteine?*—London Medical Gazette for August, 1846.

In all of these kinds of fibrine (of fish), sulphur could be readily detected; nor was it found possible by any of the methods which have been hitherto described, to obtain either from fibrinous matter or from albuminous substances, a body destitute of sulphur. The analysis of the milk detailed in a previous part of the report, afforded excellent opportunities of testing the accuracy of the idea supported by some of the continental chemists, that a substance can be obtained by the action of potash upon albuminous substances which contains no sulphur. On repeating the experiments that have been detailed in books upon a considerable scale with caseine or curd of milk, which were carefully conducted by William Parry, Esq., late of H. M. 4th Regiment, it was uniformly found that the resulting product contained sulphur. By this statement, certainly it is not meant to infer that such a substance may not exist, but only that the writer has not been able to procure such a substance as proteine, by following most scrupulously the directions supplied by its original describer, and others who have copied his descriptions. His scepticism on this subject originated some years ago, when engaged in researches on the brain, an abstract only of which has been published in Liebig's edition of Geiger's *Pharmacie.* The process of analysis for this intricate combination consisted in dissolving the albuminous part of the nervous system in dilute caustic potash; a reagent which exerts no soluble power on the peculiar matter of the brain, but combines with it, forming an insoluble salt. The potash solution, on being withdrawn

* In two experiments since made with a mercurial bath, gun-cotton exploded at 425°; but gunpowder did not explode until the thermometer rose to 545°.

from the insoluble matters, yielded by neutralization with acetic acid a substance which ought to have been *proteine*, because it was obtained by precisely the same process as that which has been described as the best for procuring that substance. But on dissolving after washing in potash, adding acetate of lead, and boiling, it gave an abundant black precipitate, indicating the presence of sulphur. This experiment was shown to Prof. Liebig by the writer at the time (1842), and it is believed that that distinguished chemist considers the existence of proteine problematical.—*Dr. R. D. Thomson.*

4.—*On the Presence of Sugar in Healthy Blood.* London Medical Gazette for October, 1846.

For some years past the attention of chemists has been directed to the remarkable property possessed by certain organic substances, of acting in the manner of ferments on other organic substances—and of transforming them into proximate principles such as glucose, dextrine, sugar of milk, lactic acid, butyric acid, &c. Numerous facts of high interest to physiology, especially in relation to the changes undergone by starch during the process of digestion and assimilation, have been latterly furnished by MM. Payen, Bernard and Barreswil, Bouchardat, Mialhe, and others.* Amongst these facts, there is one which now seems to be generally admitted as proved, namely, that mixed saliva, the pancreatic fluid, and the gastric fluid, when alkaline, have each the property of converting by *catalysis* (that is, simply by contact) the starchy principles of food into *dextrine*, and then into grape-sugar, or *glucose.* Having been lately engaged in delivering a course of lectures on digestion, Magendie has repeated the greater number of the published experiments performed by the above-mentioned chemists on this subject. Shortly after commencing his researches, Magendie found that the power of effecting these transformations of starch is far from belonging exclusively to the salivary, pancreatic, and gastric secretions, for he discovered that it was possessed by all the animal fluids which he examined: as the bile, acid urine, the seminal fluid, &c. Moreover, on applying to the different animal tissues and organs the process practised by Bouchardat and Sandras with the pancreas, namely, digesting them separately in water at a temperature of 40° Cent. (104° F.) he found that the filtered fluid, when kept at this temperature, exerted a decided transforming action on starch placed in it. He tried in this way portions of brain, heart, lung, liver, kidney, spleen, muscle, membrane, &c., &c., and observed that they effected the transformation with different degrees of energy and rapidity; yet as to the fact of their transforming power there could be no doubt. Among the various animal fluids which he found to act on starch, serum of the blood was one. On mixing a portion of starch with fresh serum at a temperature of 104° F., he found that in a few moments this substance could no longer be detected by its ordinary tests, and at the end of about a quarter of an hour the mixture quite manifestly contained sugar, and an insipid mucilaginous substance which was converted into sugar by the action of acids and by alkalies, and which was nothing else than dextrine. Blood itself, immediately after being withdrawn from a vein, possesses the same transforming power over starch: if to 200 grammes of blood 5 grammes of starch, boiled in 100 grammes of water, be added, the transformation will be found complete at the end of about four hours, and not a trace of starch will be detected in the liquid when deprived of its fibrine, globules, and albumen, whilst the presence of dextrine and glucose in it will be quite evident, and both substances may be easily extracted.

Having obtained such results, it appeared to Magendie that it would be interesting to find out whether the blood during its circulation in the living animal possesses the same property of thus acting on starch. In order to ascertain

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* For an account of these researches, see Med. Gazette, vol. xxxvii, p. 788.

this, he injected a certain quantity of starch-paste into the jugular of a rabbit, which (for reasons presently to be mentioned) had been kept fasting for three days. The blood of the animal was examined previous to the injection, and presented not a trace of sugar. It was examined again immediately after, and not any of the starch which had been poured into it could be detected, by means of iodine; but in place of it there was certain evidence of the existence of sugar. The blood was now analysed from hour to hour, and the quantity of glucose (or grape-sugar) was found to increase progressively for about five hours; after which it gradually diminished, and had disappeared entirely at the end of seven hours after the introduction of the starch into the veins.*

This experiment, which proves the power of the blood to produce, and probably also to destroy glucose, has been repeated, with like results, on dogs. It was also performed on horses, but generally produced such severe disturbance in the circulation, often occasioning death, that no sure result could be obtained: even the introduction of a small quantity of milk into the veins of a horse almost invariably causes death.

MM. Bernard and Barreswil have recently found that after herbivorous animals have fasted for some time the composition of their urine is exactly similar to that of the urine of carnivora.† Acquainted with this fact, Magendie kept fasting for three days the rabbit into whose veins he was going to inject the starch. At the time of the experiment its urine was in consequence acid, limpid, and loaded with urea. When examined a few moments after the injection, it was highly interesting to observe that it had undergone a comeplete change, having resumed in this short time the character common to the healthy urine of rabbit,—being alkaline, turbid, and containing no perceptible quantity of urea. This result, which was several times verified on rabbits and horses, is one of considerable interest, and tends to prove still more clearly the close connection which evidently exists between the composition of the blood and that of the urine.

The introduction of starch into the blood by means of injection, being, however, an unnatural proceeding, it became desirable to ascertain whether, when taken into the circulation during the ordinary process of digestion, starch undergoes conversion into sugar. To determine this point, a dog was fed for several days exclusively on cooked potatoes, mixed with a small quantity of lard. As soon as its urine became turbid, alkaline, and devoid of urea, some blood was drawn and examined. It was found to possess a notable proportion of glucose, and a quantity of another principle, soluble in water, insoluble in alcohol, and presenting the other characters of dextrine. The urine of this dog, however, contained no sugar; a fact which is important in relation to the etiology of diabetes, inasmuch as it shows that sugar may exist in the blood, yet without being found in the urine. This circumstance was noticed by MM. Bernard and Barreswil on introducing glucose directly into the circulation: the same observers found, however, that when cane-sugar was introduced into the veins, it very shortly appeared in the urine. In the blood of horses fed exclusively on oats, Magendie found both dextrine and sugar, although the urine was acid, clear, and contained urea. He has not yet made the experiment on man, but he considers it tolerably certain that during the digestion of amylaceous principles of food, sugar will be naturally present in human blood. There is reason therefore to suppose, that, although found in the blood of diabetic patients, yet it may also occur in the blood of perfectly healthy persons, as a natu-

* In order to detect the presence of sugar, Magendie states that the blood is to be received into boiling water, which separates and coagulates the albumen and serum, and takes up the soluble principles: the liquid is then filtered, rendered neutral by a few drops of acid, evaporated, and treated with alcohol, &c.

† See Medical Gazette, vol. xxxvii, p. 870.

ral consequence of the digestion and absorption of starch.* The following experiment may serve to show how the applications of chemistry to physiology may still further throw light on some of the mysteries of the blood. If into the veins of an herbivorous animal whose urine is alkaline, turbid, and almost devoid of urea, a certain quantity of recently prepared meat-broth be injected, the urine will in a very few moments assume the character peculiar to the urine of animals feeding on animal food; that is to say, it will become limpid, acid, and loaded with urea. May one not conclude from such an experiment, that the presence of urea in the urine is dependent on the composition of the blood, and that the origin of this substance is not always the one usually attributed to it?—*Comptes Rendus*, 27 Juillet, 1846.

AMERICAN MEDICAL INTELLIGENCE.

1. *Fracture of the Neck of the Femur.* By E. Geddings, M. D., Professor of Surgery in the Medical College of the State of South Carolina.

Under this title we find an exceedingly interesting paper in the November No. of the *Southern Journal of Medicine and Pharmacy.* Dr. Geddings first discusses the question whether it is possible for ossific union to take place in fractures of the neck of the femur within the capsular ligament, and cites the leading authorities on the subject both ancient and modern. The following is the result of this investigation:—

"Much discussion has grown out of a subject, supposed by many to be new, but which is as old as the science; but after all that has been said and written, we are obliged to settle down upon what was well known before,—that although fractures of the neck of the femur are capable, under favorable circumstances, of being healed by bony union, in a large proportion of cases this fortunate result cannot be realized."

He then cites a large number of "authentic cases of ossific union of this species of fracture," and adds to them the following which fell under his own observation and management:—

"Cyrus, a very healthy and active negro boy, aged about 18 years, resident on my plantation, early in the spring of 1843, attempted to accompany two other fellows on a visit to a neighboring plantation. His comrades, averse to his going with them (which he persisted in doing) when, about a quarter of a mile from the negro quarter, and while passing over a bridge, one of them

* To prove that the digestion of starch may occur quite independently of being acted on by the pancreatic fluid, Magendie had the pancreatic ducts in a pigeon destroyed, and after six weeks had the bird killed in such a way that its blood was all preserved. Previous to its death it was fed on grain, and was in good condition.—The blood was found to contain a notable quantity of sugar. The pancreas was in great measure atrophied, and its ducts did not communicate with the intestine. This result seems singular, for one would suppose that a bird deprived of its salivary glands and pancreas would not be able to digest starch; it is proved, however, by the above experiments that bile also acts upon starch, and that when absorbed and carried into the circulation it soon becomes transformed.

seized him, and threw him down with great force upon the bridge, where he was left alone. Notwithstanding the severity of the injury, he managed, unassisted, to return to the quarter. On entering his mother's house, she, finding that he complained of great pain, questioned him as to the cause, when he informed her, that in passing through the yard in the dark, he had fallen over a log of wood, and sprained his hip. He was seen the next morning by the overseer, who, misled by his false statement, contented himself with directing frictions with some stimulating liniment. This course was pursued for about a week, at the end of which time finding no improvement, a careful examination was made, when a very perceptible crepitus demonstrated the existence of a fracture of the femur. On being now closely questioned, he confessed, for the first time, how the accident had occurred. Under these circumstances, the overseer thought it most prudent to put him carefully in a cart, and send him to me in the city, a distance of fifteen miles. On his arrival, I found him suffering great pain, particularly when moved. A careful examination revealed the following conditions. Eversion of the foot; an audible crepitus in the vicinity of the hip-joint, especially when the member was rotated—the great trochanter describing a smaller circle than natural, and a very trivial shortening of the limb, which, when moved in the direction of the axis of the body, could be easily made to perform a limited motion upwards and downwards, attended with a very obvious grating crepitus.

From all these circumstances, I did not hesitate to diagnosticate the existence of fracture of the cervix femoris, and thought it probable the fracture was within the capsular ligament. Considering the youth and good health of the patient, I deemed it a favorable case for the procurement of ossific union. With this view, he was placed on his back, upon a proper mattress, and the limb was carefully put up in a semiflexed position, in the fracture apparatus invented by my friend, and former colleague, Professr N. R. Smith, of Baltimore. But in addition to the pelvic strap, with the view of restraining, as far as possible, all motion at the seat of fracture, the hip-piece of the apparatus was still more securely fixed, by successive turns of a spica bandage, extending, alternately, around the pelvis, and the upper part of the thigh.

He was kept carefully confined in this apparatus for tenweeks, suffering but little, except from his constrained position. At the end of this time, the adjustment was removed, and he was required to exercise the limb gently every day. By the end of the third month, he could walk with considerable ease with a stick, and before the end of the season he was able to resume his duties on the plantation. The limb, however, was shortened about an inch and a half, and the foot turned slightly outward, which occasioned a little halt in his gait. Notwithstanding this, Cyrus soon became one of the most active and sprightly negroes of the gang, and during the crop season of 1844-5, he was constantly engaged at the plough, without experiencing any detriment from his previous accident.

During the winter of 1845, he was, from some unknown cause, seized with idiopathic tetanus, which resisted all treatment, and carried him off in about six weeks.

A careful examination of the hip-joint was made after death, which exhibited the following appearances: The capsular ligament was considerably thickened. A fracture had extended through the neck of the bone at its junction with the head. The cervix was entirely absorbed, and a vertical section through the head and neck, showed the former solidly united to the shaft, by a white line of bony union, extending upwards, in a perpendicular direction, from the tip of the trochanter minor. A considerable solid bony deposit had taken place externally, corresponding to the line of fracture. This is especially the case upon the anterior and superior part. At the former point, this deposite overlaps the corresponding portion of the head of the bone, throughout one-third its extent, and is perfectly consolidated with it, the corresponding cartilaginous covering having been absorbed; while above, the new bony matter projects at least half an inch. The subjoined lithographic plate will serve to convey a general idea of the appearance of the specimen, but, unfortunately, many of the details cannot be delineated in a drawing.

This case furnishes, I conceive, unequivocal evidence of the possibility of of the osseous union of fractures of the neck of the femur, taking place within the capsular ligament, and when added to the numerous facts and authorities quoted in the preceding pages, we have a body of testimony adequate to convince the most skeptical, that, although our efforts to attain this end often fail, success, under judicious management, may be realized in a very cousiderable proportion of cases. In several instances referred to above, bony union took place, even though all treatment, except rest, was neglected, and it is reasonable to infer, that, if instead of abandoning these cases to nature, as hopeless, —a practice which, it is feared, the high authority of Sir A. Cooper has contributed to render too general, they were submitted to careful and judicious management, perfect recoveries would very often take place.

A review of the preceding observations will show, that from the earliest periods of the healing art, great difficulty has been experienced in obtaining bony union of fractures occurring in the neck of the femur, within the capsular ligament. The numerous authorities quoted exhibit a striking concurrence of opinion upon this subject.

That notwithstanding this difficulty, union by ossific matter has been obtained in a sufficient number of well anthenticated instances, to make it incumbent upon the surgeon, in all cases, to use his best efforts to secure such a result. With the view of placing this important maxim in the strongest point of view, I have taken the pains to collect and embody, from all the available sources within my reach, as far as my limited time would admit, every well authenticated case of this variety of fracture, which has been found united by bone. I could very easily have swelled the list, but have carefully avoided every case, where I had reason to suspect the fracture to have been external to the capsule. In this way, some have no doubt been excluded which ought to have been introduced, and others have probably been overlooked, for want of time to extend the examination of authorities.

It still remains to inquire into the causes which impede bony union; to elucidate more clearly the diagnosis of this species of fracture; and to determine the most successful method of treating it. These questions I propose to discuss in a subsequent article, to which this should be considered merely as preliminary."

2.—*Two Cases of Double Vagina.*—By Professor Meigs. (Medi- Examiner, December, 1846.)

To the Editor of the Examiner.

On the — October, 1846, I was called to see Mrs. ———, aged 20 years, in labour of her first child. She is a remarkably well formed and comely woman.

The pains were sharp and frequent, evidently of the kind called dolores præparantes, or grinding pains. After some time, as they had become more violent, I examined the state of the os uteri, which was of the size of a half-dollar, the head of the child presenting, and the ovum unruptured. In the course of an hour more, I examined again, and the os uteri was then nearly dilated. While pressing the palp of my index finger to the left side of the pelvis, it caught in a seeming bridle, which at the instant made me fear the cervix uteri had been broken, so as to detach a semi-circular portion of the os uteri, for the pains had been exceeding sharp, and their returns had been announced by violent cries. It was but a moment that I indulged the idea of a rupture of the cervix, for upon pushing the index farther, and flexing the finger, I found I could draw the point of it outwards, pulling along with it the bridle in question. Still I did not understand the case until, having withdrawn the indicator, I examined with it the structure of the external parts, and then learned that the lady was possessed of a double vagina. Supposing that such a revelation would not be agreeable to her, I kept my own counsel, hoping that the child's head would come down through the right or the left channel without injuring the septum. But after the head escaped from the circle of the os uteri, the bridle or partition would not go definitively to the left or to the right, although I thrust it first one way and then the other. The tie was so strong that the fleshy septum extending from the anterior to the posterior columna of the vagina, would not admit of the dilatation of the lower or outer third of the tube. And as the lady was very strong, and had powerful uterine pains, I began to perceive some danger of the vagina being ruptured by the vain efforts for expulsion.

I now explained to the monthly nurse, and to a relative of my patient, the cause of the delay and the necessity that had arisen. I therefore procured the requisite permission to expose the parts to an inspection. Upon this, the two orifices of the vagina were seen to be exactly alike, and the partition stretched across the head from front to rear of the passage, which by it was wholly prevented from dilating.

I now with a strong scissors divided the wall by a single stroke of the instrument, whereupon the child's head advanced, dilated the os magnum, and was speedily delivered with safety to both the mother and her infant. She never complained afterwards relative to the operation, and within in a month I met her on foot in the streets.

A week later, I was called to a lady in her 30th year, in labor of her first child. Upon examining the state of the os uteri, I found the circle not much bigger than a quarter dollar, with thin margin, and within it the penis of the child; the scrotum being detected within the os uteri after the pain ceased.—As it was night, I went to another apartment and slept an hour, when being called, I found the os uteri very much dilated, and a buttock, near which was the right foot, presenting.

While inquiring into the state of the cervix, I hooked my finger into a bridle, just as I had done in the case above mentioned, and I confess that the same thought was obvious to me, viz: that she had broken off a half ring of the circle of the os uteri, but I immediately afterwards discovered that I had another case of double vagina under management. In this case the partition was very firm and thick, extending from the os magnum almost up to the os tincæ. I inspected the external structures, and the two vaginas were each perfect and alike, included within labia pudendi common to both.

I was glad to find that only one foot of the child would come down, being fearful that if both should descend, I might not readily prevent one from entering the right and the other the left vagina.

I now disengaged the right foot and brought it down the right channel, the left leg was flexed upon the belly and thorax of the fœtus. With a little assistance the foot was delivered and the buttock of the child coming downwards, thrust the vaginal wall to the left, and so the trunk was delivered.—I had great difficulty to extricate the head of the child, which remained long in the vagina; the infant breathing from time to time the air that I admitted through the hollow of my hand and fingers to its mouth and nostrils. The child, a male, was alive, and is in good health; the mother is quite well recovered.

Some years ago I was called by the late venerable Dr. Ruan to consultation upon a case of double vagina in a primiparous woman. I delivered the child with the forceps through the right canal, without difficulty or any injury, and had some five weeks later an inspection of the parts, which, as I remember, were very similar to those described in my second case above.

November 14th, 1846.

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3. *Practical Remarks on Congestive Fever.* By E. F. Bouchelle, M. D., of Columbus, Mississippi.

In perusing the last edition of Stokes and Bell's Practice of Physic, a work embodying many valuable principles of medicine, with great experience and learning, I am forcibly impressed with the views of Dr. Bell, as almost coincident with my own, as it regards the efficacy of opium in the treatment of congestive fever. I have long been satisfied in my own mind, that the usual mode of treating congestive fever, the plan pursued by most of the physicians of the South West, is not only improper but dangerous; as its direct tendency is to strengthen the disease and hasten the stage of colapse. The views which I now entertain on the subject of congestive fever have been promulgated throughout the sphere of my acquaintance, since the summer of 1837.

It is perhaps unnecessary to advance, in detail, a theory of the disease in question, suffice, for all practical purposes, to remark that all the leading phenomena of the disease are referaible to derangement of the organic system of nerves, more particularly; the excitement of congestive fever is irritable excitement, and in most cases so excessive, that it soon sinks the system into collapse unless moderated.

Such being a *syllabus* of my pathology, it necessarily follows that in its treatment I invariably call in requisition those remedies whose known tendency is to allay nervous irritation, tranquilise the system, and produce sleep. Such remedies are to be found under the class of *narcotics*, and in another *great remedy*, belonging to no particular class, which the hand of a merciful and all-wise providence has disseminated throughout the universe; a remedy equally accessible to the rich man and the poor man, as it abounds in all places, and can be procured "without money and without price." I allude to *cold water*. The most powerful combination, however, to prevent the recurrence of a paroxysm of congestive fever, when the disease observes a remittent or intermittent character, is morphine and quinine. In the whole course of my observation I have never known the congestive fever to observe any other than the intermittent or remittent type; unless the constitution is so frail, or the disease so violent as to destroy the patient in the first paroxysm, which it often does; moreover, it is a rare circumstance if an individual, with the most robust constitution, survives a second or third paroxysm. Most usually, during the paroxysm, I prescribe *laudanum* and cold water, which rarely fail to conduct the patient safely through; and during the interval, morphine and quinine, to prevent a recurrence. The following is the prescription usually observed:—R. Sulph. quinine, grs. xxiv.; Sulph. morphine. grs. ij.; M. f. 12 pills,—to take one sufficiently often to keep up a slight state of stupor or narcotism; that is, every hour or two, *pro re nata*.

I, for one, am unfriendly to large doses of quinine, and am certain that two or three grain doses repeated at proper intervals, will insure all the good effects of that potent salt, without incurring the risk of loosing them; not only loosing, but inflicting an injury to the nervous system. Our firm belief is, and that opinion is founded on experience, that as an antiperiodic, two grain doses of quinine are as efficacious as larger doses; and that in the same proportion that we augment the dose, in the same or a greater degree do we diminish the specific action of the article; also, that its combination with a narcotic enhances its antiperiodic powers in an eminent degree.

There is a secret in connection with quinine, which, probably, very few physicians have observed; that is, that its administration during the stage of excitement in fever is often hurtful, and at best, uncertain; in order to insure a favorable influence in such cases, we have only to combine it with an anodyne. It is rare that quinine will exert any other than a favorable influence during the hot stage of fever, provided morphine be blended with it. Its most common effect under such circumstances, is to lessen the force and frequency of the pulse, relax the skin, and produce sleep. The above combination is an admirable prescription in summer fevers attended with great gastric irritability,—it must be given in the form of pills. Another valuable combination, where the excitement is inordinate, is quinine, tartar emetic and morphine,—provided there is no great nausea. The above remarks refer only to summer and autumnal fevers, of open excitement.

Before leaving this subject, I will remark that 20, 60, and 100 grain doses of quinine are very common these days. However, such doses are not prescribed, or if so, by *very few* of the scientific physicians of Mississippi and Alabama. In the meanwhile I will not presume to deny that peculiar modifications of disease, may render *such doses* applicable in more southern latitudes; generally speaking, these huge doses are given by that numerous class of mountebanks and imposters who infest our country; men who recognise no essential difference between the stomach of a human being and that of an ostrich; between the constitution of a man and that of a horse! Would to God that the prescribing of large doses of quinine was the only species of quackery practised in the West! Calomel, and other remedies, are given in equally as large quantities; the success of which *energetic empiricism*, our numerous graveyards bear melancholy though silent testimony, to say nothing of the thousands of constitutions literally destroyed by as *many anomalous diseases!*

There is a maximum and a minimum dose for any article of the materia medica—a fact which should never be forgotten in clinical practice—and when we transcend either degree, we either produce no effect at all, or we do mischief.

There is no class of remedies, however, whose dose is more variable than that of narcotics. Indeed, we can sometimes give them *ad libitum*, with very little effect; as we all know that under certain states of the nervous system arising from excessive pain, the system can scarcely be composed by opiates. Who has not seen this verified in prescribing for acute gout, the passage of biliary calculi, spasmodic cholic, tetanus, &c., &c. One of these peculiar conditions of the system occurs in congestive fever,—as we are certain that during one of its paroxysms nothing short of mammoth doses will conduct the patient safely through, and prevent collapse; which extraordinary resistance to the usual influence of opiates only argues the propriety and necessity of such remedies. I do hope, for the sake of human life, and the honor of medicine, that the day will ere long arrive when physicians will be convinced, that calomel, and purgatives generally, French brandy and other stimulents, mustard cataplasms, blistering plasters, &c., are not the remedies for congestive fever, the endemic of the Mississippi valley, whose very name, in many places, is associated with all the horrors ot the grave, in consequence of its great fatality. All purgatives, all stimulants, internal or external; all irritants—are injurious in congestive fever. So long as I pursued the plan of *correcting the secretions, and stimulating by brandy, camphor, camphor and quinine, ammonia, pepper, &c., &c.*, I lost patients. But when, on the other hand, after much reflection, I had changed my pathology of the disease, and adopted the *cold water* and *anodyne* practice, my labors were crowned with success, and have been ever since. In truth the most violent forms of congestive fever will as certainly yield to the anodyne treatment, as will a local inflammation yield under depletion. I do not regard quinine as a stimulant, it has tonic properties, and in combination with an anodyne, is the most powerful sedative in general use. (There are many sedatives very active, which are not used in the common routine of clinical practice.)

We have said nothing definite as yet about cold water in congestive fever, but will do so in very few words. How is the cold water used in congestive fever? Internally and externally; a pleasant remedy, and one which any patient will grasp eagerly, and without much persuasion. I use the could douche in collapse to arrouse the system to reaction, which it will more often do, than any other means that I have ever seen essayed. I have seen many patients, as it were, moribund; cold and clammy skin, thready pulse, sunken features, blue finger nails and lips, great epigastric oppression, and breathlessness, rescued, as it were, from the grave, by the *magic influence* of the cold douche. The cold water is not less useful during the paroxysm, to allay general anxiety, distressing vomiting, thirst, and internal heat. I allow the patient to drink it freely,—it gives great relief; it removes, in connection with laudanum, irritation of the ganglionic nerves, upon which the miserable epigastric oppression and gastric irritability depend, and seldom fails to conduct the patient safely through the paroxysm. How much more rational such treatment is, and, at the same time, how much more grateful to the languishing sick man, than the opposite plan of tormenting him *unto death* with heating stimulants and *blistering plasters!* How much more rational, than the opposite *vile* system of cramming his stomach with horse doses of calomel "to remove congestion" of the *darkest* and *foulest* of all places, "*the venous cavity*"!!! Would to God that Mississippi and Alabama could be relieved of the curse of R. A. C. quackery! Oh! ye shades of departed worth! ye ghosts of Hippocrates. Æsculapius, and Galen, how long will ye endure such humbuggery! Oh! "*venous cavity!*" Oh! calomel, and R. C. A. pills! inexorable monsters, who have slain your hundreds, why seek to demolish thousands! I am not jesting; no, I am serious.*

But, for the purpose of illustrating the most rational practice in congestive fever, I will submit one of the most violent cases I ever saw in Mississippi.

Case.—A particular friend, of vigorous constitution, was seized about midnight, on the 20th of September, 1845, with a slight chill, which was succeeded by vomiting and profuse liquid evacuations from the bowels. I saw the patient about nine o'clock on the 21st; his head was hanging over the side of the bed, and he incessantly vomiting or heaving; his features were sunken and pale; breathing rapid and difficult from congestion of the lungs: pulse feeble and very rapid, almost imperceptible at the extremities; lips blue, and tongue pale and moist; with a clammy exudation of viscid perspiration all over the surface. Indeed, I was surprised to find my friend on the very verge of the grave: that he was sinking rapidly into a deadly collapse. He complained of great thirst and universal heat; he would cry out, "My God, I must have fresh air, or I'll die; I am burning up!" when the pulse was gone at the extremities, and the skin cold. The friends around implored me to stimulate him, and apply sinapisms to the extremities. I refused, and immediately went to work in my own way. I gave him 100 drops of laudanum forthwith, and in a half hour gave 50 drops more, which he drank; seeing that the irritability of the system was so excessive, that the laudanum would not take effect unless repeated at short intervals,—in a half hour more, I gave him one 100 drops by enema. In an hour the vomiting stopped. My friend drinking cold water by the pitcherful. He very soon became tranquil, and fell into a deep sleep, with his mouth and eyes half closed,—the spectators around thought that he was dying; but I knew better, when I took hold of his hand and found that it was getting warm, and that the pulse was rising at the wrist. In the course of two hours more, my patient was under a full reaction; his skin warm and pulse full, beating eighty in the minute. He did not wake until sundown,—when he *got up*, dressed himself, and *went about his usual business!!*

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* I don't allude to Prof. Cooke; but to those who endeavour to treat the fevers of Mississippi, Alabama, &c., according to his theory. I respect the Professor; at the same time I am convinced of his delusion.

The next morning (22d,) I entreated him to take to his bed, and commence with the quinine and morphine, to prevent a recurrence of the paroxysm, which would take place about midnight,—he declined, stating that he was well.—However, the poor fellow was seized again at 1 o'clock on the 23d. In two hours he was vomiting forcibly, with frequent liquid dejections from the bowels; great dyspnœa, and small and rapid pulse, with cold skin. At daylight I saw him, and gave the first dose, which was 100 drops of laudanum. Seeing that he became worse, complaining of indescribable epigastric heat and oppression, I repeated the dose, which had no effect, as he soon became wild and unmanageable. I ordered 100 drops more by enema, in starch; at the same time allowing him to drink freely of cold water, acidulated with citric acid, which he drank in his derangement with all the avidity of a famishing animal. He soon became cold from head to foot; *no pulse*, skin cold and bathed in a viscid sweat, lips blue, eyes sunken, and features shrivelled; breathing slow and oppressed from congestion of the lungs. Indeed, the dyspnœa was so great, that he looked very much like a man suffocating. I ordered 100 drops more in enema, and applied two small sinapisms to the neck, one over each pneumogastric nerve, recollecting to have read of such things being useful in asphyxia, &c. In a short time the patient seemed more quiet,—drinking freely of cold water occasionally. At this juncture, a medical friend of experience, formerly of the United States Navy, stepped in and pronounced my patient *in articulo mortis;* however, before he had been present one hour, the pulse was rising at the wrist, and the skin began to get warm, and the patient to breathe with more ease.

In two hours more, my patient was lying in a profound sleep, with hot skin, and good pulse; with the warm sweat standing in great drops on his forehead. He awoke late in the evening, very much prostrated indeed. In a short time, I put him under the morphine and quinine, keeping up slight narcotism until the next period had passed in safety; when I gave a little blue pill occasionally, to restore the secretions. It is proper to mention here, that the use of laudanum and quinine, as above recommended, almost always leaves the system in a torpid condition, as manifested by a coated and dry tongue; so that convalescence will be tedious without the occasional use of a little blue pill, &c.

The patient whose case I have given, cannot bear the smallest quantity of laudanum when well. I could, if necessary, adduce other cases, showing conclusively that laudanum, cold water, and quinine, are the remedies for congestive fever. It is probable that the above patient would have died, had it not been for the plasters (size of a dollar) to the neck; or it may be that the laudanum had not taken effect until then.

The treatment which has just been detailed in a detached and hurried manner, with some little modification, is applicable to any form of summer and autumnal fever in Mississippi. There is no prescription better, in common fevers, to prepare the system for quinine, than morphine and tart. emet. in solution. Ordinary febrile excitement can resist its influence but a few hours. In conclusion: There is no class of remedies which exert so favorable an influence in all of the fevers of this latitude as the class of narcotics.

4.—*Quarantine in Philadelphia.*—(From the N. Y. Journal of Medicine.)

PHILADELPHIA, July 27th, 1846.

"DR. C. A. LEE,

Sir,—The following are the answers to your questions according to our quarantine laws, which are in force from June 1st to Oct. 1st, in each year.

QUEST. 1st. When vessels are required to be cleansed and purified, what is the mode and usual time required?

Ans. The cleansing and purification in the Port of Philadelphia has been as follows: The bilge water is changed until it is as pure as our river water; this is done in all cases on the arrival of a vessel at the Quarantine Station. No vessel is ever permitted to pass without this being done. When sickness prevails on board, all clothing is washed, bed and bedding well aired, cabin and forecastle washed and well cleansed, fore and aft runnings washed and perfectly cleansed and frequently thoroughly white-washed; it will require two or three days to perform the above.

Q. 2d. Are vessels coming from the intertropical ports in the summer months required to undergo a stated quarantine, and how long on an average?

A. No. When the passengers and crew are all well, no vessels are ever detained in consequence of their coming from certain ports.

Q. 3d. Has Yellow Fever been traced to vessels thus arriving within the last 20 years in your city?

A. No. We have had few or no cases within the last twenty years; if any, they were sporadic cases. The disease prevailed here in an epidemic form about the year 1819 or 21, and was satisfactorily traced by Prof. Jackson to domestic causes.

Q. 4th. The opinion of your physicians as to the contagiousness of the disease.

A. Almost every respectable physician believes in its non-contagious character.

Q. 5th. Are hides, skins, rags, hair and wool, unladen at your wharves in the summer months without undergoing quarantine?

A. Yes. But it is a practice prevalent at our quarantine station, to open all hatches and examine skins and hides: if found unsound and offensive, to remove them from the hold to dry and purify; if very foul, they are removed to the store-house and the vessel permitted to pass.

Q. 6th. How often are you obliged to order vessels laden with such cargoes (skins, hides, &c.), to discharge elsewhere than at your wharves?

A. Vessels are seldom required to discharge their cargo elsewhere; but when they have arrived at the port, and the cargo is ascertained to be in an offensive state, the vessel is required to lie out in the stream, and the cargo removed by lighters to some of the distant factories.

Q. 7th. When vessels are ordered to undergo Quarantine are passengers allowed to land?

A. Yes. By giving bonds in the sum of five hundred dollars, that they will not enter any of our populous cities within twenty days. In case the vessel is discharged within that time, they are also relieved from this obligation. This might happen in four or five days, all depending upon the cause of the vessel's detention.

Respectfully yours,
H. D. DIETRICH, M. D.
Port Physician."

5.—*Case of Cæsarian Section.* By Brodie S. Hernon, M. D., of Fredericksburg, Va.

Mrs. Russel, aged about 30, moderately robust, the mother of several children, fell in labour on the evening of the 1st October, 1845. She quickened early in May, and reckoned herself at her full time. In the course of the night the pains which had been paroxysmal became permanent, and there supervened tenderness to pressure, vomiting and high vascular disturbance. The labour gave place to peritonitis. The duration of the phlegmasia was a month, during which time the fluid contents of the uterus escaped, the abdomen shrunk, and the vaginal discharge became very offensive. On the 6th of November, the os uteri admitting two fingers, I made persevering efforts to dilate it and introduce the hand; the cranial bones of the child denuded of scalp, rough and sharp, rested on the uterine aperture. These attempts failing, ergot was freely given, which occasioned unequivocal pains, and procured the expulsion of a very putrid placenta. The patient continued to waste under irritating fever; the discharge from the uterus filling the room with stench. On the 16th of November, it being now manifest that the woman must die soon without relief, I determined on the Cæsarian section. She was placed on the table and the catheter introduced: an assistant gathering up transversely a large fold of the abdominal integuments. I thrust a bistoury through them, and, cutting outwards, made at one stroke an incision of the full length required. With the scalpel, division was now cautiously made at the upper point of the wound of all the tissues, including the uterus, which was found adherent to the parietes. The finger being introduced, the incisions were extended upon it, laying open the parts from the umbilicus to the pubis. A full-sized putrid child was readily extracted. The operation lasted half an hour, and was performed in the presence of Drs. Brown, Wallace, Willford, and M'Guire. Only one artery was tied, and that was in the integuments; there were no utero-placental vessels; the uterus did not contract in the least, but remained a large flaccid sac, receiving the impression of the liver at its upper part, this last organ being somewhat enlarged. It was sponged out, and then the integuments were brought together with stitches and strips. The patient took an anodyne, and a few hours after the operation expressed herself as feeling much more comfortable. With morphia at night, and quinine and nourishment in the day, the case did very well. The wound united by adhesions in nearly its whole extent. The woman is now—three months since the operation—quite well.

The obstetric student will find in Dr. Davis' great work, some interesting cases of gestation, in which the instinct of parturient expulsion was lost, and children were borne in utero for years, undergoing decomposition and being discharged by piecemeal. In Mrs. Russel's case, the operation was simplified, and the danger usually due to hysterotomy materially lessened by the adhesions of the uterus to the abdominal parietes. Extensive wounds of the abdomen seem often to be readily recovered from. A case occurred to Dr. Carmichael (now of Richmond), and Dr. Welford, in which a lad was ripped up and completely disemboweled by a boar, who, nevertheless, did very well; nay, even wounds involving the viscera may end favorably, as in the case of the lunatic related by Dr. Brigham, where a woman plunged a pair of scissors into her belly, tore out seventeen inches of bowel and cut it off, and yet recovered. In 1843, I performed the operation for inguinal hernia on a young man who had suffered strangulation a week. The gut was mortified, but all the symptoms were relieved, dejections were procured through the wound, and the patient recovered. There was artificial anus for a time, and, on one occasion, the bowel was prolapsed five or six inches through it, occasioning much difficulty in the reduction; but by the use of a truss, a recurrence of the accident was prevented, and defecation per anum became gradually established.

It must be admitted that these are fortunate cases, exemplifying a part of the aphorism "there are some you cannot kill," whilst we full often find in our experience abundant verification of its alternate, "there are others you may not touch."

6.—*Reply of Dr. A. L. to Dr. J. C. N., on the subject of Mesmerism.*

[In our September number appeared a review of a "Lecture on Animal Magnetism"—delivered by Dr. J. C. Nott, of Mobile. We sincerely regret, that it was inserted in our journal, as it has given rise to controversy of a personal character. Having admitted the criticism however, we felt bound in justice to admit the rejoinder, and the same feelings oblige us to insert the following. But, all the parties have now had a hearing, and this controversy, so far at least as this journal is concerned, must cease.—Eds.]

[*For the New Orleans Medical Journal.*]

" Ce n'est pas la *Science* que fait le medecin heureux, *c'est l'effronterie et le jargon.*"—Pasquerel.

" * * * * If she hath learned of an old wife in a chimney end, Pax, Max, Fax for a spell, or can say Sir Jno. Grantham's curse for the miller's eels,—why then beware, look about you my neighbours! * * * * * * * If any of you have a sheep sick of the giddies, or a hog of the mumps, or a horse of the staggers; or a knavish boy of the school, or an idle girl of the wheel, or a young drap of the sullens, and hath not fat enough for hen porrage, or butter enough for her bread, and she hath a little help of the *epilepsy, or cramp, to teach her to roll her eyes, wry her mouth, gnash her teeth, startle with her body, hold her arms stiff, &c. &c.*, then, no doubt, mother Nobs is the witch, and the young girl is *owl blasted.*"—[*Dr. Harsnett's Overthrow of Demonology.*]

A writer, in the last number of the New Orleans Medical Journal, signing himself "J. C. N., of Mobile, Alabama," whose identity certain remarkable phenomena have long since established as a mesmerizer of some notoriety, has in a short note made an onslaught upon me, for being, as he asserts, the author of a "feeble" article, published in the Mobile Register on 25th May last. In this article, he professes to find personal grievance of a mortal character, and yet, he has satisfied himself, by compromising his own dignity in a manner, for which, upon more mature reflection, he must feel thoroughly ashamed. Now, if the Dr. had made due application, in the due form prescribed on such occasions, I very much doubt if the author's name would have been withheld from him, but he has preferred, it would seem, to "walk around the spider's web for fear of being soiled." This, even if he were sincere, is too laughable, and if no other cause existed, would alone suffice to keep me in a good humour, and prompt me to regard the Doctor's spleen, as I verily do, with a spirit "more in sorrow than in anger."

Who could have believed, that reeking as he did with the sponsorship of that irrevocable *little* note, by which he has so shocked his readers, and discredited himself, who I ask would believe that *He* feared to be soiled? It is Lord Byron, I think, who speaks of the difficulty of soiling certain personages by pelting them with mud— : A dying patriot left this prayer "Let no man write my Epitaph," but our Dr. cannot trust posterity, *he has written his own in advance*, he has taken his apotheosis in his own hands, and if he also covets a verdict in advance, let me inform him, that he had succeeded in writing that epitaph in a manner calculated to afford his worst enemy a triumph, and his best friends the bitterest mortification. Verily, there has been seen nothing to equal the phraseology and decorum of that extraordinary *little* note, since the days of Urban Flatees, he who signalized himself by the gorgeous description, which he wrote of his flatulent family.

Or, was it really the feeble character of the anonymous article which induced him to walk around it? If so, it was a potent specimen of a feeble thing, if his readers are permitted to judge from its influence upon the equanimity of a gentleman professing to be so peculiarly scrupulous of his professional etiquette and decorum. It is an axiom, that "the flesh will quiver when the pincers tear," and by the writhings and contortions of the Doctor's, it is clear, that this "feeble" article has produced the most astounding results.

True, something of this may be due to that *Inspiration*, which the Dr's mesmeric prevision has attributed to it. One thing, however, is certain, should the Dr. be wrong, and it turn out, that neither *Moses* nor *St. Patrick* inspired its thunderbolts, he assuredly has acknowledged by his loud and piteous lamentation, that there is some potency in a rod, which, his thin skin confesseth, has "budded, blossomed and borne fruit in a day."

A few words, here, however merely to convince the Dr., that he has thrown a spent ball. At this stage of the world's advancement, which lifts it so far above the by-gone influences of ignorance and illiberality, a gentleman should not stoop to convert religious tenets into instruments of offence, for it is the grossest evidence of a weak cause ; nor could a "well balanced mind" so far forget its own propriety, as to be driven into resentment for such an abuse of good taste. A very sensible writer once remarked, that, "when a man takes up a pair of tongs, and exclaims, what a wonderful contrivance ! *it is clear that he has no tongs at home.*" Will the Doctor be pleased to apply this homely test to the sanction of *his* religious creed ? Would he not deem that man ungenerous, who would taunt his neighbour's faith, if it so happened that *he himself* (to the best belief of all who knew him) *had no such weak point of attack?* Or, if he pretended to have one, and upon close enquiry it turned out to be a sort of *Hybrid* faith, like the clown's frog—a creature anomalous and inexplicable, with "two hind legs before, and two fore legs behind, and no tail at all most ?"

Again, could the Dr., rigid sectarian as he is, excuse a man *notorious* for his unbelief of all religious creeds, should he assume to hold himself up as the scourge of unbelievers ? Or, would he not rather caution him to beware the brand, which the poet Burns applies so scathingly to that class of men, who, "like Judas Iscariot preaching the gospel, might melt and mould the hearts of all around him, *while his own kept its own incorrigibility" ?* Hence, when Jews, Gentiles or Catholics again awaken the long dormant sensibilities of the Doctor's piety and stimulate his apprehension for *their* souls, he might, though it bring down upon him the scorn of every liberal and refined community, at least console himself, that, like the Bishop of Novogorod, he erred in being "*over delicate*," and like that self constituted defender of the Faith, think assassination less odious to heaven, than three Lutheran churches.

Enough of this.—And now for the unkindest cut of all.—The Doctor knew my weak point—my vanity was a sore spot to touch, and its locale could not escape that *phreno-mesmeric* power, the indispensable handmaid to humbug. Like one of those immortal spiders, he has pounced down upon two of my essays, read to the Mobile Medical Society, in the absence of more important matter. Happy am I, if the "innocent mirth" they afforded, carried with it no greater sin— ; if it was free from grosser elements and *more questionable qualities.* The Dr. should not have envied me their renown, for surely *he was gorged with distinction.*

He had delivered *Two* lectures on the "*Natural History of man*," in which he undertook to treat of every thing in the known world, *et quibusdam aliis*, and came *very near* producing that desideratum in Cosmography, for which a waggish sophomore once gravely searched the college library, to wit, *A History of the World before its Creation ! ! !*—: He had insured to himself the distinguished notice of an able critic, who meted out to him a never to be forgotten justice through the pages of the *Southern Quarterly* :—He had made *discoveries* and proclaimed "new facts" on the properties &c. of *Black vomit*, *although the same thing had been done for science as far back as* 1828 *! ! !* *—He had

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* This casualty may be the result of magnetic sympathy, and need not imply design. Nor should it have been thought essential, had not the Dr. twitted me for troubling the society with endeavouring to "*prove points long since perfectly established*," which, by the way, I think very problematical. Moreover, as he is such a stickler for medical men, keeping themselves *pari passu* with the history of their profession, it does occur to me, that one who searches so much more thoroughly, than the "small potatoes" of the faculty, ought at least to have been informed of so important an item in the history of Yellow Fever. The opinions under consideration can thus be seen.

J. C. N., 1844.

—"A compound, evidently intermidiate between *blood* and *black vomit*, *this I shall show* hereafter, was blood in the transition state, *gradually turning black from the action of acid in the Secretion of the Intestines*." p. 280.

*** "There have been many speculations on the nature and formation of this fluid, *all of which have been unsatisfactory*. They are well known to the profession, *and I shall here merely state my opinions, and the facts on which they are based*."

*** "*And my opinion* is, that the black vomit is blood exhaled &c., and *changed black by the secretions with which it comes in contact. This chemical change, my facts go to show, is produced by one or more acids*." p. 281

*** "I have tested the black vomit in a considerable number of cases this summer, and in every instance *I have found it to be acid &c*: and the *aqueous portion* of that, which was taken from the stomach after death and *filtered*, in several cases *effervesced strongly with carbonates*." p. 280.

*** "*We have then established two important links in the chain: The black*

London Med. & Phys. Jour., 1828.

"*On Black Vomit*."

"Mr. *Lyons* says, that he soon convinced himself from an attentive examination of the Black vomit when ejected from the stomach, and the state and contents of the stomach after death, and likewise from an examination of the liver and tubuli biliferi, *of the identity of that fluid with altered blood. But he was at a loss to account for the mutation of the blood's colour to black*.

The black coloring matter of this fluid *was separated by filtration* in the easiest manner, and *its powerful acid qualities were proved by its active effervescence with alkalies*, as well as by its action on the enamel of the patient's teeth and by its powerfull acescent smell.

But it was not until I observed in the examination of recruits at this station, that an immense quantity of *muriatic acid* is constantly secreted from the skin with the insensible perspiration, that I brought to mind *Dr. Prout's discovery of this acid being the principal one secreted in morbid conditions of the stomach*, and with it, the idea of ascertaining *the effects of this acid upon the blood*.

Having made experiments of adding *muriatic acid* to blood, *the colour of the blood was instantly altered to a deep black*, and when diluted with water, evolved more "new facts" on the *Nervous System*, which we are to presume will hereafter be adopted as *fundamental* principles of that branch of Pathology.—And lastly, (quem deus vult perdere, prius dementat) has he not published that "little Folio of twelve pages, happy work!" *that* lecture on *mesmerism*, "published by request," *and pamphletized spontaneously?* Was not all this, glory enough for one day? Could he not have spared the spiders of the Fœtus? Or did he dread the stings of the one, and see in the other a type of his own abortions? But since the Dr. has seen fit, under the *sacra fames auctoris*, to follow the example of Dr. Johnson's schoolmaster, and like him, dedicate his *Primer* to the universal world, it would be on my part a sad want of professional liberality (seeing he has done as much for me) were I to withhold my "*feeble*" aid, in promulgating to that world *a specimen or two* from the many extraordinary revelations of his genius.

Imprimis :—"I some time ago left some powders of calomel and ipe cacuanha, to be given to a boy of 14 years old, every two hours through the night. In the morning, when I called, *I found the patient much better*, and the father, who sat up with him, informed me that the medicine had been given regularly, that *every dose had nauseated and vomited, and had also purged and sweated him.*—All the effects I desired had been produced, the fever had gone off, and the patient was very much better. (Now comes the cream)—I happened to cast my eye on a bureau in the room, (I wonder that did not puke and purge and sweat also) and to my surprize *there lay the powders.*—On investigation, it turned out, that the old gentleman, who had been much fatigued by watching, had given the boy every two hours, a tea spoonful of molasses, *but forgot to put the powder in. The Patient wasnotwithstanding vomited freely by every dose, and all the other effects which* HE *expected, were produced by his* IMAGINATION!!!"—[Lecture on Animal Magnetism—by J. C. N., South. Jour. Med. & Pharm. May 1846 ; page 277.]

Pause, gentle reader, and take breath. "Lusisti satis, tempus est abire," for if you can again read that boy's case, and the history of *those* molasses (to which the sympathetic powders of Sir Kenelm Digby, were a mere circumstance), without imminent peril to your peristaltic integrity, it must be, because you have the utmost confidence in it. What? a sleepy headed old man sits up with a sick boy, forgets to administer his medicine—the boy, by some inward gift, and under the po-

vomit in yellow fever of 1844 *was acid, and acids turn the blood black.*" p. 282

[See Amer. Jour. Med. Sciences, April, 1845.]

presented a liquid, which I should have declared from mere inspection to be black vomit.

This experiment has been tried at my suggestion by *Dr. Hope,* and the results have been the same, even when the muriatic acid has been so far reduced with water, as to be hardly capable of turning the edges of the teeth, and is supposed by Dr. Hope to be *an interesting adaptation of Dr. Prout's discovery to the explanation of the symptom of Black Vomit.*"

Do not these things "approximate?"

tent spell of the physician, pukes, purges and sweats *by imagination*, and is straightway healed ! and this, too, the "effect expected by the boy ! Is it usual for medical men to forewarn children, and invest them with the modus operandi of their formulæ ? Oh, what a jewel such a doctor must be, who thus converts drugs into sweets ! and for the disgusting compounds of the Materia Medica, supplies his patients with potent and sweet doses of *Imagination*, by which alone

> " He pukes, he purges and he sweats 'em,
> And if they dies, why then he lets 'em. "

It is Homœopathy in the ascendant, and is the most infinitesimal amount of operative medicine on record. The boiling of soup in the sunshine, with the shadow of a starved chicken is aqua fortis in comparison. To judge from the Doctor's writings and his witchcraft, he must have a very *loose* way of doing things. Verily, there is no element of nature capable of resisting his dominion. Actions of animal life, voluntary and involuntary, submits, if he but flourishes his thumb : Like Macbeth, he can murder sleep* and the agents of his Elfin genius walk unseen like spirits about the world, spell-binding unconscious mortals, "both when they sleep and when they wake."

Pass we on now, to the Doctor's *second Revelation :*—" I will mention also a *singular fact* with regard to myself, viz : that I have never been able to procure a watch, which could keep time in my pocket, though I have made great efforts to procure one which would. I have owned half a dozen high priced levers and they would all vary 10 or 15 minutes in a week. No one is more careful of a watch than I am, and two of these watches, which my brothers wore afterwards for years, kept time admirably. *I have been unable to explain the fact in any other way than by the supposition, that there was some magnetic influence about me, which acts (acted?) upon them* " ! ?—[*Lecture ut supra, p.* 275.]

Shall we receive the Dr's rationale ? was *he* or the watch in fault ? whose internal organization stood most in need of being regulated ? Perhaps it would be wiser to lay these questions over for future deliberation, and leave them to be decided at the same time, when the learned shall agree, whether or not the "*Mulatto be a Hybrid.*"

Meantime, as a rule of action for the forthcoming solution, it will not be out of place to receive such aid, as the high authority of Sir *Jno. Herschel* can afford. This astronomer being asked to account for a *deviation of the plumb line* at a certain trigonometrical station, replied, that he *supposed* it was owing to some *superior densities*, (our Dr., you know, believes in the different densities of *skulls*) which would attract the line from the true perpendicular, such as might exist in *large nodules of metallic masses.*" Sir John must here allude to lead, which is sometimes an amalgam of brains.

Now if these two cases of the Dr. are not sufficient to throw mine in the distance, and to satisfy any moderately reasonable man with the

* "I have also produced the rigid state of the muscles of the arm of one of my subjects, *when in a profound sleep*, when the mind of the subject could have nothing to do with this physical effect."—[*Lecture on Animal Magnetism by* "J C. N."]

superiority they confer upon him, that man's appetite for posthumous fame must be unappeasable.

There are points of difference, however, in our respective cases worthy of note. The Dr's are unique, remarkable and differ essentially from mine, thus,—The spiders and the fœtus present their pathology through the unfortunate patients, who were their subjects—but, the pathology of the *molasses boy* and that *animalized watch*, (*me cunique celeste*) can only be sought for in that portion of the Dr's altitude, where, (according to his own authority) anatomists are wont to bore, when searching for "*mashed potatoes without butter.*"

I utterly disclaim all intention of impeaching the Dr's veracity, this would be indeed a flagrant departure from professional etiquette, and would be at variance with what I sincerely believe of him—but may not *imagination* have worked as wonderfully upon the Dr's brain as it did upon the boy's bowels? Now the Dr. must in his waking moments admit, that there is an amount of credibility to which all things were entitled, and an amount, which every man is at liberty to give or withhold, influenced by extrinsic evidence. Without this right, we would be bound to place implicit reliance upon all the vagaries of witchcraft, which so disgraced the close of the 17th century, with the tragic and bloody fanaticism it involved, sustained and promoted by the *testimony upon oath*, of men and women, conspicuous throughout New England for *piety, respectability, virtue and learning;* individuals, who would have died martyrs in their faith of the powers of *Moll Pitcher*, she, whose renown, according to her biographer "had gone to the farthest regions, and her memory *perpetuated in the annals of credulity and imposture!*" It cannot be that our learned friend descries such immortality.

For myself, I am content without it, and am willing to watch without envy his "feeble" soarings, for every man, who aspires to renown, has the right to attain it in his own *peculiar way*, but in so doing, he would be wise so to circumscribe his gyrations as to avoid that "*frontier line, which separates the reasonable from the insane.*" He must not constrain every one to believe, that by a few manœuvres imperfectly conceived, and as imperfectly developed, his is a mission by which "the rocks of the sea shall be made to burst silence, and tell what they have been thinking on from eternity." This would indeed be a despotism inadmissible and insufferable.

I have devoted for many years, much more time than the Dr. gives me credit for, to a close enquiry on the subject of mesmerism. I have attended exhibitions wherever and whenever I could—in public halls and *private parlours*, and have given them scrupulous and honest attention. My *convictions against the doctrine remain unshaken.* But, say its advocates, itinerant jugglers are not to be taken as standards, by which to judge the craft, because acting for lucre, they fall under suspicion. To this I reply that the privilege of rejecting them rests with *us who disbelieve*, and not with the initiated and faithful, and for these reasons: the experiments performed and contended for are the same with both classes of mesmerizers. The *public showman* aims at and affects to attain the same ends with the more learned and scientific experimentalist. These last recognise the modus operandi of the former as legitimate, although they cannot mumble their *abacadabra* in as pure latin as the Savans of the occult science.

Further, I maintain that the moment an intelligent and respectable gentleman proclaims to the world his belief, announces under his sign manual his co-fraternity, and exhibits his cases in public—that moment he is of his own free will and accord *par nobile Fratrum* with every other mesmerizer, due allowances being made of course for difference of birth, education and other adventitious circumstances—but, as far as the grand principle at issue is concerned and the verdict of society, *they must forever, in the eyes of all reasoning men, stand upon the same platform.*

In a word, a mesmerizer in full feather, is almost beyond the ordinary power of description, and our readers must be content if they can "approximate" his natural history through means of the following quaint description by which that prince of humourists, *Rabelais* describes *Quaresmeprenant*:—

"He was diligent in doing nothing, and did nothing in his diligence:—He prophesied in his sleep, and slept in prophesying; but he kept his eyes open like the hares of Champagne fearing some surprise from his old enemies: He laughed when he bit, and he bit when he laughed:—He ate nothing when he fasted, and he fasted when he ate nothing: He *masticated by suspicion, and drank by* IMAGINATION: bathed beneath high belfries, and dried himself in ponds and rivers: He fished in the air, and caught tithe crabs—he hunted in the depths of the sea, and found there the Ibex and the Chamois." * * * * * * * *

"Truly," said Pantagruel, "he is a strange and monstrous man, *if man he should be termed:* You remind me of the forms of Amodaunt and Discord."

"And pray who are *they?*" asked Brother John.

"The Lord forgive me! I have never heard of them."

To conclude, Messrs. Editors, the terrible visitation with which I have been afflicted, did not come unexpectedly, as tidings had prepared me for an approaching storm—but I confess that considering the omenous forebodings, I have been amusingly disappointed. *The Doctor's last act, before any coroner's jury, would unanimously be pronounced a case of* SUICIDE. I shall trouble you no more.

Very resp'ly, your ob'nt. servant,

A. L., M. D.,

Mobile, Ala., Nov. 26, 1846.

8.—*National Medical Convention.*

It affords us much pleasure to witness the growing interest displayed in all parts of the Union, in behalf of this great project. We learn from our late exchanges that Pennsylvania University, the Philadelphia Medical Society, the Ohio State Medical Society, and many others, have already appointed delegates, and we sincerely hope that every respectable medical institution in the country will be represented in the assembly which is to meet in Philadelphia in May next. Some of the leading introductory lectures of the season have been devoted to the consideration of *medical reform.* Most persons admit the necessity of it, though to a few the obstacles seem insurmountable. But let the effort be made, and some good will surely be the result. The Medical Profession must be placed on a better footing than it now occupies in our country, or it will soon sink irretrievably into the catalogue of tricks, humbugs, and charlatanry of the day. If we take a glance at the favorable state of health with which our country is blessed from year to year, the large number of doctors throughout the land who make but a meagre support, and then look at the crowds of young men flocking to our Medical Colleges, it must be apparent that the annual *increase* of physicians is in an *inverse ratio* to the demand. But the prevailing delusion among our young men must have sway until it finds a corrective either among themselves, their preceptors, or the community at large. *Reform* is the order of the day, and *Progress* the distinguishing feature of the age! The *ball is in motion, no matter how started;* and it will be found easier *to guide* than *to resist it.* Europe is moving on the subject of *medical reform*, and young America should not withhold her efforts for the advancement of "*Young Physic.*"

We commend the following temperate and dignified Address to the physicians of the South, and hope they will attend to their own interests by sending representatives to the Convention: F.

Address to the Medical Profession, in relation to the objects of the Nationa Medical Association, by the Committee appointed for that purpose.

Abstract from the Minutes of the Proceedings of the National Medical Convention, held in the City of New York, in the month of May, 1846—

"*Resolved*, That a committee of seven be appointed to prepare and issue an address to the different regularly organized medical societies, and chartered medical schools of the United States, setting forth the objects of the National Medical Association, and inviting them to send delegates to a Convention to be held in Philadelphia, on the first Wednesday in May, 1847."

In obedience to the above resolution, the committee appointed for the purpose present the following remarks to the members of the medical profession generally, as well as to the several bodies named in the resolution:

In compliance with an invitation from the Medical Society of the State of New York, delegates from various medical societies and schools met in Convention in the city of New York, on the first Tuesday in May, 1846. About one hundred members were present, representing thirty-four different medical associations, and residents in sixteen different states of the Union. The object of the Convention was, by a concert of action of medical men from every part of the country, to advance the interests, the honor, and the usefulness of the profession. The result of their deliberations, as shown in their published proceedings, has been widely circulated, so that all who take an interest in such matters, are probably fully informed of it; still, some remarks upon the course which was pursued, together with the reasons for it, may be appropriate.

The first object which engaged the attention of the Convention was the necessity of forming some plan of organization, by which medical men in every part of the country may communicate with each other, and act in concert, in regard to their common interests. The general opinion was, that this desirable object would be best attained by the formation of a National Medical Association, to consist of members from every part of the country, who should meet together for consultation and action, at such times and places as they might deem expedient. Although no doubt was expressed of the propriety of such an organization, there were other subjects connected with it, such as, who should be members of the association? how and by whom should they be selected? if delegates, what should be their number? what power should be conferred upon them, and how far should their acts be binding upon the bodies whom they represent? concerning which there would probably be difference of opinion. As no one had been charged with the consideration of these subjects, no definite plan of organization had been prepared, upon which the Convention could act with that careful deliberation which the importance of the business demanded. It was thought best, therefore, to postpone all definite action upon it until a future Convention. This opinion was strengthened by the fact, that although the number of members present was larger than had been anticipated, yet many most respectable medical societies and schools, who are doubtless equally solicitous with those who were present, for the welfare of the profession, and as ready to promote its interests, were not represented. This deficiency, it was hoped, would be remedied in a future Convention. In the meantime, the consideration of these subjects was intrusted to a committee, who will prepare a plan for a National Medical Association, which will be presented to the proposed future Convention.

The other subjects which engaged the attention of the Convention, related principally to medical education; such as the qualifications which should be required of those about to engage in the study of medicine; the course of study which should be pursued by them; the mode of examination which should be adopted, and others of a similar kind. For reasons like those given above, these several matters were also placed in the hands of committees to examine, and report upon them. The same course was also pursued in regard to the preparation of a code of medical ethics, by which the intercourse of physicians with each other, and with the public, should be regulated.

From this statement it will be seen that matters of great interest are presented for consideration, and that if wise measures in regard to them are adopted, such errors and abuses, as may be found to exist, will be corrected; the profession will be guarded against the admission of incompetent or unworthy members; the purity, professional and moral, of those who are allowed to continue in it, will be preserved; and that thus full assurance, such as it has a right to demand, will be given to the community, that all who are acknowledged by medical men to be of their number, are worthy and competent to perform the duties, and to sustain the responsibilities of an arduous and honorable profession.

It remains to be seen whether these desirable objects shall be accomplished. It is obvious that this can be done only by general and united exertion. All partial or divided action will be unavailing. It is equally true, that whatever is done, must be accomplished by medical men themselves. In some countries, the interests of the medical profession, like others of great importance to the community, are regulated, sustained, and protected by public law. In this country, no general law, even if it were desirable, can ever exist. In many of the states, no laws upon this subject have ever been enacted; in several, where they have formerly existed, they have been repealed, and in those, where they still remain, there is a general complaint of their inefficiency. In this state of things, the only resource which remains, is, for medical men to establish and enforce among themselves such regulations as shall purify and elevate their own body, and thus more fully command the respect and confidence of their fellow-men. The proposed association, if it becomes general, may be the means of accomplishing much of this good work. The opinions of such a body of the most respectable members of the profession, enjoying the confidence of their brethren, and of the public, freely expressed after full consultation and careful deliberation, although not clothed with the authority of the law, will still command respect, and for the most part, compliance. A public opinion in regard to the subjects decided upon, will be created, which will be more controlling than law. It is by creating and sustaining a sound and healthy public opinion, that the association will prove most beneficial.

In calling the attention of the physicians of this country to such an effort at this time, it is not intended to express the opinion, for no such opinion is entertained, that they are behind those of other countries, or the members of the other professions in this, in general intelligence, in scientific attainments, or in practical skill. Still it is to be remembered that the present is peculiarly an age of advance in every department of science, and that at such a time to rest satisfied with present attainments, and to make no provisions for increased acquisitions, is practical retrocession.

In this state of things, the committee feel themselves fully authorized to call upon the medical profession throughout the country to consider, carefully and deliberately, the matters which have been presented to them, and upon which a future convention will be called on to act. They also earnestly invite all medical societies, the faculties of all medical colleges, and all similar associations to appoint delegates to meet in Convention in Philadelphia, on the first Wednesday in May, 1847.

It is confidently believed that a Convention thus constituted, embodying the wisdom, and acting under the sanction, and with the authority of the united profession, will devise such measures as shall command the respect of all who are interested in the promotion of medical science, and the physical welfare of man. In behalf of the Committee, J. KNIGHT, M. D., *Chairman.*

8.—*Report of Interments in the City of Charleston, with the name and number of each disease, from* 1828 *to* 1846. *The prevailing diseases in each month, and the Thermometrical range, &c., from* 1834 *to* 1846. By John L. Dawson, M. D., City Register.

Dr. Dawson deserves the thanks of the profession for this interesting report. It contains very valuable statistics and must have required much labour. We extract from it the following table, which shows the number of interments and the proportion of deaths to population for eighteen years.

INTERMENTS.

	Males.	Females.	Total.	Bl'ck & Col'd.		Proportion of deaths to Population.
1828	454	339	793	435	one in	37.81
1829	388	374	762	455	—	38.14
1830	408	355	763	434	—	39.31
1831	382	351	733	455	—	41.00
1832	303	257	560	310	—	53.57
1833	281	261	542	306	—	55.35
1834	350	342	692	384	—	43.77
1835	365	299	664	363	—	45.55
1836	639	533	1172	853	—	25.84
1837	352	278	630	356	—	48 00
1838	828	381	1209	500	—	25.05
1839	502	354	856	422	—	35.38
1840	361	244	605	348	—	49.52
1841	336	258	594	335	—	50.44
1842	307	253	560	360	—	54.47
1843	368	329	697	483	—	44.59
1844	282	271	553	365	—	54.18
1845	272	298	570	324	—	52.18

From this it would appear that the grand total of deaths for the 18 years was 12,955—consisting of males 7,178—females 5,777—whites 5,470; black and coloured 7,485.

Upon examining Dr. Dawson's catalogue of diseases, we find the greatest number of deaths from the following, viz: consumption 1,780—

dropsy of all kinds 1,144—stranger's fever 646—teething 594—convulsions 398—locked-jaw and tetanus 387—apoplexy 322—debility 323—bilious fever 269—intemperance 219—typhus fever 205. There are set down to *old age* 838. It is gratifying to find a place where so large a number attain the full measure of their days.

It would appear that the deaths from *consumption* fall but little short of all the fevers combined—consumption 1,780; all fevers 2,024. It is by looking over reports of this kind that we are made to feel our great want of a general nomenclature of diseases, which we hope to see supplied by the National Convention.

Dr. Dawson gives an interesting summary of the *prevailing diseases of each month from* 1834 *to* 1846, a period of twelve years; from which we learn that *yellow fever* prevailed on 8 years—*scarlet fever* on ten—and *country fever* on 6. We wish Dr. D. had given us a definition of "*country fever.*" It is certainly a strange cognomen for a *city* complaint.

We were struck with *one remarkable fact* shown by this summary; which is, that *on almost every year that yellow fever prevailed, and during the same months, there prevailed* likewise bilious, intermittent, typhus, and scarlet fevers, also bowel complaints. Is there not some sort of affinity between them all? F.

9.—*Prof. Bartlett's Inquiry concerning continued Fever.*

We take pleasure in complying with the request of Prof. Bartlett, to publish the following communication, and hope it will be promptly responded to by some of our Southern practitioners. Whilst our brethren in the old world are mostly engaged in perfecting the fundamental principles of Medical Science, we of the new avail ourselves of their labors, and are endeavouring to apply them to practice. To us in the South-west it is an object of primary importance to obtain a correct knowledge of the diseases that prevail in our region, and the best methods of treating them. It is to this object that our Medical Journals are mainly devoted, and their contents are chiefly composed of isolated and separate observations. We have but few men who are either qualified or inclined to undertake an enlarged or comprehensive view of any disease or class of diseases. When one does come forward—especially one so capable as Prof. Bartlett, and puts forth a polite invitation for facts and observations, all should feel the obligation to contribute their quota. We therefore cheerfully second the invitation of Prof. Bartlett to our collaborators, and should be pleased to make our Journal the medium of communication for any thing that may be offered on the subject of *continued fever*:

EDRS.

TRANSYLVANIA UNIVERSITY,
Lexington, Ky., Dec. 10th, 1846.

GENTLEMEN:—I am engaged in the preparation of a second edition of my work on Fever, and I am exceedingly desirous of obtaining information from careful and competent observers, upon certain points, in regard to continued fever in the Southern and South-western States. I wish to know, first of all, if there is, throughout this region, a distinct form of *continued fever*, answering either to the *typhoid* of France and America, or to the *typhus* of Ireland. The typhoid form of continued fever is the only fever of the Eastern States; it is becoming very common in Ohio; it is altogether the most common form of fever in most parts of this State. There is an opinion that it is gradually taking the place of malarious fevers. One thing at least is quite clear, and that is, that this form of disease is much more common in many portions of the middle and Western States, than it was supposed to be, before the attention of the profession had been particularly turned to the subject. Dr. Boling, of Montgomery, Ala., in an elaborate and valuable article on Remittent Fever, published in the American Journal, speaks of *protracted cases, attended with spontaneous diarrhea, and tympanitic distension of the abdomen*, and a feeling of pain or uneasiness *between the umbilicus and the right iliac fossa.* He also reports two *protracted cases*, in which be found ulceration of Peyer's glands after death. Now it seems to me hardly possible to doubt, that these were, all of them, cases of *true, continued, typhoid fever.* I should be exceedingly gratified if some of your subscribers would enlighten me upon the subject of my letter, between this time and the middle of March next. *Are there, or are there not, cases of fever corresponding in their march and symptoms to the typhoid fever of New England; running on from three to five weeks, and in grave cases nearly always marked by prominent abdominal symptoms, tympanitis, diarrhœa, and so on! and differing essentially in their whole aspect and physiognomy from even the protracted cases of genuine remittent fever?* There are multitudes of such cases in Ohio and Kentucky: I desire greatly to know whether there are such in the South-western region of the United States. Any information upon a subject in which we are all alike interested, either through the medium of your Journal, or communicated directly to me here, will be gratefully received and acknowledged. I have the honor to be, very respectfully, your friend,

ELISHA BARTLETT.

Editors of the N. O. Med. and Surg. Journal.

10.—*Attakapas Medical Society.*

"In answer to a call for that purpose, a number of physicians convened at New Iberia, on Tuesday the 10th inst., and formed themselves into an association to be called the Attakapas Medical Society. The number of medical men in attendance was less than anticipated, but assurances of co-operation were received from many who were prevented by other engagements from attending.

The following persons were elected officers of the Society, for the ensuing year:—

JEROME MUDD, A. M., M. D., *President.*
JOSEPH W. LYMAN, M. D., *Vice President.*
J. B. HACKER, M. D. *Recording Secretary.*
JAS. B. DUNGAN, M. D., *Corresponding Secretary.*
JOHN AUGUSTINE SMITH, M. D., *Treasurer.*

Censors:

JAS. T. SMITH, M. D. C. R. FASSIT, M. D.	St. Mary.	EDWARD SHIEL, M. D. EUGENE WELD, M. D.	St. Martin.
JOHN G. HOWARD, G. W. SCRANTON,	Lafayette.	H. J. SANDERS, M. D. G. MILLS, M. D.	Vermillion.

The society will hold meetings twice in each year, at New Iberia—on the second Tuesday in November, and on the second Tuesday in May.

Measures will be taken to have the Society incorporated by the Legislature."

(We hail with pleasure the formation of this new society, and hope to see the like established in all parts of the State. We would respectfully suggest to the medical societies of Louisiana the importance of publishing something officially every year. We know that with proper exertion they can easily prepare something worthy of publication, and this is the only stimulus that can excite them to vigorous action.)

11.—*University of Louisville.*

The late Medical Institute of Louisville has become the Medical Department of the University of Louisville, and is in a most flourishing condition. The number of students in attendance upon the lectures this winter, is 339 against 345 last year. This is now one of the most prominent institutions in the country.

12.—*Prize Essay of the Alabama State Medical Society.*

We are informed that the prize of a Silver Cup offered by this Society for the best Essay on *the Medical History of Alabama*, has been awarded to Dr. P. H. Lewis, of Mobile. We are pleased to learn that the Essay will probably be sent to us for publication ; if so, it will appear in due season in our Journal. As medical science is just emerging from an age of darkness in the Southern States, we should collect and record every thing of interest relating to its early history. We are glad to learn that so able and industrious a physiciau as Dr. Lewis, has undertaken the task for Alabama. Who will do the same for Louisiana and Mississippi ?

13.—*Bulletin of Medical Science ;* Philadelphia, August, 1846.

Under the title of "Waste of Sympathy," the editor of this journal has a long article in reply to some remarks recorded in our May number, concerning Professor Mutter's edition of Liston's Lectures. The editor appears to be very sore and very angry. We said something about independence of thought and action in the medical profession of the United States, but he declares that syllabub and gingerpop are just as "swelling and resonant," and seems to think that they are of equal importance.—In this, we cannot agree with the editor.

We bear no ill-will to the editor of the Bulletin ; a personal feeling would be impossible, for we have never had the pleasure of his acquaintance. But our duty as journalists frequently calls upon us to denounce abuses which we think injurious to the progress of the profession, and in so doing it is but natural that we should select the most glaring examples.

We place before our readers, in his own words, the causes which prompted him to leap into the saddle before Dr. Stokes, and the manner in which it was done—leaving the subject to the consideration of the profession at large, with the exception of a few remarks, which we shall append after we shall have got through the quotation. The verdict of the profession we cannot doubt.

"The proportion of matter contributed by Dr. Bell was upwards of eleven hundred pages, Dr. Stokes's remaining the same. The publishers, thinking that bulk, in these days of trade, might count for something, and having ascertained, by the experience of two former editions, that the American partner, whom they had introduced as the associate of Dr. Stokes, must have contributed somewhat to the success of the entire work, promoted him to the head of the firm on the title page. He might have demurred to this step; but probably he was not without some ambitious aspirations for that "independence of thought and action" recommended by some critics, and a step to which was made in the promotion that has drawn forth the sneer of our brother of the New Orleans Journal.—Temporary precedence does not, however, as he must know, imply assumption of greater rank; an usher of the black rod or master of the ceremonies may appropriately enough go before, saying: *Par-ici, mon Prince.*"

If Dr. Bell "was not without some ambitious aspirations for that 'independence of thought and action' recommended by some critics," why not publish his lectures alone? He could not have conceived that Dr. Stokes could add any thing to his fame, since he says that when the Dr's. "excellent work on the Diseases of the Lungs and Wind-pipe was noticed in a French journal, it was well spoken of, but, at the same time, it was described as a skilful make-up from the writings of Laennec, Andral, and Louis."

Many of the profession would perhaps like to possess the lectures of Dr. Bell alone; many those of Dr. Stokes; but is it fair or just that they shall be obliged to purchase them conjointly. To a gentleman inquiring for the lectures of Dr. Stokes, we think it a natural cause of astonishment when he is told that he is also to purchase the "upwards of eleven hundred pages" of Dr. Bell.

* * * * * * *

Since writing the above, (intended for our last number,) we have met with the British and Foreign Medical Review for October, 1846—from which we make the following extract—we shall make no comments upon it, as it carries its own. EDRS.

"We should not have remarked this little fact, had it not been a part only of the evidence the work affords of being composed with ill-judged haste; a haste which clearly originated in an anxiety on the part of the publisher to secure the profits of a promising speculation,—making it, as it would seem, quite a secondary consideration whether or not the work should be as worthy as possible of the two distinguished names under whose authority it appears. For this we do not conceive, nor would we for one moment be understood to imply, that the learned Professor of Jefferson College is answerable; but we cannot acquit him of all responsibility. In default of a better and juster code of international law for protecting the fair interests and vested rights of authors (years of thought and study vested in print and paper,) and defending them from the privateering system of the trade, we should like to see men of Dr.

Mütter's character and position in the profession and in society, take their stand on the highest ground, and decline to append their names to hurried productions such as the present undoubtedly is. Nor is it expecting too much from them to do so, for lookers-on can see evident proofs of the ill effect the present system has upon their writings and upon themselves."

14.—*Insensibility during Surgical Operations produced by Inhalation.*

We fully concur in the following remarks of the editor of the Medical Examiner respecting this *new discovery.* We have read Dr. Bigelow's paper in the Dec. No. of the Boston Medical and Surgical Journal, but shall give no extracts, as the *gist* of the matter is contained in the following annalysis of it by Dr. Huston. That the leading surgeons of Boston could be captivated by *such an invention as this,* heralded to the world under the auspices of a *patent right,* and upon *such evidences* of utility and safety as are presented by Dr. Bigelow, excites our amazement. Why, *Mesmerism,* which is repudiated by the *savans* of Boston, has done a thousand times greater wonders, and without any of the dangers here threatened. What shall we hear next? Eds.

"A certain Dr. Morton, a practising dentist in Boston, is advertising in the newspapers of this city, that he has secured a *patent* for what he calls 'his improvement, whereby pain may be prevented in dentistical and surgical operations,' and he now offers to sell 'licenses to use said improvement,' to 'dentists, surgeons, and other suitable persons.' Looking upon this as nothing more nor less than a new scheme to tax the pockets of the 'enlightened public,' we should not consider it entitled to the least notice, but that we perceive by the Boston Medical and Surgical Journal, that prominent members of the profession in that city have been caught in its meshes.

"From a paper by Dr. H. J. Bigelow, 'one of the Surgeons of the Massachusetts General Hospital,' contained in the Boston Journal of the 18th of November, 1846, we derive the astounding information that Dr. Warren and Dr. Haywood—men at the very top of our profession—have allowed Morton to administer his 'preparation'—'a secret remedy,' for which he has taken out a patent—to patients on whom they were about to operate! Dr. Bigelow says, in extenuation of the course pursued by Morton, in taking out a patent, that 'it is capable of abuse, and can readily be applied to nefarious ends;' that 'its action is not yet thoroughly understood, and its use should be restricted to responsible persons;' and that, one of its greatest fields is the mechanical art of dentistry, many of whose processes are, by convention, secret, or protected by patent rights. It is especially with reference to this art, that the patent has been secured.'

"Now, we would like to know of Dr. Bigelow, whether any such restricted object is contained in the patent? None such appears in the proprietor's advertisement, and we apprehend that time will show that the sale is only limited by the price and disposition to purchase.

"'We understand,' says Dr. B. 'already, that the proprietor has ceded its use to the Massachusetts General Hospital, and *and that his intentions are extremely liberal with regard to the medical profession generally.*' Not a word of the sort is in the proprietor's advertisement. Did not Swaim give his *panacea* to the poor gratis, and a lot of ground to build a church on to boot? And did not John Williams, the oculist, with a trunk full of seals and royal testimonials, invite all the reverend clergy to come to him, and to bring with them all the poor blind people of their parishes, that he might cure them without money and without price?

"The '*preparation*' is inhaled from 'a small two-necked glass globe,' and smells of ether, and is, we have little doubt, an ethereal solution of some narcotic substance. The patient is rendered insensible for a period of from five or ten minutes to an hour; the pupils are dilated; 'very young subjects are affected with nausea and vomiting, and, for this reason, Dr. M. has refused to administer it to children.' In one case, a patient of Dr. Dix, 'the respiration was very slow, the hands cold, and the patient insensible.' Various active measures were found necessary to restore the patient, and 'complete consciousness returned only at the expiration of an hour.'

"We are persuaded that the Surgeons of Philadelphia will not be seduced from the high professional path of duty, into the quagmire of quackery, by this will-o'-the wisp; and, if any of our respectable dentists should be tempted to try this new '*patent medicine*,' we advise them to consider how great must be the influence of an agent over the nervous system, to render a person unconscious of pain—the danger there must necessarily be from such overpowering medication; and that, if a fatal result should happen to one of their patients, what would be the effect upon their conscience, their reputation and business, and how the practice would be likely to be viewed by a Philadelphia court and jury? We cannot close these remarks without again expressing our deep mortification and regret, that the eminent men who have so long adorned the profession in Boston, should have consented for a moment to set so bad an example to their younger brethren, as we conceive them to have done in this instance. If such things are to be sanctioned by the profession, there is little need of reform conventions, or any other efforts to elevate the professional character---physicians and quacks will soon constitute one fraternity."---*Med. Exam.*

NEW ORLEANS, JANUARY 1, 1847.

HEALTH OF THE CITY.

The health of the city has been remarkably good since we last went to press. A few cases of yellow fever were to be seen at the Charity Hospital late in December, but they were objects of *curiosity* rather than of *terror*. We refer the reader to Dr. Fenner's paper and the report of the Board of Health, in the first part of the present number, for a full account of the yellow fever of the season. From the fact that the greatest number of cases were congregated at the Charity Hospital, the committee of the Board of Health suppose there must have existed, either in the house or immediate vicinity, some local cause of greater power than in other parts of the city. We are not inclined to concur in this conclusion. We believe that nine-tenths of the cases there were attacked out of the Hospital, and that three-fourths came from the lower part of the city.

We commend the report of the Board of Health to the special attention of our municipal authorities, as containing many valuable suggestions in regard to the preservation and improvement of the health of the city.

With the exception of yellow fever we have had no sickness worthy of particular notice. The weather has been quite changeable, but for the most part very mild.

In our next number we shall give our *Bill of Mortality* for the year 1846. In the mean time we hope to receive the annual reports of other cities, and shall thus be able to make a comparison, which, with the lights now before us, will probably be as favourable to New Orleans as that of the previous year.

HEALTH OF THE COUNTRY.

At the close of the year, we may remark that the general health of the South-western country has been unusually good. We have heard of a few sections which were scourged with the customary summer and autumnal fevers—these sections were the neighbourhoods of Nashville, Tenn., Huntsville, Ala., and Columbus, Miss. We give such letters as we have received from correspondents. The letter from Huntsville was intended for our last number, but came too late. However it is worthy of publication, as it comes from one of the ablest and most extensive practitioners of the country.

HUNTSVILLE, ALA., Oct. 19th, 1846.

Messrs. Editors :—From the date of my last communication, up to the latter part of July or first of August, the health of our town and country continued very good.

More rain fell, from early in the spring until some time in August, than usual, and consequently the ponds and low places throughout the country, became filled with water. During the latter part of August and the month of September the weather was generally clear, dry and hot. Intermittent and remittent fevers (our usual diseases) prevailed throughout these two months to an extent almost unprecedented. In no one year, for the last ten or twelve, has the writer heard so much complaint, amongst the inhabitants, of chills and fevers. The disease, however, was generally mild and manageable, yielding readily to the use of quinine and such febrifuge remedies. Some few cases assumed the congestive form, and proved more serious. An unusual number of intermittent pains, principally located in the head and eyes, unaccompanied with fever, or much derangement of the general system, were discovered by us. These yielded readily to large doses of quinine. During the month of September, and up to the present time, we have had prevailing an epidemic catarrh, accompanied with fever of a remittent form.

The hooping cough has also been amongst the children of our town and country. Very respectfully, A. E.

ST. MARY'S, Dec. 15th, 1846.

Messrs. Editors :—Since the date of my last letter, but little sickness has existed. The weather, with the exception of a few cold days of a freeze on the night of the 25th ult., has been mild, pleasant and dry, reminding one more of May than December. A few weeks since I met with another fatal case of *trismus nascentium*, in which I did not find the "occiput shoved in ;" and also a fatal case of *traumatic tetanus*. I have seen also two or three cases of pneumonia.

Now that the "season" is past, it may not be amiss to compare the past with the previous summers in reference to the amount of sickness as connected with asmospherical vicissitudes. It may be well to premise that this is a prarie country—with numerous small ponds and low marshy places scattered over it. In the upper part of the parish of St. Mary, exists a large pond or marsh called the *Grand Marais*—probably formerly the bed of a lake. It commences at the upper line of the parish, and extends down six or seven miles of an average width of half a mile, and at the distance of ¼ to ½ mile from the Bayou Teche, on its west side, emptying itself by a narrow outlet into the sea marsh. Few inhabitants are living upon its immediate borders, but more upon or in the vicinity of its outlet. In a dry season this marsh becomes perfectly dry, but in a wet one it is covered with two or three feet of water.

In 1841 and '42 frequent rainy spells occurred, alternating with dry and hot weather. Much sickness occurred during the summer and fall of these years. 1843 and '44 were dry seasons; the droughts in each year commencing about the middle of March and continuing for three months, followed by occasional rains alternating with hot and dry weather. Each of these years the *Grand Marais* was perfectly dry for a time; events which, say the old settlers, never occurred before. Both of these years were unusually sickly. In 1843, in one family living on the borders of the *Grand Marais*, I prescribed for seven cases of intermittent fever in the white family at one time. The summers of 1845 and '46 were excessively rainy, and the weather hot and sultry—the *Grand Marais* always covered with water. These two seasons have been remarkably healthy—more so it is said than any previous seasons for the past ten years.

This fall the *Grand Marais* has been drained by the planters owning lands in the same, by cutting ditches to the sea marsh, and in future years we shall see what influence, if any, it will have upon the health of the vicinity. Having no favorite theory to support, I present the above facts without any comment. Respectfully, J. B. D.

HEALTH OF THE ARMY ON THE RIO GRANDE.

We learn from Dr. Craig, chief Medical Director, who has been at Monterey ever since Gen. Taylor marched upon that place, that the general health of the Army has greatly improved since cool weather set in; though many are still suffering from intermittent fever, dysentery and diarrhœa. Dr. Craig says that the surgical operations performed at Monterey, for the most part did very well. He performed one amputation at the shoulder joint, on the field, which recovered. We should be much pleased if the surgeons on duty would keep us regularly advised of the health of the Army.

MEDICAL COLLEGE OF LOUISIANA.

Judging from the increase of students, this institution must be growing rapidly in public favour. The present class numbers 163, against 103 last winter.

STATE MEDICAL CONVENTION.

We have received several communications from physicians of the interior, responding to a remark that we made in our September number, on the subject of a State Medical Convention. There seems to be a strong desire that such a convention should be called, but we have not heard of any movement towards it, by either of the Medical Societies or the Eastern Board of Examiners. There are several important objects to be effected, which require the united action of the profession throughout the State; among which we may mention the formation of a State Medical Association, the more efficient enforcement of the laws regulating the practice of Medicine and Pharmacy, and the repeal of the *unrighteous tax* upon physicians. In short, the general interests of the profession, as well as the best means of improving it, can only be secured by the united action of its licensed members. Will neither of the bodies above named move in the matter of a State medical convention? Our Legislature is to meet in February, and we have reason to hope it would make any amendments to the existing laws that might be desired, and redress any grievances properly laid before them.

HOSPITAL REPORTS.

Charity Hospital—Erection of an Amphitheatre.

We are gratified to learn, that the Administrators of this institution have at last determined to erect an amphitheatre for the performance of surgical operations. Such a thing has been long wanted. Heretofore operations were performed in the wards, to the great terror of the surrounding sick, with much inconvenience to the operating surgeon, and where it was impossible for a large number of spectators to witness what was done. A good amphitheatre is indispensably necessary to a large hospital, and we are glad to hear, that the one proposed is to be erected as soon as possible.

The number admitted into this hospital during the year, will approximate 7000; notwithstanding its general healthiness. The sick and disabled soldiers returning from the seat of war—persons belonging to other States—have contributed in no small degree to the formation of this *unusual* number. In consideration of this, it would be but right for the General Government to make an appropriation in aid of this liberal Institution, whose doors are open to the *afflicted* of every State and Nation. Other cities think they have done enough, when they have provided for *their own sick*, but the portals of the New Orleans Charity Hospital are open to *all*, come from where they may.

As our monthly reports were crowded out of our last number, we bring them up in the present, from where we left off. Among the principal diseases prevailing at the hospital at this time, we find an unusual amount of *dysentery*, for the season. Yellow fever has entirely disappeared.

MONTHLY REPORTS.

MAIN BUILDING.

August—Admitted: Males, 639; Females, 96. Total 735.
Discharged: Males, 534; Females, 80. Total 614.
Died: Males, 45; Females, 11. Total 56.
Remaining on the 1st of September, 390.

LUNATIC ASYLUM.

Admitted: Males, 24; Females, 11. Total 35.
Discharged: Males, 21; Females, 2. Total 23.
Died: Males, 5; Females, 2. Total 7.
Remaining on the 1st of September, 72.

MAIN BUILDING.

September—Admitted: Males, 812; Females, 111. Total 923.
Discharged: Males, 707; Females, 100. Total 807.
Died: Males, 70; Females, 5. Total 75.
Remaining on the 1st of October, 419.

LUNATIC ASYLUM.

Admitted: Males, 53; Females, 8. Total 61.
Discharged: Males, 38; Females 6. Total 44.
Died: Males, 5; Females, 1. Total 6.
Remaining on the 1st of October, 85.

MAIN BUILDING.

October—Admitted: Males, 862; Females, 122. Total 984.
Discharged: Males, 704; Females, 103. Total 807.
Died: Males, 112; Females, 13. Total 125.
Remaining on the 1st of November, 469.

LUNATIC ASYLUM.

Admitted: Males, 33; Females, 9. Total 42.
Discharged: Males, 30; Females, 7. Total 37.
Died: Males, 6; Females, 2. Total 8.
Remaining on the 1st of November, 81.

MAIN BUILDING.

November—Admitted: Males, 810; Females, 119. Total 929.
Discharged: Males, 707; Females, 107. Total 814.
Died: Males, 111; Females, 15. Total 126.
Remaining on the 1st of December, 458.

LUNATIC ASYLUM.

Admitted: Males, 29; Females, 7. Total 36.
Discharged: Males, 11; Females, 21. Total 32.
Died: Males, 3; Females, 5. Total 8.
Remaining on the 1st of December, 72

ABSTRACT OF A METEOROLOGICAL JOURNAL FOR 1846.

By D. T. LILLIE, at the City of New Orleans.

Latitude, 29 deg. 57 min.; Longitude, 90 deg. 07 min. west of Greenwich.

WEEKLY. — 1846.	THERMOMETER.			BAROMETER.			COURSE OF WIND.	FORCE OF WIND, Ratio 1 to 10.	Rainy Days.	Quantity of Rain. — *Inches.*
	Max.	Min.	Range.	Max.	Min.	Range.				
Oct. - 31	76.2	58.0	18.2	30.22	29.97	0.25	N.E.	$2\frac{3}{4}$	1	1.120
Nov. - 7	70.7	56.0	14.7	30.16	29.97	0.19	N.	$2\frac{1}{2}$	0	00.00
" - 14	76.0	61.5	14.5	30.22	29.98	0.24	N.E.	$2\frac{1}{2}$	0	0.000
" - 21	75.0	44.0	31.0	30.26	29.93	0.33	S.E.	$3\frac{3}{4}$	2	0.687
" - 28	75.0	35.0	40.0	30.41	28.88	0.53	N.W.	$3\frac{1}{4}$	1	0.875
Dec. - 5	77.0	60.0	17.0	30.37	30.11	0.26	N.E.	$2\frac{3}{4}$	0	0.000
" - 12	72.7	44.0	28.7	30.36	29.86	0.50	N.W.	$2\frac{3}{4}$	3	1.025
" - 19	71.0	39.0	32.0	30.41	29.68	0.73	N.	$3\frac{1}{4}$	2	0.350
" - 26	76.0	43.5	32.5	30.46	30.28	0.18	E.	3	2	0.152

Remarks.—The Thermometer used for these observations is not attached to the Barometer, but is a self-registering one, and is placed in a fair exposure. Regular hours of observation, 8 A.M., 2 P.M. and 8 P.M.

The Barometer is located at an elevation of 19 feet above the level of the ocean, and is suspended clear of the wall of the building.

The Rain Guage is graduated to the thousandth part of an inch, and the receiver is elevated 40 feet from the ground.

During the above period the range of the Thermometer has been unusually large, even for the season of the year, and the transitions of temperature sudden and frequent; we have had, as will be perceived, but an undue quantity of rain, but the mornings have been very frequently attended by mists and fogs, which, however, have generally given way, after meridian, to a clear sky, or the clouds *cirri*, *cirro-cumuli* and *cirro-strati*. The barometer has steadily indicated a rather unusually heavy atmosphere.

THE

NEW ORLEANS

MEDICAL AND SURGICAL JOURNAL,

DEVOTED TO MEDICINE

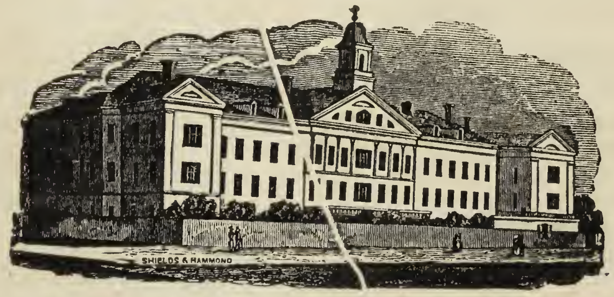
AND

THE COLLATERAL SCIENCES.

EDITED BY

W. M. CARPENTER, M. D.
E. D. FENNER, M. D.
J. HARRISON, M. D.
A. HESTER, M. D.

" Summum bonum Medicinæ, sanitas."—GALEN.



NEW-ORLEANS CHARITY HOSPITAL.

MARCH, 1847.

NEW-ORLEANS.
PUBLISHED BY S. WOODALL, 49, CAMP STREET.
1847.

TO READERS AND CORRESPONDENTS.

Owing to the multiplicity of our engagements during the winter season, we have not been able to notice as fully as we would desire a large number of Introductory Addresses, College Circulars, etc., that have been received. Our grateful acknowledgments are due for the favours.

Dr. Hulse's Report of the sickness at Pensacola is unavoidably postponed to our next No.

We hope to give a review of *Coxe's Hippocrates* and *Galen* in our next.

A communication has been received from Dr. Guild of Tuskaloosa, Ala.

The following new works have been received from the Publishers :—

A Practical Treatise on Inflammation, Ulceration and Induration of the Neck of the Uterus, etc. By J. Hughes Bennett, M. D., &c., Philadelphia, Lea & Blanckard ; 1847, 12mo. pp. 146.

Woman and Her Diseases, from the Cradle to the Grave ; adapted exclusively to her instruction in the Physiology of her System, and all the diseases of her Critical periods. By Edward H. Dixon, M. D. Published by the Author, 1846, pp. 309.

Royle's Materia Medica and *Silliman's Chemistry*, have reached the city, but are not yet delivered. These works will be noticed in our next number.

Our customary Exchanges have been received together with *La Lancette Canadienne*, a new Medical Journal published at Montreal, Canada.

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

Mobile, Feb. 5th, 1847.

Gentlemen :—If you insert the following Errata, which appeared in my "reply to J. C. N." you will oblige.

Yours very respectfully,

A. L.

Page 541, 8th line from bottom,	for	*Flatees*,	read	Flatus.
" 542, 1st line,	"	*Doctor's*	"	Doctor.
" " 20th from top	"	*Sanction*	"	Sanctum.
" 544, 7th " "	"	*Spiders of*	"	Spiders and,
" 545, 16th " "	"	*Submits*	"	Submit.
" 546, 7th " "	"	*me cunique*	"	mecanique.
" " 18th " "	"	*influenced*	"	uninfluenced.
" " 16th " "	"	*were*	"	are.
" " 27th " "	"	*descries*	"	desires.
" 547, 3d line, last paragraph	"	*omenous*	"	ominous.

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EXCERPTA.

PART FOURTH.

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Contents.

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Part First.

ORIGINAL COMMUNICATIONS.

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

In a former publication I remarked that the special cause of yellow fever is utterly unknown. This is unquestionably true, but it does not follow because such is the case, that we may not obtain correct notions of the *general* nature of this cause. Though we may not be able to present the matter creating this fever so as to subject it to chemical analysis and determine its exact constitution, still, it may be possible, from a wide range of observation and experiment, and from analogy, to determine the *class* of substances it belongs to in natural history. Nor would such a result, could it be obtained, be an unimportant one. The strong bearings that it would have upon our views of the pathology, and consequently, upon the treatment of the disease, as well as upon the subject of hygiene, cannot be a matter of dispute;—they must strike every one. The inquiry is one indeed of great difficulty, and I approach it, I hope, with the diffidence and caution such investigations demand. It will be seen, in fact, that I pretend to offer nothing original—my object is to bring to bear upon this important subject, the facts and arguments upon which my own opinion is founded and which I believe to present the true view of the case. They are scattered through various works and embrace several sciences, and I shall therefore be obliged to use quotations freely. Were the paper written solely for medical men, these might, in a great measure, be spared, or at least greatly condensed; but convinced, as I am, that yellow fever may be banished from our cities, my whole object is to put the subject in detail before the minds of the citizens generally, and of those who have control over municipal affairs, and who only have the power of applying the proper correctives. To this class of readers, medical writings, are of course, unfamiliar, In beginning this subject it may be worth our while to sum up in a brief and concise manner, certain general facts connected with it; and in so doing, I shall limit myself to those which are, I believe, undisputed, and several of which are of general notoriety.

1. The yellow fever of Louisiana only makes its appearance where persons are collected in crowds, as in cities, watering places, etc.—Persons who live in the country and confine themselves to it, though they be as unacclimated as any others, and as liable to the disease, are nevertheless, perfectly safe from attack.

2. The production of yellow fever in New Orleans cannot be attributed to marsh malaria, or to any kind of agents generated by swamps, marshes, pools, or standing water of any kind; since we know that unacclimated persons may reside in the midst of swamps, and enjoy perfect health, whilst the city is being ravaged by the pestilence. This is a truth known to most of the inhabitants of this city; and a striking example occurred within my own experience during the epidemic of 1837. The New Orleans and Nashville Rail Road Co., owned a number of unacclimated negroes who were at work on the line of road traversing the swamp from the city to the lake. On the breaking out of the epidemic, I directed the overseer to permit none of them to enter the city. In one instance the order was neglected: the slave was kept at work in the city for two or three days;—returned to the Metairie Ridge where the negroes were quartered, and was seized the same day with the fever. This was the only case that occurred among them.

3. The opinion that the disease is owing to miasm, brought by the north wind which generally prevails during the epidemic season, is therefore erroneous, since persons living in those very swamps which the north wind traverses are exempt from attack, provided they keep away from the city. On the other hand, the south and south-west winds, which prevail at other seasons, traverse to reach New Orleans, swamps even greater than those passed over by the north wind.

4. The disease has been attributed to miasm generated by a part of the bed of the Mississippi laid bare at low water; but it is well known that a healthier region than that called the *Coast* is rarely to be met with in any country. This tract of land lies immediately on both banks of the river; and it is well known that unacclimated persons who spend the summer there, enjoy perfect health. Moreover, most of the inhabitants are themselves unacclimated and are as liable to yellow fever as any other people when so imprudent as to visit the city during an epidemic.

5. Persons who arrive in the city during an epidemic from the healthiest regions—even by the ocean, are subject to attack on the 6th, 5th, 4th, and even as early as the third day after their arrival. Cases of attack on the 3rd day after arrival were not uncommon during the epidemic of 1837. It is plain that those persons were subjected to the influence of some powerful local agent, which existed previous to their arrival.

From these facts it is obvious that the yellow fever of New Orleans arises from causes peculiar to the city, and which are confined to it, or to it and its immediate neighbourhood.

We can only form three possible conjectures with regard to the causes of an epidemic local disease. It may arise from the influence of climate, from inorganic poisons, or from organic poisons.

Some epidemics, the influenza for instance, are supposed to arise from certain meteorological changes in the atmosphere; and they probably do; for they affect whole districts—nay, even continents—the country as well as the city. Their simultaneous appearance in far distant places—and in other instances, their rapid transit from one place to another, forbids the supposition that they are propagated by the agency of man. The peculiar changes which produce such diseases are unknown, but it is obvious from what has been already said, that yellow fever cannot depend upon any such causes. The only conjecture capable of commanding attention for a moment, is, that the disease is caused by the long and continued action of heat upon the system. But the difference between the range of the thermometer in the city and country must be exceedingly small, and the fact, already mentioned, of persons being attacked a few days after their arrival in the city, at once overthrows such a supposition.

We must then in the next step of our inquiry turn to inorganic poisons. But as yellow fever only appears at certain seasons of the year, and also only in certain years, our inquiry must be limited to the production of such poisons at such seasons. We cannot imagine, therefore, that any such poison is endemic, or pertaining to the nature of the country. We cannot attribute it to the nature of the water we drink, or the food we eat. The same water and the same kind of food is used by all who live along the Mississippi; who never see the disease, but who infallibly suffer if they approach the city when an epidemic prevails. We must, therefore, limit our inquiries to such poisons as may be generated from the soil at particular seasons, and under peculiar circumstances.

Such being the case we are confined to one class of inorganic poisons—the gases, which might be evolved from the soil or from water containing organic matter. But assuredly the cause of yellow-fever is no known gas. It cannot be carburetted, phosphoretted, arseniuretted hydrogen; neither can it be carbonic acid gas, etc. These and many others of the known gases produce death; but their effects on the system are known. Never has it been observed that any of them produce symptoms like those of yellow fever. When fatal, their effects are rapid—generally instantaneous—when not fatal, the immediate effects in general pass away leaving the patient sometimes merely debilitated.—In these cases they may produce peculiar organic lesions. Many of them, besides, are appreciable by the senses—by their odour for instance. Again the chemist in his laboratory is frequently exposed to them, but if not of sufficient concentration, he escapes uninjured. No such symptoms as are observed in yellow fever have ever been known to follow. Besides all this, any one of the known gases can be produced in all parts of the world, but yellow fever is met with only in certain places, and within certain latitudes.

Driven from all the known gases, we must then presume that the poison or cause (if it be a gas) is one as yet undetected. But can we believe this. Analysis of the gases has been brought to such perfection that chemists are able to detect 3.15 parts of carbonic acid gas in 10.000

parts of atmospheric air. Assuredly were the poison a gas, it could not fail to be detected during the presence of epidemics like those which occurred in New Orleans during the years 1833–37–39–41. But though the best chemists have been employed upon this subject and in various parts of the world, and in the most unhealthy localities, they have found the proportions of oxygen, nitrogen, and carbonic acid gas nearly the same, and have failed to detect any gaseous body that could possibly produce yellow fever.

We must admit, indeed, that various accidental substances are being continually added to the principal constituents of the atmosphere. Thus —"volatile exhalations from the organized matter of the soil,—products of the volcanoes on the earth's surface, and of the artificial burning of fuel,—gaseous fluids escaping from mines,—vapors of volatile solid substances,—hundreds of volatile oils from odoriferous plants,—putrid volatile products of animal and vegetable decomposition,—the muriatic acid arising from salt water in shallow lakes and lagoons,*—exhalations of men and animals,—an innumerable multitude of substances ascending in vapor from manufactures and chemical processes,—and finally, the volatile products of animal excrements; all these are sources of pollution to the atmosphere, which thereby undergoes innumerable alterations in its composition."† But it is obvious, from what has gone before, that several of these causes of pollution must be excluded from a theory which attempts to account for the poison or cause of yellow fever. This disease cannot arise from the products of volcanoes, nor the combustion of fuel, nor gaseous bodies escaping from mines, or from vapors of volatile solid substances, nor from the oils of odoriferous plants, nor from muriatic acid, nor from substances escaping from manufactures and chemical processes; because all these conditions are absolutely wanting in the city of New Orleans. There remains then "the volatile exhalations from the organized matter of the soil,"—the "putrid volatile products of animal and vegetable decomposition;" the "exhalations of men and animals;" and above all, "the volatile products of animal excrements." Here then we come upon our third category—organic poisons.

It is plain for obvious reasons, that in this inquiry we must limit ourselves to such poisons, as may be produced by what is termed the spontaneous decomposition of organic matter,—in other words, decomposition by putrefaction or decay.—The time has long since gone by, when to account for the prevalence of an epidemic, it was sufficient to say, that the wells of the city had been poisoned.

But organic matter is of two kinds, vegetable and animal. Does this poison arise from the decomposition of the first, or second, or from both?

That it does not arise from any thing like marsh malaria has already been affirmed, and the affirmation is unquestionably true; for unacclimated persons, residing out of the city, even in the midst of the swamps, are

* Mulder found free muriatic acid in the rain water of Amsterdam, which he ascribes to the decomposition of the chloride of magnesium, contained in the waters of the Lake of Haarlem, by the action of the sun's rays.—Johnstone.

† The Chemistry of Vegetable and Animal Physiology: by Dr. G. J. Mulder. Amer. edit., p. 94.

perfectly safe so long as they avoid the city. The whole Island of New Orleans is surrounded by swamps, but yellow fever is confined to the cities and villages. It might be supposed, however, that as in New Orleans we have many ware-houses filled with a peculiar vegetable substance—sugar, and from which there is a constant drainage giving rise to offensive effluvia, the cause was generated from it.—But the reply to this is, that on all the sugar plantations, the sugar houses produce the same effluvia without ever giving rise to yellow fever—that the decomposition of vegetable matter under certain circumstances may produce epidemic diseases, I have no doubt, but I am now speaking of the Yellow Fever of New Orleans.

In what respect does the city of New Orleans differ from the surrounding country, which is generally perfectly healthy, when the city is being ravaged by the pestilence? It is situated upon exactly the same sort of soil, of exactly the same elevation, with all the surrounding circumstances the same. But it differs, in the want of vegetation; in masses of mankind being crowded together; in the want of free ventilation; in the accumulation of vast quantities, from year to year, of human excrements (both urine and night soil);—of those of other animals, and of dead vegetable and animal matter. In these circumstances, therefore, we must seek the cause of Yellow Fever.

This brings us to consider organic poisons of animal origin.

It cannot be necessary for me to prove, that the constituents of animal matter may be so arranged as to produce the most deleterious effects when introduced into the system.—The existence of venomous reptiles and insects—of such diseases as small-pox, hydrophobia, &c., sufficiently settle such a question.

But we must here pause and reflect a little upon the differences between vegetable and animal matter. In certain respects they differ most widely;—in other respects, they are nearly the same. In other words certain constituents of plants—such as gum, pure sugar, starch, woody fibre, &c., possess no nitrogen in their composition; and we have no reason to believe, that the changes which these substances undergo, ever generate epidemic diseases. But there are other principles, found only in exceedingly small quantities in most vegetables, but in much larger quantities in a small class; and this class is precisely that, which gives sustenance to man and animals. The plants in this class furnish in fact the proximate elements of the tissues, which make up the bodies of animals. The animal makes nothing;—it consumes. In the laboratory of plants of the class last mentioned, are really formed the proximate principles of the animal tissues. These proximate principles are albumen, fibrine and casein; and they contain nitrogen—and not only nitrogen, they contain likewise sulphur. In short, during the putrefactive process, vegetables, which possess these nitrogenized principles must give rise to products totally different from those, which do not possess them; and these products must be the same (or similar) with those derived from the putrefaction of animal substances; that is, supposing all circumstances to be the same. We cannot, therefore, make an absolute distinction between vegetable and animal poisons, derived from putrefaction. Hereafter, therefore, instead of contrasting vegetable and animal poisons, we shall speak of the poisons causing epidemic diseases as *nitrogenized organic poisons.* This phrase, if it be limited to the products of putrefaction, will comprise those of both kingdoms.

The point then is narrowed down to this: Have we any proof, that poisons may be generated by the putrefaction of nitrogenized organic matter, and if so, do such poisons produce any effects resembling the symptoms and lesions of yellow fever?

The following extracts will show, that the question has been answered in the affirmative.

The first is a translation of some experiments of Gaspard, to be found in the second volume of Magendie's Journal of Physiology.

"Exp. 14. On the 19th of June 1809, I injected into the jugular vein of a little bitch, half an ounce of fetid liquid arising from the simultaneous putrefaction of beef-meat and dog's blood. On the instant, the animal made many movements of deglutition, and very soon afterwards experienced dyspnæa, *malaise* and depression. She lay on her side, refusing all food and soon voided first her excrements, then her urine. In an hour's time, prostration of strength, gelatinous and bloody alvine discharges often repeated, dysentery, redness of conjunctiva. Afterwards, chest painful; belly hard and painful when touched; gradual extinction of strength; bilious, gelatinous, and bloody vomitings. Death, three hours after injection. On opening the body yet warm, lungs inflammed, or rather engorged in a singular manner, but little crepitant, of a violet or blackish hue, with many ecchymosed or petechial spots, which likewise existed in the left ventricle of the heart, in the spleen, mesenteric glands, gall-bladder, and even in the sub-cutaneous cellular tissue. The peritoneum contained some spoonfuls of a reddish serosity; but the mucous membrane of the alimentary canal was most affected. That of the stomach was slightly inflamed; that of the intestines, above all, of the duodenum and rectum, was considerably so,—the colour livid, with black points, and covered with a gelatinous and bloody substance, resembling the lees of wine or the washings of flesh. In addition, this inflammation was accompanied with a slight thickening of the tissues, and possessed a hemorrhagic or scorbutic appearance."

"Exp. 16. The 14th of July 1821, I injected into the right jugular of a large dog two ounces and a half of a fetid liquid, that had arisen from the fermentation for two days of cabbage leaves, at a temperature of 20° R. It was thick, not at all acid and was mixed with an equal quantity of water. While injecting, the animal often swallowed, and before long commenced vomiting, which was frequently repeated, and soon fell into a state of depression. Some hours afterwards, great general uneasiness—pain in chest upon pressure—respiration embarrassed, difficult and plaintive—appearance of peripneumony—then vomiting anew and great depression all day—at the expiration of nine hours, he had in the night a very copious and very fetid liquid stool, black as soot, analogous to the evacuations in melæna,* and formed of a little excrements and mucus, with a great deal of apparently putrified blood. Some

* *Note by Magendie.*—This fact, which Mr. Gaspard was the first to observe, is one most remarkable; it evidently points to the cause of the black vomits, which take place in yellow fever, certain typhus fevers, &c.

time afterwards, the dog had another stool, but it was merely muco-sanguineous.

July 15th, depression more considerable—adynamia—recumbent on the side or vacillating walk—pulse, small and febrile—ardent and seemingly inextinguishable thirst—same aversion—urine natural and abundant enough—respiration free and weak. But what above all struck me many times during the day, was, that the pulsations of the heart would return at intervals with an extraordinary strength and noise, resembling what occurs in the highest degree of aneurism, combined with hypertrophy of the organ.

The 16th, a little better—less depression—cessation of the disordered pulsation of the heart, but still ardent thirst—refusal of food—fever, and sometimes vomiting of drink.

The 17th, the same condition as the night before.

The 18th, symptoms aggravated—extreme debility—walk altogether tottering—excessive thirst—eyes, red, inflamed and blear—nostrils swollen and filled with mucus, obstructing the passage of air—mucous membrane of the mouth, violet-red and phlogosed. At mid-day, liquid stool of a whitish gray colour, mixed with grumous blood of a purulent character and odour—Death, during the night at the termination of the fifth day of the experiment.

Upon opening the body, the skin, sub-cutaneous cellular tissue and muscles presented the same appearance as after death from asphyxia from want of air, and did not appear exempt from inflammation. Conjunctiva, pituitary and buccal membranes red or violet, and covered with a thick mucus, very abundant and unusual—lungs of a gluey (*poisseux*) feel, slightly phlogosed in some spots, but crepitant enough. The left ventricle of the heart presented many brown spots or sort of ecchymoses, penetrating even into its tissue; it was besides of the colour of lees of wine which contrasted singularly with the natural colour which the right ventricle preserved. But, in return, this last was in part filled up with a hard albumino-fibrous concretion, with a yellowish white hue like that of fat, very homogeneous, undistinguishable from the molecules of the injected liquid, weighing $2\frac{1}{2}$ grs, almost entirely free, and only adhering to the ventricle by a fingernail's breadth, apparently inflamed and somewhat torn. This concretion, with ramifications of the same colour and consistence, extended into the pulmonary artery and into the superior vena cava, and also into the azygos, axillary, and even to the right jugular. Probably, it had been the cause of the violent pulsation of the heart, of which I have already spoken. The œsophagus and stomach appeared healthy, but the mucous membranes of the intestines, and particularly of the duodenum, rectum, and a small portion of the small intestines, was of a violet-red,—coloured, chiefly in longitudinal wrinkles and irregular patches, which gave a party-colour to the outside of the intestines before they were cut open. Otherwise, this inflammation was without thickening of the tissues—without ulcerations, and much resembled ecchymosis or hemorrhage. In the duodenum, I observed many kinds of open blisters, whence a large quantity of sanious blood was made to flow by pressure on the neighbouring mesenteric vein. The internal membrane of the rectum was still more affected, and its mucous glands were very swollen and apparent. This intestine contained puriform matters resembling those of the last evacuation. The other intestines contained mucous matter of a whitish-gray colour, and very thick. The mesenteric glands appeared as if penetrated by blood and altogether inflamed. The gall-bladder, stained exteriorly by brown and violet spots, was filled with a black, thick bile, as ropy as melted glue."

Gaspard performed many other experiments by injecting putrid water, and the results were on the whole the same as the above, the differences being merely those of a minor character.—No one, I think, can fail to be struck with the extraordinary resemblance of these symptoms and post-mortem lesions to those of yellow fever. The characteristics of the disease, its rapid course, its hemorrhagic tendency, its peculiar lesions, are all to be met with in these experiments. We have black vomit, bloody alvine discharges, redness of conjunctiva, extreme tenderness over the abdomen, great and rapid prostration of strength, burning thirst, anorexia, &c., all so characteristic of yellow fever. In his other experiments he speaks of other characteristic symptoms—suppression of urine—intersusceptions of the intestines, the existence of fetid fuliginous matter in the bowels, ecchymosis of the mucous membranes, congestion of the lungs, &c. In short, hardly any symptom mentioned by authors as occurring in yellow fever, may not be found in these experiments; and it is the same with regard to the post-mortem lesions.

Now, it may be asked, whether other substances when thus injected into the veins, do not produce similar effects? This question, experimental physiology has answered in the negative. Gaspard, and after him Magendie, have injected a vast number of substances into the veins of animals. Many of them cause death, but none of them the symptoms or post mortem lesions described above. Those produced by ammonia come nearest to them.

The next extract, to which I call attention, consists of some remarks by Magendie. He observes:—"That since medicine has existed, the pernicious influence of ponds, marshes, neglected harbours, and in general, all places, in which animal or vegetable matters were undergoing putrefaction, has been the subject of observation. Men, and frequently animals, inhabiting the neighbourhood of these infected *foci*, were subjected to serious maladies, which authors have designated under very different names, such as the plague, intermittent fever, malignant fever, dysentery, cholera-morbus, typhus, yellow fever, &c. The bad effects of these *foci* of putrefaction are, therefore, well known; but the manner, in which these putrefied animal or vegetable matters, act upon the healthy individual; the nature of the change produced, and consequently the means to remedy it, are so many questions for which hitherto we have had but conjectures and hypotheses offered. Suffice it to say, they are still problems to solve. And nevertheless, what physician does not recognize their importance! What a vivid and unreflected light would be thrown upon the science of life, if we could treat of them in a satisfactory manner! At the present day, we catch a glimpse of the possibility of treading this new path; and the probability of success will be sufficiently strong if we can persuade ourselves, that complicated questions in natural science can only be answered by the experimental method, and that they cannot at all be so by that of simple observation ;—a method, the only one which has been, and is, pursued by physicians.

Palpable proof of what I advance may be found in the interesting treatise of Dr. Gaspard on putrefied substances. (Journal de Physiologie, tom. 2.) Using the experimental method, he succeeded in some hours in producing at will many of the diseases caused in man by putrid exhalations. The black vomit and black dejections are evidently the effect of an alteration in the blood, caused by the introduction of putrid substances into the circulation.

I have repeated with the greatest care the experiments of M. Gaspard, and have pursued them with an eye to their application to medicine. I affirm, and it will be believed without difficulty, that his results are perfectly exact; in addition, I have observed, that different kinds of flesh have not the same activity in their putrefaction. The muscles of herbivorous mammalia appear less active than those of the carnivora. Putrefied oyster-water did not cause very violent effects ; but the deleterious matter *par excellence*, is putrid fish-water ;—some drops of this water, injected into the veins, producing in less than an hour, symptoms, which have the greatest analogy with those of typhus and yellow fever. Death usually ensued in 24 hours, and upon opening the body, all the traces of a chemical alteration of the blood are discovered. The blood remains for the most part fluid ; it has transuded through the walls of the vessels into the different tissues,—particularly is it found to have traversed the intestinal mucous membrane ; it, as well as mucus, is accumulated in the stomach and intestines, where it prevents all the intermediate hues between bright red and deep black. This phenomenon merits all the attention of physiologists ; it leads us to examine the influence, which viscidity of the blood exercises over the capillary circulation and over the exhalations ;—it seems to announce, that the healthy state of the blood in which its tendency to coagulation is very strong, prevents the transudation of this liquid through the walls of the smaller vessels. Under this supposition, a serosity, the most tenuous, could alone find a passage through these vessels in a healthy state of the blood.

In following up these experiments, another fact struck me. The same putrid water, so deleterious when injected into the veins, has no bad effect when introduced, even in a strong dose, into the stomach or large intestines of animals ; nevertheless I assured myself, that it is absorbed by the intestinal mucous membrane ; but it is not impossible, that the mucus which covers the membrane and the pores of the small vessels, performs the office of a filter, which arrests the putrefied animal particles, —permitting only those to pass, which the water holds in suspension or even in solution. I propose to myself to make some researches on this new and interesting question. I have tried as yet but one experiment ; —the following : I took, in separate portions, two ounces each of putrid fish-water, which was cloudy by reason of the animal matters held in suspension. I filtered one of these portions through paper ; it became nearly limpid. I injected the two liquids into two dogs, nearly the same with regard to age, size, &c. The animal which received the filtered liquid, experienced symptoms much less intense and much more prolonged, than that which received the filtered liquid. This last dog died six hours after the injection;—the other survived two days.

Filtration through a simple paper, then, has some influence, and we can reasonably suppose that it might have been more perfect—the putrid water losing all its deleterious qualities, as takes place when we employ filters of charcoal.

The pulmonary mucous membrane and the tissue of the lung presented an analogous phenomenon. An equal quantity of the same putrid water injected into the veins, or introduced with suitable precautions, into the divisions of the bronchia, do not produce the same effects. Injection into the lung produces less serious consequences than injection into the veins. Such is not the case, however, with liquids which cannot be decomposed, or, to be more exact, which cannot be modified by filtration;—the effects are nearly the same, be they introduced into a vein, or injected into the trachea.

There is a kind of research which might conduce to very important results: it is, to study upon animals the effects of the effluvia or miasms which spring from substances in putrefaction.

During the course of the last summer I made some trials on this point. I placed a cask in such a way that its bottom might contain putrefied substances, and so, that over these an animal might abide, supported by a grating with a double bottom, and remain thus exposed to the miasms which were continually escaping.

At first I placed some pigeons, rabbits, and Guinea pigs on the grate. Their food was choice and abundant. None of these animals were affected, although they here sojourned nearly a month, and although, at the bottom of the cask I had kept up a very active *focus* of putrefaction. I then put upon the grating a very sound dog; he was there well fed—visited often—and caressed, in order that he might patiently support his captivity. The four first days he retained his position well enough, but afterwards began to grow thin; and although he preserved his gayety and his appetite, he died emaciated at the end of ten days. His case offers then no sign which recalls to mind the effects of putrid substances injected into the veins, and above all there was no black vomit; but at last he died, evidently from the influence of the miasms which he had inhaled, and which he had swallowed with his food.

Upon opening his body, there was an almost total absence of fat—some food in the stomach,—and chyle, contained in the lacteal vessels and thoracic duct. The mucous membrane of the intestines was inflamed, but much less so than in cases in which putrid substances had been injected into the veins.

I have often performed this experiment, and the results have not sensibly differed, unless we except the period of death, which, in one case, did not arrive until the twentieth day.

I must not pass over a remarkable exception. A dog, three years old, (*griffon*) experienced no derangement of health during a sojourn of six weeks in the cask—he was, as we may say, acclimated. In order to assure myself if he were really beyond the usual conditions relative to the effects of putrefied substances, I injected into his veins a quantity of putrid liquid, more than sufficient to produce serious lesions, and I was not a little satisfied to see him experience almost no effect from it.

From the preceding facts, it is seen, that putrefied liquids when they are injected into the veins cause death, or effects which have the greatest analogy with those of yellow fever and of typhus; that the prolonged respiration of putrid miasms produces death also, but in a period much longer and with symptoms, which differ from the diseases I have just named. What can be the cause of such a difference of the mode of action in the same substances? Why this diversity in their deleterious properties?

It would be so much the more important to be able to answer these questions, as therein lies the whole difficulty, relative to the epidemic diseases which have recently occupied the public mind.

Among the conjectures which may be offered, there is one which merits particular attention. We may presume that different atmospheric conditions, and particularly temperature and moisture, ought to have a great influence upon the mode of action of putrid miasms. I have begun some observations on this point;—I will give an account of them on another occasion."*

Following this in the same volume appears an interesting paper from the pen of M. A. Desmoulins—which is here translated.

Of the Anatomical condition of the skin and sub-cutaneous cellular tissue in Yellow Fever.

"On the 3rd of December, 1821, I read to the French Institute some observations upon the anatomical state of the skin, and of the sub-cutaneous cellular tissue in yellow fever, and from this note the following conclusions may be drawn:

1st. That in yellow fever there is no increase of biliary secretion.

2d. That the mucous surface of the intestinal tube exhales the matter of black vomit, both when thrown up by the stomach, or passed per anum.

3d. That the yellow colour of the skin is the effect of a peculiar elaboration from the blood contained in the capillary vessels of the dermoid tissue, in which is established a congestion or a fluxionary movement analogous to that which takes place at the same time in the hemorrhages of the intestinal membranes.

4th. That when the texture of the skin is very dense, hemorrhages do not occur.

5th. That the yellow tinge, almost always preceded by petechiæ, is in truth, nothing more than a sort of general ecchymosis.

6th. Lastly, that yellow fever is nothing but a congestion or a sanguineous fluxion taking place at the same time upon the skin and mucous membranes, chiefly upon that lining the digestive tube, with different degrees of intensity upon each of these membranes, which are moreover, equally permeable by blood.

Relative to the origin of those matters rejected by the stomach, Dr. Ffirth has remarked that the black vomit did not come from the liver, as had been supposed; for after death, he had found these matters in the stomach of patients who had had black vomit, and where the pylorus was completely obstructed by scirrhus. As in the case in question,

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* Journal de Physiologie, tom. 3.

yellow fever did not deviate from its ordinary type, we cannot, from a single exception, argue against those facts which have been offered.—The matter of black vomit, then, does not come from the liver ; it is then, the product of a vascular exhalation. This necessary conclusion becomes then a certainty, if, as has been affirmed, Dr. Ffirth found this matter perfectly formed in the arteries of the stomach.

In relation to the cause of the yellow colour of the skin, and the origin of the colouring matter which produces this tinge, I have already observed that Autenrieth and others have seen the serum of the blood assume a yellow colour in patients free from all bilious complications ; that in old persons, the skin sometimes assumes a yellow colour in full health ; that M. Breschet, in the icteric cadavers of the new-born, has witnessed no sign nor augmentation of hepatic secretion, nor any change in the bile. To this I will add that the chemical analysis of the blood of the newly-born, jaundiced for fifteen days, made by M. Lassaigne, he was unable to detect, either in the serosity, or in the fibrin, or in the cruor, a single atom of any of the elements of the bile, or any of that resin which imparts to the bile its peculiar colour. I have already observed that the icterus of new-born children is sometimes partial, and that foundlings are much more exposed than others, because they are not so well preserved from the effects of cold, in passing from the uterus into the world ; that M. Dalmas has often failed to find any thing morbid in the liver of yellow fever subjects, and the yellowness first shows itself in striæ along the course of the great blood vessels, and not upon the conjunctivæ as in hepatic icterus. Finally I have observed that some men are universally icteric, that biliary diseases are not more common among this class than others ; that this colour depends either upon the chemical constitution of the blood, or upon the peculiar elaboration which this fluid undergoes in the capillary system of the dermoid structures,—structures known by the name of the mucous bodies of Malpighi ;—that finally, certain conditions, either physiological, as pregnancy, or pathological, determine in the different species or races of men, anomalous coloration of the skin, both local and universal.

From all these facts, it follows that the yellow colour of the skin does not require the previous increase of the biliary secretion, since this colour may coincide with the permanent physiological state of the individual or even with entire species ; nor does it require a deviation or the transportation of this humour out of its ordinary channels, nor its transfusion into the blood, since in the case of icterus, chemical analysis cannot detect in this liquid a single atom of the materials of the bile. Since I read my note to the Institute, experiments on dogs, upon the production of diseases quite analogous to yellow fever both in reference to the course of the phenomena and the pathological state of the organs and the fluids of the body, have been published in the physiological memoirs of M. Gaspard, upon purulent diseases, etc., inserted in the January Number for 1822, of the journal of experimental physiology. The conformity, in all respects, of his fourteenth, fifteenth, and particularly, of his sixteenth experiments with the case which I have reported, is too striking to be passed by in silence.

The following is my observation : In July 1815, a man arrived at Havre, from Fort-Royal, Martinique, after a short passage of 20 or 22 days; on the day succeeding, he came to Rouen, fell sick, and was carried on the same day to the Hotel-Dieu, when he suffered successively with pain of the head, delirium, great agitation, brilliant redness of the eyes, with alternate loss and recovery of sight; hiccough; slight hemorrhages from nose, mouth, anus; a perspiration which tinged his linen yellow; ecchymoses;—on the third day, general and sudden attack of jaundice; coma,—partial convulsion of all the limbs and of the face; carphologia; on the third day the agitation increased, and on the fifth he died.

In the 16th experiment, M. Gaspard injected into the right jugular vein of a dog, two ounces and a half of a fetid fluid, thick, free from acidity, obtained from the stems of fermented cabbage. During the injection movements of deglutition, then vomiting and prostration took place; some hours afterwards, great *malaise*, chest painful on pressure; symptoms of pneumonia, then renewed vomiting and great prostration at the end of 9 hours; during the night, copious liquid stools and very fetid, as black as soot, resembling the dejections of melæna, and composed of a small portion of excrements and mucosity, with a great deal of decomposed blood.

Some time afterwards, a second stool, of a muco-sanguinolent character. On the 15th July, (the 2d day,) increased prostration, adynamia —burning thirst; disgust; urine normal, repeated returns of feeble action of the heart, with irregular action. On the 16th, prostration less; cessation of palpitations; but always intense thirst, loss of appetite and sometimes vomiting of drinks.

On the 17th, same state—18th, extreme debility—excessive thirst, eyes red, inflamed and filled with tears, nose tumefied, obstructed with mucosities, mucous membrane of the mouth red, violet-coloured and phlogosed. About the middle of the day, whitish dejections with some decomposed fetid blood; died during the night, at the close of the fifth day. We observe that all the external tissues were highly injected with blood; their surfaces exhaled a remarkable and very abundant fluid. If we observe no discoloration of the skin, it was because the hair prevented this examination, since the post-mortem convinced us, that it too participated in that eccessive injection of the capilary vessels of all the membrances. It is not necessary to make any comments on the resemblance of the symptoms, their nature and duration during the career of the case.

But let us compare the state of the organs. I opened the body five or six hours after death; it was yet warm, the skin yellow, particularly about the cheeks, axilla, groins, and generally wherever the sub-cutaneous cellular tissue was most abundant.

The skin when inscised, poured out, as in the living, small drops of blood. We may observe something similar, but in a less degree, in cutting the skin when blisters have been applied, or suppuration has commenced. Hardly had the last layer of the integuments been divided and the instrument reached the cellular tissue, when gases escaped through the cut; the cellular tissue then shrank; its layers were injected with a net-work of very fine vessels, resembling an inflamed conjunctiva. Its colour was of a brownish red. It was in the region of the parotid, that I observed this colour; it was the same thing with regard to the axilla and groins. The stomach, the heart, and the colon contained the same black and putrid matters, which had been discharged by stool and vomiting.

All the intestinal canal, the stomach, and especially the large intestines were of a brown red colour, with here and there, patches of a deeper colour; the liver presented nothing peculiar, and the gall-bladder, without being distended, contained a brownish yellow bile.

At this time, we had in the hospital some cases of typhus, arising from the army after the battle of Waterloo. The bodies which I opened before being cold, likewise contained gas, distending the cellular tissue, and escaping with considerable noise through the small punctures made in the skin—and also a high degree of injection in the layers of cellular tissue, and a discharge of blood from the divided surfaces of the true skin.

On opening the *warm* cadavers in the 14th and 15th experiments of M. Gaspard, in which the morbid phenomena resembled those of the 16th experiment already cited, there were found many ecchymosed or petechial spots in the tissue of the heart, of the spleen, mesenteric glands, of the gall-bladder, and *even in the sub-cutaneous cellular tissue.* Inflammation of the digestive mucous membrane, lined with a gelatiuous and sanguinolent layer, similar in appearance to the lees of wine, and the washings of flesh, was accompanied with moderate thickening of the tissues, and presented a hemorrhagic or scorbutic appearance.—In these two experiments the injection was composed of putrid animal matters.

On inspection of the body, in the 16th experiment, the skin, the *subcutaneous cellular tissue*, and the muscles presented the same aspect as after death from asphyxia, and were not entirely exempt from inflammation.

The conjunctivæ, pituitary, and buccal mucous membranes were red, or of a violet colour, and lined with a *thick coat of mucus*, of an unusual *appearance.*

A large fibrinous clot of a yellowish white filled the right ventricle extending into the pulmonary artery, the veins, superior cava, azygos, etc., and explains the powerful palpitations observed during life. The intestinal mucous membrane, particularly near the duodenum and rectum, and less so in the small intestines, was of a violet red, inflamed in longitudinal lines, and *irregular plaques* which even tinged the outside of the intestine. Again, this inflammation was unaccompanied with thickening of the tissues, free from ulceration, and bore a marked resemblance to ecchymosis and hemorrhage. In the duodenum there existed open blisters from which much dark fluid blood was made to escape in pressing the neighbouring mesenteric vein. This last phenomenon is similar to that reported by Dr. Ffirth. To comment upon the striking similitude between these facts and those reported by M. Gaspard would be superfluous. The difference between them are not more striking than between observations taken by different physicians in the same epidemic, either of yellow fever or any other disease.

In the cases reported, the cause of all the pathological phenomena, and the subsequent organic changes is evident. It is the introduction into the blood, of putrid matters. In the case of yellow fever and of typhus, the same agents produced the same or similar effects. This fact has already been demonstrated. Only in epidemics it is under another form, by other vehicles, and upon other surfaces, that the agents act. But through these surfaces they do not less promptly enter the circulation. It is, however, of little consequence what part of the circle is described—it suffices to know that the blood and the putrid matters are brought into contact.

The chemical changes brought about in the condition of the blood, a species of hemorrhagic inflammation of all the mucous and cutaneous tissues, and the different exhalations of blood more or less elaborated, which take place, constitute the characteristics of the cases in point.—Nothing is seen which might lead to the supposition, that there is either an increase of hepatic secretion, or that the biliary fluids escape from their accustomed channels and enter the circulation. I persist then in the conclusions which I have already stated, and insist upon the importance in pathological physiology of the two anatomical facts, which I was not perhaps the first to describe, because I made the post-mortems too late.

The following are the facts:—1st. The exhalation of gas in the subcutaneous tissue, and 2d, the injection of the layers of this tissue, and of the entire thickness of the true skin by blood, which does not usually penetrate these tissues; injection whence results, as well as from the chemical alteration of the blood, and a peculiar elaboration effected in the skin, the yellow colour of the membranes.

This fluxion is evidently the same in regard to the dermoid system as are the hemorrhagic congestions upon the neighbouring mucous surfaces. If there is no hemorrhage from the skin, it is because its texture is too close to allow this fluid to escape. I insist upon this injection of the vascular net-work, pervading the cellular and cutaneous tissues,—a system of vessels through which the blood cannot penetrate in a state of health;—for it is important, in a therapeutic and physiological point of view, not to confound a mechanical phenomenon, such as the effusion of blood by contusion with a chemical phenomenon, such as the change in the condition of an altered fluid, either from the effects of a particular elaboration which is impressed upon it by the system of vessels through which it circulates, or by some re-agent which is introduced into it.

We may then from the facts which I have stated, deduce this singular consequence; (viz:) an explanation of the frequent alternations of epidemic yellow fever, with yellowness and vomiting, with epidemics characterised by vomiting without jaundice. It is clear, that, when the fluxion towards the mucous membrane is very great, and the predominant symptom, then the skin is less injected; and *vice versa*, when this fluxion predominates in the skin, the mucous membranes should be less likely to be the seats of an active hyperæmia."

Gaspard, following up his experiments, showed that putrid liquids injected into the cavity of the pleura &c., produced the same effects as injections into the veins. Upon Magendie's experiments, he makes the following observations.

"Magendie, indeed, has not discovered this absorption to be very injurious to the pulmonary, gastric or intestinal mucous surfaces; but in my opinion, this would have become so, had the experiments been continued for a much longer time. This distinguished Physiologist should have been less struck perhaps by this result, than any other, since he had observed on a former occasion, that the most energetic vegetable poisons are absorbed more feebly and cause death more slowly on the application of them to mucous surfaces than on serous, or even by being introduced into various organs. Such is the case with the inoculation of the venom of the viper as compared with its ingestion, likewise with the poison of Indian arrows, so pernicious in wounds, but slightly so in the stomach. It becomes us in this instance to admire the protecting wisdom of nature, for if putrid or any poisonous substances whatever, could have been readily absorbed by the respiratory or digestive passages, there is no doubt, that the species of carnivorous animals, as an example, that so often live on rotten flesh for want of fresh, would no longer be in existence; by another wise precaution, they have received very short intestines, compared with those of herbivorous animals, in order, perhaps, that their putrid excrements should be retained for a very little while, and might not have time to become fatal. Moreover, there is no doubt, that but for this difficult absorption of putrid bodies, the human race itself would have been decimated oftener than it is, and that, for example, the lovers of tainted meat and strong game, would have become its victims on every indulgence, as well as those, who breathe the emanations of putrid bodies. Nevertheless, in spite of this admirable precaution, men and domestic animals but too generally succumb to these terrible maladies, especially epidemics, which bear the greatest resemblance to those produced artificially in animals by the injection of putrid liquids into their veins, the causes and symptoms of which are traceable entirely to putrefaction. In all these diseases, the blood plays the chief part and becomes the essential seat of the evil; it is manifestly altered, very black as well as viscid, to a great extent robbed of its plasticity and its fibrine, and very different from what it is in febrile and inflammatory affections. Under these diseases, it exudes from the economy by a species of vascular transudation, after the manner of what is called passive hemorrhages, which continue or happen even after death, under the form of petechiæ or ecchymoses, &c. In all these maladies, in a word, there are general and local symptoms of putrescence, such as fetid breath, urine, sweat, alvine dejections, &c., such also as meteorism, emphysema, the disengagement of gas in the intestines, in the cellular tissue, and even in the blood;—such, in short, as anthrax, partial gangrene, softening of the flesh and the very rapid decay of the dead body. At least, this is what numerous and credible observers have well established in scurvy, plague, the yellow fever of Siam and America, the adynamic or putrid fever, fitly so called, various forms of typhus and pernicious fevers, in gangrene, gangrenous sore throat, dysentery, in gangrenous ergotism, carbuncle, in the epizotic form of carbuncula fever, and finally in malignant pustule. We might add to this list of putrid disorders, that which hunger causes in its last degree; that which results from the abuse of mercury;—in short, that which the venom of many snakes produces."

The last extract, which we shall make upon the subject, is from

Christison.* It would be easy to extend these extracts, but perhaps more than enough have already been given.

"The tendency of putrefaction to impart deleterious qualities to animal matters, originally wholesome, has been long known, and is quite unequivocal. To those, who are not accustomed to the use of tainted meat, the mere commencement of decay is sufficient to render meat insupportable and noxious. Game, only decayed enough to please the palate of the epicure, has caused severe cholera in persons not accustomed to eat it in that state. The power of habit, however, in reconciling the stomach to the digestion of decayed meat, is inconceivable. Some epicures in civilized countries prefer a slight taint even in their beef and mutton: and there are tribes of savages still further advanced in the cultivation of this department of gastronomy, who eat with impunity rancid oil, putrid blubber, and stinking offal. How far putrefaction may be allowed to advance without overpowering the preservative tendency of habit, it is not easy to tell. But with the present habits of this and other civilized nations, the limit appears very confined.

Putrid animal matter, when injected into the veins of healthy animals, proves quickly fatal; and from the experiments of Gaspard and Magendie, together with the more recent researches of M. M. Leuret and Hamont,† the disease induced, seems to resemble closely the typhoid fever of man.

Similar effects were observed by Magendie, when dogs were confined over vessels, in which animal matter was decaying, so that they were obliged always to breathe the exhalations. These discoveries throw some light on the question regarding the tendency of putrid effluvia, to engender fever in man; and notwithstanding many well ascertained facts of an opposite import, they show, that probably in peculiar circumstances, decaying animal matter may excite epidemic fevers. A detailed investigation of this important topic would be misplaced here, as it belongs more to medical police than to medical jurisprudence; but the two works quoted below are refered to for examples, in my opinion, of the unequivocal origin of continued fever in the cause now alluded to;‡ and other instances of the like kind will be found in the Report of the Parliamentary Commission on the health of towns.

Another affection sometimes brought on by putrid exhalations, is violent diarrhæa or dysentery, of which a remarkable instance lately occurred in the person of a well-known French physician, M. Ollivier. While visiting a cellar where old bones were stored, he was seized with giddiness, nausea, tendency to vomit, and general uneasiness; and subsequently he suffered from violent colic with profuse diarrhæa, which put on the dysenteric character and lasted for three days.§ Chevallier, in noticing this accident. mentions his having been affected somewhat in the same way when exposed to the emanations of dead bodies; and

* Christison on Poisons, p. 490; Amer. Edit.

† Journal des Progrès des Sciences Médicales, 1827, v. i. 181.

‡ De divers accidens graves, occasionés par les miasmes d'animaux en putrefaction. Mém. de la Soc. de Med., v. i. 97.—London Med.Chirurg. Review, v. i. 202.

§ Annales d'Hyg. Publique et de Med. Légale, v. ii. 216.

it is a familiar fact, that medical men, who engage in anatomical researches after long disuse, are apt to suffer at first from smart diarrhæa.

The same remark must be applied here as at the close of the observations in the last section. Without peculiar concurring circumstances, no bad effect results. This will follow from many facts illustrative of the innocuous nature of various trades, where the workmen are perpetually exposed to the most noisome putrid effluvia. But no facts of the kind are so remarkable as those collected in regard to the establishment at Montfauçon by Parent-Duchatelet, who makes it appear, that this most abominable concentration of the worst of all possible nuisanses is not merely not injurious to the health of the men and animals employed in and around it, but actually even preserves them from epidemic or epizootic diseases.*

The effects of putrid animal matter, when applied to wounds, have been investigated experimentally by Professor Orfila, who found, that putrid blood, bile, or brain, caused death in this way within twenty-four hours,—producing extensive local inflammation of the diffuse kind, and great constitutional fever. In man also, several instances of diffuse cellular inflammation have been observed as the consequence of pricks received during the dissection of putrid bodies. The disease, as formerly observed, certainly arises in general from pricks received in dissecting recent bodies. At the same time, a few cases have been traced quite unequivocally to inoculation with putrid matter;† and if any doubts existed on this point, the experiments of Orfila would remove them.

Mr. Lassaigne has examined chemically the putrid matter formed by keeping flesh long in close vessels, and has found it to consist of carbonate of ammonia, much caseate of ammonia, and a stinking volatile oil,—the last of which is probably the poisonous ingredient."

The theory then, of the etiology of yellow fever, may be thus stated: From the accumulation of filth in large cities (chiefly night-soil and the animal matter of urine,) putrefaction must necessarily take place, and from this putrefaction, *under certain meteorological conditions*, there is generated a poison, which, either in the form of a volatile oil, or other organic matter, held in solution by ammonia, floats in the atmosphere; is inhaled during the respiratory movements; is taken into the circulation and poisons the system. It produces specific effects, as much so, as the matter of small-pox or scarlatina.

The formation of this poison begins under certain meteorological conditions, which are utterly unknown to us, continues while they last, and ceases with them.—As we have said before. the poison is not a gas, but a volatile substance, constituted of organic matter, as much so constituted, as the matter of small pox or hydrophobia.

To these views certain objections have been offered, which we shall now reply to.

1st. It is objected, that the closest chemical analysis of the atmosphere can detect no such substances. The chemists themselves must

* Annales d'Hyg. Publique et du Med. Légale, v. iii. and ix.

† Dr. Duncan, Edin. Med. Chirurg. Trans., i. 502 and 520.

answer this objection, and for this purpose I shall select the authority of Sir Humphrey Davy, of Mülder and of Raspail.

The opinions of Sir H. Davy are uttered from the lips of one, whom he chooses to call 'a stranger,' but are evidently his own.

"I said, 'I believe several scientific persons, Brocchi amongst others, have doubted the existence of any specific matter in the atmosphere, producing intermittent fevers, in marshy countries and hot climates; and have been more disposed to attribute the disease to physical causes, dependent upon the great differences of temperature between day and night, and to the refrigerating effects of the dense fogs, common in such situations, in the evening and morning; and, on this hypothesis, they have recommended warm woolen clothing and fires at night, as the best preventives against these destructive diseases, so fatal to the peasants who remain in the summer and autumn in the neighbourhood of the maremme of Rome, Tuscany, or Naples."

"The stranger said, 'I am acquainted with the opinions of the gentlemen, and they undoubtedly have weight; *but, that a specific matter of contagion has not been detected by chemical means, in the atmosphere of marshes, does not prove its non-existence.* We know so little of those agents that affect the human constitution, that it is of no use to reason on this subject. There can be no doubt that the line of malaria, above the Pontine marshes is marked by a dense fog morning and evening, and most of the old Roman towns were placed upon eminences out of the reach of this fog. I have myself experienced a peculiar effect upon the organs of smell in the neighbourhood of marshes in the evening after a very hot day; and the instances in which people have been seized with intermittents by a single exposure, in a place infested by malaria in the season of fevers, gives, I think, a strong support to something like a poisonous material existing in the atmosphere in such spots; but I merely offer doubts. I hope the progress of physiology and of chemistry will at no very distant time solve this important problem."*

The italics are my own. Instead of 'a specific matter of *contagion*,' it is obvious that the words should be 'a specific poison.'

I shall now cite Mülder. "Finally, Boussaingault, Verver, and others, have shown the presence in the atmosphere of the substances, which contain hydrogen and carbon. Boussaingault and Verver passed atmospheric air, *freed from carbonic acid*, over red-hot copper, and obtained small quantities of water and carbonic acid. By means of the oxygen of the air, the hydrogen and the carbon—no matter in what state they existed in it—would be changed into water and carbonic acid, while passing together over red-hot copper.

"We cannot determine in what state this hydrogen and carbon are contained in the atmosphere; they may be so, in the form of hydrogen gas, carburetted hydrogen, and carbonic oxide, or possibly in that of volatile organic substances. As to this point, nothing has yet been ascertained, nor can it be determined by such experiments as these. It is certain, however, that before the constituents of organized bodies are reduced to their most intimate combinations, they can assume a great many intermediate states—supplying the atmosphere with either solid,

* Consolations in Travel, etc.

liquid, or gaseous products. Thus, in every kind of putrefaction, peculiar volatile substances are diffused through the air, which may contain the four organic elements combined in various ways. Further, in a great many diseases—such, for example, as cause eruptions on the skin—volatile compounds escape from the patients, and are diffused through the atmosphere.* Many substances, also, which, both at ordinary and at more elevated temperatures, are regarded as fixed and not volatile, are constantly emitting particles in a state of vapor, and diffusing them through the atmosphere—such for instance are potash, soda, and even iron, when smelted in blast furnaces. Thus, the atmosphere must contain not only substances consisting of carbon, hydrogen, nitrogen, and oxygen, but a great many others besides, which it would really be of importance to investigate, but on which it is unnecessary for our present purpose to dwell."†

Raspail's language is still stronger. "The eudiometric analysis of the atmospheric air," says he, "will show a certain invariableness and a certain uniformity in its principal constituents in whatever place, and at whatever elevation, observation has carried this instrument of analysis. It will result from it that the air is a constant mixture or a combination in weight of 21 parts of oxygen, and 79 parts of nitrogen, together with a variable quantity of watery vapour, and an exceedingly small quantity of carbonic acid—a quantity more variable than the first.

Without doubt, it is permitted us to consider this analytic composition as representing the normal condition of the atmosphere,—that which suffices for, and is best suited to organic development. But it is repugnant to logic and to observation, to admit it as the constant and invariable constitution of a medium, which, at every instant, is the receptacle and absorbent of so many, and such various gaseous emissions;—experiment, in this respect, with all its graphic apparel of exactness and precision, does wrong to analogy.

How, in fact, suppose that the air of a theatre during the representation can be as simple in its composition as that in the glades of the forest? Is it not contradictory in terms to admit that the air which is breathed on the borders of swamps at the season when the miasms cause fevers, is composed of only the four elements which we breathe every where else for the maintainance of our general health? How vanish away then these ammoniacal, phosphorescent, sulphurous, hydrocyanic emanations, etc., which are discharged into the air from thousands of gaping apertures, such as our privies, our manufactories, our chimnies, our sewers?—from all that ferments and is decomposed; from every one who breathes and gives back to the atmosphere a disoxygenated air impregnated with all the vapours which the respiratory surfaces exhale? Why then has not analysis discovered these emana-

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* See on this subject the elaborate treatise of Dr. Van Guens, Natuur- en Geneeskundige Beschouwingen van moerrassen en moerasziekten. Amsterdam. 1839. (Physical and Medical considerations on Marshes and Marsh Diseases. Amsterdam, 1839.) Mulder's Verhandeling over de Wateren en de Lucht van Amsterdam, (Treatise on the waters and the air of Amsterdam,) p. 163, may also be consulted on this subject.

† The Chemistry of Vegetable and Animal Physiology, by Dr. G. J. Mülder, etc. Amer. edit., p. 101.

tions? Because, at first, she has never thought about them. She has begun to tread this path since we have pointed it out, and to be conscious that the former processes possessed but an apparent and conventional precision. In fact, to estimate the respective quantities of oxygen and nitrogen, recourse is had either to detonation by electricity or to the action of phosphorus. In the first case, to the air employed, there is added a quantity of hydrogen greater by 42 volumes; the eudiometer is exploded, and the quantity of oxygen is computed by the quantity of hydrogen transformed into water; the quantity of nitrogen being estimated by the difference. In the second case, a stick of phosphorus is introduced into the instrument for the purpose of absorbing the oxygen and transforming it into phosphoric acid. The gaseous portion not absorbed, represents a mixture of nitrogen and carbonic acid. To absorb this last, a solution of fixed alkali is used, and the analysis goes no further. The volume remaining, separated from its oxygen by the phosphorus or by the explosion, and from its carbonic acid by the potash, can be nothing else than the nitrogen.

Now let us suppose that the volume of atmospheric air subjected to experiment had contained other gaseous elements in a state of combination or mixture; let us examine if, by such processes as the preceding, the analysis could detect them. Some examples will suffice to show us the value of such a supposition. Admit, that the air contains a certain quantity of free ammonia;—the Phosphorus will convert it into fixed phosphate of ammonia, absorbing oxygen at the same time. If the ammonia exist, as an alkaline salt with excess of base, the phosphorus, now become phosphoric acid, will fix this salt, by saturating it and transforming it into a double salt of ammoniacal base. But in both cases, this quantity of ammoniacal gas will go, unknown to the experimenter, to the account of the oxygen. Admit in the air the existence of some acid emanation, of any nature whatsoever;—this acid, in the proof with potash, will pass to the account of carbonic acid. Finally, the gases which neither phosphorus nor potash can absorb—sulphuretted and carburetted hydrogen, carbonic oxide, &c., neutral volatile salts &c.,—all these will go to the account of the nitrogen, the residue of the analysis, which the analysis measures but does not seek to absorb or decompose.

Consequently the atmospheric air is not, at every moment, as pure as the eudiometrical analysis would seem to indicate. Doubtless the existence of these emanations into the air cannot be neither permanent nor invariable; and it must be admitted, that the electric power of the solar ray,—that the lightning which ploughs through the immense eudiometer of the atmosphere—combines, decomposes in a thousand different ways these elements, already so diverse. Why should it be otherwise in the expanse of the atmosphere, than in the vessels of our laboratories!"*

So much for this objection, which is so frequently repeated and so confidently relied on.

Indeed, from certain facts recently brought to light, it would appear, that we have good reason to hope, that the special poison of yellow

* Histoire Naturelle de la Santé et de la Maladie; tom. i. p. 40.

fever may itself be detected and chemically examined. We are told by Liebig, that "all the observations hitherto made upon gaseous contagious matters prove, that they also are substances in a state af decomposition. When vessels filled with ice are placed in air impregnated with gaseous contagious matter, their outer surfaces become covered with water, containing a certain quantity of this matter in solution. This water soon becomes turbid, and in common language putrefies, or, to describe the change more correctly, the state of decomposition of the dissolved contagious matter is completed in the water."*

I have already spoken of the probability of this poison being held in solution by ammonia. The following quotation bears upon that point. "The odor of gaseous contagious matters is owing to the same cause; (i. e. eremacausis, or oxidation at common temperatures,) but it is also generally accompanied by ammonia, which may be considered in many cases as the means through which the contagious matter receives a gaseous form, just as it is the means of causing the smell of innumerable substances of little volatility, and of many which have no odor.

Ammonia is very generally produced in cases of disease; it is always emitted in those in which contagion is generated, and is an invariable product of the decomposition of animal matter. The presence of ammonia in the air of chambers, in which diseased patients lie, particularly of those afflicted with a contagious disease, may be readily detected; for the moisture condensed by ice in the manner just described, produces a white precipitate in a solution of corrosive sublimate, just as a solution of ammonia does. The ammoniacal salts also, which are obtained by the evaporation of rain water after an acid has been added, when treated with lime, so as to set free their ammonia, emit an odor most closely resembling that of corpses, or the peculiar smell of dunghills."†

In a paper published by Dr. Hort in this Journal, (Jan. 1846) we also meet with some very interesting remarks, and some experiments performed by Prof. Riddell. As they are relevant, I quote them.

"It has been established by the experiments of Moscati and Boussingault, that organic matter exists in extremely small quantities in the noxious air that hovers over marshes. Moscati, many years ago, suspended in the air, over the rice grounds of Tuscany, a globular glass filled with ice. An abundant deposition of dew took place upon its surface, which, when collected, appeared at first to be pure limpid water. There was soon, however, an appearance of little flakes, possessed of properties peculiar to animalized matters, and finally, at the end of some days, the liquid putrefied completely. (*Riddell.*)

"These facts are testimony directly to the point, and they are not yet exhausted. In a memoir read before the French Academy of Sciences, in 1834, M. Boussingault reports some striking experiments tried by him at Cartago, in South America. In the middle of a swampy meadow, in every instance, carbonaceous matter was detected in the dew, by the addition of sulphuric acid.— He remarks: "The results obtained prove very clearly that in marshy places, during the precipitation of dew, there is an organic matter deposited with it."
(*Riddell.*)

"The following experiments made by Professor Riddell, "with a view of detecting the aerial miasm of small pox," are so much in point, and so interesting, that I should do injustice to the subject were I to omit to quote them. The ap-

* Agricultural Chemistry. Amer. Edit., p. 407. † Op. cit.

paratus is thus described: A perfectly clean ounce phial was half filled with distilled water; a small glass tube, with a capillary orifice, was made to terminate near the bottom, the upper and much larger portion of the tube bending horizontally to receive the silver nozzle of a delicate pair of bellows; several turns of gauze were passed around the mouth of the phial, embracing the tube, and the whole was securely fixed in an appropriate frame-work. This apparatus was carried to the city* pest house, and under the superintendence of Dr. Herron, it was placed on a table two or three feet from a small pox patient, just in that stage of the disease when the circumambient air was supposed to be most contagious. The bellows were blown by the nurses pretty constantly for twelve hours, thus presenting a great amount of noxious air to the distilled water. The apparatus was left undisturbed until it came into my possession, three days after, when I made the following experiments.

1. One-fourth of a drachm of the water contained in the phial, evaporated very slowly in a watch glass, over an alcohol lamp, left concentric circles of a whitish substance. Upon bringing this residue under the object glass of a good microscope, I discovered that it consisted mostly of long crystals, which shot from each other at right angles. The outer margin of each concentric band was less distinctly crystalline, and evidently contained some other substance.

2. A minute drop of sulphuric acid, carefully distilled and collected on a glass rod, so as not to leave the slightest trace when evaporated from clean glass, was placed upon some of the residue. [Experiment No. 1.] The application of heat rendered the acid black, and upon complete evaporation a dark stain was left, thus showing the presence of organic matter.

3. Upon adding a drop of pure sulphuric acid, to near an eighth of a drachm of the water, and expelling the water by a careful heat, the acid became black. This experiment, as well as the one which follows, was performed upon a piece of Florence flask, rinsed in clean water, and then heated to redness over an alcohol lamp, in order to remove every trace of organic matter.

"A drop of the water hastily evaporated, left a whitish residue, not crystalline to appearance, but consisting of extremely minute grains. Upon the application of a high heat short of redness, it became dark colored, indicating the presence of organic matter by the charcoal liberated. A still higher heat, in contact with air, removed the dark color, and left a mere trace of white adherent powder."

Now, though we do not at all believe yellow fever to be contagious, still, it is plain, that if its cause be a poison floating in the atmosphere, we ought not to despair, that it may be detected, when chemical analysis is further advanced. Add to this, the fact that Lassaigne detected, in his analysis of putrid meat-water, a stinking volatile oil, which he presumed to be the poisonous agent, which produced the singular effects developed in the experiments of Gaspard and Magendie: and the probability of detection is still further enhanced.

2. It may again be objected, 'that Yellow Fever in men is not caused by injections into the veins, and that, therefore, the experiments of Gaspard and Magendie are inconclusive. Certain similar effects are doubtless produced in these experiments, but death takes place too soon,—the symptoms are too rapid and violent, to constitute such affections entitled to the name of yellow fever—at all events, there is but a crude similitude;—the animal is poisoned in a peculiar manner, but we can predicate nothing more.'

In reply, we may say, that, admitting the facts concerning the rapid and fatal symptoms, they so closely resemble those of yellow fever, and

* Cincinnati.

the post-mortem lesions of the two are so much alike, that we must infer similar causes have been in operation. The rapidity with which death ensues is doubtless owing, partly to the manner in which the poison is introduced, partly to the concentration of the poison. That such is the case, we learn from Magendie, who states, that when the poisonous liquid was placed in the stomach or alimentary canal, or injected into the trachea, there were scarcely any effects produced, and also, that when the poisonous matter was filtered, its effects were much less violent and the animals survived much longer.

3. It is again objected, 'that, even after Magendie's own showing, when the animal merely breathed the effluvia, though it died, there were no symptoms of yellow fever,—they were rather those of typhoid fever. Therefore, we cannot with propriety contend, that mere exhalations from putrefied matter are capable of producing yellow fever. The experiment, indeed, which Magendie says he has often repeated, seems to prove the contrary.'

The conclusion is hasty and much too general in its tenor. It is not contended, that *all exhalations* from putrid organic matter are capable of producing yellow fever, but that it is caused by emanations of a peculiar kind, produced under peculiar circumstances.

Although putrid matters injected into the veins cause even more violent effects than we usually witness in yellow fever, (these effects bearing all the strongest characteristics of that disease) ; though mere typhoid is generated by inhalation, still it does not follow, that, under other conditions of putrefaction, a poison may not be generated sufficiently strong to produce yellow fever ;—at least in man. It comes to this at last :—that a poison is generated by the decomposition of nitrogenized organic matter, which, if injected into the veins of animals, causes speedy death with peculiar symptoms and post-mortem lesions, strongly resembling those of yellow fever ;—that if this poison be injected in a less concentrated state, sickness ensues with similar symptoms—ending in death, but at a longer interval. If the poison be merely inhaled, death ensues, but the symptoms are modified,—instead of those of yellow fever we have typhoid. But of the existence of the same identical poison operating in all these cases, there can be no doubt.

4. Another objection is, 'that our domestic animals are not subject to yellow fever.'

It is a common opinion in New Orleans, that animals recently imported, are, generally speaking, indisposed during an epidemic : but be this so or not, it proves nothing. For, it is a well known fact, that all races of animals and even individuals of the same race are not equally susceptible of morbific influences. A striking difference in this respect may be witnessed between the negro and the white, during an epidemic of yellow fever ; and even among individuals of the white race. Some have the disease in its most malignant form—some have it very mildly—some, not at all. There are peculiarities of constitution &c., which make the difference. It is probable, that if a human being had been placed in the situation of Magendie's dogs over the grating, they would have perished much sooner and with more violent symptoms.

5. The next objection has been much insisted on. Our attention is called to the fact that butchers, *vidangeurs*, knackers, etc., who live in

the midst of the most disgusting filth, are not only not subject to epidemic diseases more than other people, but, in fact, appear to be more exempt from them. "In the year 1828, a committee was appointed in Paris, to inquire into the circumstances connected with the knacker's operations. Every one, examined by the committee, agreed, that they were offensive and disgusting, but no one that they were unwholesome. It was even inferred that they were conducive to health. All the men, women, and children, concerned in the works, had unvarying health, were remarkably well in appearance, and strong in body. The workmen commonly attained old age, and were generally free from the usual infirmities that accompany it. Sixty, seventy, and even eighty were common ages. Persons, living close to the places, or going thither daily, shared these advantages with the workmen. During the time that an epidemic fever was in full force at two neighboring places, not one of the workmen in the establishment of Montfauçon was affected by it. Nor did it seem that this freedom from disease applied altogether to the men that were habituated to the works; for when from press of business, new workmen were taken into the establishment they did not suffer in health from the exhalations."*

Of that delightful place, Montfauçon, Christison gives the following description. "The Voirie et Chantier d'Ecarrissage of Montfauçon, which has existed close to the walls of Paris, for several centuries, is an enclosure of many acres, where the contents of the necessaries of the city are collected in enormous pits, and where horses, dogs, and cats are flayed to the amount of forty or fifty thousand annually. The fat is melted for blow-pipe lamps; the bones are in a great measure burnt on the premises for fuel; the intestines are made into coarse gut for machinery; the flesh, blood, and garbage are heaped to putrefy for manure, and in summer a bed of compost is spread to breed maggots for feeding poultry. There is no drain. Description cannot convey an idea of the stench. The committee of the Board of Health, appointed to make inquiries into the best mode of abating the nuisance, in vain attempted to penetrate into the place. Yet the workmen and their families are stout, healthy, and long lived." †

We must admit the health of the workmen, as the fact is established on good authority, but we cannot admit there is any thing preventive of disease in the abominable emanations of such a place. It is plain that the epidemic fever spoken of as raging in two neighboring places, could not have arisen from the decomposition of animal matter. There are various causes of epidemic diseases, of which organic putrefaction is one, but not the only one. Situations, as pure as possible, might have been equally exempt from these epidemics.

But admitting the healthiness of this and similar places; what does it prove! At the best, it is but a negative fact. Because disease is not generated at Montfauçon, does that prove that animal putrefaction in other latitudes or under different circumstances cannot generate it? Not at all. On the other hand, we have abundance of *positive* proof that many and exceedingly malignant disorders have been traced directly to

* Dunglison's Elements of Hygiena p. 108.
† Op. cit. p. 490, Amer. Edit.

such causes. Take the following instance ; "according to Percy, a Doctor Chambon was required by the Dean of the Faculté de médecine of Paris, to demonstrate the liver and its appendages before the "Faculty," on applying for his license. The decomposition of the subject, given him for the demonstration, was so far advanced, that Chambon drew the attention of the Dean to it, but he was required to go on. One of the four candidates, Corion, struck by the putrid emanations, which escaped from the body as soon as it was opened, fainted, was carried home, and died in seventy hours : another—the celebrated Fourcroy—was attacked with a burning exanthematous eruption ; and two others, Laguerenne and Dufresnoy, remained a long time feeble, and the latter never completely recovered. 'As for Chambon,' says M. Londe, 'indignant at the obstinacy of the Dean, he remained firm in his place ; finished his lecture in the midst of the commissioners, who inundated their handkerchiefs with essences, and doubtless owed his safety to his cerebral excitement, which, during the night after a slight febrile attack, gave occasion to a profuse cutaneous exhalation.' "*

One such fact must overthrow a thousand which possess a mere negative character.

It does not follow by any means that because the nostrils are offended by disgusting odors that the putrescent mass must also omit substances deleterious to human health. But the great fallacy of this objection consists in supposing the putrefactive process and its results to be invariably the same. Never was there a greater mistake.

All organic substances capable of undergoing the putrefactive process are highly complex in their chemical constitution ;—of all bodies the most so. The process indeed belongs to a class of chemical actions altogether different in form and manifestation from ordinary decomposition. There occurs a transformation among the organic molecules into simpler compounds, sometimes with the accession of oxygen and water ; sometimes without. But what these compounds shall be, is dependent upon a thousand circumstances. The slightest variation in the quantity of heat, of light, of moisture, of more or less oxygen,—even the character of the bodies in contact with the substances undergoing the process, exercise a wonderful influence, and give rise to entirely different products. This perhaps will be made more apparent by the language of Liebig.

"The juices of carrots, beet-roots, onions, &c., are rich in saccharine matter; when fermented at common temperatures they yield the same products as the juice of the grape, namely, carbonic acid and an alcoholic fluid, and the nitrogenous constituent of the juice is deposited in the form of a sediment or dregs. At a higher temperature, from 100° to 120°, for instance, the manner of the whole transformation is changed. There is less evolution of gas, and no alcohol is formed. When the process of fermentation is completed, and we examine the fluid, we find none of the sugar which was originally present; its elements have become transformed into a large proportion of lactic acid; and besides this, there is a substance perfectly analogous to gum, and another substance, which, moreover, is the most remarkable of these products, of a crystalline appearance, identical in properties and constitution with the sweet constituent of manna.

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* Dunglison op. cit. p. 110.

"*Alcohol and carbonic acid* are the products of the transposition of sugar atoms at the common temperature. At a higher temperature the results of the transformations effected by fermentation are, carbonic acid, mannite, lactic acid, and gum.

"We have every reason to believe that at high temperatures the manner of transformation in the elements of the substance exciting fermentation (the ferment) becomes altered, and that the change in the directions in which its atoms transpose and arrange themselves, determines the new arrangement of the sugar atoms, and the new positions which they assume, because it is not only by different temperatures that different products may be formed out of one and the same substance, but the same result is obtained if an alteration is made in the nature of the ferment transmitting its own action to the substance upon which it operates.

"Milk becomes sour from the sugar of milk which it contains being converted into lactic acid; this transformation caused by the presence and contact of the caseous matter, which is itself undergoing a state of change, of transposition of its atoms, from the influence of the atmospheric oxygen. If, after the entire disappearance of the milk-sugar, we add a new portion, the process of fermentation will continue as long as any cheese remains in the fluid to act upon the sugar.

"This manner of fermentation of milk-sugar without evolution of gas, is confined to the common temperature. At higher temperatures, as from 76° to 86°, the products are totally different; the cheese assumes the properties of ordinary yeast, and two consecutive processes of transformation take place in the milk-sugar. In the first place, the sugar enters into chemical combination with a certain amount of water, and becomes converted into grape-sugar; that is, a sugar possessing precisely the same character as that found in grapes; and subsequently to this it is further transformed, while still in contact with the cheese, into alcohol and carbonic acid.

"Thus, whilst milk at common temperatures yields lactic acid as the principal product of the decomposition of its sugar, at a higher temperature we obtain as the product, an alcoholic fluid, which, upon distillation, furnishes a true brandy.

"If instead of yeast a small quantity of white cheese-curd is added to a solution of common sugar, and kept at a temperature of 76° to 86°, and moreover, some chalk added to maintain the fluid in a neutral state, a lively evolution of gas soon takes place, the sugar totally disappears. Carbonic acid and hydrogen are given off in a gaseous state; and we find in the fluid a copious amount of *butyric acid*, one of the most interesting of the organic acids, which, until recently, was known only as a constituent of milk or of butter.

"In ordinary fermentation the atom of sugar divides itself into two products, but in the fermentation I have just described three new substances are produced; in the former, alcohol and carbonic acid, in the latter, instead of these, we obtain, *butyric acid*, hydrogen gas, and carbonic acid. The relation of these substances to each other cannot be mistaken. Alcohol is *butyric acid+hydrogen.* The atom of butyric acid is an alcohol atom, from which two atoms of hydrogen have separated.

"Thus, alterations in the nature of the products of fermentation ensue with every variation in the process induced by changes of temperature or the presence of matters accidentally drawn in to participate in the transformations.—The same grape-juice, when fermented at various temperatures, yields wine of dissimilar qualities and nature, depending upon the circumstance of the temperature of the air being higher or lower during autumn, and according to the depth of the cellar in which the fermentation is conducted, which vary the quality, the odour, and the flavour of the wine. An uniform temperature of the place where the fermentation is conducted, ensuring its slow and gradual progress, are the principal conditions depending upon our own control for the production of the best kinds of wine. The growers of wine will soon universally give the preference to deep rocky cellars or vaults for conducting the process of fermentation; such vaults have been found particularly appropriate for the fabrication of the superior varieties of beer, and the advantages of these vaults mainly depend upon their constant temperature.

"The influence which extraneous substances exercise upon the products of vinous fermentation is strikingly exemplified in the fermentation of potato-mash. It is well known that in the manufacture of potato-spirit an oily liquid is obtained, besides the alcohol, possessing poisonous properties, a highly disagreeable smell, and nauseous taste; this is called *fusel oil*. It does not exist ready formed in the potato, but is a product of the transformation of sugar, for it is produced not only in the fermented potato-mash, but also in the fermentation of the last syrups obtained during the preparation of the beet-root sugar."

To be sure, Liebig is speaking rather of fermentation than putrefaction; but the difference is more in words than in reality. To use his own language, "putrefaction is the process of fermentation in organic substances containing nitrogen and sulphur, which give rise to the formation of products of a disagreeable odour."*

It is obvious, therefore, that the putrefactive process may or may not give rise to the production of poisons, according to circumstances.

6. The next objection that occurs to me, is, that the meteorological conditions spoken of as being necessary to the production of the poison of yellow fever, only exist in our own imagination. The assumption of any such conditions is a pure hypothesis, unproved, and unwarranted by any facts. Thus Hillary on the Diseases of Barbadoes:

"It does not appear from the most accurate observations of the variations of the weather, or any difference of the seasons which I have been able to make for several years past, that the fever is in any way caused or much influenced by them: for I have seen it at all seasons of the year, in the coolest as well as the hottest time of the year, except that I have always observed that the symptoms of this, (as well as most other fevers,) are generally more acute, and the fever usually higher in a very hot season, especially if it was preceded by warm moist weather, than it usually is when more cool."† Again, according to Imray, (from whose paper the above remarks are taken,) M. Rufz coincides with the opinions of Hillary. He says, "now whether the thermometer was high or low, whether it rained or was fine weather, hot or cool, yellow fever always prevailed with the same intensity, without the character of the seasons appearing in any way to influence its progress."

Whatever it may be in Barbadoes, such is certainly not the case in New Orleans. The first frost sufficiently severe to kill the leaves of our forest trees, or our annual plants, puts a sudden and effectual stop to the disease as an epidemic. Its production and continuance must, therefore, depend on certain meteorological influences.

Again, the appearance of yellow fever in New Orleans is limited to a few months of the year. We never see an epidemic of it in winter or in the spring.

Again, when violent epidemics—such as those of 1837–39–41—make their appearance in New Orleans, it is notorious that almost every Southern city is also afflicted with the disease. These facts appear to

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* London Lancet, June 29, 1844.

† Observations on the Nature, Causes, and Treatment of Yellow Fever. By John Imray, of Dominica, M. D., etc. Edin. Med. and Surg. Journal, Oct. 1, 1845.

me to be conclusive with regard to the existence of some general epidemic influence.

It is no answer to tell us that we do not know what this influence consists in. Meteorology cannot pretend to the dignity of a science: its facts are too scattered, broken, unconnected.

To overlook meteorological influences when epidemic diseases prevail simultaneously in distant cities, within certain latitudes, seems to me as wise as to contend that effects may happen without causes.

7. But there is another objection, and one in my opinion by far the most fraught with mischief. It has a practical bearing, and is therefore the more dangerous. This consists in supposing that this meteorological influence is the *origo mali*;—the sole and only cause of the disease. If such be the fact our case is hopeless; for no human power can control the meteorological conditions of the earth or atmosphere, and we must, therefore, abandon forever all expectation of eradicating the disease from our city.

But such a gloomy view of the matter is not at all tenable. Unacclimated persons may live within a few miles of the city, and be perfectly safe from attack, provided they keep away. Persons arriving in the city from abroad are liable to attack as early as the second or third day. Now it is obvious that these facts are altogether opposed to the meteorological theory; without indeed it be contended that the meteorological change is confined to the city, which is an absurdity.

If yellow fever arose from a vicious constitution of the atmosphere, it would follow, that almost all persons liable to the disease and living in such an atmosphere, would be stricken at the same time, or nearly so. Making every allowance for more or less susceptibility, one could not live for any length of time in an atmosphere, in which another is poisoned.

The whole rise, course, and progress of our epidemics show, that yellow fever cannot arise from any such general cause. Persons spend months in the very midst of it, to be taken down at last, just as the epidemic is disappearing. Its whole course seems rather to indicate, that it springs from points or *foci*, in which the poison is being continually formed during the existence of the meteorological conditions necessary for its formation; and these foci may be far apart.

In concluding this paper, I may sum up by saying, that in my opinion yellow fever does not depend on putrefaction alone, nor on meteorological influences alone, but on both conjoined; when this conjunction occurs, seething laboratories of poison are put in operation at different points of the city.

If such be the case, the remedy is within our own reach. Keep the city clean—remove all filth as soon as discovered, and let it be cast in the river. But if we really wish to derive a practical benefit, these operations must be carried on upon a far greater scale than has ever heretofore been done.—Sending a few scavenger carts about the streets, is but a poor and piddling affair. A board of health *with police powers* should be created, whose duty should be to inspect private yards and lots—to see that the owners filled them up—that the privies were emptied at least once in three months,—and particularly that no nuisance should be permitted to lie about the levee. Besides all this, the streets should be paved, efficient water-works constructed, and the back of the city thoroughly drained.—Running water will keep sweet—or if foul, when commencing to run, will become sweet by being put in motion. This fact was particularly called to my attention recently by the very able engineer of the draining company. The water of the gutters, foul in the extreme, towards the upper part of Common street, was comparatively purified before it reached Claiborne street. The fact needs no commentary.

New York, Philadelphia, and Baltimore have now by their wise police regulations enjoyed immunity from yellow fever for many years. New Orleans may do the same if she has energy to put her shoulders to the wheel.

II.—*Case of Difficult Labor*—in which Embryotomy was resorted to. Communicated by Trasimond Dupuy, M. D., of Iberville, La.

The attending physician, Dr. F. I. B. Romer, has kindly permitted me to give publicity to the following highly interesting case of dystocia, and I claim for it a space in the columns of the New Orleans Medical and Surgical Journal. The Doctor having furnished me with the necessary data, I have liberally availed myself of them in preparing the present article. Having witnessed the extreme suffering and critical situation of the patient, and thereby fully realized under what embarrassing circumstances our ministrations are often required—it cannot but be gratifying to make known, that an operation so trying has been crowned with success.

June 19th, 1846.—Dr. Romer was called to see Charlotte, belonging to W. H. Avery, Esq.,—ætat 38—well formed and now in labor—having previously borne several children—two or three of whom were still-born. On examination—found an arm protruded through the vagina—the presentation being that of the left shoulder. By some mismanagement of the mid-wife, who, before his arrival, had made considerable traction on the arm—the head was bent on the breast, or rather, the child was bent double.

The child was dead, and so firmly fixed betwixt the sacrum and pubis that the Doctor was unable to turn it. Called in Doctors Pritchard and Dupuy, who were equally unsuccessful in all their attempts to that effect. Thinking that amputation of the arm might assist in bringing about so desirable a result, it was resorted to by Dr. Romer, without, however, facilitating our operations. At every effort of the uterus to free itself—the cries of the poor sufferer were heart-rending in the extreme ; yet we saw that all that had been done, was utterly unavailing. Something more must be attempted, and that quickly. Upon deliberation, it was unanimously decided, that embryotomy would afford the readiest means of relief. With a pair of common shears, therefore, and such instruments as a pocket case affords, (it being inconvenient to procure others in time,) the abdominal and thoracic viscera were removed; the ribs broken, and the spine severed; so that after three hour's very hard work, the whole was extracted, and the patient rescued from immediate danger. During the operation, opiates were exhibited to allay pain, and though some degree of irritation afterwards ensued, it was subdued by antiphlogistics. The patient is now in the enjoyment of excellent health.

III.—*Two Cases of Dropsy, one of which was tapped fifty-seven times in three years.* By Andrew R. Kilpatrick, M. D., of Woodville, Mississippi.

In 1838, I saw Mr. J. S., of Avoyelles Parish, La., who was laboring under general dropsy. He was aged 36 years; small frame; phlegmatic temperament; blue eyes; fair, soft hair, inclined to baldness.—He was fond of spirituous liquors, which he often drank to excess; and this, together with the low marshy country in which he lived, and some hereditary taint, were the causes of his malady. His mother had died of the same disease.

He had had hydrothorax some years before, which was cured by my friend Dr. G. E. Elmer, who was at this time (1838) his medical attendant. He was treated upon general principles, and by the use of the remedies usually employed in such cases, till the dropsy was removed. But by indulgence in ardent spirits, the disease was again brought on the ensuing spring, and became worse than at any previous time. Remedies which formerly relieved him were now powerless—he became swollen to an immense size—excessive dyspnœa, impending suffocation—locomotion was attended with the greatest pain; and he could not lie down at all, but was obliged to sleep sitting in a large arm chair.

On the 2d July, 1839, I saw him in consultation with Dr. Elmer, when we concluded to resort to the operation of paracentesis abdominis. The patient was not only willing to submit to it, but requested that it should be done speedily. Accordingly on the next day, 3rd July, he was tapped by Dr. E. in presence of the writer and several neighbours. He bore it remarkably well; the pulse varied but little, and he was sustained by the use of brandy toddy while the fluid was passing off. Discovering that he could bear it, we drew off all the water we could, and upon measuring it, found we had *eight gallons.*—It was of a pale straw colour and coagulated when heated.

He was very much relieved in every respect. The incision was dressed with an oiled rag and compress, and the entire abdomen and thorax firmly compressed by a roller. His extremities, head and neck, were still distended with fluid, and when we visited him next day, we discovered that they were now shrunk, and the skin hung in huge loose folds and wrinkles, while the abdomen was again enlarged so much that he was obliged to relax the bandage to procure relief. I tapped him and drew off *two gallons* more, making ten gallons in twenty-four hours. This is more than any case on record, save one mentioned by Baron Stoerk, which yielded twelve gallons at one time.

While conversing on the subject, Dr. Elmer proposed, that as hydrocele could be cured by injecting a stimulating fluid into the sac, might not ascites be cured in the same way? The great size of the peritoneal sac and the danger of inflammation in that membrane would seem to forbid a resort to this measure. But might not a part of the same fluid, after being exposed to the air, be injected back into the sac and produce the adhesive inflammation?

The patient at this time placed himself under my care. I found all remedies of no avail in the prevention of the disease. In two months the water had reaccumulated to the same amount and I drew off eight gallons. I continued to visit him and tap him whenever the fluid would distress him; sometimes twice a month, and sometimes once in six or seven weeks. He did not regard the tapping as much as some do a cupping.

From the 3d, July 1839, to the time of his death in Oct. 1842, he was tapped *fifty-seven* times and the fluid discharged amounted in all to about *three hundred and sixty gallons*, being more than *eleven barrels*. I regret very much that I was absent at the time of his death and there was, consequently, no *post-mortem* examination.

Case 2. Amanda, negress, belonging to Mrs. E. W., very black; ordinary size, aged 17; has never menstruated. She had the yellow fever in 1844, and in a few weeks afterwards her extremities became œdematous—she was very languid and inclined to sleep incessantly.—In January 1845, she was placed under my charge. At this time she was very much bloated and evidently dropsical, as the skin pitted on pressure and the urine was albuminous, coagulating when heated. In addition to this she had syphilis.

She was first put upon a course of treatment for this last named disease, which required three weeks to accomplish a cure; but the medicines had no effect upon the ascites even to check it, for at the end of three weeks, the water had accumulated to a great amount in the abdomen and extremities—she was much larger than a woman at the full term of utero-gestation.

Treatment. Her diet was regulated and constant exercise enjoined. The following formula was prepared, viz:

℞ Pulv. Jalapæ		
" Scillæ	àà	℥ ij ss.
Pot. Nitr:		℥ v.
Vin. Teneriffe,		℔j. M.

and often letting it digest twenty-four hours, half a wine-glassful was given thrice a day. Before the first bottle full was consumed she was reduced more than half. It acted freely upon the bowels, producing watery stools from four to six times a day, and copious diuresis.

Another bottle full was prepared and before it was all taken she was completely reduced down to her natural size. She was still able to walk about and had a keen appetite. This last amount did not act so freely as the first had. She was then placed on the use of a more generous diet and some vegetable tonics, and in a short time was discharged completely cured. She has had no return of the disease, and at this time is quite well.

IV.—*Cases in Hospital Practice.* Reported by A. HESTER, M. D., late Visiting Surgeon to the New Orleans Charity Hospital.

1.—*Case of Diabetes, with abscess of the Liver.*

William Ward, ætat. 43, native of Ireland, in New Orleans 3 years; usually in good health; stout and finely developed muscular system, entered ward 24, of the Charity Hospital (service of Dr. Hester) on the 8th April 1844; sick three days. He had been for some time a Porter to the National House, Tchoupitoulas street, had had unrestrained access to the wine cellars; lived almost exclusively on fermented liquors, as porter, ale, &c., &c; no meat; lived on vegetables almost exclusively, and drank two or three bottles of porter during the 24 hours. He was attacked with a chill, pains in left scapula, extending down that side of the chest, across the epigastrium, and terminating in the right hypochondriac region.

The thoracic pains were attended with dyspnœa, frontal cephalalgia, slight fever, etc., frequent micturition, but unattended with pain; a sense of debility across the lumbar spine; great thirst and general *malaise.*

On the day of his entrance, had high fever, pains in various parts of his body; bowels costive, and thirst. He was bled to ℥ x, which relieved his head. Took a cathartic, hot cataplasm to abdomen, &c. On the 9th, *cups* to *epigast.*, now the seat of well-defined pains and considerable oppression; demulcent drinks and low diet.

On the 10th, at 8 o'clock, A. M., apyrexia, not much thirst; cups dissipated epigastric tenderness. Prescribed *sulph. quiniæ, sulphuric lemonade.* Up to this time, and without our knowledge, he had discharged about half a gallon of urine during the night.

The debility increased with this abundant flow of urine.—On the 11th, his bowels being costive, the oil was repeated; he was restricted to mild acidulated drinks, and a light, but nutritious and digestible diet, up to the 14th. His tongue was clean at the point, but the base was coated with a yellow fur; pulse nearly natural. On the morning of the 14th, I found him with some fever; great tenderness on pressure over the epigastrium, red tongue and thirst; *ordered cups to epigastrium and lemonade.*—On the 15th was relieved by the cupping, and nothing occurred up to the 18th to require a change of treatment, or any active measures.

During a part of this time had been up, but on the 19th complained of dysuria; frequent but ineffectual efforts to urinate, attended with pain in pelvic region, and perineum; unquenchable thirst; pain in lumbar region.—*Ordered a demi-bath at night.* This relieved the pains and caused a free flow of urine. From this time the urine flowed abundantly, inducing great prostration, thirst, emaciation, &c.

It was now quite evident that I had to combat formidable symptoms —that I had to contend against a disease, to cure which, medicine had hitherto generally proved powerless—it was manifestly a case of *Diabetes mellitus.*

During the night he filled a large gallon pot with a pale light-straw coloured and sweetish urine.

Believing this hyper-secretion of urine to be merely symptomatic, and finding him free of fever, and other evidences of excitement, and recollecting that the best practitioners advocated tonic medicines and a highly azotised diet, I ventured to prescribe the ferruginous preparations—and the meat of young animals: as roasted lamb, veal, &c.—During the night of the 22d, he slept well, and thinks he urinated less than usual; skin, dry, pale and shrivelled; great thirst. Continued the preparations of iron and roast meat—*ut supra.*

On the 24th April, he remained up the greater part of the day, and states, that he passes less urine in the erect than in the horizontal position.

He is forced to rise several times through the night to evacuate the bladder; pulse 60—and rather quick.—I requested him to abstain from all drinks for 24 hours—a request, to which he rigidly adhered, but with no diminution in the urinary secretion.

Continued same treatment.

The symptoms already described, persisted to the 25th, when to these were superadded pains at the epigastric center, and a sense of soreness in the lumbar region; no appetite; costive.

Treatment.—Suspend the martial preparations and the meat diet; substitute the blue pills and a little aloes; sinapised cataplasm to epigastrium. An enema was required the following day, to evacuate the bowels, which procured considerable relief; pains across the region of the kidneys, especially over the left; diminution of urinary secretion; skin pale, dry and withered. *Ordered dry cups to the region of the kidneys; anodyne cataplasm to follow cups; lemonade.*—The 27th, increased flow of urine, thirst, bowels loose, persistence of pain in the renal region. Only temporary relief from dry cups.

Ordered 20 *large leeches over the seat of pain, and the following pill:*

℞ *Tannin pur.*, *grs. iv.*
Pulvis Opii, *gr. ij. M.*

F. Pil. No. viij—one every 3 hours. Sulphuric lemonade.

The leeches dissipated the pains, and the pills (perhaps ?) lessened the flow of urine.—His pulse continued regular, full, and slow; his skin pale, corrugated and perfectly dry; the thirst without any abatement; and the flow of urine varied from day to day.

Ordered Carbonate of Ammonia, suspended in mucilage of gum arabic. The last medicine was continued for 24 hours, but it rather tended to aggravate the symptoms, than to subdue the disease. Symptoms, as last described, with the addition of a jerk in the pulse.

I now suspended the ammonia, and gave a Terebinthinate mixture, but with no better success; *mild bitter infusions.*

For 24 hours the urine was less abundant, and pulse had lost its tension and jerk.—The skin feels dry and rough; the thirst unabated, and bowels costive.

Ordered to take the following:

℞ *Mass. Hydrargyri*, *gr. x.*
Ext. Conii, *gr. iv.*
Emetic Tartar, *gr. ij. M.*

F. Pills No. x.—One four times daily. Full tepid baths, night and morning, with a view, if possible, to soften the skin and determine to the surface.

He entered the bath, but feeling faint and exhausted, he refused to remain in it any length of time. The pills produced no nausea: one consistent evacuation during the day—sound sleep during the night—pulse better, the forehead was moist, and the entire surface soft, and perspirable. During the night of the 3rd of May, patient drank his urine as fast as he discharged it—declaring that it was more palatable than lemonade, and that it "quenched his thirst."

On the 4th May—skin better—pulse 38—with a slight jerk; urine more scanty, and not so decidedly sugared. Abdomen full, and rather tense, some dyspnœa. At this time we found the abdomen covered with a great number of small phlyctonæ or vesicles, filled with a colourless fluid. *Continue same treatment.* On the morning of the 5th, the surface of the body decidedly moist, especially beneath the bed clothes; paroxysms of dyspnœa; hesitates in his answers; tongue clean and smooth; less thirst. Pulse 50, regular. *Continue pills.*

About noon on the 5th, was seized with a chill, which continued for one hour, followed by considerable reaction, headache, thirst, &c.

During the night following, the febrile phenomena gave way to a profuse and abundant sweat, which continned until morning, and so profuse as to saturate the bed-linen. In the meantime, the urinary secretion was almost entirely suspended. On the 6th, he was cheerful and gay, and expressed the belief that he would survive. This remission—this deceitful calm only served to proclaim the danger at hand, and to direct our attention to the thoracic organs and the brain; for we began to fear either the formation of pus or serous effusion. Pulse slow, regular, and without any jerk.

To prevent another paroxysm, which I regarded as critical, quinine was ordered. The quinine arrested the paroxysm on the 7th, and maintained a free sweat through that day. The urine, both as to colour, taste and quantity, was normal; thirst not troublesome; slept well.—During the 8th and 9th, he gradually improved; felt stronger; skin soft and perspirable; no chill; flow of urine natural. On the 10th, we found his pulse slow and regular, skin covered with large drops of sweat, but warm; complains of soreness in abdomen.

Took *sulphuric lemonade*, and a cath. enema, which procured a copious evacuation, since which, the urine has been more abundant. On the 14th, his countenance indicated some uneasiness—brow corrugated, as if the brain was intently engaged in the solution of a difficult problem; prostrated; thirsty; tongue smooth, rather red, but moist; dimness of vision; supra-orbital pain; pupils contracted; hesitates in his speech.

Rept: the pills already given, which had been suspended.

It was now obvious that our apprehensions would be fully realized,—that the encephalon was the seat of considerable congestion, which would eventually, if not timely and promptly dissipated, terminate in effusion. The duration of the disease, the great drain from the circulation through the kidneys—the recent copious and spoliative sweats—the atrophy, prostration, and a uniformly, soft, regular, and slow pulse, sustained by the well-established fact—that in secondary affections of the brain without febrile phenomena, no benefit can be obtained by depletion; all these considerations conjointly deterred me from adopting the usual anti-phlogistic means in cerebral affections. I ordered a *blister* to the *nucha*, with the hope, though feeble, of acting derivatively on the brain and its membranes.

The morning after the application of the epispastic, I found him with his eyes closed; mouth half opened, features discomposed, and when aroused, states that he feels "*curious*," and again relapses into his former somnolent condition. His intellect is confused; pulse slow, not above 60—soft and regular; skin cool and moist. His case presents this singular feature: at one moment his surface may be free from moisture; at another, large drops of sweat collect over the whole body. His respiration, at longer or shorter intervals, becomes irregular and hurried; complains of palpitation; asks for wine and water. Ordered blisters to inside of each thigh; he refuses to cover his chest, lest he should suffocate. Percussion over the heart elicited a dull flat sound; the ear revealed a rough jarring *bruit*, announcing a pericarditis, with slight effusion. He continued to slumber; hiccough is excited by drinks; dysphagia; no urine; blisters drew well. He was allowed claret and water, which he relished; suppurative dressing to blistered surface.—The first sound of the heart, (May 18th,) is very distinct, the second obscure. Spasmodic action of the glottis when he attempts to swallow.

Prostration extreme, pulse 46, although sitting up in bed; drowsy; stricture across the chest.

Ordered *gum-camphor*, *quinine*, and *mucilage of gum acaciæ*.

The medicine was repeated three or four times, and the patient then refused to continue it. It, however, aroused the nervous system, and gave a temporary impulse to the heart and arteries.

Porter and ice, nitro-muriatic acid foot bath.

Continues to sink—singultus; pulse 42 and feeble—expresses a wish to have his chest examined. Substitute wine whey—flying sinapisms to extremities. The powers of life continued to wane; the pulse intermittent at every third stroke, and fell to 36—he became comatose, and quietly expired about 7 o'clock, on the morning of the 24th May.

Autopsy three hours after death.

Exterior habitude:—Body greatly emaciated; muscles soft and flaccid.

Head. Dura mater, of healthy and of a bright silvery appearance; no disease. Three or four ounces of a clear limpid serum under the arachnoid; the vessels on the surface of the hemispheres gorged with blood. The ventricles contained four or five ounces of a clear colourless fluid, of a faint urinous smell. The substance of the brain, especially the enterior lobes quite firm.

Thorax. Right lung collapsed, and of a light pink color; lower lobe of the left lung adherent to the adjacent part of the diaphragm. No effusion in the pleural cavity.

Organs of Circulation. The pericardium contained six or eight ounces of a pale straw-coloured serum; its inner, or cardiac surface was lined with a distinct false membrane, which could be readily peeled up entire with the handle of the scalpel. It was as thick as ordinary mucous membrane. It was rough to the touch;—thicker at some points than at others, but lined the entire surface. The part of the Pericardium, already mentioned as being in contact with the diaphragm, was intimately blended with that organ.

The surface of the heart was covered with a similar and equally distinct false membrane. Its physical and pathological characters were the same. The heart appeared pale and soft, with dilatation of its ventricles. The cavities were enlarged at the expense of the walls of the organ. No valvular disease.

Abdomen. Liver, right lobe of which was thick, and rounder than usual, of a deep nutmeg color,—of a granular feel and appearance, and bearing evident traces of chronic inflammation, leaving a number of small cicatrices, with thickening of its peritoneal covering on its convex face. The omentum adhered to the surface of the liver, where the latter is in contact with the diaphragm. The right lobe was softened, and easily torn. On the convex surface of the left lobe, corresponding with the cartilages of the floating ribs, behind and to the left of the ensiform cartilage, and contiguous to the diaphragm, was an abscess in the liver, containing three or four ounces of thick pus, pressing against the diaphragm, and opposite the Pericardium. Ulceration had evidently commenced its work upon the diaphragm, and if death had not put a period to this morbid action, an opening from this cause, would most probably have been effected first through the diaphragm and then through the Pericardium, and thus the contents of the hepatic abscess must have been discharged into the pericardium—a result almost without a parallel.

At this point, the liver was closely adherent to the diaphragm, as already stated ; this union was so intimate that they seemed as one tissue, and the most careful dissection could not separate them.

Will not this anomalous adhesion satisfactorily account for the singultus ?

The *Gall-bladder* was highly vascular and distended with a fine golden coloured bile. The *stomach* contained one or two pints of a thin bilious looking fluid; its mucous membrane was thickened, softened and highly phlogosed.

Kidneys. These organs were unusually pale, somewhat enlarged, and softened in their texture—no other lesion was perceptible.

What connection existed betwen the gastritis in this case and the diabetes ? Was the latter produced by the former ; and if so, why do not diabetic symptoms more frequently accompany gastritis? Without a blind adherence to all the doctrines of Broussais, we are half inclined to agree with that gifted man when he declares that a knowledge of the diseases of the stomach is a key to pathology. Could the hepatic abscess have been the cause of the diabetes ? why then does it not attend in all similar cases ?

2.—*Fracture of the Os Frontis, of three years standing, with a circular depression.* Epileptic convulsions ; hepatization of one lung ; inflammatory engorgement of the other.

Michael Maffit, an Irish laborer, aged about 22 years, of a robust constitution and sanguine temperament, was picked up in the streets, in a state of complete insensibility, in the afternoon of May the 19th, 1845, and brought to the Charity Hospital. When examined by the House Surgeon, he was found unconscious of surrounding objects, was seized with epileptiform convulsions every 30 minutes, and in the interval of these seizures, was stupid and comatose. Revulsive cathartic enemas and powerful cutaneous counterirritants and cold lotions to the head were ordered for the night.

The epileptic convulsions continued to recur at short intervals, through that night, and on the morning following, we found him with a full, tense and bounding pulse; ranging between 120 and 30; skin intensely hot, but moist about the chest and head; surface of a leaden hue and capillaries gorged with blood; respiration laborious and rapid, inspirations deep and prolonged, expirations quick and spasmodic, numbering 40 per minute. During expiration, the buccinator muscles were protruded as in smoking a pipe, hence M. Rostan has designated this as "la respiration a la fumer la pipe," quite expressive of the fact. Pupils dilated and fixed; eyes constantly closed; tongue compressed between the teeth, and of a dark blueish aspect; frothing at the mouth; temporal arteries full and throbbing; jugular veins distended and the blood in them undulating.

The case was truly hideous and death seemed near at hand. The depression was five or six times below the level of the bone, and seated just over the orbital ridge of the left eye. It was more than one inch in diameter and seemed to involve the superior orbital plate of the frontal bone. A free bleeding from the arm, a cathartic enema, flying sinapisms to the extremities, and iced applications to the head, were prescribed.

This was done at our morning visit; we returned at 6 P. M. determined, if the symptoms of compression persisted, to apply the trephine and elevate or remove the depressed bone. We found his breathing still hurried and stertorous; in other respects, much as before the above prescription was enforced. In consultation with the Resident Surgeon, it was decided that the operation should be performed.

Some time having elapsed, before the necessary preparations could be made, we entered the ward, and much to our astonishment, found the patient sitting up in bed, and casting a wild, frantic look about the room. This change, as inexplicable as it was sudden, induced us to suspend the operation. Cups were now applied to the nucha; a cathartic of calomel and comp. ext. Colocynth was given, and the cold lotions continued to the head. He became restive under the cups, and endeavored to remove them; he also gave other evidences of returning consciousness. Through the night of the 20th, he remained without any material change; breathing more or less stertorous; pulse 120 and soft; eyes half closed—pupils dilated; no return of convulsions. On the morning of the 20th, he seemed decidedly better; the violence of all the symptoms had sensibly abated,—and he was semiconscious. At this crisis, the nurse raised him in bed to change his shirt, and whilst in the act of thrusting his arms into the sleeves, the patient suddenly fell back and expired.

In 3 hours after death, the body was inspected. No rigidity of the limbs; some discoloration about the most dependent parts of the body—considerable *embonpoint.*

Cranium. Occipital protuberances unusually developed, giving an elongated appearance to the head. It was evidently congenital. In removing the scalp a large quantity of very dark thick fluid blood escaped from the divided vessels. The membranes covering the brain were turgid with dark blood, and the arachnoid much thickened and opaque in that portion spread over the two hemispheres. The pia-mater was extremely vascular; the substance of the brain, when sliced presented a number of bloody points, indicating much cerebral congestion. The lateral ventricles were filled with a clear fluid.

Turning our attention, then, to the seat of the depression, we discovered a small flat spiculum of bone, nearly three quarters of an inch in length, projecting from the internal face of the superior orbital plate, and pointing backwards and towards the *crista galli*. Thus situated, it acted upon the under surface of the anterior lobe of the left hemisphere.

That part of the brain, upon which the spiculum acted, was reduced to a pulpy mass, of a yellowish straw-colour, and larger than a five-franc-piece. The softening was about three lines in depth—and the surrounding tissues much engorged. The edges of the softened structure were irregular, and undulating.

The depressed bone was much thickened; the original fracture extended along the floor of the superior orbital plate towards the sella tursica, leaving traces of an ancient fracture.

Chest. Some old pleuritic adhesions in both cavities; no effusion. Left lung the seat of a high degree of inflammatory engorgement, for, when cut and pressed, dark fluid blood, mixed with froth, escaped.—Right lung, in the upper and middle lobe, presented a fine specimen of red hepatization—the "hepatization rouge" of Andral and others.

Liver and gall bladder normal—stomach contained a large quantity of mucus mixed with healthy bile. Heart and appendages healthy.—It is unnecessary to say that trephining in this case could have availed nothing, since the state of the lungs was alone sufficient to cause death.

3.—*Compound Comminuted Fracture of right Tibia and Fibula. Gangrene—Amputation—Death.*

Was brought to the hospital in the afternoon of the 14th May, 1845, P. B., an Irish labourer, ætat 36, of stout frame, and apparently robust constitution. A few hours previous, being engaged in hoisting some packages of heavy goods, he fell a considerable distance, and one of these packages striking him in the fall, upon the shoulders caused the entire weight of his body to rest upon one leg. The bones of the leg were fractured about 10 inches above the ankle; the integuments were torn about three inches transversely by the triangular point of the upper fragment. The point of the fractured bone was driven through the integuments with such violence as to penetrate the dorsum of the foot of the same side, wounding the *arteria dorsalis pedis* and causing profuse hemorrhage. The house-surgeon received the patient in this state, and on examination, found it necessary, in order to reduce the fractured bone, to saw off more than an inch of the triangular end of the upper fragment. This being done, the reduction was easily effected; the edges of the wound made through the integuments of the leg were drawn together by a few interrupted sutures; a roller was applied from the points of the toes up to the knee, and a soft pillow and two splints completed the dressings. At the point of the compound fractures, although the soft parts were much lacerated, but trifling hemorrhage took place. Cold lotions were ordered to the leg, and an opiate administered to allay the irritability of the system. On the following morning, (15th,) the patient presented the appearance usual after the reception of grave injuries, yet he complained but little; the pulse was excited, and about 100; the skin rather hot, but soft; some thirst. The bandages were not too tight, although some swelling had developed itself as high as the patella. We ordered a cathartic enema—cold evaporating lotions to be constantly used—cooling drinks—and the chest, &c., to be occasionally spunged, if the surface should become hot and dry.

Unwilling to interfere with the dressings which had been carefully applied, we left this duty to the resident-surgeon, who promised to examine the leg in the course of the day. In the afternoon of the 15th, 24 hours after the accident, the dressings were removed and the leg was found to be gangrenous;—a dark fetid sanies was pouring from the stitched wound;—the spacelus quite encircled the limb and was progressing towards the foot. Free inscisions were practised without pain or hemorrhage; in truth there was complete death in the whole limb below the point of fracture. The countenance of the patient had assumed a pale, anxious look; the pulse rather contracted and quick; and the tongue coated and clammy. In this dilemma what was to be done. Either wait until a distinct line of demarcation should be established between the dead and sound tissues, or amputate without loss of time. We prefered the latter as the best alternative, partly, because the *gangrene* seemed to travel *downwards*, and because the parts *above* the point of fracture appeared quite healthy, with the exception of slight swelling. After administering an anodyne in a glass of brandy, we amputated below the knee, it being our aim to cut as far above the diseased structures as possible. In dividing the integuments and raising the flap, the blood flowed freely, leading us to suppose a free and healthy circulation in the part. The stump, after the vessels were secured, was dressed as usual and some slight stimulant given the patient; a cloth dipped in cold water was kept constantly to the stump which rested on a pillow. About 9 o'clock the same evening, we found the patient quiet and in good spirits. On the following day, (16th,) trifling secondary hemorrhage—a mere oozing of venous blood from the stump took place. It was arrested by cold water dressings. Pulse and skin,—the former 100, the latter hot, but soft, and in some parts, decidedly moist. We returned in the afternoon and found the skin bathed in perspiration; pulse 120, full and soft. This excitement of pulse was ascribable to the too frequent use of wine during the day. It was instantly discontinued, and cold drinks substituted. Chlorine lotions to the stumps; emollient enema and cold applications to head which was very hot. The patient rested quietly and slept soundly, notwithstanding the general excitement of the system. He complained of soreness about the stumps. In the course of the night, the excitement gradually passed off, and at the morning visit, the pulse stood at 90; skin warm and moist, and patient cheerful. For the first time, we observed a reddish streak wending its way along the inner surface of the thigh, in the tract of the large superficial veins and lymphatics. Appropriate topicals were prescribed, and in the evening, the issue was no longer doubtful; the red was converted into a dark gangrenous hue; the track spread around the thigh and travelled up towards the groin. Its progress was rapid, almost visible; the pulse became frequent; the skin bathed in a cold clammy perspiration; the countenance haggard, and the features decomposed. During the night of the 18th, vomiting supervened; finally, delirium with subsultus. In a few hours after the development of the gangrene, it reached Poupart's ligament, when death took place on the morning of the 19th.

The body was so fetid and the temperature so high that an examination was not made.

We, however, inspected the leg that had been amputated, and found the tibia literally crushed and cleft into five or six distinct fragments.—This bone not only had been broken above transversely, but split longitudinally down within an inch of the astragulus. Coagulated blood filled the cancellated structure of the bone, and was also diffused through the surrounding muscular and cellular tissues. None of the large arterial branches seemed to be torn, although the soft parts were seriously and extensively lacerated. The fibula was fractured obliquely some distance below the point at which the tibia had given way. If the serious and complicated nature of the fracture had been known at the time of the accident, amputation would have been justifiable on the instant. The man urged his willingness to hazard any thing to save his limb. His request was respected and the result has already been stated. On inspecting the parts through which the knife was carried in removing the limb, all seemed quite healthy as far as the eye could determine.

4.—*Pus in the Vena Portæ.*

Martin Shannon, ætat. 35, a native of Ireland, stout, tolerably muscular, habitually enjoys good health; large chest, light complexion; labourer; entered Ward No. 5, bed 56, on the 5th of June 1843. He had been quite *ill* for *ten* days, before he applied to the Hospital for admission

Condition at the time of admission: Complained of considerable pain in the right side of thorax; occasional paroxysms of coughing; abundant muco-purulent expectoration; respiration about 30 per minute; much thirst, nausea, and frequent efforts to vomit; repeated and thin watery evacuations; lips of a blueish livid appearance; tongue loaded with a dense, rough, yellowish fur; great tenderness on pressure over the epigastrium. Pulse 95—full, soft and regular; great restlessness &c.

House surgeon prescribed ol. Ricini., Hot Pediluv., Gum water.

June 6th *mané:* Persistence of same symptoms, perhaps augmented in violence; vomited several times through the last night; abundant purulent expectoration; bowels freely moved by the Ol. Ricini.; pulse full and compressible; thirst unabated; cephalalgia, mucous ronchus in the division of the right bronchus; but little tenderness over the right hypochondrium. *Ordered cups to epigastrium, followed by warm emoll. cataplasm; and Lemonade.*

June 7th *mané:* Spent a restless night, vomited and purged a dark greenish fluid. He expectorated nearly a pint of a thick green and yellowish pus; skin quite cool and saturated with perspiration. Tongue as heretofore; unquenchable thirst, jactitation, lips dark leaden hue. The cups were also applied over epigastrium and right hypochondrium.

They removed the tenderness, but failed to check the vomiting. Profound prostration, respiration nearly normal, features much contracted, and expressive of great anxiety.

I now began to apprehend immediate danger for the patient; it was now that I strongly suspected something wrong in the liver, but the closest scrutiny and the most careful manipulation of that organ failed to confirm my suspicions. Percussion over the upper part of the right lung, in the subclavicular region, elicited a dull flat sound—vesicular re spiration no longer distinct.

Prescribed an Expectorant mixt., composed of Syrp. Scill., Tolu., et Syrp. Pappav. somnif. Emplast. Vesicat. 8 × 10 *to right side of thorax—Cataplasm. Drink: Infus. of Flaxseed Tea. Diet: Boiled milk, diluted.*

June 8th *mané*: Spent a sleepless night; continued to purge and vomit; the matters discharged from the bowels were of a yellow-bilious character; and from stomach and bronchi, of a mixed nature: partly bilious, and partly purulent. Thirst; pulse small, feeble, and soft;—120 per minute; the surface of the body was bathed in a profuse cold perspiration; orthopnæa; cold extremities; prostration; inquietude; rejects every thing from stomach as soon as swallowed; thin watery discharges.

This case offered a striking resemblance in its symptoms, to one of poisoning from Emetic Tartar. Magendie's experiments upon animals, were in their effects somewhat similar to the case before us.—This distinguished Physiologist injected *putrid water* into the veins of certain animals, and the result was constant purging and vomiting—cold extremities—great prostration, and in a few hours, death from exhaustion.

The case of Shannon was evidently becoming serious.—*Ordered Repeated Anodyne Enema—and Morphine by the mouth*—to allay the vomiting and restrain the bowels—neither of which succeeded to my satisfaction;—sinapisms to extremities, &c. 6 o'clock, P. M.—Repeat, Prescript. by House Surgeon.

9th *mané*: No improvement; the symptoms are more alarming; pulse scarcely perceptible; cold, clammy perspiration; hurried respiration—eyes everted; slight stupor; talks incoherently.

Ordered:	*Carbonat. Ammoniæ,*	℈ ij.
	Syrp. Morph.,	℥ iss.
	Mucil. Arabic.	f ℥ vij. M.

Table spoonful, repeated; sinapisms to inside of each thigh—to wrists, &c.

The above mixture raised the pulse and revived the man for a short time. Free from all pain; agrypnia; vomits less frequently.

Evening of 9th: House Surgeon ordered the following:

Sodæ Bi-Carbonat.	ʒ i.
Morph. Sulph.,	gr. ij.
Aqu. Menth.,	f ℥ ij. M.
Aqu. Distill.,	f ℥ ij.

The patient continued to sink; nature seemed to be on the point of giving up the struggle; the muscles became relaxed, the circulation languished, and the heart ceased to act about 8 o'clock, A. M., on the 10th of June.

Autopsy, 50 *minutes after death*:

Slight emaciation; muscles relaxed, chest large and well developed; the right hypochondrium seemed more elevated than the left; surface of body pale in superior, and dark blueish colour over the most, dependent portion.—This is owing obviously to position.

Thorax: Left lung and pleura normal, no adhesions, no effusion. *Right lung:* Extensive adhesions, some old, others quite recent; a *cavity* in the upper lobe, capable of holding about three ounces of fluid, contained a small quantity of flaky and tenacious muco-purulent matter, resembling essentially the matters expectorated. Surrounding this cavity, the lung was dense, compact, impervious to the air, and constituted one of the stages of pulmonary inflammation, termed *gray hepatization.*

Beyond this hepatized portion, the lung was gorged with blood, heavy and destitute of any crepitation.

Pericardium and Heart—normal. *Abdomen: Liver* large and of a dark brown colour, dotted over its convex surface with a number of light coloured spots, as large as a ten cent piece; it was very heavy, and engorged with blood; the *acini* were very distinct.

On cutting into the substance of the Liver, a rather dark looking *pus* escaped from the vena portæ, which had been divided; it flowed very freely from the smaller as well as the larger branches of the portal system of vessels; the quantity could be increased by making pressure upon the Liver.

The matters discharged, were precisely such as are found in an ordinary abscess.—The vessels which were loaded with pus, were traced up by Drs. Slade and Fenner, and the purulent matters were found locked up in the *vena portæ*, beyond a doubt. The Hepatic vein contained a large quantity of blood: such as is usually found to exist in engorged livers. No trace of any abscess could be found in any part of the liver, from whence it might be supposed, the purulent fluid could have made its *transit* into the Portal system.—Hence, we may conclude, that the extreme or minute vessels of the vena portæ, which ramify, and are distributed to the parenchymatous substance of the liver, where we found evidences of inflammation, here took up the purulent fluid, as fast as it was formed, and thus prevented any accumulation of *pus*, or the formation of a regular Hepatic abscess.—The *pori biliari* were loaded with a fine golden coloured bile. The gall-bladder was full. The Hepatic duct was also distended with bile. The stomach was perfectly healthy, and without the slightest trace of phlogosis. It was even paler than usual. It contained about a pint and a half of a yellow-greenish fluid, which, no doubt, was derived from the liver. The intestines, as far as inspected, were healthy. No communication could be discovered between the abscess in the lung, and the portal system of vessels. Then I would ask, how did the pus find its was into the vena portæ; or was it formed in the vessels themselves? I state facts, and attempt nothing more.

In reporting cases, we are too apt to select only those that end favourably, but we believe that all those that embrace interesting points, whether fatal or otherwise, should be given to the profession. This we have done.

V.—*A Case of Hydrophobia, with the appearances upon dissection.* Reported by C. Glidden Young, M. D., Greenwood, La.

A negro girl, Jane, aged about 12 years, the property of Miss Wilson, of previous good health, complained on the evening of the 13th July 1845, of stiff neck, something like crick. On Monday morning, the 14th, same instant, she was quite ill; and without the supervention of any manifest fever, she continued to grow worse throughout the day. Towards evening she complained of difficulty of swallowing. On Tuesday morning she complained of chilliness when exposed to a slight current of air; was thirsty, yet refused to drink, alleging her inability to swallow; was occasionally startled, agitated, and partially convulsed. I found her in the evening laboring under considerable difficulty of breathing. The respiration was sobbing, and performed chiefly by the respiratory muscles of the chest. She had vomited up bilious matter several times in the course of the day. Her bowels had not been moved. The tongue presented a white milky appearance, with a little redness at the end; pulse 120, feeble; urine scanty and highly coloured. The extremities were cold, and a cold perspiration covered the face and breast. Her countenance expressed anxiety and apprehension. She complains of want of sleep, and says, she has not slept any since the night of the 13th. I gave her a cathartic dose of calomel, which she took with a good deal of difficulty.

I saw her early in the morning of the 16th. She has slept none during the night. Medicine not operated. Extremities cold; pulse very irregular. The bowels were opened by an enema. I attempted to administer some quinine in solution, but she was unable to take it; indeed, I could not prevail upon her to attempt it, or even to look at it. I then mixed some quinine in a small quantity of water in a spoon, and insisted upon her taking it. At first she turned away from it with a wild and agitated look, then turning quickly back again, she seized the spoon, bent her body forward, and swallowed it with a convulsive motion of her throat, chest, and upper extremities. She continued to take quinine in the same way, at intervals of two hours, until she had taken 20 grs. without any other perceptible effect, than a gradual return of warmth to the extremities.

12 o'clock, M. Countenance occasionally expressive of languor and fatigue, then suddenly assuming a wild, haggard, and furious expression. The door was thrown open to allow the air to circulate freely in the room. No sooner had this been done, than she screamed out for them to shut it, jumped upon another part of the bed, drew the blanket over her, and became frightfully convulsed all over.

Up to this time, I had regarded this as a case of congestive fever, complicated with hysteria; but her conduct and appearance were now so remarkable, the effect of the slight current of air so extraordinary, and the general aspect of the case so singular, that I was startled, and, for the first time, the thought occurred to my mind, that this was Hydrophobia.

No case of canine madness had ever occurred in this section of country; I had never before witnessed the disease; nor was I aware, at the time, that any rabid animal had been seen in the country. Hence I was still disposed to attribute the hydrophobic symptoms of this case to a modification of that protean affection of the nervous system, which, like the mocking-bird, seems to delight in the perfection, with which it can imitate any one of its whole tribe.

After she became calm, I approached the bed quietly, but she sprang up in the sitting posture, and with a wild and frantic expression of countenance, told me not to fan her. She said, she was so thirsty, and her mouth and throat were so dry, she should choke to death, and that she could not drink any thing; that there was a tumor in her throat, the removal of which would enable her to drink some water. She wanted me to examine her throat, and permitted me to press her tongue down with the handle of a spoon. I could discover nothing more, than the mucous membrane was covered with a ropy, tenacious mucus. She wanted to suck a wet rag, and when it was handed to her, she seized it with both hands and commenced biting, and chewing it in a kind of convulsive manner, very like the snapping of a rabid dog. In less than a minute her whole frame became convulsed with spasms. She threw the wet rag across the room, sprang to her feet, jumped towards me, and, with mingled expressions of terror and pain, implored me to remove the "nasty stuff" from her throat, which was choking her, and commenced spitting in every direction a quantity of frothy, tenacious saliva. From this time, the bare sight or mention of water, or the noise of pouring it from one vessel into another, was sufficient to throw her into spasms. The sight of her own image, reflected from the surface of a mirror, or of any brightly polished metallic substance, or the slightest agitation of air in the room, produced the same effect. She had intervals of repose, in which she suffered but little. As the disease progressed, these became shorter, and she would spring up in the sitting posture and look wildly around the room. She spent most of her time in this posture, and finally died in it. Previous to her death, it became necessary to restrain her, and she attempted several times to bite her attendants. Towards the termination of the disease, the saliva ejected became tinged with blood; and she suffered occasionally with nausea and retching. The disease proved fatal in seventy-two hours, and although she slept not at all, her intellect was not affected, during her intervals of repose, to the last. She complained of no pain during the whole course of the disease, yet her appearance of suffering at times defies description.

Upon inquiry, she told me, she was bitten upon the thumb about three weeks before her illness, by a sort of distracted little dog. The wound was now well, but in healing had left a plainly marked cicatrix. The dog had not been supposed affected with hydrophobia, was a good natured puppy, until the day upon which it bit the subject of this report, and subsequently six other persons. It was then observed to droop, carry its head to one side, and snap at every thing that came near it. It died the next day in a fit. It was known to bite two other dogs; one of which was killed; the other was taken sick, carried its head to one side, had fits, and died. After this, quite a number of dogs were killed in the vicinity, having every appearance of being affected with canine madness; and I think, I have been able to trace the origin of the disease to a mad wolf in an adjoining parish.

Not any one of these six persons mentioned above, as having been bitten, has been as yet (January 1847) affected, though no prophylactic treatment was adopted.

The only remedial measure, which apparently afforded any mitigation of the violence of the disease, was the external use of cold water. At one time after having directed a stream of cold water from a height of several feet, for more than an hour continuously, and until she was almost pulseless, she drank a third of a tea-cup full of water with a spoon, and remained quiet about half an hour. When the paroxysms returned, the remedy was repeated, but the intervals of ease, which it procured at first, became shorter, until it ceased to have any effect. When the cold water was first applied, it threw her into the most violent spasms, which gradually abated, as the remedy was continued.

She seemed to dread it much, but said it made her feel better, and desired it to be repeated.

Post mortem twenty hours after death.—Upon removing the lid of the coffin, there was seen a collection, as large as a tea-cup, of bloody, frothy mucus protruding from the mouth and nostrils, which continued to bubble slowly from the nostrils. There was no rigidity of muscle; the trunk and extremities were flaccid and limber; the skin soft, pliant, and warm. The warmth was remarkable. I had no means with me of ascertaining the exact temperature of the body, but it must have been fully equal to that of the human body in health. The body was not at all offensive, and there was no appearance which indicated that decomposition had commenced. The parotid glands, and the sub-maxillary gland on the right side, were highly injected with blood. I believe a morbid condition of the salivary glands has not generally been observed in subjects who have died of hydrophobia, but it was quite evident in this one. The tongue was a little swollen, white, and flabby, with the glands towards its root somewhat enlarged. The mucous membrane covering the larynx presented patches of a dark mahogany color. The same membrane was highly injected throughout the Trachea and Bronchia.—The lungs were completely gorged with black, fluid blood. The mucous coat of the æsophagus was softened in its whole length; its muscular coat actively inflamed. The mucous coat of the stomach was inflamed in patches; its muscular coat thickened; its serous coat of a deep mahogany color. The small intestines were collapsed in a number of places. The peritoneal covering was highly inflamed, through the whole extent of this portion of the alimentary canal. The muscular membrane was inflamed in patches of several inches in length. The mucous membrane of the small intestines was red and inflamed in places, in other portions thickened, in others softened, in others ulcerated.

The ulcers occurred mostly in patches, and varied in size from that of a split pea to that of a Half-Eagle, United States coin. Some of these ulcers were seated on the folds of the membrane, others involved its follicles ; some were regular in their borders, others were irregular, and ragged ; some were superficial, others deep, having destroyed the mucous, and muscular coats, and almost perforated the intestine. The cellular tissue connecting the different coats of the intestines appeared to have been destroyed as if by maceration, so that the coats might be separated with the greatest facility as we tear off the paper from adhesive plaster. The cæcum was inflamed. The colon contained a quantity of mud colored inodorous bilious matter ; its descending portion was contracted—a result of former disease. The gall-bladder was distended with dark vitiated bile which had extended through its parietes and stained the surrounding tissues. The liver was a little enlarged, of a dark color, and gorged with black bile, and blood. The spleen was near its natural size and quite destitute of blood. The reflections of the peritoneum, the pancreas, the mesenteric glands, and kidneys presented evident marks of inflammation. The diaphragm exhibited a uniform dark, red appearance. The muscles of the larynx, and the scaleni muscles of the neck, looked as if they had been violently contused until their fibrous structure was almost completely destroyed. The cellular sheath surrounding the carotid artery, on each side, presented the same bruised appearance. The investing membrane of the par vagum, on each side, had a bright red appearance as far as traced down in the chest. The recurrent branch, upon the right side, was traced out until fillaments were lost in the muscles of the larynx, and its investing membrane presented a like red appearance. The thyroid gland and the thymus body, which appeared usually large, both evidently participated in the inflammatory action of the surrounding tissues. The inner surface of the heart, in all its cavities, was of a dark red color which was continued in the vena cava, the subclavian veins, and every other one which was examined. The inner coat of the veins was found thickened in a number of places, but no pus was observed. The inner coat of the Aorta had a red appearance, which gradually subsided as we left the heart, until it disappeared in the abdomen. The coats of the veins could be separated in the same manner as those of the intestines mentioned above, and to some extent, those of the arteries. The blood was accumulated at the right side of the heart. It was dissolved, and no where presented any appearance of possessing the power to coagulate, but every where appeared to be filled with bubbles of gas, like a solution of organic matter in water which is undergoing the process of decomposition. The sympathetic nerve was examined in the thorax ; its appearance was normal. In dissecting the integuments of the cranium the little blood vessels which perforate the skull bone were found very much injected ; and on removing the skull-cap, the surface of the dura mater was found covered with an effusion of fluid blood. The dura mater was actively inflamed. The veins of the pia mater were highly injected and contained numerous bubbles of air. There was no effusion of serum within the cranium? The choroid plexus was much injected. The whole substance of the cerebrum was so softened as not to permit an examination of its parts. It presented in different portions a variety of color—gray, white, and red.

The cerebellum was softened in like manner, but its color was a uniform redness. The membranes surrounding the spinal cord were actively inflamed; and the substance of the cord, for the space of two and a half inches, about the middle of its dorsal portion, was considerably softened.

VI.—*Hints on the Value of External Medication*, by Thomas D. Mitchell, M. D., Professor in Transylvania University.

The more I notice the results of remedial agents in my own experience, and in that of others, the more deeply am I impressed with the vast importance of the external use of medicinal appliances. I have been forcibly reminded of this truth, in my perusal of the tretament of what are called by some *continued* fevers; the result of which has been the indelible conviction, that very many have fallen victims to the free use of internal remedies, while no efforts have been made on the external surface.

To show the importance of external medication, I have selected some well established facts, touching the use of several remedies, which are not generally known to the profession, and which, I trust, will commend themselves to all who are in quest of truth.

The extract of Belladonna is among the articles of Materia Medica, that are never employed by some practitioners. They have fallen into a kind of routine method, unchangeable, it would seem, as their own nature; and it would, perhaps, be time wholly lost, to attempt to convince them, that belladonna is good for any thing. And yet, they know as well as we do, that it is a very ancient remedy, and has had the testimony of some of the ablest physicians in its favor. Some of those physicians have attempted to cure *Delirium tremens*, by the internal exhibition of large doses of opium, but in vain. In the language of a very sensible writer, "The large doses of opium failed, because the sensibility of the nerves of the stomach must, in a great measure, have been rendered defunct by long continued abuse, so that they were incapable of receiving the usual impressions, or transmitting them to the brain." Such is the doctrine I have been in the habit of teaching for years, in my public lectures.

The high value of the external use of extract of belladonna in *delirium tremens*, has been settled so conclusively, that I should think no practitioner, aware of the facts, would hesitate to give the remedy a trial.

From the reported cases, I select a very striking one, recorded in the London Lancet for April 1843, page 12. The patient was a regular, mature drunkard, and presented a very marked case of the disease. He had been well dosed with opiates and the like, to no good purpose; indeed, the symptoms were rather made worse by the treatment.

At the end of a week, wasted in the ordinary mode of management, a pretty large blister was applied over the spine, below the nape of the neck. The cuticle was separated from the blistered surface, and a dressing of pure extract of belladonna was laid on the part. I now quote from the report: "Previously, he had been very boisterous, but so acute was the pain of the application, that he became instantly subdued, tears rolled from his eyes, and he begged to have the plaster removed. In three minutes he ceased to complain; in five minutes, slight twitchings of the facial muscles and the arms commenced, his utterance became indistinct, and he now kept up a stupid laugh, like a man much intoxicated. The pupils, which had been very much contracted began rapidly to dilate, and in seven minutes were opened to their fullest extent. He now became very drowsy, and begged to lie down. The extract was then sponged off the surface, and a dressing of simple cerate applied. In nine minutes from the first application, he was in a profound sleep, which lasted for seven hours, during which he seemed to enjoy undisturbed repose. During the action of the extract, the pulse rose to 160, but fell in a few hours to 108. At the end of seven hours he awoke and was quiet, until he stared about apparently astonished, and then relapsed into his previous wildness."

During the day, following this happy change, he was treated with large doses of opiates and was made worse. It was then resolved to repeat the blister and reapply the extract of belladonna.

The blistered surface was denuded to the extent of an inch and a half square, and the extract laid on. The results were precisely the same as before. The pulse rose to 152 in ten minutes; the pupils were largely dilated, and in twenty-five minutes, sound sleep was induced, which continued for nine hours and a half, without interruption. At the end of this long sleep, he appeared tranquil, but soon began to relapse. In one hour after, the extract was re-applied and sleep came on in twenty-five minutes, which lasted about four hours and a half. He awoke perfectly subdued, and was quiet during the remainder of the day. The pulse fell gradually to 70." From this period, the patient continued to improve, and in a few days was quite well.

The reporter, in conclusion, has these very judicious remarks: "I am persuaded from my observations in this and other instances, not only of delirium, but of cases involving a large amount of nervous irritability and excitement, that we possess no medicinal agent, whose action is so immediate and so *irresistible*, and at the same time, with proper precautions, so safe, as the extract of belladonna applied in the manner described."

This case teaches valuable lessons touching the opiate practice. It not only failed in the first instance, when boldly pushed, for the space of a week, but its failure was equally signal, when another attempt was made with it, after the extract of belladonna had been tried only once.

This agent, externally employed, and with no adjuvant internally exhibited, controlled the disease. And although I have been an advocate for the emetic practice of Klapp, to clean the internal surface of the stomach, and to rouse its sensibility, it is my deep conviction, that the external use of belladonna is a highly important addition to our curative means in this frightful malady. Even, though we employ the emetic, it may be needful to resort to the extract, if the good effects of the former be not soon quite obvious.

On the first reading of the reports of success, by use of the belladonna, as above, it occurred to me, that another disease, very often fatal in its issue (or rather very seldom cured) and dependent, as is most probably delirium tremens, on lesion or disease of the cerebro-spinal system, could be more certainly controlled by this medicine, than by any other. An opportunity has not yet presented, to test its power; but it commends itself so forcibly, by analogy at least, that I should feel bound to give it a faithful trial. I am not aware of any practitioner, who has been more successful in the treatment of Tetanus, than Dr. Hartshorne of Philadelphia, whose main dependence was on caustic potash applied to the cervical spine, so as to make an eschar. And the most palpable effect, that I ever witnessed from any remedy in Tetanus, was in the case of a miserable sot, who was brought into the Cincinnati hospital, but a few hours before he died; and the agency was cupping to the spine. While the cups were drawing, as the phrase is, the spasms ceased entirely, not only once, but repeatedly.

In view of the prompt and salutary action of the extract of belladonna in delirium tremens, it seems to me, that no practice promises so well in Tetanus, as the separation of the cuticle from the entire spinal column, and the application to the whole surface of the pure extract. To accomplish this end speedily, let frictions be made to the spine, of Granville's lotion, or the strongest spirit of ammonia, and then persevere in the use of the extract so long as any good effect can be perceived. These remarks are not less applicable to Hydrophobia, than to Tetanus; and I very much doubt, whether the practice detailed above, would not be valuable in that stage of Southern Congestive fever, in which external stimulation is supposed to be indispensable. Let some one give it a trial, and tell us of the issue. And still further, let me risk the opinion, that in what is usually called *Typhoid* fever, this practice might be signally serviceable, in controlling the diarrhœa and high irritability of the small bowels.

The next article to which I invite attention, is *Digitalis*. This medicine has been utterly repudiated by very many physicians, who, in truth, never employed it. They have heard and read of fatal effects attributed to it, and hence their settled conviction, that it should not be resorted to at all. They concede its power, but fear to use it.

For the information of such persons, it is well to state, that the tincture, or the infusion of the fox-glove can be employed externally in dropsical affections, with entire safety, and with decidedly good results. A writer, by name Joset, published his experience on this subject some years ago, in the *Revue Medicale*. He tells us, that he has employed the remedy externally for more than 20 years. His practice, especially in ascites, was to rub into the abdominal surface, from 2 to 4 oz. of the tincture or strong infusion, three times a day. In general dropsy, he rubbed it freely into the thighs and legs. In all the cases, the urine soon began to flow most copiously, and the entire effusion passed away.

The same practice is confirmed by a Portuguese physician, in a paper published in the *Medico-Chirurgical Review* for April 1843. He employed digitalis and squill, externally, in ascites and anasarca; confining the remedy to the surface by compression and bandage to the abdomen and thighs. The effused fluid escaped by the kidneys, and with no unpleasant constitutional result.

The *marchantia hemispherica*, or *polymorphia* is next to be noticed. This plant is more generally known by the name of *liverwort*. It grows in almost every country, abounding chiefly, however, in moist and shady places, and on the banks of the rivers; it is found at all seasons of the year, but is in greatest perfection and vigor near the end of autumn.

This plant has been brought to the notice of the profession, as a valuable *diuretic*, by Dr. Thomas Short, in the Edinburg Medical and Surgical Journal, vol. 39th. He says, the remedy had been frequently employed by the lower orders in Ireland, and having accidentally learned from some of them, its diuretic agency, he was led to try it.

The manner of using the liverwort, Dr. S. informs us, is in the shape of poultice; averring, at the same time, that he never observed the slightest benefit to follow its internal administration.

The leaves of the plant being plucked when it is mature, two large handfuls are thrown into a pot containing about a quart of boiling water, and simmered by the side of a fire for 12 hours, fresh water being added if necessary. The leaves are then beaten into a pulp, and as much linseed meal stirred in as will bring the mass to the consistence of a poultice, which is to be spread on flannel and applied to the abdomen by means of compress and bandage; or to the legs, if swelling of the extremities alone exist.

The poultice is renewed every twelve hours, until the water is drained off; or continued for such a time, as to show that no benefit would result. The remedy acts powerfully on the kidneys, and although it has occasioned feelings of sinking and exhaustion, Dr. S. never knew it to do harm. No internal medicine was given while the poultice was employed, unless these feelings of exhaustion were complained of. The diuretic action was increased by the free use of warm drinks, especially weak leaf tea and chicken water. The patients were clad warmly and confined to bed during the operation. And it is also stated, that good results were obtained in cases of evident structural disease of the kidneys, as well as in those where no organic change had taken place.

The following case is given as a specimen. "J. S., aged 50, was ill with swelling of the abdomen and lower extremities, having previously had a bowel complaint. Tongue dry in the centre; much thirst; urine coagulates by heat; has no local affection; has been in the habit of drinking freely; urine very scanty. After the use of purgatives and diuretics, to no good purpose, the *marchantia* poultice was applied; and from the 27th of September to the 29th of October, a period of twenty-nine days, 309 pounds of urine were discharged, being an average of 10½ pounds per day, and he was dismissed, cured."

In the conclusion of his paper, Dr. Short very properly remarks, that the *marchantia* is no more a specific than any other diuretic; but having often succeeded after other more noted diuretics had failed, he felt it his duty to lay the facts before the profession.

Duchesne in his "Repertoire de plantes utiles et des plantes veneneuses," (1836) says, "the marchantia has been recommended as a powerful diuretic, when applied externally in form of poultice." This is three years after the publication by Dr. Short. I have not met with any other notice of its use, in this way.

The same Dr. Short has furnished important information touching the use of *blisters* in the management of hiccup, that is known to very few practitioners. We do not mean to say, that hiccup has not often been treated with blisters, but they have been generally applied to the epigastrium. Many physicians, however, would scarcely think of vesicants in this form of disease.

After many judicious remarks on the various sources of hiccup, the writer goes on to say, that the phrenic nerves and their connexion with the accessory nerves of Willis, are closely associated with the causes of many cases of this affection, and therefore he applies his blister, as near as may be over the origin and course of the phrenic nerves. To do this, it is needful to carry the blister nearly round the neck. Several cases are detailed, of long standing, in which the usual anti-spasmodics were employed and blisters laid on the epigastrium, to no good purpose; and so soon as a blister placed round the neck began to affect the skin, the hiccup ceased and did not return.

We are aware, that an ordinary case of hiccup is easily controlled. But there are some cases so protracted and distressing as to be truly alarming, and in these it is quite probable, that blisters, as employed by Dr. S., will prove most salutary.

My partiality for external, over internal medication is very great; and, as I think, rests on a solid basis. Especially is external medication important where there is decided irritability of stomach or bowels, or positive objection to take an important remedy by the mouth or by injection. This remark will hold good, even in an intermittent, attended with gastric irritability, and a determination on the part of the patient, not to take the sulphate of quinine. I have had cases of this kind with complete success from the endermic use of this heroic medicine, to the epigastric region, where I could not possibly have administered it in any other way. I know that a French writer has called in question the power of the sulphate to act efficaciously on the skin; but he happens to be a few years behind the times, and if he will pay us a visit, he may very easily get rid of his heresy on this point.

VII.—*Description of a New Genus of the family of Solanaceæ; with remarks on its Characters and Properties.*

The flora of the tropical regions is widely different from that of the temperate climates, and we were much struck with the beauty, size, and magnificence of the vegetable productions of the torrid zone. We often had opportunities, in our botanical recreations, of collecting materials which seemed of some interest, and may be allowed to communicate the following observations respecting the family of Solanaceæ, so extensive and various, in warm countries, and so rich in useful and dangerous applications. We were a little troubled in recognizing this great family, which was in part unknown to us before, and which cannot be studied to advantage in the books, treatises, or botanical collections, or gardens of our country.

In this family, some of the species are trees, others are shrubs, or herbaceous plants, and they differ so much in appearance and external characters, that it is very difficult to arrange them all in the same classification. Some of the genera, indeed, contain species which depart so far from the characteristic forms, that we must necessarily separate them. Thus, I made some observations on the genus Datura, or rather on some particular species of this genus living only in the warmest regions of the globe. It seems that the Daturas have not been very well studied, in the aspect and various properties which they present in the tropics. Some of these have characteristic features so peculiar that they cannot with propriety be longer retained in this genus:—they evidently belong to different groups, and have distinguishing generic characters.

I have, therefore, separated some species, forming a natural group, from the true Daturas, and have formed of them, the new genus, Elisia, in memory of a much esteemed friend.

General Characters of Elisia.

Elisia calice monopetalo, anguloso, longitudinaliter fisso et persistenti; corolla monopetala, margine quinque-punctata, subcampanuliformi, prismatica, marcescenti.

The Elisias are very elegant shrubs, with foliage of a light and lively green, their large terminal, snow-white, campanulate, and pendulous corollas produce a most charming and picturesque effect. They grow in fresh and fertile soils, but their shade is not only dangerous to animals, but injurious to the plants which vegetate in their vicinity. In the evening, and during the whole night, they exhale a nauseous, soporific, and carbonic odour, which must be injurious to respiration if in a great quantity, but a few flowers produce in a large open room, an agreeable perfume.

Specific Characters.

Elisia formosissima—Nobis. *Datura arborea*—Lin.

Elisia pericarpio subspinoso, scabro, quadrivalvi; et quadriloculari, foliis eliptico-ovatis, subtus leviter villosis, longe petiolatis lateraliter nervosis parum sub-dentatis.

We thought convenient to change the specific name of *arborea* to *formosissima*, the first is applicable to all the species, and on that account objectionable, the second expresses the charming effect which this beautiful shrub produces. Every one that has observed it in its native country, will certainly agree with us in adopting the specific name of *formosissima*. This plant, quite common in Venezuela, reaches the height of twenty feet or more, and is very well formed. Its large and smooth leaves are slightly villose in their inferior part: but what particularly strikes the traveller, is its numerous flowers, which hang in a graceful form, so as to cover the whole plant with a snowy hood. Its wood is useless.

Elisia mutabilis—Nobis. *Datura Bicolor*—Mor.

Elisia pericarpio scabro quadriloculari, quadrivalvi; foliis ovatis atrovirentibus, mollibus—crasse petiolatis, serratis flore mutabili.

This little shrub though apparently like the preceding, is nevertheless less elegant in its form, less lively in its color, its exhalation is more injurious to the lungs, and has more dangerous qualities. It is not so high as *E. formosissima,* and it seems diminutive in all its exterior appearance. Its corollas are not precisely of two colors, as indicated by the specific name, *bicolor*, given to it by the Botanists; but it changes in the different hours of the day, passing from the whitish to the reddish or yellowish colour, and we observed these different gradations of hue in the same plant, and at the same hour. Hence, we have called it *E. mutabilis.* This effect is produced by the influence of light, which induces chemical changes in the fluid. Whoever observes this curious phenomenon for the first time, will certainly be surprised, and consequently may be deceived by the illusory appearance, if he does not examine it attentively at different times.

Elisia lacimata—Nobis. *Species Nova (?)*

Elisia pericarpio ruvido, quadrivalvi; biloculari foliis sublaciniatis, breviter petiolatis, glabris.

This elegant shrub may be said to be a diminutive of the first species in its dimensions and properties. It is woody, perennial, with its bark lighter yellow than the *E. formosissima.* It is particularly distinguished by its deeply cut, glabrous, petiolate leaves. The color of its subcampanulate and prismatic corolla is unchangeable, except in the warmest hours of the day.

Medical properties.—The inhabitants of the plains of Venezuela and Nueva Grenada, use the leaves and flowers of E. mutabilis and other elisias in making a potion or smoking it as we do with tobacco. Some experiments, made by my friends and myself, confirm the common opinion of their beneficial effects in cases of asthma. The indians stupefy and kill their enemies with a kind of starch or white powder which they extract from its seeds. The narcotic properties of the *Solanaceæ* are generally well known. The *alkaloid principles* obtained from many of them, as *solanine*, *atropine*, *daturine* are incidentally or purposely used in materia medica: they are extracted from the *solanum nigrum*, *atropa belladonna* and *datura stramonium.* I have obtained from the leaves, flowers and tender branches of the above mentioned three species of *Elisias*, a fine white powder, with bitter, nauseous and caustic taste: treating it with sulphuric acid, it crystalizes in an acicular form, giving fine shining needles of sulphate of elisine, which name I have adopted in the belief that Elisine and daturine are essentially different. It is very like daturine in its external appearances, but I have had no opportunity of making comparative experiments to determine its specific characters. It is probable that all the alkaloid principles which are now known and those which will hereafter be recognized in the numerous family of *Solanaceæ*, possessing the same medicinal properties, are the same alkali, viz: the *solanine*, with perhaps a little modification in the proportion of the atomical number of their elements.

MILANO.

Part Second.

REVIEWS AND NOTICES OF NEW WORKS.

I.—*An Anatomical Description of the Diseases of the Organs of Circulation and Respiration.* By Charles Ewald Hasse, M. D., Professor of Pathology and Clinical Medicine at the University of Zurich, &c., &c. Translated and edited by W. E. Swaine, M. D., &c. Philadelphia: Lea & Blanchard, 1846, 8vo., p. p. 377.

The work under consideration was intended as the first volume of a series, on the pathological anatomy of all the organs, which, when completed, will constitute one of the most admirable of the works on this subject. The task of completing so great a labour, however, will occupy the author for many years, and it was desirable that the profession should be supplied with translations of the portions in the order in which they are published on the continent. "The uncertainty that necessarily attaches to the appearance of comprehensive works in distinct parts, has induced the council of the Sydenham Society to prefer publishing the present volume, which constitutes singly a complete and valuable treatise, as a separate and independent work."

This work, as it now stands, comprises two parts; the first devoted to the pathological anatomy of the organs of circulation; the second to that of the organs of respiration. The whole work is written in a plain, concise style well adapted to the subject; it opens out a mine of wealth which is stored up in the German and Italian medical literature, from which, indeed, the author has drawn more extensively than any other preceding author, not excepting Meckel; and it is refreshing to see a volume which contains some new facts, principles, or new views of things, and our author is replete with that originality and independence of thought and reasoning, which constitutes the philosopher in any branch of science. He is full of erudition, and draws extensively from all sources of information on the subject under consideration, but at the same time does not allow himself to become the slave of authority. The articles on the lesions of the organs of circulation are certainly the best we have met with; and those portions with reference to obstructions of the arteries and veins become especially interesting when viewed in connection with the recent researches of the learned Tiedemann and of Bouchat on the same subject. But the same remarks may be applied to every part of the work with almost equal justice, for all parts are characterized for clearness and precision, and indicate depth and careful induction. We could not make any thing like an abstract of the work without manifest injustice to its merits, and it would be useless to call attention to particular parts, or to make extracts, as the work can only be understood or appreciated as a whole.

To sum up, we would regard this volume as constituting the best, or at least, one of the best synopses of the facts of pathological anatomy in relation to the organs of circulation and respiration ; and most cordially recommend it to the profession generally, and especially students who desire to obtain clear and precise views on this important branch of medical knowledge. The fact of its being published by the Sydenham Society, is, however, a recommendation in itself so strong, that in this country, it will stand in need of no advocate to ensure it a favourable reception, and attentive consideration.

W. M. C.

II.—*Lectures on Natural and Difficult Parturition*—By Ed. W. Murphy, A. M. M. D., Professor of Midwifery, University College, London &c. &c., New York, Sam. S. and Wm. Wood, 1846.—8vo. 281.

The above is the title of another of the works, on Obstetric Medicine, of which our age is so prolific. The author writes like one thoroughly acquainted with the literature and practice of this branch of the profession ; and the work must certainly be regarded as a most excellent one of its kind. The view of the author seems to be to condense within a moderate compass the great mass of materials furnished by other and more voluminous works. There is but little, if any thing, in it known that can be regarded as original, and nothing that gives it superiority over the many excellent works which have preceded it in our own day. The fact is, there is too great a tendency among authors to favour the prevalent passion for shortening the road to learning ; and we have, every day, new works produced, which are only abstracts of the science, to which they relate. This, it is true, saves, the student labour, but it by no means supplies the knowledge that would be gained thereby, and then remain satisfied with the partial information such works supply, instead of aiming at the profound and complete instruction that is to be gained by works which are more full. This may be regarded, to some extent, as the prevailing defect of the works intended for the use of students at this time ; and it has the effect of fostering the spirit which now leads the student to prefer the superficial knowledge, which is easily gained in such works, to the erudition and profound knowledge which can only be attained by the assiduous and laborious study of those great and excellent works which we possess on many branches of science.—These abbreviations, it is true, possess a degree of value on account of the facility with which they may be referred to, on the spur of the moment, by those, who are already familiar with the subjects ; and the fault, in fact, is rather in their present, too frequent application, as books of instruction, that we would find fault with them.

Taken as a book of reference only, the work under consideration promises some value, and it may be regarded as a good abstract of the facts and principles in reference to Obstetric Medicine.

III.—*Elements of Pathological Anatomy; illustrated by 250 wood-cuts.* By Samuel D. Gross, Professor of Surgery in the Louisville Med. Institute, &c., &c. Second edition, greatly enlarged. Philadelphia, Barrington and Haswell; Louisville, Ky., Maxwell, (1845) pp. 822.

This is the second edition of Professor Gross' Pathological Anatomy; the first of which was published, in two volumes, in 1839. During the intervening six years, the science has been enriched by the addition of many valuable facts, in part the result of microscopic investigation, and a more intimate acquaintance with the chemistry of man. The present edition, our author thinks, "exhibits an accurate and comprehensive view of the science of which it treats." The attentive reader will be, no doubt, gratified, to find on the first page which meets his eye, the above declaration.

It will arm him with courage to follow our author through his elaborate pages, buoyed up with the hope, that when he shall have read the book with care, he will become thoroughly acquainted with Pathological Anatomy. About three hundred pages of new matter has been added to the present edition, besides a thorough revision. We think, our author would have enhanced the value of his book, if he had expunged twice as much, as he has appended to the original. It contains nothing, either novel, or that may not be found in the writings of Muller, Laennec, Lobstein, Gulliver, Cooper, Cruveilhier, and other celebrated authors on pathological anatomy. Indeed, Professor Gross acknowledges in his preface, his large indebtedness to these authors, for the matter contained in his work. Some of the views laid down in this work, we would fain believe, are peculiar to Professor Gross; as doubtless they would be repudiated by those, from whose writings he has drawn so largely. We have looked over the "*Elements of Pathological Anatomy*," and can discover nothing to recommend it to the attention of the profession.—It is nothing more than a common place history of pathological anatomy—a compilation of the opinions of others, without being dressed up in a fascinating and elegant style.

We predict, that this work will not add much to the reputation of its distinguished author. It cannot be expected, that such a work will attract attention, when we have so many master-minds engaged in the same subject. Had our author continued his experiments, so well begun, on "wounds of the intestines," he might have erected a monument to his memory, more durable than brass. Many of the views of Prof. Gross on the nature of Inflammation, are at variance with those, entertained at the present day, by the great majority of the profession. He contends, that all organic diseases, of whatever nature or extent, are the result of acute or chronic inflammation. Is this true of tubercular deposit? Again, he makes inflammation produce prodigies! In one case, it determines hypertrophy, and in another, atrophy.

He says, for example:—If an organ remain for a considerable time inflamed, the particles which are requisite for its growth and nourishment are withheld, and in consequence, it gradually sinks into a state of atrophy. This is indeed quite a novel explanation of the effects of an inflammation. Physiology tells us that the blood is the material from which the tissues and organs of the body derive their nutritive elements.

This being admitted, and further, that one of the conditions of an inflamation, consists in a preternatural accumulation of blood, and other fluids in a part, it would seem that, so far from the particles requisite for nutrition, being withheld, thereby causing *atrophy*, the very opposite must be the result; the nutritive element is furnished in excess to the part, and hypertrophy may be the consequence. Nature, we have been taught to believe, never achieves absurdities, and we leave it with the candid reader, to say, whether the professor or nature be correct! Such are only a few of the contradictions (to us) which have met the eye, in glancing over the pages of the work; yet the book may be read with advantage by those who wish to acquire some knowledge of the outlines of pathological anatomy. It is creditable to the author as a whole, for it proves him a man of industrious habits, and anxious to diffuse a taste for this department of medical science. The wood cuts add greatly to the value of the work.

A. H.

IV.—*On Diseases of the Liver.* By GEORGE BUDD, M. D., F. R. S. Professor of Medicine in King's College, London; and Fellow of Caius College, Cambridge, 8vo., p. p. 392. Philadelphia: Lea & Blanchard, 1846.

The appearance of a new work on Diseases of the Liver, must be an event of interest to all medical readers, if for nothing else than the comparative rarity of the occurrence lately; and especially so, to physicians in our climate, from the great practical importance of the subjects treated of. The author had (as he states in his preface) considerable opportunities for the study of hepatic diseases, from his connection with a seamen's hospital; and from his self-known ability and celebrity as a teacher, we might *a priori* vouch for the excellence of the work before us.

The introductory chapter is taken up with an account of the structure of the liver, its function as a secreting organ, the chemical composition, source and uses of its secretion. The author speaks first of the great comparative difficulty of investigating diseases of the liver, owing to our not being able to detect changes in its physical structure, (except in regard to size,) as in the heart and lungs, nor, as in the case of renal disease, can we make a daily examination and analysis of the excreted product; so that we are thrown upon the purely functional or rational signs, which when they exist, are few in number and difficult of interpretation from intimate association with those belonging to other organs. Another and perhaps the main barrier to our progress in this investigation has been, our ignorance of the minute structure of the liver, on account of which we were unable to explain the most common post-mortem appearances; this difficulty has been done away with, by the recent researches into the minute anatomy of the liver; so that now it is placed on the same footing with other organs, in its morbid anatomy. Another and important step has been made by the more correct and satisfactory analysis of the bile lately made, by which means we have a point of comparison, for its altered conditions.

The result of all the researches into the intimate structure of the liver may be summed up in the words of the author's definition, "the mass of the liver is made of a plexus of capillary blood-vessels, the meshes of which are filled with nucleated cells, containing the peculiar principles of the biliary secretion." He adopts the views of Mr. Kiernan, in regard to the intimate structure of the liver, with one exception, and that is in regard to the isolated condition of the lobules; this he states, on the authority of Mr. Bowman, to be an error arising from the use of a magnifying power not sufficiently great to bring plainly into view the entire capillary structure. As this is a subject of some interest, we cannot do better than transcribe the words of the author, to illustrate his views or rather Mr. Bowman's as different from Kiernan's, in regard to the existence of lobules and of areolar tissue or Glisson's capsule between them. "The injected preparations of Mr. Bowman show, I think, clearly that this opinion is erroneous—that the lobules are not distinct, isolated bodies, but merely isolated masses tolerably defined by the ultimate twigs of the portal vein and the injected or uninjected capillaries immediately contiguous to them." Again in speaking of the areolar tissue of Glissen's capsule he says, "it cannot be traced further than the ultimate twigs of the artery and duct, and seems not to enter the capillary network." We come now to the author's consideration of the really essential parts of the liver as a secreting organ, viz. the "nucleated cells" occupying the little islets of the capillary network. It having been shown, that from the follicular appendages to the stomach of the polype, up to the complex and most highly developed liver, as found in the carniverous animals, these cells are the "secreting organs," the consideration of these little bodies, both in health and disease becomes important.

When examined by the microscope, they are seen to contain besides a nucleus, an amorphous biliary matter and oil-globules, an alteration in the quantity of which constituents results in some well known pathological states. The increase of the latter or oil-globules is seen in the fatty liver of phthisis, and different proportions of the former give various degrees of size and consistence to the liver, and blended in its effect, with that of a greater or less quantity of blood in the capillary vessels, produce those striking variations of colour, which we more frequently witness in the liver than any other organ. Bile is easily acted on by chemical agents, producing thereby a great number of apparently component parts, and a total want of conformity in the results of different analytic chemists; this has led most recent writers on the subject, to incline to the old and simple view of the soapy nature of the bile, which doctrine has been revived by Demarçay and Liebig; in this list we must include our author. After giving the "oft quoted analysis of Berzelius," he proceeds to notice the doctrine of the bile being composed of an "isolable electro-negative body" termed choleic acid and soda, pointing out at the same time, the resemblance between this compound and soaps, but omitting to mention the only stumbling block to the universal reception of this theory, viz. that whilst choleate of soda is decomposed by any, even the weakest acids, bile is not.

In the following paragraph the author alludes to the probability of a greater difference between hepatic and cystic bile, than has yet been discovered by analysis or even alluded to by physiological writers.—"There are probably more important differences between the cystic and hepatic bile than those that result from different degrees of concentration, but little is known on this point. It is very difficult to collect bile from the hepatic ducts in quantity enough for a complete analysis, and consequently chemists, in their study of this fluid, have confined themselves almost exclusively to bile taken from the gall-bladder.—Most chemists, indeed, have been content with bile from the gall-bladder of the ox, which can be more readily got in a healthy state, and can be obtained in a larger quantity than human bile." That cystic bile contains more mucus, and less water than the fluid of the hepatic ducts; that the former in some states of the gall-bladder abounds in cholesterine, while this principle forms but a very small part of the latter, are facts, of which we are well aware ; but that any essential or even "important differences should be hereafter detected in these two fluids, seems improbable when we consider, that in the elaboration of the bile from the blood, its secretion and subsequent uses, the gall-bladder is not an essential but merely an accessory part of the biliary apparatus, that it is not present in all animals having a liver, nor absolutely constant in any species usually possessing it.

The remainder of this chapter is taken up with a consideration of the source and uses of the bile, and the action of cholagogue medicines.—That the elements of the bile, as they exist in the blood, and are separated by the liver, are derived from the decomposition of the blood seems undeniable, but whether any portion of the food goes to this formation appears questionable. The author, in this place, takes occasion to show the fallacy of Liebig's deductions upon this subject, founded upon the alledged fact, of the excess of carbon in the bile, secreted in any given time. over that of all the proteine elements (which must necessarily replace the tissues,) being derived from the food. The quantity of bile daily secreted is here greatly over estimated, being reckoned on the extravagant notion of Schultz, by the quantity of alkali necessary to neutralize the acid of the chyme.

In regard to the uses of the bile, the following brief summary of the author's conclusions may not prove uninteresting to the reader. The bile is not an excrementitious product, (at least in man and the higher animals,) but is absorbed into the system, and being principally composed of carbon and hydrogen, passes off by the lungs in the shape of carbonic acid and water, having by its union with oxygen played an important part in the production of animal heat. In the digestive process bile does not act as a chemical precipitant, separating the nutritious and soluble from the fecal parts of the food ; but as the gastric juice is necessary for the solution, of the "staminal principles of our food" i. e., the proteine compounds, so the bile acting as a soap, effects such a change in the fatty matters as to render them absorbable by the lacteals. An important collateral effect of the bile is "to promote the due discharge of the contents of the bowel," calling into action the muscular contraction of the bowel, by its immediate presence upon the mucous coat. Though the functions of the liver and lungs cannot be considered as supplementary to each other, yet the relation of the bile to respiration is "direct and fundamental;" upon this the author insists and makes some important practical deductions, which we think well worthy of quotation.

"Thus it appears, on any supposition, that the relation of bile to respiration is direct and fundamental. Fortunately, the activity and effects of the respiratory process are largely under our control. In the vast power we have of modifying these by appropriate regulations, having reference to the great conditions of air, exercise, temperature, and food, we have means much more effectual than any other, in dealing with biliary disorders.

"Of these disorders, on the other hand, the neglect of such regulations is by far the most fruitful source.

"Thus, for example, may be explained many of the bilious disorders of hot climates. If, in such climates, the food be not regulated in accordance with the smaller needs of the economy as to animal heat, an excess of bile is formed, and disorder of the stomach and intestines—bilious vomiting, and diarrhœa—are the consequence."

The author next alludes to a defective secretion of bile or "feebleness of the liver, which may be inherited and has its counterpart in hereditary vesicular emphysema, or it may be acquired, being then the result of an injury done by disease to the cells or "secreting element of the liver," (the author reckons yellow fever, as one of the diseases, likely thus to cripple the functions of the liver.) Persons so situated, when "temperate in all things" do well enough, but when from "a hot climate, indolent habits, gross living, constipation, &c.," an accumulation of the biliary elements in the blood takes place, we have sallowness of skin and other derangements which mark this peculiar constitution.

Passing by the chapter devoted to the elucidation of Mr. Kiernan's well known views upon Congestion of the Liver, we came to the important subject of the inflammatory diseases of that organ. In treating this subject, the author discarding the old terms acute and chronic as inapplicable, divides inflammatory affections of the liver, according to their causes, or rather by the nearest approximation, which the present limited state of our knowledge affords, i. e. according to the effects of the different causes. The enumeration of these affects is as follows. "1st. Suppurative Inflammation ; 2nd. Gangrenous Inflammation ; 3d. Adhesive Inflammation ; 4th. Inflammation of the veins of the Liver, and 5th. Inflammation of the Gall-bladder and Ducts. In reference to the first species of inflammation, the author has tabulated the chief circumstances of sixty cases of abscess of the liver, some of which were observed by himself, and the remainder recorded in the works of Andral, Louis and Annesley. As a result of this investigation into the causes of this affection, he draws conclusions, of which the following is a brief summary : That by far "the most frequent cause of abscess of the liver is suppurative inflammation of some vein, and the consequent contamination of the blood by pus."

It has been shown by experiment, that mercury injected into a vein, produces a circumscribed abscess in some of the internal organs, generally the liver or lungs, the relative frequency of the occurrence in these two organs depending on the situation of the vein, whether a branch of general or portal venous systems. The occurrence of metastatic abscess of the liver, after injuries and operations with suppuration in a vein, is well known ; now, the protected state of all the branches of the portal system would lead us to expect this result from injury but rarely, yet one or two such cases are recorded; one in particular is related by the author, where an abscess of the liver immediately followed injury of a prolapsed rectum, in an attempt to return it. In the opinion of our author, the most common source of the pus, which "contaminates the blood of the portal system," and by "being arrested in the capillaries of the liver forms abscess," is inflammation of some of the mesentery veins from ulceration of the large intestines in dysentery; to this cause he refers a majority of the cases of abscess of the liver. That there must be some peculiarity in the lesion of these cases of dysentery with hepatic abscess, must be evident from the rarity of its occurrence, as derived from the recorded evidence of writers on dysentery, and the result of the experience of our own physicians in that prevalent disease here. In noticing the commonly received causes, viz. congestion of the liver, spirit drinking, and a hot climate, he asserts that the first and second never produce it, and the third only secondarily, by exciting first dysentery or inflammation of gall-bladder and ducts, with ulceration.

The great obsurity of the symptoms of abscess of the liver is known to every physician, the presence or abscence of these depend principally upon the seat and size of the abscess, i. e. whether internal or approaching the peritoneal surface, whether small or involving a great amount of the "secreting element of the liver." The following sympathetic phenomena have been, for a long time, said to occur in abscess of the liver viz. "pain in the right shoulder; vomiting; a short, dry cough; and permanent rigidity of the muscles of the abdominal parietes, but especially of the right rectus muscle." Upon the first three of these, as signs of hepatic disease, much discredit has been thrown by the remarks of Andral and Louis; but with regard to these, the author remarks, that though "they are less frequent in cases of hepatic abscess than is generally imagined," yet they occur in a number of cases and "in some of these cases, there could be no doubt of their dependance on the disease of the liver." Every practitioner will perhaps readily agree with Dr. Budd, "that the treatment of abscess of the liver is very unsatisfactory;" he joins with Annesley and others, in reprobating in strong terms, the use of mercury in this affection, as perfectly nugatory in the first stage, when it might possibly do good, could the system be brought under its influence; and as manifestly injurious after suppuration has commenced. The author objects to opening an abscess of the liver, as "an occasional instance of success is a poor sett-off against the cases, in which the operation has done mischief, or failed of doing good."

"Sect. II. *Gangrenous Inflammation.*" The author remarks that cases of unequivocal gangrene of the liver are of extreme rare occurrence, the appearances resembling this state, which are seen around abscesses and immediately under the peritoneal surface of the liver in suppurative perotinitis, are the result of the action of sulphuretted hydrogen gas, (developed after death) upon the adjacent tissue of the liver. When gangrene of the liver does occur, it is generally accompanied by the same condition of some other portion of the body, (which is the cause, according to our author) "the septic agency being conveyed by the blood." In the most striking case met with by the author, the first cause was mortification of the toes from cold. In this connection we will mention a case, observed in the Philadelphia Hospital, by the writer of this article when a house pupil, which shows that gangrene of the liver may occur, without any contamination of the blood from a gangrenous mass exterior to that organ. A young man in the wards with phthisis, was seized with all the symptoms of gangrene of the lung, had fetid sputa and the signs indicative of the formation of a cavity in the right lung at its lower part. Shortly after the supervention of these symptoms, the patient died and upon examination after death, a large gangrenous cavity was found in the upper convex part of the right lobe of the liver, communicating with a cavity of much less size, in the lung, and through it, with one of the large bronchial tubes. The substance of the liver was thickly studded with tubercles of various sizes, around most of which was a distinct ring, of dark and softened hepatic tissue having an offensive odour; the ring was in some instances nearly half an inch in thickness. No corresponding appearance of gangrene was seen about the tubercles which existed in the lungs, some of which had gone to the stage of softening.

"Sect. III." Treats of "*Adhesive Inflammation of the capsule and substance of the liver—Cirrhosis.*" This being the most common of the organic diseases of the liver and the one most disastrous in its consequences, the author by his ample consideration, admits its importance. A clear and graphic description of the appearances observed in cirrhosis and of the cause of these appearances, can no where be better found, than in the words of the book, before us.

"The tissue of the liver is paler than natural, from the presence of this white fibrous tissue, and from its containing but a small quantity of blood; and it is often yellowish from accumulation of biliary matter in the cells. When this is the case, a section has the grayish and yellow colour of impure bees-wax, and, in consequence, the disease has been called by the French, *cirrhosis.*

"In other cases again, the quantity of this adventitious fibrous tissue is much greater, and by its contraction the lobular substance of the liver is drawn into round nodules, which being of a deep yellow colour from accumulation of biliary matter, are in strong contrast with the gray fibrous tissue between them.

"The ordinary appearances in cirrhosis, and the changes just described, seem the consequence of adhesive inflammation in the areolar tissue about the small twigs of the portal vein, by which serum and coagulable lymph are poured out. The serous part of the effusion gets absorbed, and the fibrine contracts and becomes converted into dense fibrous tissue, which divides the lobular substance of the liver into well-defined masses, gives great density and toughness to the organ, and by compressing the small twigs of the portal vein and the small gall-ducts, and thus impeding the flow of blood and the escape of bile, causes the pale yellowish colour of the masses of lobules.

"In the early stage of cirrhosis, the liver is much enlarged by the serum and lymph effused within it. In time, the watery part of the effusion is absorbed, the fibrine contracts, the small twigs of the portal veins are compressed by the new tissue, and the lobular substance of the liver, receiving less blood than it should do, wastes. On all these accounts, the liver diminishes in size, and in protracted cases, from the small quantity of blood it contains, and the great atrophy of the lobular substance, it is usually very much smaller than in health."

In some cases this disease appears to arise from disordered digestion, long continued, but in the vast majority of instances spirit-drinking is the cause, the alcohol being absorbed by the portal veins, and owing to its volatile nature, readily permeates the tissues and acts directly in producing an effusion of Lymph in the different portoins of Glisson's capsule. There are two accessory causes which powerfully aid the principal one, viz. congestion of the liver from disease of the heart, and a hot climate. The symptoms of the early stage of this disorder, when alone it is amenable to treatment, are obscure, but we should be led to suspect its occurrence in any intemperate person, from the appearance of slight febrile symptoms, with pain and fulness in the right hypochondrium. If combated now by mild antiphlogistic means and the removal of the cause —alcohol, the patient may be saved; but if these premonitions be disregarded (as they generally are,) and the well known symptoms of the advanced stage, (ascites, tendency to hemorrhage from different portions of the portal system, sallowness of skin, emaciation, &c.,) supervene, little or nothing is left for the practitioner, but to smooth the path of the sufferer to the grave.

"The case is analogous to that of stricture of the intestine from the contraction and organization of lymph effused under the mucous coat, or of disease of the valves of the heart. There is a permanent mechanical impediment to the due performance of the functions of the organ. The disease will, sooner or later, but inevitably, prove fatal."

The author condemns the use of diuretics and hydragogue cathartics, as having little or no effect upon the ascites, and doing injury "by lowering the strength of the patient." The operation of paracentesis abdominis is never advised but in the most extreme cases, for the relief of the embarrassment of the respiration and other functions.

"Sect. IV. *Inflammation of the veins of the Liver.*" This affection is rarely met with; when it occurs, it is some times the result of direct injury; of this the author reports a case, where a fish-bone penetrated the stomach and vena portæ or of an arrest of pus-globules in the veins, as a consequence of some operation or injury.

"Sect. V. *Inflammation of the Gall-bladder and ducts.*" This inflammation may be of various kinds, as "mucous, purulent, plastic or ulcerative," in its mild or mucous and purulent form, it constitutes the lesion of a majority of cases of "inflammatory jaundice." The author has bestowed great labour and research upon this obscure and insidious, but very important disease, from its mechanical results. The most common, or perhaps the only cause, is an altered secretion of bile, for in regard to gall stones, owing to their being of less specific gravity than the bile, they can exert little or no pressure upon the coats of the gall-bladder. The general result of this variety of disease is closure of some duct, the cystic, hepatic or common, or all of them; when closure of the cystic duct takes place, if the disease is arrested without ulceration of the coats of the gall-bladder and effusion of its contents, (cases of which are reported in the work before us,) the effects are comparatively unimportant. But closure of the common duct has far more serious effects; if the patient survive for some time, the liver after death will be found entirely destitute of the "nucleated cells," or the proper secreting organs of the bile; this fact has been fully proved by the microscopic researches of our author and Dr. Williams, in two well marked cases. The symptoms of this variety of hepatic disease are sudden jaundice, with pain in the right side, and if the disease involve the cystic or common ducts, there will also be, "a small pear-like tumour, not tender to the touch immediately beneath the right ribs." A curious observation is made by the author, in regard to two cases of continued jaundice from closure of the common duct, where from the want of cells in the liver, it was probable that no bile had been secreted, for some time before death. In these cases none of those grave symptoms usually attributed to the action of bile upon the nervous centres appeared, the intellect and other nervous functions being perfect up to the time of death. In the treatment of this class of affections, after general and local depletion. Dr. Budd recommends the use of mercury, soda or taraxacum, as likely to prove very beneficial, in regard to the former he says: "It probably acts in two ways, 1st. by increasing the quantity and promoting the flow of the bile, and 2nd. by producing changes in its quality, which render it less irritating." "It is to the striking benefit sometimes derived from mercury used in this way, that this medicine owes the reputation it has long had as a remedy in liver diseases." In this chapter the author has noticed a peculiar disease of the gall-bladder and pointed out its analogy with that very common affection of the arteries in old people, termed atheromatous degeneration.

"Another circumstance, worthy of notice in the case of Mrs. Diprose, is the state of the coats of the gall-bladder, which were thickened and opaque, and when examined under the microscope, exhibited numerous oil globules and transparent scales of cholesterine. This disease of the gall-bladder is analogous to the "atheromatous" disease of arteries, which Mr. Gulliver has lately designated, "fatty degeneration of arteries," from having discovered that the atheromatous matter is chiefly composed of fat, in the form of oil-globules and scales of cholesterine. This disease of the gall-bladder may, therefore, be termed with equal propriety, *fatty degeneration of the gall-bladder;* an expression, which has the merit of involving no theory as to the cause of the disease, but merely announcing a fact. In the gall-bladder, as in the arteries, phosphate of lime is often deposited with the fatty matter, and sometimes in such quantity as to form large bony plates, which on the inside of the gall-bladder are usually bare, or merely covered by a soft pulpy matter, which may be readily scraped away.—Sometimes, the earthy matter is in such quantity that the gall-bladder is almost converted into a bony cyst.

"This disease of the gall-bladder is very important, from its being always attended by a large secretion of cholesterine in the gall-bladder, which frequently leads to the formation of gall-stones and all the evils they occasion. It is perhaps confined to persons advanced in life; and, according to my own observation, it is much more common in women than in men. Sedentary habits and modes of life conducive to fat, probably favour this degeneration."

"Chapter III. *Diseases which result from faulty nutrition of the liver, or faulty secretion.* Sect. 1. *Softening of the Liver—destruction of the hepatic cells—fatal jaundice.*"

"Having considered the inflammatory diseases of the liver, we may pass on to a class of diseases, at present less understood; diseases in which, seemingly without inflammation, the secreting power, or the nutrition of the hepatic cells and other tissues of the liver, is seriously disordered. These diseases may be divided into two principal groups. One of these groups is characterized by suspension of the secretion of bile; the principal feature of the other is, that the hepatic cells separate from the blood some abnormal matter, which, instead of passing freely out of the liver in the bile, is retained there, adding to the size of the liver, and more or less changing its appearance and texture."

In cases of fatal jaundice, a condition of the liver, under the terms, "softening of the liver." "yellow atrophy of the liver," has been long known and described; but it is only since the application of the microscope to the study of morbid anatomy, that the real peculiarity of this alteration has been detected and found to be, not softening of the substance alone, but a destruction of the "nucleated cells," attended with diminution of the size of the liver and softening of its tissue. In the sudden and rapidly fatal jaundice, which is produced by some violent mental emotion and in a form of it, described by the author, as occurring successively in different inmates of a house or ship, this condition of liver is seen, when a piece of it is placed under the microscope, and from the analogy of symptoms, the author thinks, will be found in the jaundice, produced by the bite of venomous serpents; the poison of yellow and other malignant fevers; and the altered condition of the blood in purulent phlebitis. In the detection of this terrible malady, the author thinks, that we should be principally guided by a knowledge of the circumstances under which it generally occurs, and next, by the diminished size of the liver, the vomiting of bile, and the early supervention of delirium, coma, &c., in all of which it differs from sudden jaundice, resulting from closure of the common duct. Of the treatment he has the following remarks:

"Until more is known of the causes of this form of disease, and until it can be detected with more certainty, we cannot expect to have satisfactory proofs of the good or ill effects of particular plans of treatment. The conclusion that may be most safely drawn from the foregoing cases, is, that in some instances, coma may probably be prevented or removed, and the life of the patient saved, by active purging."

"Sect. II. *Fatty degeneration of the liver.*" The appearances shown in this state of the liver, must be familiar to every one; as a very common accompaniment of phthisis, especially in women, it was pointed out by Louis as early as 1825; but it was not until 1841, that the exact seat of the fatty matter was known, when the oil globules were detected in the nucleated cells by Mr. Bowman, in a piece of fatty liver, sent him by Dr. Budd for examination. The quantity of fat varies in different instances; where it is small in amount, it produces the "nutmeg appearance" of liver, and where very great, the "waxy liver;" in the one case, the fat can hardly be detected by ordinary means, and in the other, it constitutes sometimes one half of the weight of the entire organ. Fatty liver may be produced by gross living; here the fat accumulates in the liver, as in the other organs. Fatty liver occurs in other diseases besides phthisis; the author has furnished cases of it along with cancer and chronic dysentery, and according to Rayer and Biett, it is common in persons affected with chronic pemphigus.

"It would seem, from these instances, that the fatty condition of the liver so common in phthisis, does not result from the office of the lung being interfered with, or from the presence of tuberculous matter in any particular organ, but rather that it is connected in some way with the general constitutional disturbance—the abundant suppuration, the wasting, the hectic,—so common in advanced stages of phthisis.

The opinion was some years ago advanced by the late Baron Larrey, that the fatty condition of the liver in these cases results from solution of the fat previously laid up in the body. He considered this opinion strongly supported by the method then employed in France to make the livers of geese fatty, and of which he gives the following account: "To procure the large livers of geese, for the making of patties, fatted birds are confined in close cages, and then exposed to a graduated heat, being kept at the same time entirely without food, even without water. They become feverish, the fat undergoes a kind of fusion, and the liver grows enormously large. The liver is considered to be in the desired state, when the animal is *extremely wasted*, and the fever increases."

Dr. Budd regards fatty deposit in the liver as exactly analogous in its relation to that organ, to that between diabetes and the kidneys, both being diseases of the general economy, and not of the particular secreting organs, by which the fat and sugar are separated from the blood, with diminution of fat every where else; in phthisis it accumulates about the heart and in the liver, in the first position, it sustains an important mechanical office, and in the second, it obeys the general laws of elimination from the blood. But how came so much of it in the blood, in phthisis, rather than in other chronic diseases equally wasting? we know not.

"Sect. III. *Scrofulous enlargement of the liver and other kindred states.*" The most common of these "kindred states" is an enlargement of the liver, consequent upon the abuse of mercury, for the cure of syphilis, they all resemble fatty liver in the fact of there being a secreted product, retained in the cells of the liver. This substance being in these cases supposed to be albuminous, interferes more with the passage of blood and the secreting function, than the oil globules; Ascites and Jaundice are more common than in fatty liver. These are all then "diseases of the nutrition of the cells of the liver," and not cases of hypertrophy, a term, which cannot, according to our author, be applied to any state of the liver, or any other secreting organ, at least in the sense we use it, when speaking of the muscles. These cases yield to treatment, when the peculiar cachexy, upon which they depend, is skilfully managed; the author speaks in high terms, of frictions with iodine ointment externally, together with the iodide of potassium internally, both being continued for a considerable time.

"Sect. IV. *Excessive and defective secretion of bile,—unhealthy states of the bile.*" These affections, though imperfectly understood at present, from the want of a good analysis of the bile, and from its being retained for the process of digestion, and not excreted like the urine, are of great importance, both from their great frequency and the readiness with which they yield to appropriate treatment. Of the quantity and quality of the bile secreted, we can during the life of the patient only judge by the symptoms, which present themselves.

"The secretion of bile may be disordered from organic disease of the liver, which renders it incapable of adequately performing its functions: but it may also be disordered without this, when the portal blood, from which the materials of the bile are drawn, is rendered unhealthy by medicines, by unwholesome food, by faulty digestion or assimilation, or by defective action of some other excreting organ. It may probably be disordered, too, from the direct influence of anxiety or strong mental emotion. In any case, the disordered secretion of bile is the effect of some other disease, or of some cause that disorders other organs as well."

"But the bile has a long course before it passes out of the body, and serves an important office in digestion, and, on these accounts, if it be in undue quantity, or unhealthy, however the change in its quantity or quality may have been brought about, it may cause various secondary disorders. In the first place, it may inflame or irritate the gall-ducts, or the parts of the intestine with which it is brought into contact. There is reason to believe that most of the diseases of the gall-bladder and ducts, are produced by irritating bile; and there can be no doubt that various disorders of the bowels result from the bile being in improper quantity or unhealthy. But, besides these mere local effects, a faulty state of the bile may render digestion imperfect, and in this way, may impair nutrition; and the noxious products of imperfect digestion may be absorbed into the blood, and from this, again, many secondary evils may spring."

The symptoms ensuing from an occasional redundance of bile, commonly termed a bilious attack, are familiar to every practitioner; they are easily relieved by mercurial and saline purgatives; these, though they afford temporary relief, do not strike at the root of the evil; this can only be done by adopting such measures, as will prevent an accumulation of the bile in the blood, and a recurrence of the disease. The means advised by the author are of two kinds, 1st. the increase of the function of respiration, so as to "burn off" the additional hydro-carbon, and 2nd, to cut off the supplies, viz. "spirituous liquors, cream, fat and sugar."

"In the degree of confidence he places in these resources, and in the preponderance he gives them over mere drugging in the treatment of disorders of this class, the practitioner will give the best evidence of his real insight into their nature, and of practical skill founded upon it. It adds not a little to the value and importance of these means that they are so free from hazard, and that they act in a way in which no others can act, and therefore have no perfect substitute in any direct medication. By appropriate purgatives, we may temporarily drain the liver and intestines of redundant bile; but by the means here pointed out, we prevent its formation, and attack the evil in its source."

Diminished secretion of bile may be the result of purely functional derangement of the liver; in this form it constitutes the efficient cause of that acidity of the contents of the alimentary canal, producing in some persons a peculiar diarrhœa, which ceases when the flow of bile is re-established; and in others, that peculiarly distressing and common train of symptoms, known as nervous headache. In regard to the treatment of these affections, the author agrees with Dr. Prout, in reprobating the use of drastic purgatives, "which sometimes afford immediate relief, but usually leave the patient more inveterately disposed to the diseases," and advises the exhibition of small doses of rhubarb, aloes, and ipecacuanha daily, in conjunction with a regulated diet.

"Another class of disorders is where the secretion of bile is defective, not as regards digestion merely, but as regards the blood; where the blood is not sufficiently freed from the principles of the bile, and the complexion is jaundiced, or bilious."

This condition of things is sometimes the result of too great a formation of bile and follows indolent habits, gross living, residence in a hot climate, &c.; this "deficiency of secretion" is easily got rid of by calomel and purgatives. But most commonly it is a consequence of some injury to the secreting cells, resulting in their atrophy, as "in cirrhosis; adhesive inflammation of the portal veins; fevers and bilious disorders of hot climates; or long continued indigestion in any climate." When the liver is thus crippled we must endeavor to prolong the existence of the patient, by the agency of proper hygienic measures, such as, increased respiration from exercise in the open air, and regulation of diet, (avoiding all rich and fatty food, sugar, &c.); together with the use of "various medicines, some of which appear to act by rendering the secretion of less bile necessary; others, by rendering the liver more active, and in this way increasing its secretion." Of the former class of medicines nitro-muriatic acid has the first rank, and of the latter, mercury and taraxacum. In regard, however, to the frequent use of mercury as a cholagogue in defective secretion of bile, from organic injury of the liver, the author has the following judicious remarks.

"Occasionally, and under these circumstances, and especially in persons of full habit, mercury may be given with great advantage. But its frequent use, in any case, may lead to much mischief. When the liver has been accustomed to the stimulus of mercury, no other medicine will sufficiently excite its action. The person is thus led to the habitual use of this medicine, and, after a time, the constitution is undermined by it. In the class of cases we have just been considering, where, from organic disease, the liver is inadequate to its office, and nutrition has suffered much in consequence, mercury, although even here it may relieve for the moment, will almost invariably do harm. It increases the activity of the liver, at first, but seems to leave it weaker than before, and if frequently resorted to, the nutrition of the patient, impaired by the original disease, is still further impaired by the drug. In all such cases, we should be content with milder medicines, which increase the secretion of the liver without having any permanent deleterious effect on the system. The best medicine of this class is taraxacum; which may be given alone, or in conjunction with the nitro-muriatic acid."

The most important effect of an altered state of the bile is the formation of gall-stones, to the consideration of which, in detail, the author has devoted the next section. As the most important effect of the formation and passage of these bodies, i. e., inflammation, ulceration and closure of the ducts, has been treated of in first part of the book, we omit any further mention of it. The remainder of the work is devoted to "diseases which result from some growth foreign to the natural structure;" our limits forbid any notice of these chapters, though of almost equal interest with the foregoing. Having thus attempted to give a brief summary of the more important contents of this work, we would, in conclusion, recommend it to every practitioner and student, as well worthy of a careful and patient perusal.

R. J. F.

V.—*A Text-Book on Chemistry for the use of Schools and Colleges.*—By John William Draper M. D., etc., with nearly three hundred illustrations. 3d Edition. Harper & Brothers—octavo, pp. 408.

In this work we have a satisfactory compendium of the most important facts and principles in chemistry. The style is perspicuous and appropriate, and altogether the work is well adapted for the use of the students for whom it was designed. As a general work of reference for chemical facts and numerical data, it is excelled by the more extended treatises of Turner, Kane and Graham. Yet it contains more than our medical and literary students generally have time to master. It has been adopted this session as a text-book in the Medical College of Louisiana.

R.

Part Third.

EXCERPTA.

[From the Medico-Chirurgical Review.]

[Concluded.]

1.—*Rapport à l'Academie Royale de Medécine sur la Peste et les Quarantaines, fait, au nom d'une Commission*, par M. le Dr. *Prus;* accompagné de Pieces et Documents, et suivi de la Discussion dans le Sein de l'Academie.—I. et II. Parties. 8vo. pp. 663. Paris, 1846. Bailliere.

2.—*Correspondence respecting the Quarantine Laws, since the Correspondence last presented to Parliament.* Presented by Command to the House of Commons, in pursuance of their Address of May 19, 1846. Folio, pp. 48.

Third Part.

We now approach the question of the transmissibility of the plague, away from or beyond, as well as within, epidemic foci—a question, we need scarcely say, of the highest importance and, at the same time, of very difficult solution. The first point that we shall discuss is this:—

Chap. I.—*Is the plague transmissible by inoculation either of the blood drawn from a vein, or of the pus from a bubo, or of the serosity from the phlyctena of a carbuncle?*

"It is obvious," says M. Prus most justly, "that if the plague be truly a virulent disease (*à virus fixe*, to use the expression of certain writers,) the possibility of the inoculation of its virus would approximate it to epidemic contagious diseases; whereas if it does not furnish any principle, liquid or solid, that is susceptible of being inoculated and of producing a virus similar to that which gave it birth, the disease must be withdrawn from the class of diseases that are properly contagious, such as Small-pox, and would approach in this respect the character of Typhus, which is propagated by peculiar miasms, but which gives out no inoculable element."

And here it should be noted as an important fact that, if the diseases that are indubitably contagious—Small-pox, Hydrophobia, Glanders, and Syphilis, for example—all present us with a palpable liquid which contains the poisonous principle, such is certainly not the case with the Plague. Hence medical men have operated, by turns and almost indifferently, with the pus of a bubo, the serosity of a carbuncle, or even with the blood itself of a pest-patient.

The experiments that were made in Egypt by Desgenettes, White, and a few other physicians, about the beginning of the present century, to ascertain the effect of the direct inoculation of the matter taken from plague buboes, are anything but satisfactory or conclusive. We shall therefore not dwell upon them, but at once proceed to notice those which were instituted in 1835, at the Cairo Hospital, in the presence of Gaetani-Bey, Clot-Bey, and Drs. Lacheze and Bulard, and which are deserving of all confidence.

Five criminals, who had been condemned to death, were the subjects of the experiments. A lancet, wetted with the blood drawn from a pest-patient, was passed under the epidermis on the inside of the arm of one of these criminals, at two different points. On the third day afterwards, the man was affected with confirmed plague—so, at least, says Dr. Lacheze, who reports the experiment; Clot-Bey thought the case doubtful. Three days subsequently, the man was convalescent.

In three other cases, no effects followed the inoculation of the blood. In two cases, the serosity from a carbuncle, and in one the pus from a bubo, was used for the purpose of inoculation: in none of these cases, was the disease induced.

With respect to the single case, in which the disease (mild indeed) occurred after inoculation with the blood of a pest-patient, it must be kept in mind not only that the man was exposed, as a matter of course, to the epidemic atmospheric influence then existing in Cairo, but also that, for three days before the performance of the experiment, he had been living in a pest-hospital, which was necessarily a focus of pestilential infection.

Clot-bey inoculated himself, in six different punctures, with the blood of a pest-patient; no constitutional effects followed. A few days subsequently, he inserted some pus from a bubo on the inner part of his left arm; this was followed by a slight indisposition, which he attributed to the absorption of the purulent matter, but which bore no resemblance to the symptoms of plague.

The results of certain trials made by Professor Pruner in 1829, and by Dr. Rossi in 1841, were altogether similar.

The general *conclusion* of the Commissioners upon the important point under consideration is to this effect:

"The results of the inoculation of the blood drawn from the vein of a plague patient, or from the pus of a pestilential bubo, have been equivocal; the inoculation of the serosity taken from the phlyctenæ of a pestilential carbuncle has never given the disease. It is therefore not proved that the plague can be transmitted by inoculation, even under the influence of a pestilential constitution.

"We are not acquainted with any experiments that have been made upon the same subject, at a distance from an epidemic focus.

"It is useless to observe that the study of the effects which might have been obtained from inoculation of the plague, a study so important for the knowledge of the nature of the disease and consequently of its transmissibility, presents, nevertheless, no direct application to the question of quarantine. There can be no fear that the mass of a population will ever allow themselves to be inoculated with the plague."

Chap. II.—*Is the plague observed, in epidemic foci, to be transmissible by immediate and direct contact with the sick?*

The Arabian physicians, as well as their predecessors, regarded the disease as purely and simply epidemic, and seem therefore not to have troubled themselves in seeking to determine if the disease be communicable from one person to another. We must come down to the middle of the 15th century, the time of Frascatorius, before we meet with any formal exposition of this doctrine.—The celebrated physician of Verona recognized three modes in which the plague may be communicated:—1. direct contact with the sick; 2. the infection or contamination of goods, clothes, &c.; and 3. diffusion of morbific miasms through the atmosphere. The relative frequency of these three modes was believed to be in the order that they are here enumerated; the first being supposed to be by far the most common, and the last to be comparatively very rare.—These opinions of Frascatorius prevailed almost universally down to the year 1720. In that year, Chicoyneau, Verney, and Deidier of Montpelier maintained with considerable eclat the doctrine of the non-contagiousness of the plague; they regarded it as purely epidemic. Their chief argument was, that they had touched the bodies of plague-patients without taking any precautions, and that they had not caught the disease. The opposite and older opinion, however, continued to be very generally held in the schools.

In 1771, Mertens, Orræus and Samoilowitz, who had an opportunity of watching the plague at Moscow, declared their belief that it was propagable only by direct or indirect contact with the infected, and never through the mere medium of the air. Stoll, however—who is characterised by Dr. Prus as the most able observer, after Hippocrates and Sydenham, of epidemic diseases—was not at all satisfied with the prevailing opinions on the sucject in question, and pointed out in the following (ironical?) passage the necessity of re-examining them with care and candour.

"He who would deny," says this truly enlightened physician, "the contagion of the plague, and attribute a very grave disease to an epidemic cause, acting equally upon all the same effects, and would ascribe it either to the constitution of the year, or to an alteration in the air more fit to produce putrid diseases than in other years, that person, I say, would assert (what would be considered) a paradox. But, at the same time, whatever truth he might utter, and whatever service he might render in the calamitous conjuncture, it were well for him to be at a distance. He, who would hold this opinion, might find abundance of arguments, which could not be refuted, in all the authors who have written on the plague, even in those who have defended contagion; unless, indeed, the love of the marvellous should make him despise or overlook the most simple causes, which he might find at his very feet."

All the medical men who accompanied the French expedition to Egypt, Assalini alone excepted, were of the opinion that the plague is propagated by contact with the infected. For nearly forty years after their return, this opinion has been universally received, and acted upon. It was not till 1835, that a change of sentiment began to be manifested among medical men on this most important subject In the course of that year, as we have already seen, a number of European physicians had an opportunity of studying the terrible pestilential epidemic that ravaged Egypt. Impressed at first most firmly with the belief of the transmissibility of the disease by contact with the sick, they have all, with scarcely one exception, completely changed their opinion; as, indeed, M. M. Brayer and Cholet, who had observed the epidemics of 1819, 1826, and 1834 at Constantinople, had previously done. The writings of these last-named gentlemen, and subsequently of Clot-Bey and Aubert-Roche, have mainly contributed to effect this very remarkable revolution in medical doctrine. We shall briefly note a few of the most interesting facts, which have been of late years made public.

During the pestilence of 1824, upwards of 30,000 persons died in Cairo, while not more than two or three cases occurred in Alexandria, although the communication between these two cities was constant and uninterrupted. In 1834, on the other hand, the plague broke out and continued in Alexandria for a very considerable time, before it made its appearance at Cairo; and it had existed for fully eight months in the former city, before there was any sign of it in Mansoura and Damietta, although the daily intercourse between these places remained entirely free. Dr. Coch, principal physician of the Egyptian fleet, mentions an interesting fact observed by him in 1835. Ten men had gone from Sakkarah, a populous village, to Cairo, where the plague then existed.—On their return home, every one of these men sickened, and died; yet not a single member of their families, who had assiduously waited upon them, took the disease. "Such a fact," it is emphatically added, "was observed hundreds of times during the course of this great epidemic." The same gentleman states that, the Viceroy having ordered that all vessels in which the plague appeared should be subjected to a quarantine of 11 days, the sick were immediately disembarked and carried on shore by the sailors of the fleet; and, although these sailors returned on board and communicated freely with the rest of the crews, not a single case of infection was the result.

We owe the following facts to Dr. Roche. The ports of Suez and Cosseir on the Red Sea draw the chief supply of their provisions, the one from Cairo, the other from Keneh in Upper Egypt. In 1835, the plague broke out at the latter place about the same time that it made its appearance at Cairo. Suez was attacked by the pestilence ; but Cosseir remained quite exempt. The first of these places is surrounded by stagnant marshes, a state of things not unlike to what exists in all the towns of the Delta ; the second is built upon rocks, and is surrounded by bare arid hills. During this epidemic, Djedda, Yambo, and Moka enjoyed the same immunity as Cosseir, although the sick from Suez and other infected localities often died in the midst of them. Still more convincing is the following statement :—

Every year pilgrims depart from all parts of the country, subject to the laws of Mahomet, to go to Mecca. Caravans from Morocco, Darfour, Egypt, Constantinople, Persia, Asia Minor, and Syria converge at Djedda, at Medina, then at Mecca, the central point. They carry merchandise with them, for this pilgrimage is also a fair. Has the plague ever broken out at the place of meeting of all this population and all this merchandise, which have often, be it remembered, come from places infected by it? No. On the contrary, it is proved that, from time immemorial, the plague has never been seen in Arabia. The epidemic plagues, which desolated a great part of Lower Egypt in 1825 and 1835, had not one victim in Arabia, notwithstanding the daily and perfectly free communication which existed between these countries. This has also invariably been the case with respect to the pestilential epidemics of Constantinople, Smyrna, or Syria. The Arabian historians pretend that their country owes this immunity to the protection of the Prophet !

Nubia, Sennaar, and Abyssinia, notwithstanding their close connexion with Egypt, are not acquainted with the plague. If it may be said regarding Arabia, Sennaar, and Nubia, that the heat in these places prevents the condensation of pestilential miasms ; the same reason cannot be alleged for Abyssinia, which is a temperate country, the thermometer varying from 16 to 25 degrees Cent. above Zero. Here the salubrity of the climate alone serves to keep the disease at bay. Abyssinia is a mountainous country with inclined plains, where there are neither marshes nor stagnant waters.*

For the extract which follows we are indebted to the work of Clot-Bey ; allusion has been made to it in a former page.

"During the five months that the epidemic of 1835 lasted, MM. Gaëtani, Lachèze, Bulard, and myself at Cairo, MM. Duvigneau, Scisson, Perron, Fischer, at Abouz-Abel, and MM. Rigaud and Aubert, at Alexandria, visited the infected in the hospitals and in private houses. None of us took the least prophylactic precaution. We were in immediate contact with the sick during all the stages of the disease. We received upon our clothes and upon our hands the matter that was rejected by vomiting, the blood of those who were bled, the pus from the thousands of bubos which we opened. More than a hundred dissections were made at Cairo, and we passed whole hours in endeavoring to detect, in the bodies of those who had just expired, the pathological alterations which had hitherto been so little attended to. The same researches were made with equal care at Alexandria.

"Dr. Rigaud is the only one among us who fell a victim to the reigning epidemic.

"It is remarkable that many physicians, who scrupulously avoided all contact with the sick and with suspected objects, were attacked with the plague and died. Of this number are Dr. Mannucchi, sen., Leopold and Lardoni."

The observations made by different medical men, during the subsequent epidemics of 1837 in Syria and of 1841 in Egypt, amply confirm these statements of Dr. Roche and Clot-Bey. M. Granet was the chief medical officer of the troops stationed in the province of Adana (Upper Syria,) when the plague

* Aubert-Roche, *ouvrage cité*, p. 100.

broke out there in 1837. He was entrusted with all the sanitary regulations ordered by the governor. At first, it was attempted to arrest the extension of the pestilence by establishing a cordon around the infected spot. This was speedily found to be wholly useless. Upwards of 15 new cases were received every day into the military hospital, in which there were usually from 40 to 60 cases at a time. No precaution to guard against the risk of contagion was employed either by the medical men or by the other attendants of the sick; and yet not a single case of the disease occurred among them, although the epidemic lasted for three months. "How can we believe," says M. Granet after relating these particulars, "that, if the plague was really transmissible by contact with the sick, we should not have had a single case of this transmission?"

The evidence of Dr. Ibrahim, a native physician of Cairo, is highly satisfactory and convincing. He adduces many cases where one member only of a large household was affected with the plague, although the patient had been waited upon by the whole body of the domestics. The case of the wife of Hassan-Pacha, who died on the 35th day of the disease, is more than usually instructive; she had no fewer than two dozen white and black slaves, two keios, two eunuchs, and four pages, in constant attendance upon her!

Dr. Delong also, and M. Euzieres report many cases that occurred under their own immediate notice in Cairo, during the epidemic of 1841, in which the relatives of the sick, who had most assiduously nursed them up to the hour of death, entirely escaped. It seems unnecessary to give the details of any of them. In two cases, one of which proved fatal on the fifth day of the attack, the patients continued to suckle their infants; the children were not affected. In several instances, the disease attacked those who had sequestrated themselves by the most strict quarantine from all communication with the city; while others of their household, who were less timid, remained intact.

The testimony of Dr. Arnoux, surgeon-major of the 43d regiment (Egyptian army) stationed in 1841 at Nabaro, of Dr. Dieterich of the 5th at Damietta, of Dr. Penay of the 5th cavalry at Neguille, and of Dr. Chedufau, chief physician of the central military hospital at Cairo during the same year, all serves to prove that the immediate attendants upon plague-patients are, on the whole, very rarely infected;—always provided due regard be paid to the ordinary hygienic precautions. The last-named gentleman informs us that, in consequence of there being no separate plague-hospital prepared when the pestilence broke out, the sick were received at first into the general hospital along with the other patients. There were no fewer than 182 cases of plague treated in this hospital; and, although the number of the ordinary inmates and attendants during this period amounted to 2,000, no instance of infection could be directly made out. None of the "officiers de santé," consisting of 92 Europeans and 300 Arabs, remained in quarantine, but waited upon the sick without taking any precautions. Only three of the Europeans were attacked; and, of these, two recovered. Three also of the Arabs suffered; they all died. M. Chedufau himself, besides his hospital duties, treated many cases of plague in the town, opening bubos, dressing carbuncles, and executing all the necessary medical duties to his patients. He performed, moreover, 17 *post-mortem* examinations. During the whole of the epidemic, he was in continual intercourse with the members of his own family; and yet, although no preservative means were employed, neither he nor any of his household suffered.

The experience of Dr. Perron, the director of the medical school at Cairo, confirms in every respect the truth of these statements. Not one of the professors or pupils, who were in daily attendance upon the sick, was infected.*

After relating numerous other facts and statements, all bearing upon the

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* We shall afterwards however find it stated that most of the *pharmaciens* in one of the hospitals at Alexandria caught the disease and died, and also that very many of the pupils in the hospitals at Cairo perished.

question proposed at the beginning of this chapter, the conclusion is at length arrived at, that,

"On the one hand, immediate contact with thousands of plague-patients has not been followed by any dangerous consequences to those who have been exposed to it in the open air or in well-ventilated chambers; and on the other, that there is not a single fact which indisputably proves the transmissibility of the plague by mere contact with the sick."

CHAP. III.—*Is the plague transmissible by contact with the clothes or effects of the sick, in localities which are, or have been, recently exposed to the epidemic influence?*

After the plague of 1835 at Cairo, the clothing, effects, &c., of upwards of 50,000 plague-patients, who had been carried off by the pestilence, were sold in the public bazaars, without communicating, as far as is known, the disease in a single instance. More than 600 houses remained tenantless for several months after this frightful epidemic; they were then ordered to be visited, and an inventory taken of their contents. Not one of the persons engaged in this service fell sick.

Three thousand plague-patients were received into the large hospital of Esbequié during the year 1835. On the cessation of the pestilence, the ordinary class of patients was admitted and put into the very same beds, which had been occupied by those who had died of the plague. The sheets indeed were changed, but the coverlets, which had never been subjected to any process of disinfection nor even freely exposed to the air, were used without alarm. No case of infection ensued. These facts were made known in 1840 by Clot-Bey, and have not since been disputed by any one.

Dr. Brayer informs us that it is a fact perfectly well known in Constantinople, that the Jews buy up the clothes, &c., of persons who have died of the plague, however virulent this may have been, for sale at Fit-Bazar, where most of their stores are. No one dreams of using any means of disinfection. If the deaths be numerous, the bazar is full; and *vice versa*. All the poorer classes resort thither for their clothing; and, generally, bundle after bundle is turned over and examined, before a purchase is made. In 1812, the "depouilles" of 150,000 victims of the dreadful pestilence of that year were brought together into that market! One portion was speedily bought by the inhabitants of the city; another portion was forthwith sent away into all the principal towns throughout the Turkish dominions; while what remained unsold was kept in close confined magazines, to be disposed of next year. Notwithstanding this dispersion of infected *fomites* to such a vast extent, nothing was heard of the spread of the pestilence thereby. It deserves also to be noticed that a smaller proportion of the Jews died than the Greeks, who, we may remark, have always had great fear of the contagion of the plague.

It would be easy to mention many other instances were the clothing, goods, equipage, &c., of persons affected with the plague, have been taken possession of by others, without any injurious results. But this is unnecessary; suffice it to say that, in Egypt, Constantinople, Smyrna, &c., an epidemic of the plague is almost invariably found to subside and cease at a certain period known beforehand, whether any sanitary regulations have been taken to arrest its course or not; and that then the clothes and other property of the victims freely circulate in the bazaars of the place. If these objects communicated the disease to those who handled them, it is quite obvious that it would last much beyond the period at which it disappears with a truly remarkable regularity.

Such are some of the facts that have led most of the medical men resident in the East to believe, that the plague is not transmissible through the medium of *fomites*. Even although we do not go so far as this, it may surely with perfect truth be asserted, that in a vast number of instances, some of them indeed of almost continual recurrence, the pestilence has not been communicated by touching, or even wearing the apparel of those who have died from it, although no means of purification or disinfection had been employed. We are not prepared utterly to deny that the disease has been, and may be, sometimes transmitted in this way;* but surely, after the facts which we have mentioned, we may very fairly withhold our credence from many of those histories on record of the plague having arisen in places previously quite healthy, from the introduction of infected objects. Most of the reported facts of this description appear to have been dictated by prejudice and accepted with credulity. Nevertheless, it must be admitted that some medical men, who have had extensive opportunities of observation, still maintain the opposite opinion. Of these M. Grassi, principal physician of the lazaretto at Alexandria, and who has been in the Egyptian service for upwards of 20 years, is the person whose opinion is entitled to perhaps most consideration. We may state, however, that the accuracy of many of the facts, which he has adduced in favour of his views, has been disputed or denied by Clot-Bey and others.

The general *conclusion*, to which the Commissioners have come, after a laborious investigation of all the particulars, is that

"Very numerous facts prove that the clothes and effects, belonging to plague patients, have not communicated the disease to persons who have used them, even without any previous purification. The facts, which seem to indicate an opposite result, can only be considered valuable, if they are confirmed by fresh observations made beyond epidemic *foci*, at a distance alike from *foci* of miasmatic infection and from countries where the plague is endemic."

CHAP. IV.—*Is the plague transmissible, in countries where it is endemic or epidemic, by merchandize suspected to contain pestiferous matters?*

It will be obvious that it must be very difficult to decide this point in places where the pestilence is indigenous, and where it is, therefore, liable to be developed spontaneously at any time; for what may be attributed by certain persons to handling some description of goods, may be altogether owing to the influence of an endemic cause. The case related by M. Sicard of Marseilles to M. Prus is unsatisfactory in several points of view, and nearly the same thing may be said of all the analogous examples which have been made public. It may, therefore. be very fairly set down that the transmissibility of the plague, under the circumstances mentioned, has at all events not been proved.

CHAP. V.—*Has the plague been observed, in epidemic foci, to be transmitted by the air being charged with pestiferous miasms?*—in other words, is the plague infectious through the medium of the atmosphere during the prevalence of an epidemic pestilence? Until of late years, the question of atmospheric *infection*

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* Clot-Bey himself reports the following narrative:—

"On the 15th of April, 1835, (in presence of MM. Gaetani, Clot, Lacheze, and Bulard) two young criminals, Ibrahim-Assan and Ben-Ali, in perfect health at the time, were placed in beds which had just been left by patients affected with well-marked plague.

"In the night of the 18th, Ibrahim's pulse was slightly affected.

"The following day, he had the plague with bubos and carbuncles; he died on the 23d.

"On the third day after he was in bed, Ben-Ali also felt the ordinary symptoms of an attack of the plague; but the disease abated, and convalescence began from the fourth day after the appearance of the characteristic symptoms.

"That Ibrahim-Assan died of the plague, after having slept in a bed recently left by an infected person, is a fact. But was it the sheets or other coverings of the bed which gave the disease? This is uncertain. Ibrahim-Assan was in a town where a pestilential epidemic raged; he was in a hospital which had contained and did still contain a great number of plague-patients, and in which several medical pupils and attendants had contracted the disease. It cannot then, in his case, be absolutely asserted that the plague developed itself by contact with contaminated objects, rather than by epidemic influence alone, or by miasmatic infection."

had been altogether superseded by and merged in that of direct and immediate *contagion*. We need scarcely say that it can never be an easy thing to determine with exactitude the infectiousness of any disease, while a pestilential constitution of the atmosphere exists, and when consequently a whole population is exposed to the morbific influence. As we have previously remarked, scarcely a single person escapes *in toto* the effects of the malarious condition of the ai; they are experienced by all to a certain degree during the prevalence of the epidemy.

We shall first enquire if the air of a plague-hospital has seemed to give the disease to those, who had most carefully avoided all contact with infected persons or objects. That great mortality occurred among the *pharmaciens* and attendants in the plague-hospitals at Cairo and Alexandria during the pestilence of 1835, notwithstanding the use of all precautionary means to avoid direct contact with the sick, or of any thing belonging to them, cannot be disputed: of 20 pupils, that were sent from Abouzabel to the hospitals of Cairo, 19 caught the disease and died. The question then comes to be, was this mortality owing to the general epidemic influence, to local atmospheric infection, or to immediate contact with the patients? The best answer to this will probably be found by referring to what took place at Abouzabel itself—only a few miles distant, it will be remembered, from Cairo—when that place was attacked by the pestilence. The barracks of the sick were situated in the open country, at some elevation above the plain, and well ventilated. Not one of the medical inmates or attendants, although in most frequent contact with infected persons and objects, suffered; and this too, be it remembered, in an epidemic focus.—The free ventilation had prevented the formation of "a focus of infection." Dr. Laidlaw, the physician of the general European hospital at Alexandria, remarks most truly that "whenever a number of plague-patients are collected together in one space, they seem to create a pestilential atmosphere, unless free ventilation be employed. Hence the importance of never having many plague patients in one room or ward, and of maintaining a most thorough ventilation through every part of the building, which should always, if possible, be situated in an elevated open position and on a dry foundation, at a distance from stagnant water or any source of malarious exhalation. If these circumstances be not attended to, hospitals are apt to become positive foci of infection, and thus to increase rather than to diminish the general mortality.

That the disease, when it has attacked one of the inmates of a house, is, under certain circumstances, apt to extend to others who are dwelling there, cannot be disputed. Dr. Grassi tells us that, in 1835, no fewer than 57 deaths occurred in the house of Hingi Osman, the treasurer of the marine at Alexandria. This is only one out of many similar instances that might be quoted. The fair conclusion therefore seems to be, that pestilential miasms or effluvia emanate from the bodies of the sick, and that these powerfully tend to act upon the system of persons whom the epidemic influence has already made liable to the disease. Almost all the resident medical men in Egypt at the present time, who believe in the transmissibility of the plague, are of opinion that the transmission is effected in this way. Dr. Grassi is the only one who maintains that the disease is communicated by direct or indirect contact with the sick, without any intervention of atmospheric agency.

Remaining long in the chamber of the infected is the thing most to be avoided by the medical and other attendants. What Dr. Rigaud said to his friend M. Lesseps, the French Consul-general, who visited him constantly to the last moment of his life—"Do come and see me twenty times a day, but don't remain more than five minutes at a time in my room"—abundantly shows what his sentiments were upon this point.

By a singular perversion, the common practice in the East is to keep a patient in a close and confined apartment, with as imperfect current of air through it as possible. What wonder then that a disease like plague spreads, wherever it makes its appearance: every sick chamber becomes a new focus of infection! There cannot, therefore, be a reasonable doubt that the diffusion and mortality of the pestilence are powerfully promoted by the contamination of the atmosphere with morbific emanations from the bodies of the infected.

Sometimes it happens that when a locality, which has contained a number of plague-patients, comes to be occupied by other persons (who may use nevertheless every possible precaution to to avoid all contact with suspected objects,) the disease re-appears to a greater extent among the new residents than can fairly be attributed to the sole agency of the epidemic influence which may be then existing.

In 1834 in the month of June, during the insurrection which broke out in Judea, the insurgents pillaged and sacked Jerusalem. A number of Roman Catholics took refuge in the convent of St. Saviour in this city.

"At the end of ten or twelve days of close confinement, I remarked," says M. Delong, "cases of plague among this distressed population, huddled together in their dormitories, upon and under the stairs, in the courts and other chambers of this vast building. After twenty-five days of expectation, Ibrahim Pacha at length arrived, and the city was relieved. The holy Fathers, full of alarm, hastened to clear their dwelling of all this mass of people, and shut themselves up in most strict quarantine. What happened? Of all those who left the convent, three only died four or five days afterwards. But, out of 63 priests who thought to save themselves by isolation, no fewer than 22 died."

"Now," continues M. Delong, "what part did infection play in this instance? I will not positively decide. It seems however clear to me, that we should, in all similar circumstances, disperse the infected into different quarters, instead of shutting them up with others, as yet intact, in a confined locality where sanitory regulations have not always been attended to."

What occurred in the musical academy at Kanke in 1835 is still more deserving of attention:—

The plague havlng broken out in this school, although it was kept in the strictest quarantine, the pupils were sent into the desert, where they continued for upwards of a month. In the mean time, all the rooms were well cleansed and purified; and no person had remained in the building. Not one case of plague occurred in the desert: but no sooner had the boys returned to their old quarters, than several were taken ill: and each day several fresh cases were reported. Again were the boys sent into the desert; and again the disease ceased to spread. While they continued in the desert, 15 soldiers were employed to go daily to the village, where the plague was raging, for provisions; but none of these men caught the disease themselves, or gave it to the boys.

During this disastrous year, many striking circumstances similar to those now mentioned occurred in the military barracks, all of which had been put into strict quarantine. Although every attention was paid to cleanliness, they seemed to remain foci of infection whenever the disease had once shewn itself in them.

It will be obvious that the facts now mentioned, coupled with similar ones observed in epidemic foci and more especially on board-ship, must receive new and very useful applications in considering the question of Quarantine.

Admitting that morbific miasms are given off from the bodies of plague-patients, and thus render the surrounding atmosphere pestiferous, we have next to enquire whether these miasms are exhaled from the lungs, or from the surface of the body of the sick, or from both; and also when they prove infectious, whether they are (most probably) absorbed by the skin, the lungs, or in the way of deglutition. There can be very little doubt in the present day that very many cases, where the disease has appeared to be the result of direct contact with a patient or with any thing he has used, may fairly be regarded as examples of infection through the medium of the atmosphere. Dr. Brayer has put this point very forcibly:

"A person finds himself affected with plague, although he has never left the house; straightway he tries to bring the mind where he was the night before, two or three days ago, the objects he touched, and so forth. If he has not been out of doors for one or two weeks, owing to indisposition, it matters little; he was out three weeks, a month ago; and as the virus may be retained for months, nay, for years, it is not surprising that the disease should have shown itself. He will not for a moment consider that the skin is guarded by the double covering of the epidermis and of the clothes which he wears, and that therefore the virus can only with the greatest difficulty be transmitted from the exterior to the interior. As to pulmonary absorption, that is never spoken of. It is a received opinion among the Franks that the air is not, never has been, and never can be the vehicle of the plague. They refuse to believe that in seasons of the plague, every individual breathes an air more or less deleterious, that the person who sees or touches an invalid is in the atmosphere of the invalid, and that when, by means of the air, a deleterious principle is conveyed into the minutest bronchial ramifications, and by the act of deglutition into the gastric passages, there is then much more than mere contact; for there is a real penetration in the first case; and, in the second, there is digestion and interior absorption. Infection therefore exists, in the strongest sense of the term; and the disease which it occasions is more or less severe, according to the quantity of miasmata introduced into the system, their intensity, and the individual predispositions or susceptibilities of those who have absorbed them."

The Academic Society of Marseilles, in the report which was unanimously adopted in August 1845, expressly recognises the following two propositions:—

"1. Writers, the most at variance in all that concerns the general history of the plague, are nearly unanimous in asserting that the simple contact of one individual with another is one of the modes of transmission the least favorable to the propagation of the pestilence.

"2. A lenghtened stay in the atmosphere of the sick, and, above all, exposure to the pestilential miasms which contaminated objects exhale are highly dangerous."

The only difference, says M. Prus, which, upon this truly important point of doctrine, exists between the Academical Society of Marseilles and the Commissioners, may be thus summed up:—

The Academic Society asserts that the simple contact of individuals is one of the modes of transmission least favorable to the propagation of the plague.

The Commissioners think that no well-authenticated fact establishes the reality of such transmission They are moreover not acquainted with any facts to authorise them to believe, with the Academical Society, in the dangers of miasms from contaminated objects.

The Academical Society and the Commissioners equally acknowledge that a lengthened stay in the atmosphere of infected persons, or, in other words, infection by pestilential miasms is that which is most to be feared.

Dr. Mead who, at the time of the plague at Marseilles in 1720, was ordered by the English government to draw up instructions for preventing, if possible, the introduction of the pestilence in England, and to arrest it if it was introduced, has much insisted upon the utility and necessity of promptly removing the sick from the seat of the infection, and transporting them to some distance.*

He has mentioned as worthy of imitation the course pursued at Rome, during the plague of 1657, by Cardinal Gastaldy, at that time invested with full power to take every sanitory measure which he judged proper.

The Cardinal prohibited any infected person, and even any person in health who was suspected, to remain in their houses. They were promptly taken to the hospital, built on the island which divides the Tiber. With respect to those who had occupied the same house, they were placed in other hospitals near the city, from whence they were removed into the island if the disease showed itself. During this time, the Cardinal was very careful to have all the furni-

* R. Mead, M. D. *A short Discourse concerning Pestilential Contagion.*—London, 1720; and *De Peste Liber*, 1723.

ture taken out of the infected houses, exposed in the open air, and the apartments left open, in order to purify them.

By these means the Cardinal, in two months, caused the plague to cease, after it had raged at Rome for two years.

But that which deserves most attention, adds Dr. Mead, is that, before these regulations, it was constantly observed that the disease rarely appeared in a house without attacking all its inhabitants; whereas, after they had been put in force, scarcely five out of a hundred of those who were removed from the proximity of the infected, were subsequently attacked with plague.*

Muratori informs us that similar measures had been adopted with equal success at Ferrara, in 1630.†

The Board of Health at Constantinople has, for the last eight or nine years, followed out the prophylactic method recommended by Gastaldy and Mead, removing the infected to a hospital, and emptying every house, in which a case occurs, alike of its inhabitants and furniture, having it well cleansed and purified, and not allowing any one to occupy it for the space of a month. It is to the adoption of these means that the board attributes the exemption of Constantinople and the principal ports in Turkey from the plague, since the year 1839. If, in place of acting in this manner, the houses of the infected are condemned with their inmates to a severe quarantine, the result will necessarily be to create fresh foci of pestilential infection, and thus increase the very evil that is vainly sought to be extinguished.

M. Scisson, principal physician to the hospital at Cairo and formerly professor of the School of Medicine at Abouzabel, observes that if, at the time of the appearance of the plague at Cairo, in 1835, after the arrival of the Maltese Giglio, who died of that disease which he had brought from Alexandria, they had dispersed the other members of his family in the country, it would probably have prevented the death of eight or ten persons who, kept by military force within the house, contracted the plague and died. Two persons fled from the focus of infection, of which the quarantine was the cause; they both remained free.

The case of Giglio, adds M. Scisson, does not prove, as it has been said, the contagion of the plague; it only proves the danger of shutting up, in a narrow space, individuals who have been in connection with an infected person. It is therefore of the first importance to abolish what are called special quarantines for houses, in which persons have been seized with plague. On the contrary, they should be at once emptied, aired, and purified, to prevent every focus of infection.‡

Dr. Mead has quoted from Gassendi a passage wherein this author attributes the frightful mortality of the pestilence that prevailed at Digne in Provence, in 1629, to the severe measures that were taken to prevent the inhabitants from leaving the town and retiring into the neighbouring country.

In 1720 the inhabitants of Marseilles were prohibited, under pain of death, from leaving that city or its suburbs. Hence, doubtless the terrible devastation that ensued.

" And yet, " says M. Prus, with an emphatic force of argument that cannot fail to make a deep impression on the public mind, "what would be done in the present day according to existing regulations, if the plague made its appearance in any town of France? It would be isolated by a cordon of troops, for the purpose of preventing any of the inhabitants leaving it; in other words, the unfortunate town would be condemned to retain, and in a concentrated form too,

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* Gastaldy, *Tractatus de avertendâ et prostigandâ* (?) *peste.* Bologna, 1684, folio.

† Muratori, *Governo della peste et delle maniere di guardasena.* Modena, 1714, 8vo.

‡ M. Scisson. *Lettre addressée au consul general d'Angleterre à Alexandrie en* 1839.

within its bosom all the various causes which serve to develope foci of pestilential infection. Is it then impossible to reconcile the advantages of the public health with the most common laws of humanity? We think not; on the contrary, we are firmly convinced that perfect security may be given to neighbouring towns and to the whole kingdom, by taking the necessary measures to remove the great majority of the inhabitants of the town, in which the plague should appear, from the danger. To obtain results so desirable, it needs only to know how to profit by all that time and experience have taught us of the epidemicity of the disease and of pestilential infection."

Conclusion.—"In epidemic foci, the plague is transmissible by the miasms which emanate from the bodies of the infected, and by the foci of infection thereby produced."

CHAP. VI.—*Is the plague transmissible beyond, or away from, epidemic foci?*

This, it will be perceived, is the most important of all the questions respecting the history of the plague, in reference at least to the subject of Quarantines; for, upon its solution, the propriety of making any change in existing regulations must depend.

A considerable number of medical men, who have studied the epidemics of 1835 and 1841 in Egypt, will answer it in the decided negative, and for the following reasons:

When the epidemic constitution ceases, all or nearly all the sick recover, and no new attacks occur.

Numerous plague-patients have been, and still are accumulated in the hospitals and houses; all the conditions favorable to the transmission of the plague by mediate or immediate contact, or to its propagation by miasmatic infection, continue to exist together; and yet, at a period that is almost known and determinate, the epidemic becomes extinguished, and with it the plague ceases entirely.

An infected person coming from an epidemic focus is now not more to be feared than a sporadic plague patient, who, by the consent of all the medical men in Egypt, never occasions any risk.

If it is doubtful whether the clothes and goods of infected persons can transmit the plague in the time of an epidemic, it is certain that, when once this has disappeared, such clothes and goods may be used with impunity.

An epidemic only appears in a country in the train of certain local and atmospheric influences, whose action has been prolonged for a greater or lesser period of time; very commonly too, privations, fatigues, physical or moral troubles have been experienced, in different degrees, by the inhabitants. From these united causes result more or less general predispositions to contract the prevailing diseases. Now, when a vessel carries one or more infected persons beyond the focus of infection, she cannot take along with them all the causes, past and present, which are necessary to the development of an epidemic.

It must be confessed that the observations, made in Egypt during the years 1835 and 1841, seem to justify these propositions and the conclusions which flow from them.

Let us now see whether the facts observed on board vessels at sea, and in the Lazarettos of Europe, are in accordance with these conclusions.

Since the year 1720 down to the present period, 25 vessels having the plague on board, have arrived in the ports of France or Italy; 10 at Marseilles, 5 at Venice, 8 at Leghorn, and one at Genoa. We shall confine our remarks to the circumstances connected with the arrivals at Marseilles, the official documentary evidence upon these being much more complete than in the other cases. The years in which these arrivals occurred in 1741, 1760, 1784, 1785, 1786 (*bis*,) 1796, 1819, 1825, and 1837. The entire number of cases of plague (omitting all the doubtful ones,) treated in the lazaretto of this port since 1720, is 32; and of these, 18 have proved fatal. Three of the quarantine surgeons caught the disease during their attendance on the infected; they all recovered. A fourth surgeon, who had arrived on board an infected ship, and subsequently acted in his professional capacity in the lazaretto, died. Four of the health-guards, who had been (most improperly) put on board infected ships, contracted the disease in the lazaretto; two died. A sailor, who acted as assistant in the lazaretto infirmary, was taken ill and died. Two other sailors, belonging to an infected vessel, but who seemed to have caught the disease in the lazaretto where they had been confined for more than 12 days, died.

In the 11 cases therefore of plague, which might have been contracted in the lazaretto, 6 of the patients recovered, and 5 died; all the latter cases occurred in men who had been on board infected vessels. Of the three health-guards who had caught the disease on board, only one recovered. Indeed it would seem that, in all the fatal cases, the patients had been for a longer or shorter period of time on board infected vessels.

It appears, also, that not one of the cases, which occurred on board a vessel at sea during the voyage to France, recovered;—a circumstance that very emphatically shews the malignancy of the disease when it occurs in a crowded confined space, and the great advantages of treating it in a large open lazaretto.

From the facts now alluded to, we are surely justified in maintaining not only that the plague may be transmitted on board-ship among individuals coming from the same infected focus, and living in the same hygienic conditions; but also that a plague-patient, received into a lazaretto in another and distant country, may become the cause of infection to others. The *conclusion*, therefore, of the Commissioners is this:

"It is indisputable that the plague is transmissible beyond or away from epidemic foci, whether on board vessels at sea, or in the lazarettos of Europe."

The question proposed in Chap. VII. is to this effect: *Is the plague transmissible, away from epidemic foci, by immediate contact with the infected?* It is at once answered in the negative; there is not a single authentic case on record to prove that the disease has ever been propagated in the way mentioned. The same thing may be said in answer to the question in the following Chapter, viz: *Is the plague transmissible, away from epidemic foci, by the clothes or other effects which have been used by the infected?* Instances indeed have been related, by some writers, of passengers appearing to be taken suddenly ill almost immediately after opening their bags or boxes, and handling their contents; but not one of the narratives of this sort is satisfactory in its details or conclusive in its evidence. Nevertheless the Commissioners, unwilling to commit themselves unqualifiedly on the point under consideration, suggest that new experiments and observations should be made with all possible precautions, at a distance from every focus of infection, and in a locality where the plague is not endemic.

CHAP. IX.—*Can articles of merchandize transport the plague beyond or away from epidemic foci?*

The evidence and facts, on which the advocates (few in number as they are in the present day) of the affirmative side of this question chiefly rest their opinion, are the following:

The plague of London in 1665 is said by Hodges to have been imported from Holland into England in bales of cotton. But it may be fairly objected to this opinion, that the registers of that city clearly shew that the disease had been endemic within its walls for some years preceding the outbreak of the great pestilence, and the destructive conflagration that occurred immediately afterwards. Since that period, the plague has never reappeared in the English metropolis.

The testimony, that has been adduced to shew that the plague of Toulon in 1721 was owing to the reception by some of the inhabitants of some suspected silk that was stolen out of quarantine, is equally unsatisfactory. These and such-like statements cannot be fairly admitted, in the present day, to be entitled to much weight in the determination of the important question under inquiry. We are not sufficiently acquainted with the accompanying circumstances of either case to warrant us in laying much stress upon them. Does the following fact,—the details of which may be relied on as in every respect authentic and perfectly accurate—communicated by Dr. Laidlaw to the English Consul at Cairo, enable us to form any thing like a decided opinion?

In 1835, the epidemic plague raged at Alexandria among all the servants and employés living in the magazines of the Egyptian government. Notwithstanding this, a vast number of bales of cotton, daily handled by the prisoners, were exported from January to June—that is to say during the whole continuance of the epidemic—to all the great ports of Europe.

There were exported this year,

To England - - - - -	31,709 bales.
To Marseilles - - - - -	33,812 "
To Leghorn - - - - -	424 "
To Holland - - - - -	150 "
To Trieste - - - - -	32,263 "
To other ports - - - - -	32 "

Now, although no precautionary means were taken in the way of disinfecting this immense quantity of an article that has always been deemed highly susceptible of retaining the infectious effluvia, not one person seems to have been infected in consequence.

Of sixteen English vessels laden with cotton, which sailed from Alexandria from the beginning of January to the end of June, eight had the plague on board; and yet their cargoes did not prove more dangerous than those of the non-infected vessels.

Besides this very conclusive evidence, the Commissioners mention upon official authority that, since the year 1720, not one of the porters employed at the lazaretto of Marseilles in discharging and landing the cargoes of suspected ships has ever caught the plague.

The *conclusion* is therefore fairly forced upon us that

"There is nothing to prove that articles of merchandise can transport the disease beyond epidemic foci."

The division of objects of merchandize in the French lazarettos into three classes, according as they are (believed to be) susceptible, doubtfully susceptible, and non-susceptible of infection, is the most arbitary and ridiculous thing imaginable; nor is it easy even to form a conjecture what possibly could have led any set of reasonable men to adopt it. Tallow and wax, for example, are declared to be non-susceptible objects; but when made into candles (from the wicks, we suppose,) they are susceptible! Pieces of old copper and other metals are conductors of the pestiferous poison; wood and other porous substances are not! Truly, as M. Prus remarks, the classification can only be regarded "as the result of most imperfect observation and of antiquated traditions prompted by fear and prejudice." It is utterly discreditable for any enlightened government to retain and act upon it.

The great point is to determine whether the clothing and baggage of plague-patients are capable of communicating the disease in our ports. If the decision upon this subject be in the affirmative, then certainly it will be right to ascertain what other objects possess the same property; but should it be in the negative, it is scarcely necessary to say that the entire catalogue of interdicted articles must be swept away.

It is quite unnecessary, as a matter of course, to say a single word respecting the comparative value of different modes of disinfection that have been proposed at different times. Most of the substances used in fumigation are utterly worthless; some of them are dangerous, and therefore inexpedient. Chlorine and its preparations are unquestionably the safest and best.

Conclusion.—"The study of the means best fitted to disinfect baggage, clothes, and articles of merchandise, remain still to be made.

"To be rational, before researches on this subject are undertaken, it should be proved that these different objects are really capable of becoming charged with the principle of the plague."

Chap. X.—*Can the plague be transmitted by pestilential miasms, beyond or away from epidemic foci?*

When, on board-ship or in a lazaretto, the plague is communicated from an infected person to one in health, it is obvious that it must be always difficult, and often impossible, to say positively that the disease has been owing to direct contact with the sick; for, whoever has been so near a patient as to have touched his body, must have inspired the pestilential atmosphere that is around him. How, then, shall we decide whether the infection has taken place by the skin or by the lungs? The task is indeed not easy, if indeed it be possible. But if we consider, on the one hand, that numerous observations have clearly shewn that immediate contact with plague patients have not given the disease, and, on the other hand, that residence in a focus of pestilential infection, but without any contact with the sick, has imparted it, we are surely almost compelled to admit the following *conclusions*, viz.

" 1. The transmission of the plague by pestilential miasms is a proved fact.

" 2. The transmission of the disease by immediate contact with the infected is not a proved fact."

It has been already seen, from the experience of the Marseilles lazaretto, that, whenever there was plague existing in a ship, the stay of the vessel in the port became dangerous not only for the crew and passengers, but also for the health-officers who were sent on board, and who, it will readily be believed. most carefully avoided all contact with the sick or with any suspected object. Too often the vessel became a genuine focus of pestilential infection. The air, loaded with the miasms that always emanate from the bodies of the sick, acts as a poison to all who inhale it. This infected state of atmosphere on board a vessel may continue for some time, after every plague-patient has been removed from on board. We are therefore fairly warranted in concluding that "the plague is transmissible by infection (in other words, by the armosphere being charged with pestilential miasms from the bodies of the sick) beyond or away from epidemic foci, as we have already seen that it is so in epidemic foci, and in countries where the disease is endemic."

To this general conclusion it may be useful to append the following three which are but as corollaries from it, as upon each of them certain quarantine measures are based.

" 1. It follows from the facts adduced in the preceding chapters relative to the transmissibility of the plague, alike within and at a distance from epidemic foci, that plague-patients, by vitiating the air of places wherein they are confined, may create foci of pestilential infection that are capable of transmitting the disease.

" 2. Foci of pestilential infection may persist in a place after the removal of plague-patients from it.

" 3. Foci of pestilential infection once formed in a vessel, by the presence of one or more plague-patients on board, may be transported to great distances."

Chap. XI.—*Is sporadic plague transmissible either by the infected themselves, or by foci of infection which they may form?*

This question has already received a partial answer; but it will be useful to recur to its consideration in this place. The grounds, upon which almost all the medical men in Egypt have adopted a negative opinion on the matter, are as follow.

After the epidemic of 1835 had ceased, the attention of these gentlemen—divided, be it remembered, in opinion as to the general question of the transmissibility of the plague—was naturally directed to the sporadic cases which occurred in the latter half of that year, and also in 1836, 37, and 38. From June

1835 to the end of December 1838, 649 cases of sporadic plague were observed in Alexandria. Now it is admitted by all that, of these 649 cases, 646 did *not* transmit the disease to any of the persons who had waited upon, and had frequently touched the sick. It is therefore in reference to the three remaining cases only that any difference of opinion has existed ; and with respect to these three cases, we think that no unprejudiced reader will hesitate to say, after carefully perusing the reports, that they are very far from proving that the disease was communicated, in a single instance, from one patient to another.

Here it deserves enquiry whether the cases of plague that have at any time been imported into Europe have, or have not, been brought by vessels which had left the producing countries of the disease while a pestilential *epidemic* was prevailing. The imported cases of the plague have never, we believe, been sufficiently considered in this point of view, although it bears very obviously on the question of the transmissibility of *sporadic* plague. The examination of the histories of the ten importations of the plague into Marseilles since 1720 has led the reporter to believe that, in every one of these instances, the pestilence was raging epidemically in the ports whence the vessels had come. This interesting fact certainly does not prove that sporadic plague is not transmissible ; it only shows that none of the numerous vessels, which have left Egypt, Syria, or Turkey since the year 1720, at a time when these countries suffered only from sporadic plague, has ever imported the pestilence into Marseilles. This consideration, coupled with the remark of intelligent and trustworthy observers in Egypt that the cases of sporadic plague, which have occurred from July 1835 to the beginning of 1839, have in no instance transmitted the disease, deserves the serious attention of all medical men and legislators. M. Brayer also has come to the same opinion respecting the sporadic plague at Constantinople.

Conclusion.—"Patients affected with sporadic plague do not seem to be capable of producing foci of infection sufficiently active to transmit the disease." *

Chap. XII.—*Is the plague more or less transmissible according to the intensity of the epidemic, the different periods of its course, and the organic susceptibilities of the individuals exposed to the action of pestilential miasms!*

There cannot be a doubt for a moment that different epidemics of this as well as of other pestilences, exhibit very different degress of intensity or malignancy, as evidenced by the varying amount of mortality produced in different seasons. Now, a question here arises, whether the risk of infection is at all proportionate to the degree of this malignancy. It will be obvious that it is scarcely possible to determine this point within the range or operation of the general exciting causes of the pestilences, or, in other words, of the epidemic focus. The observations, to be at all satisfactory, must be made on cases occurring beyond, or at a distance form, such a focus ; for there, the miasms exhaled from the bodies of the sick will operate alone. The results of past experience seem unquestionably to indicate that the liability of such cases to propagate themselves is in direct relation with the intensity of the epidemic at its place of origin. The greater this intensity, the more readily the disease is communicable ; and the reverse holds true, in proportion as this intensity diminishes. When the plague ceases to be epidemic, its transmissibility appears to cease altogether.

The period, too, of the epidemic has a decided influence on the force of the transmissibility of the disease. We have already considered this point, and need not dwell upon it here.

What has been said respecting the period of the epidemic in this point of view, is equally true of the period of the disease. Larrey was of opinion that

* In a subsequent part of the Report however, when treating of quarantine restrictions, it is stated that "the non-transmissibility of sporadic plague is not yet sufficiently determined by experience to warrant us in founding a sanitary measure upon it."

the plague was not communicable during its first period or stage. Dr. Lacheze maintains that the disease ceases to be so after the second stage ; that is, after the fourth or fifth day from its invasion. In the case of the disease, as in that of the epidemic, it is the second period which is most dangerous. Moreover, the influence of the organic dispositions or susceptibilities of individuals, in other words of their general state of health and their idiosyncrasies, either in promoting or in counteracting the liability to infection, cannot be disputed by any one. Much unquestionably depends upon the hygienic regime that is followed. Whatever tends to enfeeble or deprave the powers of life, renders the system more liable to infection. Hence excessive fatigues, the want of proper nourishment and the abuse of spirituous liquors on the one hand, and luxurious dissipation and excessive venery on the other, are all powerfully predisposing causes. Larrey observed that any tendency to the scorbutic diathesis rendered the system usually susceptible, and that all such cases proved very rapidly fatal. We have already seen that race, nationality, sex, age, the circumstance of being acclimated or not, &c., have all some influence in aiding, or otherwise, the tendency to be affected by general epidemic causes. The same is the case with respect to the action of pestilential miasms.

Chap. XIII.—*Is there reason to believe that the plague, when imported from the East into any European port, may be communicated to a sufficiently large number of persons to give rise to a pestilential epidemic?*

The medical men of Egypt answer this query in the negative. Their opinion is based on the often observed fact that, when plague-patients are transported to places not subject to the pestilential constitution, they die or recover without transmitting their disease to any one. If the infected, as we have seen, cannot communicate the disease to the inhabitants of certain places in Upper Egypt, how shall we believe that, when transported from Egypt to France, it will possess a power of transmission so strong as to occasion an epidemic ?

Some observers—and Dr. Lacheze is of this number—have indeed remarked that, in certain cases and in certain localities not subjected to a pestilential constitution, the disease has been communicated to a few individuals ; but that these latter in no instance transmitted it to any one, notwithstanding the most free and intimate intercourse. Dr. Lacheze does not admit that the plague can ever, without a pre-existing epidemic influence, attack a sufficiently large number of persons to constitute a public calamity ; and he insists that, wherever it has committed great ravages, there has uniformly been a pestilential constitution prevailing at the time. Sydenham, too, was of this opinion ; for we find it distinctly asserted in his writings that, however frequent the importation of the plague might be into England, the disease would assume an epidemic character only every thirtieth or fortieth year ; because then only would it find the atmospheric conditions and the organic predispositions that are favorable to its development aud propagation.

This doctrine is very generally received in Egpyt in the present day, and its truth has already been recognized by many of the most intellectual physicians in Europe. The legitimate inference from its public recognition would be that vessels arriving trom infected ports, and having the plague on board, might be permitted to land the sick at any place not subject to the epidemic pestilential influence, without any risk to the people of that place, whether the patients recovered or not. But then it must be remembered that, until we are better acquainted than we are yet with what are the conditions of the soil and the atmosphere which, in Europe, may give rise to a pestilential constitution, and what is the meteorological condition in which an imported plague may be liable to become diffused, prudence will authoritatively require that the very same measures should be taken in the ports of France against the possibility of the propagation of the pestilence by the infected, as if there was no room for doubt that they may become the cause and starting-point of a pestilential epidemic.* This caution is more particularly necessary in the case of Marseilles than of any other place in France, in consequence of its many local sources of insalubrity.

This great sea-port presents—in the circumstances of its climate, of its harbour being choked with filth of all sorts and containing an admixture of salt and fresh water, of its large working population, of its being hemmed in on all sides by mountains which prevent the free circulation of the air, and lastly of its proximity to large ponds—the very conditions that are favourable to the development of the plague.† It is a remarkable thing that the two cities of Europe which, after Constantinople, have suffered most from this pestilence are Venice and Marseilles:—Venice, which by its lagunes, its filthy canals, the moist heat of its climate, and by the wretched state of a large portion of its inhabitants, combines most of the causes which engender spontaneous plague, and may therefore, *a fortiori*, promote the diffusion of the disease when imported; and Marseilles, which in this respect approaches far too near to Venice.

Here we should not fail to remark that sufficient attention has not hitherto been paid to the cases, certainly very numerous, where the plague conveyed to a country has become spontaneously extinguished, for very want of being able to transmit itself. It would be very useful to ascertain with precision what are the local causes which appear to resist the transmission and propagation of the disease. This work has only just been commenced in respect of a few places in Upper Egypt.

Conclusion.—"If it has been proved that the existence of a pestilential constitution in a country, into which the plague is imported, is necessary for the transmission and propagation of the disease, it seems nevertheless certain that imported plague will not exercise any great ravages, if it does not meet with, in the character of the climate, atmosphere and population of the place, the conditions that are favourable for its development."

Fourth Part.

What is the ordinary or exceptional term of the incubation of the plague? —in other words, how long may the plague remain hidden, so to speak, in an infected individual, before it manifests itself by more or less distinct symptoms?

This period is believed to vary considerably according to the stage of the prevailing epidemic, and other less influential circumstances. The variations are nevertheless confined with certain limits, which it is important to ascertain; for upon them should depend, in a great measure, the duration of qnarantines.

All observers have remarked that, when a pestilential epidemic commences in a town, the incubation of the disease is often extremely short. We read of attacks of the plague proving fatal within a few hours, nay within a few minutes: these are the cases, which have been truly called "pestes foudroyantes."

In the second period or stage of the epidemic, the usual term of incubation is from three to five days. It is about the same in the third stage.

Upon all these points there is little or no discrepancy of opinion. It is only when we endeavour to determine the longest duration or lapse of time that may be fairly admitted for certain exceptional cases, that we find the statements of different writers to disagree. Some, and these constitute the large majority, maintain that the term of incubation never exceeds eight days; others think that this term may be prolonged to ten days, and sometimes, although very rarely, a few days more. Dr. Grassi, in his reply to certain interrogatories addressed to him in 1839 by the English minister, says upon this subject:

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* This is one of the conclusions of M. Prus, the justice of which has very fairly been impugned.—*Rev.*

† *La topographie medicale de Marseille par M. Ducros, medecin en chef de l'Hôtel Dieu de Marseille*, 1837.

"In the course of several years, some thousands of persons of every age, sex and condition, were condemned to undergo a quarantine of observation of six days for having been exposed to infection. The disease made its appearance in many of them during their isolation, but in no one case beyond the sixth day. This is an observation which I have made with great attention."

On his representation to the Egyptian government, the quarantine, which had before been eleven days in the lazaretto at Alexandria, was reduced in 1842 to seven days.

The experience of other medical men in Egypt has, on the whole, confirmed the truth of M. Grassi's observations.

This gentleman assures us that, among the multitude of people that left Cairo during the epidemic of 1835 and fled into Upper Egypt, which continued healthy, the disease manifested itself in a few persons; but, in no one instance, more than the eight days after their quitting the city.

The observations made, in the course of the same year, by the professors of the medical school at Abouzabel lead to the general conclusion, that the period of incubation never exceeds six days. According to M. Segur, it is never more than eight. If, too, we examine what has occurred in vessels that have had plague on board after leaving an infected port, we shall find that on every occasion the disease has shown itself within eight days from the time of sailing.

With respect to those cases where it has been alleged that the period of incubation exceeded eight or ten days, M. Prus does not consider them at all worthy of acceptance, in the sense in which they have been adduced; for no account has been taken by their narrators either of the epidemic action, or of the miasmatic infection which always acts an important part in places that are badly ventilated; in a ship for example. When a vessel becomes a focus of infection in consequence of having had a number of plague patients on board, the persons, who remain in this focus and breathe the vitiated air, may, and often do, contract the disease at intervals of a longer or shorter space of time. It is obvious that, under such circumstances, the sailors and passengers may be seized 15, 20, 30 days and even upwards, the one from the other, without the inference being at all warranted that the incubation of the malady has existed in any one case for more than six or eight days. We in truth cannot make out when the pestilential miasms began to act on those who had received them into their systems, in such a manner as to occasion the development of the disease.

Conclusion.—"If it be true that a fixed and absolute term cannot be assigned to the incubation of the plague, it seems, nevertheless, to be clearly proved by well-established facts that, at a distance from countries where it is endemic and beyond or away from epidemic foci, the disease has never broke out in persons who have been exposed to its influence after an isolation of eight days. The few facts, which might be regarded as exceptional to this rule, are all susceptible of another interpretation."

The concluding part of the Report is occupied with an account of the Quarantine regulations now in force in the different ports of France on vessels arriving from suspected countries, and of the changes which the Commissioners propose should be adopted. We have no intention to allude to the first of these matters; and, with respect to the second, our remarks shall be very brief. It is suggested, and the suggestion is certainly a valuable one, that medical men should be appointed by Government to reside in the different places where the plague is most apt to exist, in order that regular reports as to their sanitary condition might be transmitted home, and for the purpose of officially examining the state of every vessel, with respect to her passengers, crew, cargo, accommodations, &c., about to leave the country for any French port. The resident consular agents would then be better qualified, by the accurate professional details thus acquired, to determine when to grant and when to refuse clean bills of health to the vessels of their own nations. The affixing of the medical certificate to the ship's bill of health would also enable the officers of the port, where the vessel arrived, to judge more correctly of all the circumstances connected with her past and present condition. The Commissioners propose that no clean bill of health should be granted when a pestilential epidemic prevails or appears to be impending in the place of departure, or even when the number of sporadic cases of the plague are so numerous and severe as to occasion apprehensions that the malady may spread. In all other cases, a clean bill might be granted at the port of departure.

Although believing that the clothes and other effects of the sick are not capable of transmitting the plague, the Commissioners suggest that, until further experiments are made upon this subject, all those articles should be duly ventilated during the voyage; or, what would be better, that the trunks and boxes of the passengers and crew should be secured and stamped (plombées) at the port of departure, and not be opened until this can be done in a French lazaretto, where they should be well ventilated for three days or so.

The following are the suggestions of the Commissioners in respect of the Quarantines which they deem advisable, in lieu of those now existing in French ports.

1. For vessels having a medical man on board, and coming from Egypt, Syria, or Turkey with a clean bill of health, the quarantine to be 10 full days, *to date from their departure*, provided no case of plague or of any suspicious disease has appeared during the voyage.

The quarantine to be 15 full days, *to date from their departure*, for the same vessels arriving with a foul bill of health, if neither plague nor any suspicious disease has occurred on board during the voyage.

2. For merchant vessels arriving with a clean bill of health, but having no medical man on board, a quarantine of observation to be for ten full days, *to date from their arrival.*

When the same vessels shall arrive in a port with a foul bill of health, but without having had either plague or any suspicious disease on board while at sea, a most strict quarantine to be for 15 days, *to date from their arrival.*

If a case of plague, or of suspicious disease has occurred on board during the voyage, or should occur after the arrival of the vessel in a French port, the vessel to be subjected to a rigid quarantine, the length of which to be determined by the sanitary authorities of the port. The passengers and crew to be transferred to the lazaretto, and detained for 15 days at least, and 20 at most; the cargo to be unloaded and freely exposed to the air; the vessel herself to be well cleansed out, purified, and left empty for a month at least; and health-guards to be stationed near to, but not to be put on board of, the infected vessel.

With respect to the cargo, since it has not been proved by any authentic fact that articles or merchandize have the property of retaining pestilential miasms, and of transmitting the plague, the Commissioners confine themselves to merely recommending the employment of such means as are most simple and least oppressive or vexatious to commerce.

After exposing the barbarous absurdity of the plan followed at the Marseilles lazaretto, even to the present day, in the treatment of any one affected with, or supposed to be affected with, plague, it is suggested not only that plague-patients should be treated and waited upon like other patients, without using the cruel and ridiculous precautions that are still in force, but also that *post-mortem* examinations should be made in all fatal cases.

Should the plague break out in any house, town, or district, the rules to be followed are simple and obvious. The sick should be immediately removed from the dwelling, where the disease has appeared, to a healthy well-aired spot that has been fixed upon; while all the other inmates also should be compelled to leave it, in order that it may be duly cleansed, purified, and ventilated. On no pretext should the infected be confined and inclosed along with the healthy by sanitary cordons, or other compulsory measures, in the very place where the pestilence exists; this were only aggravating the mischief and rendering it more concentrated. The great object should be to attenuate the virulence of the atmospheric poison by separating the sick from the healthy, and by keeping both in airy, elevated situations. If necessary, tents and simple barracks should be established, and the healthy compelled to dwell in them, at a distance from the focus of infection.

The Report, of which we have now given so extended an analysis, was elaborately discussed at eight or nine sittings of the Academy of Medicine, in the months of May, June, July and August. Notwithstanding the labour expended, we do not think that either much new matter was added to the details which the Report itself contains, or that any of its more important positions were successfully impugned. The majority of the speakers, as well as of the members of the Commission, declared their cordial concurrence with the results of M. Prus' researches, while the minority were divided into two sections that maintained very opposite opinions; the one advocating almost all the extravagant notions held by the ultra-contagionist party, and the other loudly proclaiming the non-communicability of the plague under almost any circumstances, and calling for the entire abrogation of all quarantines. The reader will be able to judge for himself of the value of the extreme opinions of both parties.

By far the most valid objection to the Report is, that the practical recommendations proposed in its concluding part are not altogether consistent with the doctrines professed before; the quarantines recommended are unnecessarily stringent and severe, if the conclusions in the body of the Report be correct.

Part Fourth.

MEDICAL INTELLIGENCE.

FOREIGN.

1.—*On the Mutual Relations existing between Physiology and Pathology, Chemistry and Physics, and the Methods of Research pursued in these sciences.* By Baron Liebig.

Rise and Progress of the Natural Sciences.

The history of the natural sciences teaches us that every especial department or branch of our knowledge of Nature is constituted at first of a number of observations and facts, the result of experience, having no discernible connexion or obvious relation to one another.

Special laws of Nature.

The discovery of certain facts which connect two or more of such original detached observations, supplying, as it were, connecting links between otherwise disconnected phenomena, led to the apprehension, first of special laws, and subsequently to the deduction of general laws, or, what amounts to the same thing, to the invention of certain terms expressive of the mutual dependence or connexion existing between a more or less considerable number of natural phenomena.

General laws of Nature.

Many branches of natural philososphy—mechanics, hydrostatics, pneumatics, optics, accoustics, for examples—became elevated to the rank of sciences, when it was demonstrated that all the known instances of the phenomena of motion, whether of water, air, light, or sound, are referable to certain abstract truths—i. e., a small number of indisputable facts, which serve, not only to connect all the observed phenomena, but which also necessarily comprehend the elucidation of every possible fact left for future discovery, so that, in order to explain any new phenomena, it is not necessary to institute a new series of experiments, or to go over the same grounds of reasoning and deduction as at first.

If we may assume it to be indisputable, that not only the phenomena of inanimate matter, but also those which are peculiar to living vegetable and animal forms, stand in certain definite relations to each other—i. e., are dependent upon certain necessary causes; and further, if it be true that a clear and satisfactory insight into the nature of organic changes and processes can be obtained only through the medium of a just apprehension of these causes, it must be concluded that the investigation and elucidation of the relative and mutual position of dependency in which the phenomena of living matter stand to each other; in other words, of the causes and essential conditions of phenomena—must be considered the highest and most important aim of physiology.

The simple knowledge of the relation of cause and effect, or antecedent and consequent, amid natural phenomena, suffices, in many cases, to furnish a satisfactory explanation of them. These relations are discoverable in every branch of natural science; increased and enlarged experience, accurate observations, and correct experiments, will finally lead to a perfect elucidation of phenomena and their causes. It cannot, therefore, be questioned that, as chemistry, at a certain point of its progress, advances beyond the limits of a mere experimental art, physiology is susceptible of being raised to the rank of a true inductive science.

Order of proceeding in the investigation of Nature.

If it be essential to the systematic course, or order, of investigating Nature, that our apprehension of *special laws*, must precede that of *general laws*,—if to attain to a correct conception of life it be necessary that we should know, not only the form of every part of every living organism, but also be accurately acquainted with the functions of every organ, the mutual relations and dependence they bear to each other,—the relations between the form and the matter of which every organ consists,—the manner and degree in which the form depends upon surrounding parts, it cannot be denied that we are still at an infinite distance from the final deduction of that universal and ultimate term which is to comprise the true conception of life—the comprehension of the cause and link of connexion of all its phenomena.

Nay, so distant are we from the attainment of this exalted generalization, that the probability or possibility of the discovery of such universal laws in physiology is by many held to be utterly inconceivable; indeed, the majority of physiologists seem unable to conceive of the psychical apart from the corporeal phenomena of life, the *vis vitæ* as distinct from the form and matter of the living organ!

Preconceived notions impediments to investigation.

There is an infirmity attaching to the human mind, dependent upon certain laws which govern its perceptive faculties, from which even individuals possessed of the most excellent understandings are unable readily to emancipate themselves. When daily observation has, during a long time, presented two or more phenomena in intimate connexion, when we find they have been for centuries observed and regarded as inseparable, when no one has, either intentionally or accidentally, been led to contemplate them separately, the human mind almost loses the faculty of disassociating them; and the reason seems to revolt at the conception, when first presented to it, that the two facts, or phenomena, are really and distinctly separable.

Innumerable instances might be adduced to prove this general truth. Many of the wisest and most sagacious men have considered certain facts or views as impossible, simply because they were under the influence of this prejudice, and could not apprehend them; whilst the next generation have not only admitted the same notions to be perfectly conceivable, but, far more remarkable, they have been subsequently adopted by all men as established and indisputable truths.

Men of the highest order, most distinguished for intelligence, and far above the influence of vulgar prejudices, were for a long time incapable of conceiving the force of gravity as acting upwards as well as downwards, or that the sun, at such an immense distance, could exercise any action upon the earth, or the earth upon the moon. The great Leibnitz himself rejected the Newtonian theories, because he deemed the motion of the heavenly bodies in curves around a common centre to be impossible, without the agency of some continually propelling mechanism, or of an angel specially appointed for the task. He assumed that it was in accordance with a natural law that if a body, like a planet, were left to move unassisted by some controlling power, it must deviate from the circular course, and pass off in the direction of the tangent.

The Newtonian theory of gravitation, the operation of the force of gravity through immense distances without the intervention of material agents operating upon innumerable worlds, is now familiarly apprehended by every school-boy; nevertheless, it was for a long time rejected by philosophers as a creation of the imagination, because they had adopted as an axiom that bodies cannot act at distances on each other, and they could not divest themselves of this prejudice.

Many of the established laws of mechanics and other branches of physics, which we know to be the fruits of long, patient, and arduous labor and inquiry, appear so self-evident and obviously true, that if we disregard the history of their slow and gradual development, it seems altogether inconceivable, how any individual could at any time have questioned their truth. The simple proposition that a body once put into motion may continue to eternity to move in the same direction and with unaltered velocity, unless retarded by some equivalent external influence, seemed so contrary to the most common observations and self-evident notions, that the recognition and establishment of its truth met for a long time with the strongest opposition.

The notion that two chemical substances, possessed of definite properties, should, by their combination in *indefinite* or *unlimited* proportions, be able to form a compound of *definite* and *invariable* properties, seems absurd. It is wholly incompatible with, and contradicts all that are now considered to be, sound and correct views of chemical combinations.

Thus it will be evident that the apprehension of a natural fact or phenomenon is not dependent upon its own evidence, but that it depends altogether upon the intellectual standard of the observers. On the discovery of a new fact, if the intervening link connecting it with the habitual train of ideas and reasoning is wanting, men look upon the new fact as incomprehensible or illusory. This is one of the greatest impediments to the application of chemistry to physiology; indeed, it renders the mere mention of chemical discoveries repulsive to physiologists. And if, added to this, we consider the state of pathology; if we contemplate pathologists assuming as sure and undeniable, facts which rest upon no better foundation than the faith of their forefathers in their truth, and observe that the methods of inquiry and of reasoning pursued in this science remain altogether unchanged, it must be confessed that little hope can be reasonably entertained, that chemistry, notwithstanding all its advancement, will afford natural assistance to physiology and pathology. But it is nevertheless absolutely certain, that without the co-operation of chemistry and physics, it is impossible physiology and pathology can become worthy of the name of sciences, can have a true scientific foundation. Even amongst those who perceive the necessity of this co-operation, it is a matter of doubt and dispute in what manner and by what means it can be accomplished.

Physiology an Inductive Science.

The proposition that every empirical science (and therefore physiology) may in process of time attain to the rank and dignity of an inductive science, requires no further demonstration. Whether, or to what extent, the aid of other sciences may be required for the attainment of this object, is indeed a matter of indifference. Astronomy has now become merely a branch of mechanics, but it owes its high scientific character and foundation, precisely to this alliance.

Natural Laws.

If we bear in mind that, like every event in the world, so every manifestation or change occurring in Nature, whether in inanimate matter, in plants or animals, stands in direct relation to, or is the immediate consequence of, another phenomenon that has preceded it, every state or condition of a plant or animal body existing at any moment having been caused by, and being dependent on, certain precedent conditions, it is obvious that if we know all the causes which have produced the present state, and are acquainted with their powers, and the effects, according to certain laws, they produce both in time and and space, we are able to anticipate and foretell the state and condition which will next follow. The general deduction from these relations of antecedence and consequence is termed a natural law.

Modern Chemistry as contradistinguished from the Chemistry of the Past.

Every one conversant with the rise and progress of Chemistry, and of the various branches of physics, must be aware that the advancement of these sciences in Modern times is attributable principally to the idea, which was gradually conceived and slowly admitted, that every natural phenomenon, every state of, or change occurring in matter, in a word, every *effect* is produced by several causes; and it is simply the patient investigation of these causes, and the separation and discrimination of effects, which distinguishes the chemistry of the present day from the chemistry of the past. In the days of phlogiston all inquiry was arrested by assumption. Thus there was held to be a principle of dryness, humidity, heat, cold, combustibility, metality, acidity, volatility, colour, state. For every property observed a peculiar essence was imagined, which explained everything; the mere designation of a phenomenon was considered a satisfactory explanation.

The change of weight which bodies manifest when subjected to chemical processes, was regarded as a simple property of matter, and also the effervescence of limestone with acids. The chemists had this theory ready for the phenomena of combustion. They regarded the changes in weight which they observed, to belong rather to the natural philosopher to explain, than to themselves; they left it to him to say how a substance could become heavier by the loss of one of its constituent elements. The increase of weight observed to follow the process of calcination was an accidental property, belonging, amongst other substances, to the metals.

Physiologists of the present day.

Many of the physiologists and pathologists of the present day are precisely in the condition of the phlogistic chemists, in their mode of apprehending and explaining the vital processes and phenomena. They ascribe the phenomena of the nervous system to a *nervous force*. *Vegetation*, *irritability*, *sensibility*, action and reaction, simple effects of motion, resistance, causes of the formation and changes of form, which are comprised by them in the general term *typical forces*, these gentlemen look upon as distinct things or beings, or at least they occupy in their bearings the place of the essences of the phlogistic school in the explanation of phenomena.

Confounding of cause and Effect.

The most common phenomena assume in the minds of physiologists, even at the present day, the appearance of particular faculties. They explain them by supposing the existence of special causes differing from all others with which we are acquainted. Thus, the restoration of a state of equilibrium between two fluids, differing in their specific gravity, or two substances dissolved in dissimilar menstrua, and separative from each other by animal membrane, has received the names of *endosmose* and *exosmose;* and these names are treated as if they were distinct entities, and comprised a full explanation of the process they are intended to indicate. The phenomenon in question is simply filtration; differing from ordinary filtration only in this, that the passing of the fluids is caused by the attraction of affinity instead of by pressure.

To this mode of viewing and explaining phenomena was added, during the reign of the phlogistic school, the no less grave error of assuming that causes must necessarily be similar to their effects. Thus, something combustible must be the cause of combustibility; something intrinsically acid, of acidity. The causticity of calcined lime was considered to be a separable *causticum*, transferable from one body to another; from lime, for instance, to the mild alkalies.—A primitive alkali was supposed to exist in the alkalies, a salt in the salts, and a universal acid in acids. Analogous substances were regarded as simply varieties of one and the same prototype.

Fallacious Explanations of Physical Properties.

Many physical properties of a substance were supposed to be explained, by the imaginary nature, properties, and physical textures of its molecules. Pungency of taste was ascribed to pointed particles. Leming's notion, that the ultimate particles of an acid had lancet points, bent like tenter-hooks, and the atoms of alkalies being like sponge, blunted the acids, met with universal approbation, as it seemed satisfactorily to explain the neutralization of acids by alkalies.

The precipitation of gold from its solution, by ammonia, was perfectly intelligible to Leming's contemporaries. Ammonia had the power to break off the points from the lance-shafts. It acts, says Leming, like a club which is thrown at a walnut-tree laden with fruit. In the same way of reasoning, substances having an astringent or cooling taste were assumed to have an astringent or cooling action on the living organism, and liquids, such as alcohol, being said, in common parlance, to be strong, were supposed to have a power of imparting strength when taken as medicine internally.

It is a mistake to suppose that this way of viewing and explaining natural phenomena belongs to a time long past. The following passage from "An Attempt at an Universal Physiological Chemistry," published in 1844, will prove that it still obtains among physicians of the present day:—"We conclude, therefore, correctly," says its author, M. Mulder, "that analogous forces exist in sulphur, selenium, chromium, and manganese, and we are thus led naturally to assume that chemical deportment of these elements is independent of their material form and condition, but depends upon the forces which govern the molecules of the sulphur, selenium, &c. Thus the idea which represents to our mind the element sulphur, becomes associated with the notion of a force, and this, of the same force which is also active in selenium, and which operates not only in the production of compounds, but assists also in the determination of the principal characteristics of these compounds. This sulphur and selenium force is still manifest, even in the most remote compounds formed by these elements, &c. &c."

It will be seen, from this passage, that Mitscherlich and Copp's beautiful researches on isomorphism have been unable to remove the impression made by that peculiar mode of apprehending natural phenomena which I have described.

Every natural phenomenon is occasioned by more than one cause.

The correctness of theoretical expositions, or mere opinions, may be questioned or discussed, and whether rightly or wrongly decided on is a matter of indifference. But a phenomenon perceptible to the senses at all times and in all places cannot be disputed. The cause or causes producing the perceived effects may be a matter of discussion, nevertheless, in that field of Nature treated of by physical science, the imagination, alone, being inadequate to the discovery of causes. We know that one and the same effect—as, for example, mechanical motion, a pustule on the skin, contraction of a muscle—may be produced by many and various causes; on the other hand, one and the same cause may produce as many and various effects.

Chemical Combination.

We know that every case of chemical combination, simple as it is, depends upon at least three causes or essential conditions—namely, affinity, force of adhesion, and heat,—which three causes must stand in a certain relation to each other, and exercise an equal share in the process, if combination is to take place.

Different Effects of Heat.

We know, also, that when a given amount of heat expands a solid body, merely forcing its molecules further assunder, twice or thrice the same amount of heat totally alters its properties, and that a further change in its properties ensues when the amount of heat reaches a certain point.

It is absolutely certain that expansion, liquefaction, and the assumption of the gaseous state of bodies, are all produced by one and the same cause. But we also perceive that the effects produced are not in proportion to the cause.—To explain this we are obliged to refer to a certain other cause, resisting or counteracting the influence of heat—namely, the force of cohesion. It is precisely from the investigation of this that our notion of the force of cohesion has acquired a scientific basis.

The same heat constituting an essential condition of the combination of one constituent of the atmosphere with mercury, when raised by a few degrees, produces precisely the opposite effect—namely, it effects the separation of the oxygen from the mercury.

By a simple process of oxidation, we produce acetic acid from alcohol. The same acid is obtained by the oxidation of salicylite of potass. It may be also equally produced from sugar, wood and starch, by the mere application of heat, with the exclusion of all atmospheric oxygen. In all these cases, the product is the same, but the conditions of its formation are exceedingly different.

Discrimination between the Effects of Vitality and their Causes, the Principal Condition of Progress in Physiology.

If it be true that a scientific basis for physiology can only be laid by a complete investigation of the sum total of all the conditions upon which the phenomena of life are dependent, and that the primary task of the physiologist must consist in the discrimination of the effects of vitality and the causes producing them, it is obvious that since many causes coöperate to produce these effects, he must have a perfect acquaintance with all the forces and causes which are capable of producing in nature any motion of change of form and condition of matter. How otherwise could it be possible for him to separate the effects attributable to these causes, from those which are referable to a cause having nothing in common in its manifestations with gravity, affinity, &c.

Principles admitted, but not strictly applied.

No one can deny that these principles of investigation begin to obtain an influence in the pathology of the present day ; indeed, the difference between the method of viewing and interpreting phenomena now prevailing, differs exceedingly from the philosophic method which preceded it. But the influence of the latter is by no means yet obliterated ; at all events, not in Germany. Notwithstanding the full acknowledgement of the principles of exact research, the physiologist and pathologist too readily throw aside its control, and in every case where their way is not clear, their unfettered imagination plants a forest of errors in their path, and thus obscures and impedes their apprehension. The antitheses and periphrases, which were formerly so much in vogue, continue to this day to play a principal part in all explanations, and deprive the description of the commonest facts and phenomena of that simplicity and clearness of which they are susceptible. The error lies, not in the absence of just principles, but in the neglect of their rigid application.

Examples in illustration.

A few passages, quoted from the writings of a distinguished pathologist of the day, (Henle, "Pathological Investigations," Berlin, 1840,) will serve to justify the foregoing assertions, and to illustrate the influence of the old method of investigating and interpreting Nature upon the doctrines of the present day.—They will show how small is the chance of arriving at correct conclusions when starting from modified and vague notions, and how little Science can gain, even from the most highly intellectual persons, if they do not avail themselves of the aid of chemical and physical truths.

Indistinct notions respecting the action of External Agents upon the Animal Organism.

The effects produced upon the animal organism, or any of its parts, by various external agents, such as mechanical pressure, friction, the atmospheric air, heat, light, electricity, magnetism, chemical agents, &c., are in certain cases similar, in others, totally dissimilar. Like all effects, these depend upon two conditions—the quantity or force of the impinging agent, and the amount of resistance opposed to its operation by forces within the organism. The nature of these internal forces is determinable, and their respective amounts are measurable, by estimating the qualitative or quantitative differences in the effects produced by the external causes. These effects are the marks or indications of an altered state of the organism; the forces active within are therefore to be discovered by a judicious study and investigation, both qualitatively and quantitatively, of the effects produced by every external cause, separately considered.

A few quotations from Henle's celebrated work ("Pathological Investigations," Berlin, 1840,) will show that the method of research adopted by our modern pathologists is the very reverse of this. "A stimulus," says Henle, "is anything which, acting upon organic matter, alters its form and composition, and thereby its functions." Far from considering the disassociation of causes and their effects to be the indispensable preliminary of a correct apprehension of their nature, he strings together into one bunch all the imaginable causes of change of form and condition of organic bodies, and gives the aggregate the title of "a stimulus;" and then this new term is made to play the part of a distinct thing or being in the explanation of the various states and changes in the organism. This term is not intended to indicate the *modus operandi* of light, heat, electricity, magnetism, or chemical agents, but only a small fraction of the action of all these agents. If the reader substitutes, in the following passage, for the term "stimulus," the above definition of the word given by Henle, he will perceive at once the amount and value of the acquisition which science has gained by this method of examining and explaining phenomena. "A stimulus alters the nervous fibre and its relation to the blood; but unless it destroys the fibre altogether, the transmutations of matter continue—nay, perhaps, are promoted by the irritation, &c. &c."

False Analogies.

After the above, we need not be surprised to find, in a subsequent part of the same work, (page 221,) an hypothesis respecting the manner in which stimuli act upon the organism; an hypothesis, moreover, in which there is not the most distant allusion to the action of any thing or cause whatever which can act upon organic matter to change its form or composition. The passage is eminently characteristic—"The distinctive peculiarity of organic matter consists in its persistency, notwithstanding the many transmutations it undergoes; and that it progresses steadily towards and up to a certain end. It is not the reaction which is characteristic, but its cessation. It is not the irritability or mutability that distinguishes organic matter, but it is the power of compensation for these alterations and changes. It is the property of the organism to continue its development, according to inherent laws, amid all the various phases of transmutation through which it may pass. A chord, the sound of which is raised by mechanical pressure, continues to emit the high sound as long as the pressure lasts. A metal which has been rendered more elastic by an alloy remains elastic and alloyed. But an organic body ceases to react even while the excitement continues; and when a chemical action has altered its matter, and increased or diminished its activity, the normal composition and normal action are, after a longer or shorter interval, reëstablished. The manner in which this is accomplished I will endeavour to illustrate by a simile. Suppose a vessel full of fresh water, flowing off on one side, and constantly replaced on the other. Let a chemical agent act upon this water—place in it common salt, for instance. The reaction of the water upon the salt causes a saline taste.

very strong at first, but gradually becoming weaker, and finally, when the whole of the water is replaced by fresh, will disappear altogether. This simile, homely though it be, is exactly adapted to our case, &c."

The author proceeds to elucidate his views apparently in the same manner as is done in physical science. He attempts to explain the deportment of an organic body by its similarity or analogy with another well-known case. This method of reasoning from analogy has never been neglected either by ancient or modern philosophers. The error here consists simply in its application. It is an attempt to explain certain phenomena by comparing them with others to which they bear no resemblance, and exhibit no analogy; and by the assumption, that the causes of such phenomena are well known because the phenomena themselves are of frequent occurrence, and familiar to observation. Need it be added, that no insight is to be obtained into the nature of causes and effects by false analogies—in other words, by images and similes.

Erroneous points in these comparisons.

Henle terms mechanical pressure upon a musical chord, which heightens the note, a *stimulus*, and, according to him, all imaginable alterations of form, composition, and condition of organic matter, are produced by stimuli.

But mechanical pressure upon a musical chord simply serves to shorten the chord for the period the pressure lasts; it bears not the slighest direct relation to the sound produced. Metals are not rendered more elastic by alloy; elasticity is a property wholly independent of the composition of substances. There is not the least relation, whether of similarity or dissimilarity, between an organic body and a musical chord, a metallic alloy, or water to which salt has been added. Water is a compound of oxygen and hydrogen; the addition of salt changes neither its form nor its composition, the salt does not act as a stimulus upon the water, nor does the water react on the stimulus by a saline taste; water is altogether tasteless; it is the salt which is tasted.

Organic matter forms no exception to the great laws of Nature. It cannot cease to react whilst a cause which alters its form, composition, and functions, continues to act upon it;. and when the normal constitution of organic matter is altered by any chemical agent, it is not reëstablished. In the language of physical science, the simile of Henle would bear the following interpretation.

As water may continually flow from a constantly refilling vessel, without the slightest decrease of its level, so the vital manifestations of the animal body continue so long as the latter possesses the means of reproducing, and replacing the matter consumed or eliminated. An external influence may transiently change or modify the vital manifestations, just as salt thrown into running water alters the taste of the fluid. But as this extrinsic saline taste gradually disappears if no more salt is added, so, likewise, those modified manifestations of the vital forces die away when the causes which produced them cease to act and the normal functions are enabled to pursue their wanted course.

Thus expressed, the simile serves to show that the action of the body has nothing to do with the stimulus. Under the given conditions, the level of the water in the vessel would have been maintained; notwithstanding the non-addition of salt, the water in the vessel would have been renewed and replaced by fresh water. In the organic body, if the supply from without fails, the means of its renovation and restoration fail as the level of the water decreases, if on one side water flows off without being replaced on the other.

A portion of the skin of an animal, destroyed by a red-hot iron or by sulphuric acid, is not replaced in consequence of any reaction, but the renovation of the destroyed portion is owing to the operation of a cause existing beneath the skin, the action of which is not called forth by the red-hot iron or the sulphuric acid; it was acting, and would continue to act, without their coming into contact with the skin. Nor have these external agents the slighest share in accelerating the action of restoration; this depends on very different causes.

Typical force, a vague and indistinct term.

It can scarcely be considered a very happy explanation of certain manifestations of vitality—as, for instance, the development of the organism from an ovum, or the restoration of excised parts—to refer them to the operation, within the system, of a typical force, since terms of this kind simply designate phenomena which they are intended to explain. Why does the power of restoration in the salamander extend to whole limbs, whilst in the frog, and the higher order of animals, this regeneration is limited to a few tissues? Henle admits that his eternal typical laws do not explain this, ("Rational Pathology," p. 129.) To refer the vital phenomena to a typical force, (a mere term,) is simply to say, they take place thus and no otherwise, because they take place thus and no otherwise.

To refer phenomena to a general law pre-supposes a knowledge of the law. The conception of a law is inseparably connected with an exact knowledge of qualitative and quantitative relations.

To institute rather a rude and homely comparison, the animal organism is like one of the great transatlantic steamboats. During every moment of the voyage, these huge bodies consume fuel and oxygen, which they eliminate in the form of carbonic acid, water, and soot. In them exists a source of heat and a source of power, producing motion and preparing the food of the crew. If a wheel be damaged, a leak sprung, or a bolt broken, carpenters, smiths, and others are at hand to repair the breach, to maintain the ship in its original integrity, and to preserve its normal motion. Just so in the animal organism; beside the first part of the comparison holding exactly, there are within it agencies (like the smiths, carpenters, and engineers of the ship) to preserve and repair it, and the task of the physiologist and pathologist is, to ascertain what these are, and to study their nature and their mutual relations.

Light considered as a Stimulus.

Many pathologists apply the term stimulus both to external causes capable of altering the form and composition of animal organisms, and those which do not possess this property, such as light, sound, &c. Now this renders the whole matter unintelligible. Light is, properly speaking, a phenomenon of motion, and as such is perceived in the eye, which motion is communicated through the optic nerve to the sensorium. But this motion cannot of itself produce any change of form or composition of the nerve or brain, without the concurrence of other causes, and to these belongs the operation of the mind, through which the impression on the optic nerve becomes a distinct sensation, calling forth notions and ideas.

Surely no one will seriously maintain that a piece of white paper produces, by the light reflected from it, any alteration in the form or composition of the brain. If so, we must ascribe an equal effect to a piece of black paper, which reflects no light. But the same white and black surfaces, as letters in a book, awaken the most diversified sensations, notions, and ideas, and exercise through these, not by the stimulus of light, an influence on the condition of the brain.

Sound considered as a Stimulus.

As with light, so with sound: sound is the vibration of the air, propagated through the organs of hearing upon the acoustic nerve. This alters neither the form nor composition of the parts to which this motion extends. In a picture gallery, the sense of sight is wearied in a much shorter time than in the open air, although in the latter the eye receives far more light; the same applied to the sense of hearing.

Erroneous notions respecting Reaction.

False notions, associated with a *term* once adopted into general use, become a constant source of mistakes and misunderstanding. Such is the case with the term *reaction*, which is constantly employed by physiologists in a sense widely different from its ordinary, or etymological meaning. When the secretion of a gland is increased by the action of any external cause, the physiologists call this the reaction of the gland against a stimulus. To this they add, that it is a peculiarity of the organic body that the increased activity of the gland ceases, although the stimulus continues to act; whereas the true explanation of the phenomenon is, that the secretion ceases when the matter adapted for secretion is exhausted, and that it recommences when a fresh quantity of such matter is supplied. The action of the stimulus here is not properly an action on the gland, but on the cause which regulates the secretion of the gland; hence the argumentation of the secreted matter.

In the tail of the lizard there is a constant transposition and renovation of its component particles. When the tail is cut off, the same cause continues to act, without interruption, in the surface left in connexion with the organism. There is no reaction against the knife—the excised part has no reaction! The tail is not restored, if the creature receives no food, or if it be restored in the absence of food, it is at the expense of other parts of its body, which lose weight and volume in exactly the same proportion as the tail has gained.

The organic body is, in all respects, like other bodies. External causes produce numerous effects, which continue after the operation of the primary cause is withdrawn; other effects of external causes are counterbalanced and effaced as soon as the external cause ceases to act, because there are within the body forces, incessantly resisting external agencies.

Descriptions of Phenomena are not Explanations.

The little progress made by physiology, even during the prevalence of true philosophy, abundantly manifests the inadequacy of the most exact and accurate description of the parts and processes of organic bodies, such as digestion or respiration for example, or of diseased conditions, to afford any satisfactory elucidation of the phenomena. Descriptions afford us no insight into the nature of the processes and changes, nor can any combination of them, however ingenious, contribute to the progress of the science. To this end there must be a rigorous and minute search into every existing fact, and a thorough knowledge of everything bearing any part in the processes. The imaginative faculty does not enable us to advance one step beyond our first observed fact. A succession of views and opinions cannot be termed progress. To rest in these is to go around in a circle. It is true it has its use, and is very important for ascertaining the right direction in which to push our researches, to endeavour to present all known truths in as multifarious and diversified points of view as possible; but we must not rest here. The description of any given condition of the animal organism, as of a catarrh for instance, when it is said to be an inflammation of the mucous membrane of the nose, is no explanation of this state, and should never be regarded as the ultimate object of our inquiry into the nature and causes of catarrh. To paraphrase the word *cold* into a *noxious agent operating on the skin*, makes no real progress towards a correct conception and apprehension of that state.

The due exercise of the faculty of Perception one of the principal conditions of correct Observation and Apprehension.

The correct employment of our senses, the accurate estimation of distances, of the height and size of objects, is acquired by experience and reflection. The correct apprehension of natural phenomena, the power of representing them to others unobscured by the prejudices of the observer, are exclusively the attributes of well-practised and experienced intellects. The botanist detects at a glance the resemblances and dissimilarities of the plants around him. The painter detects numberless details in a picture, or in natural scenery, which the uninitiated fail to discover. In no branch of experimental science is the exercise and improvement of the perceptive faculties more important than in physiology and pathology. In the professors of no science do we more rarely meet with a perfect observer than amongst the professors of medicine. It is to this generally prevailing deficiency of medical men in the powers of perception and apprehension that we must ascribe the many inconsistencies and contradictions which we find in their ideas and descriptions of even the most simple diseases, the appearance and disappearance, without leaving a trace behind, of numberless books on climate, yellow fever, plague, cholera, &c., written by persons wholly unqualified as observers, and perhaps who have never seen the localities or the diseases they describe.

In chemistry and physics, no theories are admitted to be worth consideration which do not emanate from men who are known to be qualified to pursue practical investigations, and who have given adequate proofs of such qualifications. The truest and most correct theories not having this guarantee are usually equally disregarded as the wildest hypotheses. Thus it required a Berzelius, with all his acute penetration, to save Richter's great theory, respecting the laws of definite proportions in combining bodies, from oblivion, and to detect the intrinsic truth of the existence of an universal law amidst the mass of fallacies and false assumptions of Richter's work. The very first example adduced by Richter the starting-point of his first table of equivalents, was the assumption of the existence of a carbonate of alumina; this alone was enough to destroy all confidence in the correctness of the rest.

Error dependent on incorrect Observations and wrong Interpretations of observed Phenomena.

Both incorrect observation and wrong apprehensions of observed phenomena, lead to false views and theories. For example, the constant simultaneous manifestation of two phenomena is too often regarded as indicative of the existence of the relation of cause and effect between them; whereas, there are many examples of phenomena in nature occurring together, or immediately antecedent and subsequent, which have not the relation of cause and effect. The assumption of causation, where none exists, is the result of defective observation.

Observation.

To perceive a thing by means of some of our senses is certainly an essential condition of observation; but sensible perception does not constitute correct or useful observation. An observer must be able, not only to see a thing simply as a whole, but in its component parts; and to be a good observer, he must ascertain and correctly apprehend the connexion of the several parts of any phenomena with each other, and their mutual relation to the whole.

Instances of erroneous Observation—Influence of the Moon on the formation of Dew.

The moon is supposed to exert some influence on the coolness of the air on moonlight nights, and on the formation of dew and hoar frost, simply because while these processes are proceding she is always present. But it is only as a looker on!

In an otherwise excellent lecture, on the influence which the moon exerts on the earth, delivered last year at Dresden, the following passage occurs:—

"*Influence of the Atmosphere on Vaporization*

"Without the atmosphere, the existence of water on any similar fluid in a liquid form on the surface of the earth would be impossible. Were the earth suddenly stripped of its surrounding atmosphere, the ocean, lakes, and rivers, would evaporate, and the whole surface of our globe would, ere long, become completely dry, as may be demonstrated by the experiment of placing water under the exhausted receiver of an air-pump."

The lecturer here supposes and assumes a connexion between the atmosphere and evaporation which does not exist in nature. Without the atmosphere no clouds could form, no water would float upwards in the form of minute globules, as in visible steam; nor would watery vapour rise to so great a height as it attains in the atmosphere; but upon evaporation the atmosphere exerts no influence, and an equal amount of aqueous vapour is formed under the receiver of the air-pump, whether exhausted or filled with air!

The Dilution of the Atmospheric Oxygen with Nitrogen.

In how many physiological works do we find it stated that the nitrogen of the atmosphere serves to dilute the oxygen, and to retard and moderate its action upon animal organisms, and yet the amount and volume of the oxygen, in a given space, would suffer no change even were the whole nitrogen suddenly removed from the earth. The two gases composing the atmosphere exercise a certain pressure upon the earth's surface, and thus, consequently, upon the human body, but the particles of the one do not compress those of the other gas. If we take two equal bottles, one perfectly exhausted, the other filled with nitrogen, and connect them together by means of a glass tube, the nitrogen becomes equally diffused through them both, and if they are of equal capacity, the one will contain precisely the same amount of nitrogen as the other. The same will happen if one of the bottles, instead of being empty, as we have supposed, is filled with oxygen gas;—under equal pressure, the nitrogen is diffused in the bottle filled with oxygen, just as if there were no oxygen in it, and the oxygen is diffused in the other, just as if it contained no nitrogen.—*The London Lancet.*

(To be continued.)

2.—*Case of Repeated Pseudo-apoplectic Attacks, &c.* Reported by William Stokes, M. D., Physician to the Meath Hospital, &c.

Edmund Butler, aged sixty-eight, was admitted into the Meath Hospital, Feb. 9th, 1846. He stated that his health had been robust, until about three years ago, at which time he was suddenly seized with a fainting fit, in which he would have fallen if he had not been supported. This occurred several times during the day, and always left him without any unpleasant effects. Since that time he has never been free from these attacks for any considerable length of time, and has had, at least, fifty such seizures. The fits are very uncertain as to the period of their invasion, and very irregular as to their intensity, some being much milder and of shorter duration than others. They are induced by any circumstance tending to impede or oppress the heart's action, such as sudden exertion, distended stomach, or constipated bowels. There is little warning given of the approaching attack. He feels, he says, a lump first in the stomach, which passes up through the right side of the neck into the head, where it seems to explode and pass away with a loud noise resembling thunder, by which he is stupified. This is often accompanied by a fluttering sensation about the heart. He never was convulsed or frothed at the mouth during the fit, but has occasionally injured his tongue. The duration of the attack is seldom more than four or five minutes, and sometimes less; but during that time he is perfectly insensible. He never suffered unpleasant effects after the fits, nor had anything like paralysis. His last fit occurred about one month before admission. He has never heard it remarked that there was anything peculiar about his heart or pulse. At first he found that spirits was the best restorative or prophylactic, but latterly he has not used them, being "afraid to die with spirits in his belly."

On admission, he was haggard and emaciated, but seemed the wreck of what was once a fine, robust man. He lay generally in a half drowsy state, but when spoken to was perfectly lively and intelligent.

What he sought admission into the hospital for was an injury he had sustained, by a fall, on the left shoulder; this, however, was of no consequence, and he soon recovered under the use of an anodyne liniment.

He makes no complaint of his general health; his appetite is good, and he sleeps well; bowels regular, and, in fact, all the functions are in good order.—

He has, however, some cough, attended with a slight mucous expectoration.—His intellectual powers are perfect. He complains of a feeling of chilliness over the body, and is never warm except when close to the fire. This has long been the case; and he says that each day he gets a periodical chill, generally in the afternoon, which is followed by increased heat of the surface, but without sweating.

On percussion, the chest is universally resonant. The respiratory murmur loud, and combined, more especially posteriorly, with large mucous *rales*. The impulse of the heart is extremely slow, and of a dull, prolonged, heaving character, giving the idea of feeble as well as of slow action. The first sound is accompanied by a soft *bruit de soufflet*, which is prolonged until the commencement of the second sound, and is heard very distinctly up along the sternum, and even into the carotid arteries. The second sound is also imperfect, though very slightly so; the imperfection being much more evident after some beats than after others. Pulse twenty-eight in the minute, of a prolonged, sluggish character; the arteries pulsate visibly all over the body, but no *bruit* is audible in them. They appear to be in a state of permanent distension: the temporal arteries ramifying under the scalp, just as they are seen in a well-injected subject. All the other cavities and viscera appear to be in a perfectly healthy state. Urine, neither acid nor alkaline; of a light colour, clear; specific gravity 1010; and does not afford a precipitate with nitric acid. He was ordered four ounces of wine, and a liniment for the shoulder.

February 17th. The pulse has varied from twenty-eight to thirty in the minute. The cardiac murmurs continue unchanged; that with the first sound is plainly audible over the upper part of the thorax, but most evident along the course of the aorta.

21st. Pulse thirty. Cough quite gone. Has been complaining of a feeling of the "lump in the stomach" for several days, and was once threatened with the approach of a fit during the night; it passed off, however, without becoming a true attack.

23rd. An œdematous swelling has appeared behind the left ear, extending up the side of the head, slightly tender on pressure; no redness; has had no shiverings; tongue clean; bowels free. Pulse up to 36.

March 3rd. On the 24th of February the œdema had left the left side, and made its appearance on the right, from which it was dispersed on the following day by the application of poultices. The pulse fell to the usual range.

His aspect and general health are greatly improved since his admission. He gets up every day, and is much stronger. The shoulder is almost quite well. The pulse has continued at about 28 or 30. He says he has had two threatenings of fits since his admission, both occurring in bed, *and both warded off by a peculiar manœuvre: as soon as he perceives symptoms of the approaching attack, he directly turns on his hands and knees, keeping his head low, and by this means, he says, he often averts what otherwise would end in an attack.*

4th. He has mentioned, for the first time to-day, that he is much troubled with irritability of the bladder, so that he is obliged to rise very often during the night to pass water. His urine was examined and found to be healthy. Specific gravity 1015. He has been subject to this for the last twelve months, and it probably depends on the disease of the prostate so common in old men.

We remarked to-day, that on listening attentively to the heart's action, we perceived that there were occasional semi-beats between the regular contractions, very weak, unattended with impulse, and corresponding to a similar state of the pulse, which thus probably amounts to about 36 in the minute, the evident beats being only 28, so that there must be about eight of these semi-beats in the minute;—but these signs are very indistinct.

14th. Health improving; has had no fit; no cough. Both morbid sounds are loudest over the sigmoid valves, and thence along the aorta. No semi-beats audible. Pulse 29; not quite so prolonged as before.

18th. He complains to-day of palpitation, and a feeling of uneasiness about the heart;—the impulse is increased and is found to consist of two distinct pulsations. The *bruit*, with the first sound, is somewhat louder than before.—On listening attentively, there are heard occasional abortive attempts at a contraction, probably about four in the minute. They do not destroy the regular intervals between the stronger sounds, but are heard, as it were, filling up the interval. We could not recognise a corresponding state of the pulse, which counted 32 in the minute.

After this, little change was observed. His health continued improved; he had no fit, or threatening of one; and he appeared anxious to leave hospital, in order to go to work again. The pulse continued about the same standard, and regular; I believe it never exceeded 36 in the minute since his admission into hospital. The physical signs remained unchanged, as was observed the day before he left the hospital. An examination of the lungs revealed no morbid sign, the bronchial rales, heard at the time of admission, having quite disappeared.

He left the hospital in March, intending to go for some time into the country before he resumed work. He was advised to be careful not to over-exert himself; and never to allow himself to be bled when threatened with one of his fits.

Within the present month (June) this patient has been again admitted into hospital. The cardiac phenomena remain as before, but a new symptom has appeared, namely, a very remarkable pulsation in the right jugular vein. This is most evident when the patient is lying down. The number of the reflex pulsations is difficult to be established, but they are more than double the number of the manifest ventricular contractions. About every third pulsation is very strong and sudden, and may be seen at a distance; the remaining waves are much less distinct, and some very minor ones can be also perceived. These may possibly correspond with those imperfect contractions which have been already noticed in the heart. The appearance of this patient's neck is very singular, and the pulsation of the veins is of a kind which we have never before witnessed.

He has had scarcely any of the cardiac attacks since he was discharged; he refers the premonitory sensations to the right supra-clavicular region, but states that he has often experienced them without any loss of consciousness following.—*The Dublin Quarterly Journal of Medical Science, for August*, 1846.

3.—*Diseases of the Respiratory System. On the Fallacies attending Physical Diagnosis in Diseases of the Chest.* By THOMAS ADDISON, M. D.

(*Guy's Hospital Reports*, October, 1846.)

[Ranking's Abstract.]

[As Dr. Addison justly remarks in his introduction to the following communication, there is no one thing which has been more prejudicial to the study of auscultation than the overweening importance and assumption of infallibility attached to it by some of its ill-judged advocates. Great as the assistance undoubtedly is which it affords as an adjunct to the investigation of the rational signs of disease, it is indisputable that even in the hands of a proficient the stethoscope will often lead to mistakes in diagnosis if it be exclusively relied upon, to the neglect of the information to be derived from other sources. To strip the stethoscope of the extravagant and meretricious pretensions thrust upon it by injudicious friends, to make a candid acknowledgment of the manifold difficulties to be encountered in its employment, is Dr. Addison's aim, and although we are of the number of those who look upon a knowledge of auscultation as indispensable to make not only an accomplished but even mediocre physician, we are glad of the opportunity of laying before our readers so clear and candid a statement of the fallacies to which it is occasionally subservient.

The author has adopted the method of enunciating certain propositions, the principal of which are thus exhibited:]

1. It is well known that many persons while under examination entirely fail to perform the respiratory act efficiently either from nervousness, or from mistaking the manner of accomplishing it. This may lead to an erroneous belief that the respiratory murmur is deficient, or even absent, while the lungs are perfectly healthy.

This source of fallacy is avoided by desiring the patient to cough, and to inspire deeply, so as to cough a second time. This done on both sides of the chest, the actual state of either lung may be ascertained with tolerable precision.

2. Whatever lessens the freedom, mobility, or elasticity of the ribs, renders the sound on percussion more dull. Hence it is that in rickety persons, where deformity of the chest has taken place subsequent to birth, the signs furnished by percussion are often extremely unsatisfactory; and, indeed, under such circumstances, neither percussion, nor in many instances auscultation, can be much relied upon.

3. Some persons with actual deformity have naturally such fixedness of the ribs, that they at all times manifest very imperfect resonance, as well as considerable feebleness of the respiratory murmur.

4. The rigidity of the cartilages of the ribs in advanced life has a similar effect; and, moreover, often tends to throw obscurity over hypertrophy of the heart by preventing the usual heaving of the ribs at each systole of the hypertrophied organ.

5. When exploring the chest in a case of recent disease, we may be misled by the permanent effect of an ancient pleurisy.

[The physical signs of old pleuritic contraction independent of deformity palpable to the senses, are stated to be, "dullness on percussion, feebleness or absence of the respiratory murmur, constrained movements of the ribs, dry crepitation, and harsh sonorous râle during inspiration." The value of these signs is much increased by their incompatibility with the recent history of the case.]

6. When, as usually happens, rickety deformity of the chest consists in lateral flattening of the ribs, with projection of the sternum, the action of the heart is liable to beat with such violence, and over so diffused a space, as to lead to the unfounded apprehension of organic disease of the organ.

7. The dullness on percussion caused by pushing up of the diaphragm by an enlarged liver, or fluid in the peritoneum, is liable to be mistaken for dullness caused by fluid in the pleura.

8. Bronchitis is a frequent source of fallacy, it may greatly obscure pneumonia, phthisis, and pleurisy, as well as other chronic diseases of the organ.

9. When the bronchitic complication of phthisis is considerable we often fail to detect some or all of the physical signs of the latter, such as dullness on percussion, tubular respiration, and even bronchophony and pectoriloquy. This is more especially the case in the earlier stages.

[It is under these circumstances, Dr. Addison observes, that the too exclusive stethoscopist is liable to be beaten in diagnosis by those who altogether reject physical examination, but the former will obviously have the advantage if he institutes, likewise, a careful inquiry into the history of the patient, and the rational signs of his disease. According to Dr. Addison, the individual symptom which, above all others, should lead to the apprehension of tubercular disease, is occasional slight hæmoptysis just sufficient to tinge the sputa.]

10. Dullness of sound on percussion, tubular respiration, bronchophony, pectoriloquy, and gurgling, are not necessarily conclusive of phthisis. All these signs may result from changes induced by a former pleurisy, from pleuropneumonia, or whooping-cough, [? Ed.] or even from recent pneumonia or pleurisy associated with considerable bronchitis.

[Cases are given in illustration of this proposition.]

11. When, in phthisis, the larynx is so involved as to impede the entrance of air and give rise to permanent sonorous rale in the tube, the reverberation of this rale through the entire chest is apt to lead to the erroneous suspicion of disease in the lungs.

12. Complete loss of voice from disease of the larynx almost completely nullifies the results of auscultation.

13. The existence of a cavity may be overlooked if the bronchial tubes leading into it are plugged with mucus.

In every case of suspected phthisis the patient should be made to breathe and cough with violence; this will dislodge mucosities and render the existence of a cavity perceptible.

14. A patient may have all the rational signs of incipient phthisis while auscultation does not reveal any change in the lungs.

Similar symptoms may arise from relaxed uvula, and in hysteria.

15. Dilated bronchial tubes surrounded by indurated pulmonary tissue, cannot be distinguished from phthisical lesion by auscultation alone, especially if situated in apices of the lungs.

In such cases the diagnosis is chiefly formed by the history of the case.

16. Malignant disease of the lungs cannot be distinguished from other lesions by auscultation alone.

17. If acute pneumonia have proceeded to complete hepatization when we first examine the patient, the physical signs are frequently insufficient to distinguish it from tubercular consolidation, or ancient pulmonic induration. This is especially the case if the apex of the lung be the seat of the induration.

18. Pneumonia may occur without cough, and so closely resemble simple continued fever that both the stethoscopist and the non-stethoscopist are apt to be deceived.

In such a case the stethoscopist has infinitely the advantage, and will rarely fail to detect pneumonia by the physical signs.

19. When the anterior and inferior portion of the left lung is consolidated by pneumonia, it may not be detected by percussion on account of the proximity of a flatulent stomach. Under similar circumstances a marked amphoric respiration is produced, with metallic tinkling, leading to the erroneous conclusion that pneumo-thorax is present.

The respiration acquires its amphoric character by reverberating through the solid parts to the inflated stomach or bowels.

20. It cannot be determined by physical examination whether pneumonia have or have not supervened upon tubercles, although the prognosis in the two cases would be very different.

21. I doubt whether physical examination can in any instance determine, with certainty, the existence of simple tubercles in the lungs.

22. When serous effusion is very considerable, giving rise to unequivocal bronchophony, tubular respiration, and want of resonance and vocal vibration, physical examination has repeatedly led to a mistaken belief that these signs resulted from pneumonic or other consolidation of the lung.

23. When a patient presents himself with febrile affection of any kind, we may, on examination, detect dullness on percussion, tubular respiration, bronchophony, and a rale not distinguishable from the submucus crepitation commonly observed in pneumonic hepatization; and yet physical examination should not enable us to determine whether the chest affection be recent or of ancient date. When a portion of lung has been compressed by pleuritic effusion, and has been prevented from expanding again by adhesions, the physical signs may remain permanently, and be found to resemble precisely those which result from recent pleuro-pneumonia.

24. Experience leads me to the conclusion that pleuritic friction-sound cannot in all cases be distinguished from the rubbing produced between the inflamed peritoneal surfaces of the liver and diaphragm; neither can the croak-

ing sounds produced in the bronchi be always distinguished from the pleuritic rub.

25. As simple pericarditis is rarely attended with pain, and as the other symptoms of that disease are equivocal, the physical signs are chiefly to be relied upon in forming a diagnosis. Nevertheless, when effusion has taken place to a certain amount the friction-sound commonly disappears, and auscultation fails to recognise the disease.

26. Enormous accumulations of fluid in the pericardium cannot always be distinguished from effusion into the cavity of the pleura.

27. When the pericardial friction-sound is single, auscultation may fail to distinguish it from a valvular murmur, especially if it be situated over the region of the valves.

28. The double pericardial friction-sound may be confounded with the see-saw murmur of imperfect aortic valves, and vice versa.

This question may be almost decided by the characters of the pulse alone.

29. A sound closely resembling a murmur appears sometimes to be produced by the stroke of the heart against a portion of lung interposed between it and the parietes of the chest. Under such circumstances auscultation may lead to the erroneous conclusion that the heart is diseased.

This sound is most commonly heard at some point at the edge of the left lung, and resembles the *bruit de rape*. It may occasionally be made to disappear by a deep sustained inspiration. The author thinks it may be identical with the sound described by Dr. Latham as present in phthisis.

30. Auscultation fails to distinguish an aortic murmur depending on organic change from one which results from other causes; neither can it decide whether what has been called a mitral murmur is organic or functional.

31. In certain diseases of the heart it is difficult or impossible to localise the murmurs with accuracy, however pronounced they may be.

32. Auscultation cannot distinguish the murmur of an aneurismal artery from the murmur produced by external pressure upon the vessel.

33. Physical examination does not enable us to distinguish congenital malformation from disease of the heart or large vessels. (pp. 1–36.)

AMERICAN MEDICAL INTELLIGENCE.

1.—*Mortality following the Operation of tying the Iliac Arteries.*

(In the January No. of the American Journal of the Medical Sciences, we find an exceedingly interesting article on this subject from the pen of Dr. G. W. Norris, of Philadelphia, one of the Surgeons of the Pennsylvania Hospital. It consists of a table comprising most valuable statistics in reference to this important operation, but we regret that we cannot make room for it. We must content ourselves with the synopsis and preparatory remarks of the author. Dr. Norris is himself a highly talented and promising surgeon, and by these researches displays a most commendable acquaintance with what has been done in that department of medical science.)

"In the number of this Journal for July, 1845, I published a paper exhibiting the mortality following the operation of tying the subclavian artery, in which some of the difficulties of that operation—mistakes in diagnosis—accidents following it, and the causes of death, were principally dwelt upon. The tables now given were drawn out at the same time, and on a plan similar to that just mentioned, and will be found to include most of the recorded cases in which a ligature has been applied to the iliac vessels, either for the cure of disease, or the suppression of hemorrhage. In this as in all similar tables, the actual results are, I have no doubt, less favourable than they appear to be—unfortunate cases being, as is well known, less generally reported than successful ones, and in those recorded, any one who attempts to collect materials of a like character to those here presented, will have often to lament, that the comparatively unimportant steps of a simple procedure, are given and commented upon with great minuteness, while the dangers attendant upon it—the difficulties of diagnosis—and post-mortem appearances, are either passed slightingly over, or altogether omitted.

"*Mortality.*—Of the one hundred and eighteen cases included in the table, eighty-five recovered, and thirty-three died—three of the patients who recovered, undergoing amputation in consequence of gangrene of the limbs.

"*Sex.*—Of one hundred and thirteen cases in which the sex is noted, one hundred and seven were males, and six females. Of the six females, five laboured under aneurisms, and one had secondary hemorrhage.

"*Right or left side*—Of seventy-nine cases in which the affected side is noticed, forty-four were on the right, and thirty-five on the left side.

"*Age.*—The age is given in ninety-nine of the cases, of which number there were under

	20			4
between	20	and	30	23
"	30	"	40	32
"	40	"	50	25
"	50	"	60	11
"	60	"	70	3
above	70			1
				99

"*Disease or injury.*—Of the one hundred and eighteen cases of operations given in the table, ninety-seven were done for the cure of aneurisms, eighteen in consequence of wounds or secondary hemorrhages, and three for the cure of varicose aneurisms. In four of the ninety-seven cases of aneurism, that disease existed simultaneously in both the ham and the front of the thigh, and in three of these the operation succeeded in curing both tumours.

"*Period the ligature separated.*—This is noted in seventy-eight cases, in forty-four of which the ligature came away before the twentieth day, in twenty-four, between the twentieth and thirtieth days; in seven, between the thirtieth and fortieth days; and in three beyond the fortieth day. The earliest period at which the ligature came away, was the tenth day, and the longest time to which it remained, was the sixty-second day.

"*Return of pulsation in the tumour after the application of the ligature.*—This occurred in nine cases. In one of these, (No. 18,) evident pulsation was noticed in the sac on the fifth day, which gradually ceased at the end of ten or twelve days, the patient recovering. In the second case, (No. 20,) pulsation returned in the tumour more than two months after the operation, and after a time ceased. In the third, (No. 23,) aneurisms existed both in the popliteal and inguinal regions. Pulsation in the ham entirely ceased upon the application of the ligature, but continued, though feebly, in the inguinal tumour; both aneurisms were ultimately cured. In the fourth, (No. 30.) pulsation reappeared, and remained till the forty-fourth day, the patient recovering. In the fifth, (No. 49,) slight pulsation was observed a few hours after the operation, and on the following day was so considerable, that compression was made on the artery, and kept up so as to arrest it; the cure being complete. In the sixth, (No. 90,) which was a varicose inguinal aneurism, circulation through the tumour was observed to have returned on the third day, and the patient died after repeated hemorrhages, on the fifty-fourth day. In the seventh, (No. 94,) slight pulsation was noticed on the day following the operation, and the patient died on the fifth day of hemorrhage. In the eighth, (No. 104,) both femoral and popliteal aneurisms existed. On the day after the operation, both tumours were solid; no pulsation was perceived in the ham, but a slight tremulous motion was noticed in the groin. From this period, pulsation increased in the groin, and after a few days returned in the ham also, where, however, it soon entirely ceased.—At the date of the report of the case, (about six months after the ligature of the vessel,) it continued in the groin, though very feebly. In the ninth, (No. 110,) the operation was done in the month of May, and the patient discharged cured in August, but was re-admitted in the following November, with a return of pulsation in the tumour. Pressure was employed for two months, after which he was again looked upon as cured. In November, 1841, a recurrence of pulsation was again noticed. In January, 1842, all pulsation had ceased, but the tumour increased. In January, 1843, it became stationary, and some time after began to diminish in size, and so continued to do till July of the same year, when he died of phthisis. On post-mortem examination, the tumour was found to be connected with the superficial femoral artery immediately below its origin, was of the size of a full-grown fœtal head, and perfectly solid.

"*Hemorrhage after the operation.*—This is stated to have occurred in fourteen cases; of these, seven died, and seven were cured. In Nos. 1, 79, 94 and 114, it took place on the fifth day, and in them all proved fatal. In the fourth and fifth cases, (Nos. 25 and 99,) hemorrhage once occurred in each, but the patients did well. In the sixth, (No. 30,) a considerable quantity of arterial blood escaped from the wound from the twenty-fourth to the thirtieth day, which was believed to come from the inferior end of the artery, and was successfully arrested by compression. In the seventh, (No. 34,) which was a case of hemorrhage from the upper and outer part of the thigh, the bleeding continued after the external iliac had been secured, and a ligature was placed on the femoral, which restrained it The limb afterwards mortified, and was amputated, the patient doing well. In the eighth case, (No. 42,) the hemorrhage occurred at several intervals between the twenty-fourth and forty-third days after the operation, when a second ligature was placed on the vessel higher up than the first, but without success. In the ninth, (No 58,) repeated hemorrhages occurred after the nineteenth day, and the patient died. In the tenth, (No. 77,) the artery was ligatured on the 22d of August, for hemorrhage following amputation; on the 28th, bleeding took place from the groin, which was restrained by pressure with a truss, and the patient cured. In the eleventh, (No. 78,) the vessel was also tied for hemorrhage after amputation; ligature of the femoral having been first tried ineffectually. On the day following the operation, there was a slight return, which was arrested by pressure. In the twelfth case, (No. 90,) which was one of varicose aneurism, it took place on the fortieth day after the application of the ligature from the inferior end of the wound. On the forty-third day there was another frightful return of it, when the aneurismal tumour was laid open with a view of tying all the bleeding vessels; the loss of blood when this was done, was such as to make the operator fear the man would die on the table; he, however, lived eleven days. In the thirteenth, (No. 96.) the artery was tied to arrest hemorrhage from a stump, and more than six weeks after it, in consequence of a return of bleeding, it was again secured higher up than at first, and the patient cured. In the fourteenth, (No. 114,) it occurred on the seventeenth day, and proved fatal.

"*Suppuration of the sac.*—In ten cases, the tumour is stated to have suppurated after the operation, all of which did well. In one of these, (No. 16,) the integuments were in a state of mortification at the time of the operation, and on the twenty-third day after it, an incision was made into the tumour, and its contents evacuated. In another, (No. 75,) the tumour, which is stated to have been "of enormous size," suppurated and was punctured on the 25th of March. On the 31st and 2nd of April, hemorrhages occurred from it, and a fruitless effort was made to take up the profunda. On the 12th, the bleeding was renewed, and the actual cautery was applied. On the 14th, "apprehensive of another hemorrhage," an effort was made to tie the internal circumflex without success, and the actual cautery was again resorted to. In a third, (No. 95,) where the ligature had been applied close to the bifurcation of the common iliac, the tumour discharged itself through the wound on the twenty-first day, and in No. 113 the same occurrence had taken place on the twenty-ninth day.

"*Gangrene of the limb.*—This occurred in sixteen out of the one hundred and eighteen cases; three of which were cured after amputation, and twelve died. In one case, (No. 31,) the artery was tied on the 22d of January, mortification followed, which extended to the thigh, and on the 9th of February amputation was done close to the trochanter. In a second, (No. 34,) the ligature was applied August 27th, and the limb was amputated September 26th. In both of these cases the vessel was secured to restrain hemorrhage. In the third case, (No. 101,) the operation was done for femoral aneurism on the 20th of February. The ligature separated on the twenty-second day, and about a week after this occurred, gangrene was observed in the toe, and gradually extended up to within a short distance of the knee, where the limb was removed. These amputations were all successful. In one instance, (No. 61,) slight sloughing of the sole of the foot occurred from a bottle of hot water applied to the part.

"*Cause of death.*—Of the one hundred and eighteen cases, thirty-three died. Of these, six died from hemorrhage; three, from sloughing of the sac; thirteen, from mortification of the limb; one, from the bursting of an aneurism of the aorta at its bifurcation, ten weeks and six days after the operation; two, on the third and fifth days from prostration; two, of peritonitis; two of tetanus; one, on the eleventh day of some affection of the chest—probably diseased heart; one, on the second day, of delirium tremens; one, of diffuse inflammation, and in one, the cause is not noted.

"*Difficulties of, and accidents during the operation.*—In two instances, (Nos. 20 and 51,) the peritoneum was wounded in the operation. Both patients recovered. In one, (No. 74,) the sac was accidentally wounded after the ligature was applied. In one, (No. 91,) a vein—the circumflex ilii—was a source of much embarrassment to the operator, who gives with his case a plate representing it.

"*Mistakes in diagnosis.*—In four of the cases given in the table, (Nos. 22, 40, 67 and 88,) the tumours had been mistaken for abscesses and opened previous to the operation. Of these none recovered. In No. 106, the aneurism which followed a gun-shot wound, was supposed to arise from a wound of the femoral. Upon examination it was found that the ball did not pierce the fascia lata, but had passed altogether in the sub-cutaneous fat, and that the only vessel wounded was a superficial branch of the femoral artery, which was divided close under Poupart's ligament and nearly an inch from the main trunk.

"Mr. Ferguson* mentions that he has seen "a most experienced and judicious surgeon cut through the parietes of the abdomen with the intention of tying the external iliac artery for a supposed aneurism where none existed." This case is, I presume, that which has been recorded by Mr. Syme in one of hi Surgical Reports in the Edinburgh Journal, and is well calculated to show the difficulty of diagnosis which is sometimes met with. The tumour which was stated to have followed a mis-step made some eight months before his presenting himself for examination, was situated in the right iliac region of a man aged 54. It was tense, pulsated obscurely throughout its whole extent, and offered a distinct bellows sound upon the application of the stethoscope. Believing that an aneurism existed, Mr. S. made an incision into the abdomen, six inches in length, with the intention of securing the external or common iliac, but when exposed, the tumour was found to be composed of a solid cerebriform

* Elements of Surgery, p. 135. Amer. edit., 1845.

mass, and was taken away entire. Seven days after it the patient died, and on dissection, a chain of tumours similar in nature was found surrounding the great vessels on both sides.

Internal Iliac Artery.—I am aware of only seven instances in which this vessel has been ligatured. These were by Stevens, Atkinson, Thomas, White, Arendt, Mott, and J. K. Rodgers. Of these, three died, and four were cured. In all of them the operation was done for the cure of aneurisms, and in one of them, (Mott,) although the peritoneum was opened in the efforts to separate it from the parts beneath, yet the patient did well.

Common Iliac Artery.—Fifteen cases are recorded in which the operation of tying the primitive iliac has been performed. The first case in which it was done, was that of a gun-shot wound, in 1812, by Dr. W. Gibson, of this city. The patient died from peritoneal inflammation and secondary hemorrhage on the thirteenth day. The second case was that of Dr. Mott, for aneurism, in 1827, and was fully successful. The third was in 1828, by Mr. Crampton, also for aneurism, and was unsuccessful, the patient dying on the fourth day from hemorrhage. The fourth case was that of a boy, aged eight, in whom the common iliac was tied by Mr. Liston, in 1829, in consequence of secondary hemorrhage after amputation, and was unsuccessful. The fifth was the case of a lady operated on in 1833, by Mr. Guthrie, for a tumour on the right nates as large as an adult head, which presented so decidedly the characters of aneurism that it was believed to be so by Mr G., as well as by Sir Astley Cooper and others, who examined it. Pulsation was manifest in every part of it, and "on putting the ear to it, the whizzing sound attendant on the flowing of blood into an aneurism could be very decidedly heard." Diminution of the tumour to the extent of one-half followed, and recovery from the operation was complete. Five months after it the tumour again enlarged, and she gradually sunk. On post-mortem examination, eight months after the operation, the arteries were found to be perfectly healthy and the tumour to consist of cerebriform matter. The sixth was by Mr. Salomon, of St. Petersburg, in 1837. The cure was deemed perfect, the tumour almost disappeared, and the free use of the limb was restored. Ten months after, the patient is stated to have taken cold, and had an abscess to form upon the affected side, which was opened just below Poupart's ligament. He died shortly after, worn out by the suppuration. The seventh case was that of Mr. Syme, in 1838, for aneurism, and was unsuccessful, the patient dying on the fourth day. The eighth was by Deguise of Paris, in 1840, and proved successful, despite three serious accidents which happened during its performance, viz., the wounding of the sac, the giving way of the vessel under a first ligature, and the wounding of the femoral vein in the taking up of the artery of that name which was done at the same time to prevent secondary hemorrhage. The ninth was in a case of aneurism, aged twenty, which occurred to Dr. Post, of New York, in 1840. The symptoms here were deceptive, and it being judged that deep fluctuation was present, an explorative incision was made into it. On the following night there was a sudden gush of arterial blood, which was arrested by compression, and the day after, a ligature was applied to the common iliac by cutting through the peritoneum, the tumour extending so high up that it was thought impracticable to expose the vessel without it. The patient sunk twenty-four hours after the operation, from exhaustion and loss of blood. The tenth case was that of a female child, aged two months, in whom the artery was secured by the late Dr. Bushe, on account of a large aneurism by anastomosis of the left labium. The child lived five weeks. In the eleventh case the ligature was applied by Dr. Perogoff on account of hemorrhage after the removal of a ligature from the external iliac, which had been applied for the cure of aneurism. In exposing the vessel, the peritoneum which was adherent, was torn through. The wound became gangrenous, and on the eleventh day, fecal matter was discharged through it. On the fourteenth day hemorrhage occurred, and the patient died twenty-four hours after it. The twelfth case was that of my colleague at the Pennsylvania Hospital, Dr. Peace.

The operation was done in August, 1842. The tumour which extended from three inches below, to the same distance above Poupart's ligament, had become, five months after the operation, reduced to the size of a filbert, and was perfectly hard. On the 13th of November, 1843, he presented himself for re-admission at the hospital, and stated that after being last seen he had returned to his employment--that of loading boats with stone--and had continued perfectly well and able to work up to within two weeks, when his attention was directed to a reappearance of his tumour. Upon examination it was found to be of the size of a small orange, was soft, entirely free from pulsation, presenting evident marks of fluctuation, and the skin covering it discoloured. A few days afterwards an opening took place in it and was followed by considerable hemorrhage, which was arrested by compression. Several recurrences of this followed, and he died on the 24th.* The thirteenth case was an aneurism operated on by Mr. R. Hey in 1843, and was successful. The fourteenth is the interesting case of supposed aneurism which has been lately published by Mr. Stanley.--On applying the ear over the abdominal parietes a bellows sound in the tumour was plainly recognized. Compression applied to the femoral artery below the tumour produced enlargement of it, but when made upon the aorta all pulsation was arrested; after the application of the ligature to the common iliac, pulsation ceased. Death occurred on the third day from peritonitis. Upon dissection the arteries were found to have no connection with the tumour, which was composed of medullary matter. Tumours of a similar character were found in the heart and lungs, and one of the size of an orange occupied the middle and inner side of the arm, which during life was observed to be free from pulsation or pain, and was said to have existed for several years. The fifteenth and sixteenth cases are those which have been published by Dr. Garviso of Monte Video. The first which occurred in 1837, was an aneurism of the external iliac which extended from the pubis to the umbilicus, and was of the size of an adult's head. An eschar which had formed on the tumour had commenced to separate and gave rise to abundant hemorrhage immediately previous to the performance of the operation. From the size and situation of the tumour the cavity of the peritoneum was necessarily opened. Death followed in four hours. The second of his cases--also an aneurism--was in 1843. The incision was made with a view of securing the external iliac, but the disease was found to extend so high up that the common trunk was tied. The ligature separated on the thirty-sixth day, and the patient recovered. Total, sixteen cases, of which eight may be said to have been successful, and eight died.

2.—*Case of sudden formation of Hydrocele unconnected with the development of Inflammation in the Tunica Vaginalis Testis.* By HENRY H. SMITH, M. D.

In the various treatises upon hydrocele, with which the profession have at different periods been favoured, it has generally been admitted that more or less time was necessary to its formation. In the definition, even of acute hydrocele, the term has been limited "to those cases which are developed in the space of a few days, and are accompanied by inflammatory symptoms in the scrotum."† Without, then, discussing the correctness of this opinion, I offer the following case, not only as an exception to the rule, but in the hope that it may assist some one hereafter in the diagnosis of what are often difficult cases, to wit, scrotal tumours.

Hugh G——, æt. 29, married about nine months, of fine muscular development, in perfect health, and by occupation a porter, was struck upon the scrotum at 7 P. M. of March 25th, 1846, by the end of a barrel of flour, which slipped

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* A particular account of this case, after his re-admission into the hospital, accompanied by the post-mortem appearances, will be given by Dr. Peace in a forthcoming number of this Journal.

† Velpeau, Leçons Orales, par Pavillion.

against him whilst endeavouring to load a wagon, at the close of a hard day's work. The pain at the moment was so severe as to cause him to faint, and on reviving he found, to use his own expression, "that something had burst, and that he had a large tumour in his bag which prevented his closing his thighs."

He was immediately sent home, and at 8½ P. M. I saw him. He was then in bed, complaining of faintness; of pain in the testicles, especially the left one, and also in the small of his back. Between his closed thighs lay a tumour 21 inches in circumference, seated in the scrotum, but most developed on the left side. The skin of the scrotum was smooth and shining on the left, but wrinkled on the right side, and on the middle of the left side, about two inches from the external abdominal ring, and extending round towards the perineum, was an indented, discoloured mark, as if made by the end of the barrel.

The tumour was globular or nearly so; quite tense, but with an indefinite sense of fluctuation; largest below, and tolerably transparent when examined with a candle. The left testicle could be felt at the posterior and middle part of the swelling, enlarged and extremely painful. The left cord seemed thickened and indefinite, up to the external ring. On the right side of the scrotum there was no mark; no sense of fluctuation and no transparency to the candle; but, it seemed to contain a thick tenacious mass, like omentum. The right testicle was also painful, but not enlarged. The right cord was apparently surrounded by the mass of omentum.

The patient stated that up to the moment of his accident he was in perfect health, but that he had passed no urine for about four hours previous to, or since the accident, and that his bowels had not been opened since the day before.—From these circumstances, I concluded that he either had a hernia, or that his bladder or urethra had been ruptured by the blow upon the perineum. Being, however, very doubtful as to my diagnosis, and unable from want of instruments to verify the state of his bladder, I requested a consultation, and at 10 P. M. was met by Dr. Horner, of the University of Pennsylvania. A catheter being passed into the bladder proved the soundness of this viscus and drew off about one pint of clear urine, whilst a further examination of the parts led to the belief that the swelling on the right side was an effusion into the dartos, and that the tumour on the left contained fluid.

I proceeded, therefore, to open the latter, by making a careful dissection of the tunics of the scrotum, as in hernia, until I came to a serous membrane.—This I opened cautiously, lest it should prove to be the sac, and gave exit to about a pint and a half of pure serum brightly tinged with blood. This being evacuated, the position of the parts was easily recognized, and the effusion found to be limited to the tunica vaginalis testis. There being no other indications, the wound was kept open and the testicles ordered to be leeched, &c.

For about eight days after this, matter escaped from the cavity, most of which was healthy pus, but, under the use of pressure from adhesive strips and the antiphlogistic treatment generally, he was subsequently relieved, although not able to attend to his store until the forty-eighth day after the accident. At this time, the left testicle was about twice its ordinary size, though not otherwise inconvenient.

On October 29th I was again consulted by this patient on account of a strain that he had received in his knee the night previous. This had been directly followed by a swelling which I found to be a regular hydrops articuli. Under the ordinary treatment he was soon relieved.

On *Nov. 4th*, I again examined his scrotum, and found the left testicle yet about one-third larger than natural, but evidently owing to the thickness of its external coat. The right one was of the natural size.

After a careful examination of such works as I deemed likely to assist me, I have yet been unable to find a case analogous to this one. The nearest are those cited by Mr. Pott, of hematocele instantly following blows and violent efforts.* I can therefore only speculate as to the manner in which the above effusion could have occurred.

From an attendance on his wife a few weeks previous to the accident; from his own statement; and, from various incidental circumstances, I have every reason to think that his generative organs were perfectly sound. The effusion, therefore, was not a gradual one or connected with orchitis. It could not have been the result of inflammation, from the blow, because the interval between the accident and the swelling was too short. It was not owing to the rupture of a vessel, because the liquid was serum tinted with blood and not an hematocele. There was no hernia; hernial sac; rupture of urethra or bladder, nor was there any abdominal tumour, enlarged gland, or chronic induration of the parts above.

It may therefore be presumed that the effusion was owing, First, to a peculiarity of the patient's system, as shown by the sudden effusion in the injury to another part; and Second, that the blow created it, in a way similar to the welt or swelling of the skin made by the cut of a whip; or the effusion of serum resulting from a burn.

PHILADELPHIA, *Nov. 20th*, 1846.

The American Journal of Medical Sciences, Jan., 1847.

3.—*Statistics of Private Obstetric Practice.* By JAMES C. BLISS, M. D.

It will, I trust, be a matter of interest to have presented to the readers of the Journal, the Statistics of a number of cases in Private Obstetric Practice, the results of which may serve as a basis for some practical deductions. I am not aware that anything of the kind has been given to the public in this country; and our public Institutions, all comparatively in their infancy, and have not as yet been made available for this purpose. In presenting these details I am aware that the materials are too meagre to give that certainty to general results, which are to be derived from the more ample and abundant means afforded by the large Institutions of Europe; and which might be obtained also, from a wider field of observation in private practice; but it may be the means of directing the profession to this field of enquiry, and aid, to some extent, the researches of any who may be led hereafter to pursue the subject on a more extended scale.

Nothing can be more unphilosophical, and more likely to bewilder, and lead to error in the practice of medicine, than modes of treatment founded on statistical results. It is calculated to retard legitimate inquiries, and the progress of sound medical science; and it is not a little remarkable, that some of the most distinguished and popular members of the profession in our day, have deserted the only safe and rational path of investigation, to deduce systems of practice from principles so extremely fallacious. But while there are conclusive objections to our attempting to arrive at truth in the practice of medicine in this way, the same does not hold good in the obstetric art. Many of our rules of practice, if not founded upon, are at least confirmed by these investigations.—The value of the excellent work of Dr. Collins, of Dublin, is greatly enhanced by the very precise, accurate, and elaborate details and tables contained in his volume. It has been through the influence of this example of Dr. Collins, that I have been led to prepare this paper. The facts contained in such inquiries have a bearing on many branches of human knowledge, and when little items serve to swell the aggregate, they are not without their importance; and in this point of view, the motives for presenting this paper will be appreciated.

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* Treatise on Hydrocele, page 104.

I have made a record, more or less full, of *Eight hundred and twenty* cases of delivery. Of this number, 815 were born in the following months, viz.,

In the month of	January,	–	52
" "	February,	–	63
" "	March,	–	74
" "	April,	–	57
" "	May,	–	62
" "	June,	–	71
" "	July,	–	65
" "	August,	–	81
" "	September,	–	75
" "	October,	–	80
" "	November,	–	72
" "	December,	–	63

It will appear from this table, that the greatest number of births occurred in the month of August, and the fewest in January. In the former month they amounted to 81, while in the latter there were but 52. In the month of October there were 80, in September 75, and March 74. These facts show that the greatest number of conceptions occurred in the months of December, February, January, July, March, and October; and in the order in which they are named.

The sex of *Seven hundred and ninety-seven* children has been noted. Of this number 395 were males and 402 females. This shows a summing up considerably at variance with most reports, the number of male children generally preponderating from three to ten per cent. over the other sex. According to the registry of Dr. Collins, a little more than 8-15ths of the total births were males.

The presentation in *Seven hundred and seventy-one* deliveries is recorded. Of this number there were:

Natural presentations,	–	738
Breech "	–	15
Funis & Breech "	–	1
The foot "	–	8
The face "	–	1
The face towards the pubis		7
The arm and abdomen		1

It will appear from this, that in about one case in 22 the presentation was preternatural. The proportion in the Dublin Lying-in Hospital, according to Dr. Collins, was one to every thirty, but it will be remarked that in his Treatise he does not include presentations of the face, and the face towards the pubis, among preternatural presentations. Should these be included, the proportions will not probably very materially differ.

Of the 820 cases, *forty* are noted as being premature births. That is, about one in twenty were born before the full period of pregnancy was completed.

Out of the same number, viz., 820, *twenty-one* were born dead. This shows a small fraction over one still-birth to every thirty-nine deliveries. In the Dublin Lying-in Hospital, a little short of every 15th child was born dead. This discrepancy between hospital and private practice is doubtless owing to the difference in condition and character of the patients.

The Funis was around the neck once	in 127	cases.
" " " twice	in 21	"
" " " 3 times	in 6	"
" " " four	in 1	"

A little over every fifth child, according to his record, had the cord around the neck at birth.

In *seventy-nine* cases of the 820, *the membranes broke* before or at the accession of labor. The duration of labor, in seventy-five of these cases, was as follows, viz :

One hour	1	Twelve hours	9
Two "	2	Thirteen "	1
Three "	4	Fourteen "	3
Three and a half	1	Fifteen "	1
Four hours	6	Sixteen "	2
Four and a half	1	Eighteen "	2
Five hours	8	Twenty "	3
Six "	4	Twenty-four	2
Six and a half	1	Thirty "	1
Seven hours	3	Thirty-two	1
Eight "	3	Thirty-four	1
Nine "	2	Thirty-six "	3
Ten "	1	Forty-three	1

The average duration of labor, under these circumstances, 10$\frac{3}{4}$ hours. This shows that this occurrence did not very materially retard the progress of parturition.

The pregnancies of 690 cases are recorded.

Of the	First	pregnancy	there were	184	cases.
"	Second	"	"	138	"
"	Third	"	"	116	"
"	Fourth	"	"	78	"
"	Fifth	"	"	60	"
"	Sixth	"	"	26	"
"	Seventh	"	"	32	"
"	Eighth	"	"	10	"
"	Ninth	"	"	14	"
"	Tenth	"	"	12	"
"	Eleventh	"	"	10	"
"	Twelfth	"	"	5	"
"	Thirteenth	"	"	5	"

This shows that a fraction over every third birth, was the		first child.	
Every fifth	"	second	"
A little short of every sixth	"	third	"
A little short of every ninth	"	fourth	"
Every eleventh and 5-10	"	fifth	"
" Twenty-six and $\frac{1}{2}$	"	sixth	"
" Twenty-first and $\frac{1}{2}$	"	seventh	"
" Sixty-ninth	"	eighth	"
" Forty-ninth	"	ninth	"
" Fifty-eighth	"	tenth	"
" Sixty-ninth	"	eleventh	"
One hundred and thirty-eighth		twelfth	"
One hundred and thirty-eighth was also the		thirteenth	"

Of the whole number, two only of the Fœtuses were *monsters*.

There were, out of the whole number, *eight twin births*. This shows a proportion much less than occurs in the Lying-in Hospitals in Europe. The number, as above stated, is *one* to every *one hundred and two* cases, while in the European reports it is as follows, viz :

In France	one to every	ninety-five	births
In Germany	"	eighty	"
In England	"	ninety-two	"
In Scotland	"	ninety-five	"
In Ireland	"	sixty-three	"

See Collins' Treatise, Am. Ed., p. 153.

The duration of labor is noted in 809 cases, and is as follows, viz:

Ten minutes	1 case	11 hours	4 cases
$\frac{3}{4}$ of an hour	1 "	12 "	13 "
1 "	3 cases	13 "	3 "
$1\frac{1}{2}$ "	4 "	14 "	14 "
2 hours	4 "	15 "	1 "
$2\frac{1}{2}$ "	4 "	16 "	2 "
3 "	6 "	17 "	4 "
$3\frac{1}{2}$ "	5 "	18 "	6 "
4 "	13 "	19 "	2 "
5 "	8 "	21 "	1 "
6 "	18 "	24 "	5 "
$6\frac{1}{2}$ "	1 "	26 "	1 "
7 "	4 "	29 "	1 "
$7\frac{1}{2}$ "	1 "	30 "	3 "
8 "	7 "	32 "	1 "
$8\frac{1}{2}$ "	1 "	36 "	1 "
9 "	10 "	37 "	1 "
10 "	3 "	43 "	1 "
$10\frac{1}{2}$ "	1 "	46 "	1 "

The average duration of labor, in these 809 cases, was a fraction over ten hours.

Out of the whole number *three* were cases of *instrumental delivery.* The proportion is *one* to every *two hundred and seventy-third* case. This shows a proportion much less than occurs in the European Hospitals, or in the private practice of those from whom we have reports. An interesting table is given in Dr. Collins' Treatise, to which reference has been so frequently made. The following is the average proportion of instrumental deliveries which he has collected from different sources:

In Dresden, Dr. Carus delivered with instruments	1	in every	13
" Giessen, Dr. Ritger " "	1	"	9
" Berlin, Dr. Kluge " "	1	"	15
" Cologne—Drs. Minden and Merrem delivered with instruments,	1	"	12
" Breslau—Professor Andrée delivered with instruments,	1	"	35
" Heidelberg—Nagelé, " "	1	"	28
" Magdeburg—Dr. Voigtel " "	1	"	5
" Breslau—Dr. Küster, " "	1	"	36
" Marburg—Dr. Casper Siebold, " "	1	"	9
" Vienna—Dr. Boer, " "	1	"	96
" Paris—Mad. Boivin, " "	1	"	183
" London—Dr. Merriman, in private practice, "	1	"	98
" Dr. Bland, Westminster Gen. Dispensary,	1	"	158
" Dr. A. B. Granville, same institution,	1	"	80
" Dublin—Drs. Cusack and Maunsell, Wellesley Disp.,	1	"	34
" Dr. Beatty, New Lying-in Hospital,	1	"	99
" Dr. Joseph Clark, " "	1	"	162
" Dr. Collins, " "	1	"	114.

In the three cases in which instrumental delivery was resorted to, the forceps were used twice, and the perforator and crotchet once.

The first case in which forceps were employed, labor was rendered tedious by narrow pelvis, and the face of the child being towards the pubis. Delivery was accomplished at the end of 33 hours, the child being dead. The mother recovered slowly. An abscess formed around the hip some weeks after her confinement, which was attended with fever and great debility. Her health was ultimately restored, and she subsequently bore a living child without any untoward circumstances.

The second case was that of a lady 42 years of age, with her first child.—She had been attended by a practitioner 81 hours previous to my being called. The head was so far advanced that delivery was readily accomplished. The child was living, notwithstanding the long continued pressure it had suffered. The mother also had a good getting up, and without any unfavorable symptom intervening.

The third case was that of a patient laboring under violent puerperal convulsions, with whom all the usual remedies had been employed without success. Delivery was attempted with forceps, which failed in consequence of violent convulsions being brought on whenever any effort was made with the instrument. The child was delivered by diminishing the head and using the crotchet. The cellular tissue of the patient was enormously distended by dropsical effusion. The patient was not seen by the writer after the birth of the child, she being in charge of another physician, but he understood she died some days afterwards of effusion in the chest.

In the case of presentation of arm and abdomen, the child, which was large, was turned and delivered by the feet. It was born dead. The mother recovered without any unfavorable circumstance following.

Four cases were preceded by *puerperal convulsions*. This is in the proportion of one to every 205 births—while, under the mastership of Dr. Collins, only one in every 555 occurred in the Dublin Lying-in Hospital.

One of the cases is detailed under the head of instrumental deliveries. In every case, it occurred with the first child. In each instance, also, there was infiltration in the cellular membrane. In one case only was the urine examined, and in this it was found to be albuminous. The patients, with the exception of the first noticed, were relieved of the convulsions before delivery, by bleeding, stimulating injections, purgatives, and counter-irritants. The children were all born dead: two were in a state of decomposition, and there was reason to believe all died previous to the attack. The patients all did well, with the exception of the one described under the head of instrumental deliveries. Œdema existed in these cases previous to the occurrence of convulsions, and it was considerable, except in one instance, which does not correspond with the observations of Dr. Dewees.

In the detail of cases in my note book, several are noticed in which convulsions were threatened, in all of which there were bloated countenances, and more or less œdema in other parts of the body. In one of these cases the urine was albuminous. These facts would seem to show that Dr. Lyman, whom Dr. Dewees quotes, is correct in stating that œdema is a precursor of convulsions.

The detection of albumen in the urine of females, threatened with or laboring under this disease, has thrown much light on the pathology of the affection; and further inquiries will probably remove much obscurity, which, no doubt, has led practitioners, in many instances, to confound hysteria with this formidable complaint.

In one of the cases, in which the attack occurred in the seventh month, labor came on, and the fœtus was delivered three days after the convulsions had ceased. The child was in a state of partial decomposition.

The observation, that convulsions more commonly occur in the first labor than in subsequent ones, appears to be confirmed by these cases; and likewise, that they happen more frequently when the child is dead, although the latter observation is questioned by some of our best writers. In all the cases the presentation was natural.

The *placenta* was attached over the mouth of the uterus in *three instances*.—This is in the proportion of one to every 273 cases.

In one instance, the edge of the placenta was over the mouth of the uterus. The patient had repeated hemorrhage for two months previous to delivery, but it was at no time alarming. Labor commenced with profuse flooding, followed by syncope. The vagina was plugged, which arrested the hemorrhage till the head advanced so as to make pressure. The mother was safely delivered, but the child was dead and exceedingly blanched.

In the second case, I was called to a patient who was attended by a popular female. She had been flooding for ten hours,—was in a state of extreme exhaustion, and swooned immediately after I entered her room. The hand was passed up through the edge of the placenta—the feet brought down, and the child delivered in three minutes from the commencement of the operation. The child was born alive, but the mother, although she lost less than a pint of blood at the time, died at the end of an hour and a half after the birth of the child.

In the third case, the patient was taken with flooding on Sunday, but not profuse. On Wednesday, a profuse hemorrhage suddenly occurred. A physician was called in, who found, as he stated, the os uteri dilated to the size of a shilling. When seen an hour afterwards by the writer, the flooding had been so great and the exhaustion so extreme, that the patient was almost moribund. On examination, a small portion of the placenta was found protruding from the mouth of the womb. The feet of the child were brought down and secured by a tape, and attempts made to deliver in this way, but decomposition had made such progress that the feet were immediately separated from the legs; and, notwithstanding the relaxation which had taken place in the mother from the great exhaustion, it was found difficult to effect delivery on account of the condition of the fœtus—the abdomen being very much distended by effused fluid in its cavity. The finger was passed through its parietes, through which opening the fluid rushed out. The finger was then hooked over the pelvis, and in this way the body was brought down and delivered. The patient survived about twenty minutes. The loss of blood was inconsiderable at the time of delivery.

The whole of the cases might be presented in detail with some other particulars not adverted to above, but it would be extending this paper beyond what would be desirable for the Journal. The arrangement and bringing together all the various particulars has cost some labor, but the writer will be amply compensated if it shall lead other members of the profession to give to the public similar inquiries and observations.

There is one thing that perhaps ought to be adverted to in comparing the results given above, with those of public institutions, and that is, that most of the patients were among a class of females who could command the comfort and conveniences of life, and were therefore less exposed to accidents than those less favorably situated.—*New York Journal of Medicine.*

4.—*Case of Idiopathic Tetanus, treated successfully by Strychnine.* By EDWARD VANDERPOOL, M. D., of New York.

New York, 13*th October*, 1846.—Was requested to see Mr. B., aged 58 years, segar merchant. Just one month ago he thought he took cold from sitting in front of his store evenings, from a chilliness creeping up the sides of his face, which continued for several days. This was followed by a sore feeling of the face, near the ears, and inability to extend the jaws, with a difficulty of swallowing. About this time, he commenced to fall backwards when walking, which he would do every few days, from a sudden spasmodic opisthotonos attack. Two days ago he fell suddenly, in the same way, through the lower half of a window, breaking out both the sash and the glass, but without injury to himself.

I found him with tetanic expression of countenance, permanent rigidity of the masseter and temporal muscles, jaws nearly closed—could only show the edge of the tongue at its tip, which was much bitten, by being caught between his teeth whilst dozing—is frequently drawn or, if standing, thrown backwards by spasmodic action of the muscles of the back. Bowels soluble. Pulse 80, and soft. Has taken several cathartic doses of calomel since he felt unwell, and "a number of sweats." Prescribed strychnine in solu., 1-16 grain every two hours.

14th October.—Dr. Fell happening to call on me, I invited him to see the patient. 10 o'clock A. M.—Took only three doses yesterday and one this morning. Found him with a handkerchief tied around his waist from bad feeling in the region of the diaphragm—produced, as he supposed, by a fall an hour before—rigidity of the jaws about the same; his teeth are worn off, to appearance one eighth of an inch—thus allowing for the distance he can open his mouth—Spasms more frequent; takes fluid nourishment well. Bowels have moved.—R. Stryc. 1-14 grain every two hours. 7 o'clock P. M.—Jaws have yielded a little, but spasms have been very frequent. R. Stryc. 1-12 grain every two hours.

15th Oct., 9 o'clock A. M.—Patient has had a bad night. It required two men to lift him in the bed; he thought his back would break from the rigidity. Could not remain in the recumbent posture, from the greater frequency of the spasms. Missed his medicine only once, from having fallen asleep. Says he feels well, but weak. Spasms are milder this morning. Jaws more relaxed; can introduce his fingers between his teeth, and had just swallowed a raw egg without beating. R. Continue 1-12 grain stryc., and wine *ad libitum*, and generous diet. 7 o'clock P. M.—Opens his mouth wider. "Can speak, swallow, and spit with more ease." Relishes his food, and has taken only a half gill of the wine. R. Continue the prescription.

16th Oct., 9½ o'clock, A. M.—At 11 o'clock last evening patient discontinued his medicine, undressed himself and went to bed, for the first time since under treatment. His sleep was frequently interrupted by the spasms. At 7 o'clock this morning resumed the strychnine, which immediately produced its specific effects. Opens his mouth about the same as last evening, and can protrude his tongue an inch and a quarter. Pulse seventy-six, and soft. As he lies in the bed with his knees gently drawn forward, his spasms cease. Bowels have not moved in three days. R. Ol. Ricini ℥ ss. ol. Tiglii gtt. j. M. 8 o'clock P. M.—Patient says he feels twenty per cent. better every way. Jaws more relaxed; articulates easier; uses his mouth more freely; has had only two or three very slight spasms; pulse eighty; oil operated gently. Has taken beefsteak, &c. Continue the prescription.

17th Oct., 9½ o'clock, A. M.—Patient improving. Says he went to bed alone, and much easier, and rested better than the night before. Had very few spasms; omitted his medicine from 12 to 6 o'clock. Feels much better and stronger; sits erect; has walked around the room and into another; opens his mouth wider; protrudes his tongue to the full extent; complains of nothing but the uncomfortable feeling about his jaws. R. Continue the strych. Seven o'clock, P. M.—Has slept two and a half hours; had a few slight spasms, otherwise same as in the morning. R. 1-20 gr. every two hours.

18th Oct., 11 o'clock, A. M.—No particular improvement. A few spasms through the night. Had a bad fall when walking this morning, which cut his scalp badly.

Monday, 19th Oct., 10 o'clock, A. M.—Says he has had a passible night—best yet—very few spasms; general improvement. Has taken beefsteak and coffee. Seems almost well.

20th Oct.—Patient convalescent.

21st Oct., 11 o'clock, A. M.—Continues to do well, and has gained considerably since yesterday.

22d Oct.—Patient discharged cured.

The above, as far as I can ascertain, was a case of Idiopathic Tetanus, which—together with the other two of a Traumatic character, Miss McC. and Mrs. A., published in your last number, with those of Dr. Fell, and cured with strychnine—encourage me to believe that this most awful malady may be controlled, and made subject to the remedial powers of this wonderful medicine.

Within the last two years, two other cases have occurred in my practice.—

The lamented young S. Fox, aged 19 years, from an injury whilst bathing, died the fifth day; having been treated with Tart. Ant., Asafœt., Laudanum and Brandy. The other, a colored woman, two months subsequent to young Fox, recovered. Both were Traumatic Tetanus. The woman had the Opisthotonos form, permanent rigidity, with occasional spasmodic aggravations—especially of the diaphragm—which caused excruciating suffering. The fifth day, her jaws having become firmly closed, I concluded to take the responsibility and bleed her to the extent of a large wash-basin full, when her jaws yielded a little—say ⅜ths of an inch—and this state of things I endeavored to maintain by Tart. Ant. She took four grs. in solution every hour and every other hour for two days without producing emesis, when, for fear of inflammation of the stomach, I suspended its use. Her convalescence was very slow, but her health has been good since.

N. Y., Oct. 20th, 1846.

P. S. Strychnine will not dissolve in water or alcohol. A good way is to add ʒj. Acetic acid to ℥vij. aq., which will make a clear solution.

New York Journal of Medicine.

5.—*Mortality in Boston in* 1846.—The whole number of deaths in Boston during the past year, by the published General Abstract, was 3389, 804 more than the year previous, of which 1472 were children under 5 years of age; over 60 years, 254. The number by consumption is put down at 485. With a population of 120,000, the mortality of Boston during the last year would thus be 1 in 35.40, or 2.82 per cent.—*Boston Medical and Surgical Journal.*

6.—MEDICAL HISTORY OF LOUISIANA.

(We take pleasure in laying before our Louisiana Subscribers the following communication, from which it will be seen that our esteemed correspondent, Dr. Logan, proposes undertaking to give us a Medical History of our State. Such a work is much wanted, and we sincerely hope the members of the Profession, especially those who have long resided in the State, will comply with the wishes of Dr. Logan, and render him every assistance in their power. Dr. L. is a ready writer, a scholar of most respectable attainments, and we have reason to believe if properly aided and supplied with facts, will perform the task with credit to himself and satisfaction to the Profession. In our next number we shall commence the publication of Dr. Lewis's Medical History of Alabama, and we hope it will not be long ere we have the pleasure of giving that of Louisiana. Contributions may be made either directly to Dr. L. or through the medium of our Journal, and we earnestly bespeak the aid of the Profession in behalf of the undertaking.—Edrs.)

New Orleans, Feb. 20th, 1847.

Gentlemen!—Purposing, if I can procure the requisite data, to prepare a paper on "the Medical History of Louisiana," permit me, through the medium of your Journal, to invite the co-operation of the medical community as well as the public generally. Doubtless there is much valuable information respecting the topography, geology, climate, diseases, and statistics of the State, which may be rescued from oblivion, and made available for medical and scientific purposes, if occasion be afforded for its development. Any observations or facts bearing either directly or indirectly on the above topics will be duly acknowledged in the essay, or published separately in this Journal, as may be desired.

I am particularly desirous of receiving from our country practitioners the result of their experience with the prevalent diseases within the sphere of their personal observation—especially the autumnal fevers, and the circumstances under which they assume a malignant type. I should also like to ascertain what influence the progress of civilization—such as the draining of ponds and marshes, the embanking of streams and rivers, and other measures adopted for diminishing the insalubrity of Towns, Villages and Districts, &c., exercise not only on the mortality, but also in modifying disease. Are the chances of longevity increasing, and the mortality decreasing, *pari passu*, with other parts of the civilized world? This can only be positively demonstrated by comparing the proportion of deaths to the population at intervals sufficiently long to admit of a decided social amelioration.

As to *the disease* of this region, Yellow Fever, urban in its predilection, I call upon the medical men of New Orleans for all the facts that may have been rigidly observed during its prevalence. Still as this malady sometimes prevails in different parts of the State, country practitioners, from the circumstance of the sparseness of the population, can trace the mode of its origin and propagation with greater chance of success than those of the city, and thus contribute greatly to the settlement of the vexed question concerning this Protean form of fever, which agitates the profession.

It is to be regretted that so little has as yet been published, respecting the peculiarities and phases of the diseases of the South.

I appeal, therefore, to the sense of obligation every one must feel under to his profession to remove this opprobrium, and I hope my appeal may not be made in vain.

Every species of information relative to the causes, symptoms and treatment of *all orders* of the diseases of this region since the first settlement of the country down to the present epoch will be thankfully received.

Very respectfully, your obed't servant,
THOS. M. LOGAN.

NEW ORLEANS, MARCH 1, 1847.

We had marked off many interesting extracts from the American Journals which we intended to insert in this number, but find ourselves short of space and compelled to defer them. Having given an *extra* amount of matter in several of our preceding numbers, we are now *required to curtail.* The amount of valuable and interesting matter is so abundant in the Medical Periodicals, both American and Foreign, that we find it no easy task to make a proper selection. We particularly regret that our contracted limits often compel us to draw so scantily upon these rich fountains of knowledge. Those who wish to keep pace with the improvements of the day should take a number of Medical Journals.

HEALTH OF THE CITY.

We have had the customary diseases of the season since our last report, perhaps to a somewhat greater extent than usual; yet there has been nothing worthy of special notice. The weather since the first of January has been very bad—cold, wet, and very changeable. Such weather seldom fails to produce a good deal of complaint. Our city has been full to overflowing with visitors, and it is notorious that persons from home are not apt to be remarkably prudent. Catarrhs, pneumonia, bronchitis, sore throats, bowel complaints and a few cases of measles have been seen; but no epidemic of any kind.

We had prepared a notice of the sickness among the Mississippi Volunteers, who recently passed through our city, accompanied with an extract from a letter from Dr. Love, the Surgeon of the Regiment, written after they embarked, but are compelled to omit all.

HEALTH OF THE COUNTRY.

We are much obliged to our attentive Correspondent for the subjoined letter and list of cases, and most cheerfully accept his offer to furnish the like for each No. of our Journal. The plan is an excellent one, and we would be pleased to see it adopted by all our country Correspondents.

EDRS.

MONTGOMERY, ALA., Feb. 13th 1847.

Gentlemen:—In consequence of the want of variation in the health of our place at the time for issuing the January No. of the Journal, from what it had been two months before, I deemed it unnecessary then to write. Some sudden changes in the weather, since, however have been followed by an increase in the number of acute inflammatory attacks, principally of the thoracic viscera.

As conveying a better idea of the health of our place, and more especially of the relative proportions in which the various diseases occur among us, I give the following table, compiled from my own, and the case books of two of my Professional friends, who keep accurate records of their cases, and who have kindly tendered them for the present purpose. The table embraces the cases occurring between the 10th of Dec., 1846, and the 10th of Feb., 1847.

Should the plan meet your approval, I propose to furnish a similar table for each No. embracing the cases occuring during the two months preceding its appearance, instead of my usual communication.

It is proper to remark that the table is from the practice of three out of about 15 Physicians.

By the way—to the remark of Professor Bartlett, in the last No. of your Journal, page 552—"now it seems to me hardly possible to doubt that these were all of them cases of true continued Typhoid fever," I must answer, that with me there is no doubt whatever on the subject—Emphatically they were not such.

List of Diseases:—Abscess 2, Bronchitis (acute) 16, Burn 1, Cataract 1, Catarrh 2, Cancer 3 ,Cholera-morbus 3, Colic 3, Convulsions (infantile) 2, Dyspepsia 2, Dysentery (acute) 4, do. (chronic) 1, Diarrhœa 4, Engorgement of spleen (chronic) 2, Engorgement and ulceration of cervix uteri 1, Enteritis (acute) 2, Erysipelas 1, Fracture (comp. of leg) 1, do. (simple of humerus) 1, Fever, (Intermittent, simple) 15, Fever, (Remittent, simple) 14, do. (do. pernicious) 4, do. (scarlet simple,) 1, Fistula (vesico-vaginal) 1, do. (in ano) 1, Gastritis acute 1, do. chronic 1, Gleet 1, Hysteria, 1, Hæmorrhoids 1, Hæmorrhages (uterine) 2, Jaundice 2, Leucorrhœa (vaginal) 1, Menorrhagia 2, Neuralgia 4, Osteo-sarcoma (of lower jaw) 1, Orchitis 1, Procedentia uteri 1, Paralysis 1, Paronychia 1, Phthisis pulmonalis (still under treatment) 1, Pertussis 1, Parotitis 6, Parturition (Tedious) 1, Pneumonia (acute) 17, Syphilis (primary) 3, do. secondary 2, Suppression of Catamenia 1, Spinal irritation 1, Sprain (of ankle joint) 1, Tumour (Fatty) 1, Trismus Nascentium 1, Tonsilitis 2, Urticaria 3, Vicarious Menstruation 1, Wounds—gun-shot 1, do. Punctured 3, do. Incised 1, do. Contused 1, do. Lacerated 2.

Total number of cases 138—Deaths 6; viz: 1 from Burn, 2 from Pernicious Fever, and 3 from acute Pneumonia.

W. M. B.

Parish of St. Mary's, 19th Feb. 1847.

Messrs. Editors :—Since the first of January, the weather for the most part has been extremely unpleasant. We have had frequent and excessive rains, the changes of temperature have been sudden and extreme—we have had much severe cold weather—more so than any I have experienced during a residence of ten years in this State. The Orange trees have sustained much injury. A great mortality has prevailed among Cattle throughout the Country; arising from disease—exposure to the weather, and scarcity of food.

As a consequence of the unfavorable weather Catarrhs accompanied with more or less febrile excitement have been universal—few have escaped. An Epidemic Pneumonia, has been prevailing and still continues in my neighborhood. Negroes are the principal subjects ; I have met with but 2 or 3 cases among the whites, many of the cases have early assumed a typhoid appearance. In some of the cases extensive hepatization of one lung was found to exist when the cases first came under observation. Some plantations have suffered much more than others; on one Plantation 11 cases occurred, most of them of a severe form.

Since the first of January I have treated about 23 cases, 3 deaths have thus far occured, one an old gentleman of 60, and 2 negro boys. I have several cases now under treatment and cannot now predict their issue. Most of the cases I have seen have been accompanied by great gastric irritability, so much so as entirely to forbid the use of the Tartar Emetic. In its treatment I have derived the greatest advantage from the use of Quinine. In the management of this disease I consider no remedy equal to this. In many cases it acted like a charm in lessening the frequency of the pulse—in diminishing the number of respirations and sustaining the vital powers. Two or three of the first cases in which I trusted too long to the antiphlogistic treatment succumbed as soon as I made free use of Quinine conjoined with mercurials and opiates, the succeeding cases assumed a favorable aspect. In all cases where I saw the patient in season, the treatment was commenced by a copious bleeding. I have given the Quinine in doses of 5 grains every four or six hours, and I attribute the recovery of several of my patients solely to its use.

In my next communication you may hear more from me on this subject.

J. B. D.

MORTALITY IN NEW ORLEANS IN 1846.

We promised in our last number to give in this our Bill of Mortality for the year, but we regret to say that owing the derangement in the operations of the Board of Health, the Reports from the Cemeteries were not received during the first half of the year, and it has therefore been found impossible to obtain a full and correct report of the deaths and diseases. The Board was re-organized in August, and our Colleague Dr. Hester having been appointed Secretary, has supplied us with a full report since the 23d of that month. We cannot make room for the list of diseases. The whole number of deaths for the six months, from the 23d August to the 20th February, was 2096. Of this amount a large number was supplied by the Army of operations in Mexico. Many sick and discharged Volunteers have terminated their career in this City. Of the 2096 deaths given, 707 were under 10 years of age—471 were colored, and 1625 white.

THE LETHEON.—*Insensibility produced by the inhalation of Sulphuric Ether.—Amputation of the Thigh in New Orleans.*

In our announcement of this new discovery in the last number of our Journal, we confess that our impressions, with the lights then before us, were decidedly unfavourable to it. In whatever light we viewed it—whether as a *humbug fraught with danger*, or *as a powerful remedial agent presented to the world under the odious auspices of a patent right*, we felt prejudiced against it, and expressed our surprize that its use, *whilst a secret remedy*, should have been sanctioned by the honorable members of the medical profession in Boston. We classed it with *mesmerism*, and thought it had then done nothing more extraordinary than that mysterious agency, whilst the *threatening dangers* of its use rendered it much more objectionable. Since that time the Northern Journals have teemed with reports of its wonderful powers and criticisms upon its use, and we have felt considerable curiosity to witness its effects. We are sorry we are not able to give copious extracts on the subject in the present number. What the remedy *really is*, is still held *a secret* by the inventors, Jackson and Morton ; but even the *claims to the invention* by these gentlemen have been strongly contested, and as to their attempt to monopolize its benefits under cover of a *patent right*, it has not only been viewed with disgust, but the force of the patent fairly doubted. The remedy is generally supposed to be *pure sulphuric ether*, but the inventors have not the magnanimity to disclose it. Whatever it be, its use has attracted much attention wherever it has been tried, and the object of these remarks is not to discuss the various opinions that have been expressed in regard to it, but rather to mention that it has just been introduced into this city, and that we have this day (25th February,) witnessed an important operation under its use. We have barely room to state that Dr. Stone, this morning at the Charity Hospital, amputated the thigh whilst the patient was in a state of complete insensibility, and the result so far has been very satisfactory both to the operator and a large number of spectators. When the man awoke, he could only realize that the operation had been performed by having the amputated limb held up before his eyes. The case will be fully reported in our next number.

F.

Resection of One Half of the Lower Jaw, by *Dr. A. Mercier.*

On the 3rd of February, we were invited with several others, among whom were Drs. John C. Cheesman, of New York City, and T. Rush Spencer, of Geneva, N. Y., then on a visit to our city, to witness this operation. It was skillfully performed, and the result is most satisfactory. We had prepared an account of the case and operation, but are compelled to defer it for the present for want of space.

F.

ANNUAL REPORT OF THE NEW ORLEANS CHARITY HOSPITAL, For 1846.

From this Report made to the Legislature of the State, we learn that the total number of admissions into the Hospital during the year was 8,044—of whom 7,074 were discharged, and 855 died. There were 7,934 white persons, and 110 black. The number who had resided in New Orleans less than three years was 6,010—over three years, 2,034. The number coming from the different United States was 1,773—from different foreign countries 6,150—unknown 121.

The number of admissions for the different months was as follows:

January,	520	May,	530	September,	923
February,	431	June,	586	October,	984
March,	794	July,	649	November,	929
April,	495	August,	735	December,	783

Remaining on the 1st January, 1847, 501.

These statistics include the Insane Department.

The following will show the number supplied by the different States of the Union—or rather their places of nativity, for many of them were not directly from those States at the time of admission.

ADMISSIONS INTO THE CHARITY HOSPITAL, DURING THE YEAR 1846, *From the United States.*

New York, ...	320	New Hamshire, . . .	21
Pennsylvania, . . .	249	Rhode Island, . . .	15
Massachusetts, . . .	91	Connecticut, ...	38
Louisiana, ...	145	Alabama, ...	14
Kentucky, ...	114	Vermont, ...	25
Virginia, ...	130	Indiana, ...	29
South Carolina, . . .	46	Arkansas, ...	9
North Carolina, . . .	65	Delaware, ...	13
Maryland, ...	87	District of Columbia, .	5
Maine, ...	67	Georgia, ...	20
Ohio, ...	105	Florida, ...	4
Missouri, ...	23	Illinois, ...	12
Mississippi, ...	26	Texas, ...	1
Tennessee, ...	69	Michigan, ...	1
New Jersey, ...	29		1773

The following will show the number coming from different foreign countries in the same way.

ADMISSIONS INTO THE CHARITY HOSPITAL, DURING THE YEAR 1846,
From Foreign Countries.

Ireland, ...	3107	Belgium, ...	22
Germany, ...	1109	Switzerland, . . .	130
France, ...	506	Holland, ...	25
England, ...	369	Greece, ...	3
Scotland, ...	152	Brit. Pos. of N. Amer.,	26
Spain, ...	94	Canada, ...	54
Portugal, ...	47	Mexico, ...	14
Italy, ...	28	Chili, ...	3
Sardinia, ...	14	Peru, ...	1
Austria, ...	29	Hayti Island, . . .	3
Poland, ...	14	East Indies, ...	4
Russia, ...	5	West Indies, . . .	14
Prussia, ...	240	Repub. of Colombia, .	3
Denmark, ...	42	Africa, ...	1
Sweden, ...	64	Malta Island, . . .	6
Norway, ...	25	Brazil, ...	1
			6150

The following will show the complaints for which there was the greatest number of admissions.

Intermittent Fever, . .	1995	Typhoid Fever, ...	292
Rheumatism, ...	515	Delirium Tremens, . . .	175
Dysentery, ...	469	Yellow Fever, ...	146
Ulcer, ...	468	Remittent, ...	111
Syphilis, ...	389	Pleuritis, ...	66
Bronchitis, ...	266	Neuralgia, ...	52
Consumption, ...	238	Congestive Fever, . . .	42
Diarrhœa, ...	229	Pernicious Intermittent, .	27

NUMBER OF DEATHS DURING THE YEAR 1846,
Divided into periods of age.

Ages	Males.	Females.	Totals.
Under 10 years,	11	5	16
From 10 to 20 years,	37	4	41
" 20 " 30 "	237	36	273
" 30 " 40 "	218	31	249
" 40 " 50 "	107	9	110
" 50 " 60 "	39	6	45
" 60 " 70 "	24	4	28
" 70 " 80 "	2	1	3
" 80 " 100 "	"	"	none
At 100 "	1	"	1
Unknown "	65	18	83
Grand totals,	741	114	855

It will be seen that the mortality was but a small fraction over 10 per ct.

The number of admissions into this hospital during the year is larger than was ever known before; yet the year was a healthy one, and free from epidemics. This extraordinary number (8,044) affords sufficient evidence that access to the hospital is altogether too free and easy.—One half the amount of *real objects of charity* suffering under sickness could not be found in our city during any year.

MONTHLY REPORTS
OF THE NEW ORLEANS CHARITY HOSPITAL.

MAIN BUILDING.

December—Admitted : Males, 667 ; Females, 116. Total 783.
Discharged : Males, 587 ; Females, 91. Total 678.
Died : Males, 96 ; Females, 8. Total 104.
Remaining on the 1st of January, 427.

LUNATIC ASYLUM.

Admitted : Males, 34 ; Females, 15. Total 49.
Discharged : Males, 30 ; Females, 10. Total 40.
Died : Males, 5 ; Females, 3. Total 8.
Remaining on the 1st of January, 74.

MAIN BUILDING.

January—Admitted : Males, 602 ; Females, 115. Total 717.
Discharged : Males, 480; Females, 102. Total 582.
Died : Males, 86 ; Females, 11. Total 97.
Remaining on the 1st of February, 378.

LUNATIC ASYLUM.

Admitted : Males, 42 ; Females, 8. Total 50.
Discharged : Males, 28 ; Females 8. Total 36.
Died : Males, 4 ; Females, 2. Total 6.
Remaining on the 1st of February, 83.

ABSTRACT OF A METEOROLOGICAL JOURNAL FOR 1847.

By D. T. LILLIE, at the City of New Orleans.

Latitude, 29 deg. 57 min.; Longitude, 90 deg. 07 min. west of Greenwich.

WEEKLY. — 1847.	THERMOMETER.			BAROMETER.			COURSE OF WIND.	FORCE OF WIND, Ratio 1 to 10.	Rainy Days.	Quantity of Rain. — Inches.
	Max.	Min.	Range.	Max.	Min.	Range.				
Jan. - 2	77.5	50.0	27.5	30.27	30.01	0.26	S.W.	2½	1	0.250
" - 9	68.5	29.5	39.0	30.49	29.83	0.66	N.W.	3½	0	0.000
" - 16	75.0	30.0	45.0	30.51	30.03	0.48	N.W.	3	1	0.380
" - 23	73.0	30.0	43.0	30.55	30.05	0.50	N.W.	2¾	2	8.070
" - 30	65.0	41.0	24.0	30.24	29.79	0.45	N.W.	2¾	4	4.800
Feb. - 6	64.5	32.5	32.0	30.34	29.88	0.46	N.E.	3	3	3.123
" - 13	70.0	35.5	34.5	30.46	29.76	0.70	N.W.	3¼	0	0.000
" - 20	75.5	43.5	32.0	30.25	30.05	0.20	W.	2¾	3	0.931

Remarks.—The Thermometer used for these observations is not attached to the Barometer, but is a self-registering one, and is placed in a fair exposure. Regular hours of observation, 8 A.M., 2 P.M. and 8 P.M.

The Barometer is located at an elevation of 19 feet above the level of the ocean, and is suspended clear of the wall of the building.

The Rain Guage is graduated to the thousandth part of an inch, and the receiveris elevated 40 feet from the ground.

THE

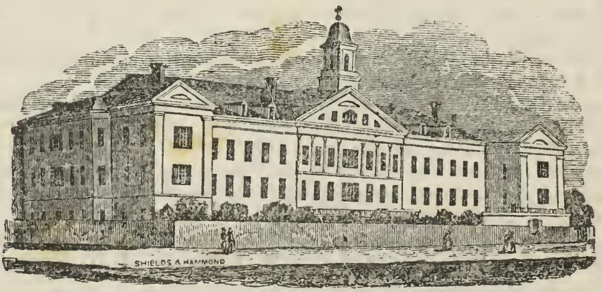
NEW ORLEANS MEDICAL AND SURGICAL JOURNAL,

DEVOTED TO MEDICINE AND THE COLLATERAL SCIENCES.

EDITED BY

W. M. CARPENTER, M. D.
E. D. FENNER, M. D.
J. HARRISON, M. D.
A. HESTER, M. D.

"Summum bonum Medicinæ, sanitas."—GALEN.



NEW-ORLEANS CHARITY HOSPITAL.

MAY, 1847.

NEW-ORLEANS.
PUBLISHED BY S. WOODALL, 49, CAMP STREET.
1847.

TO READERS AND CORRESPONDENTS.

Dr. Guild's communication will appear in our next number. Also one from Dr. B. J. Hicks, of Vicksburg, Miss.

The following books and pamphlets have been received:

New Elements of Operative Surgery. By ALF. A. M. VELPEAU, with Notes and Additions, by Valentine Mott, M. D. Volume 3d and last, New York; Samuel G. & W. Wood, 1847, pp. 1162.

A System of Human Anatomy, General and Special. By ERASMUS WILSON, M. D. *Lecturer on Anatomy, London. Third American from the third London Edition.* Edited by PAUL B. GODDARD, A. M., M. D. Philadelphia: Lea & Blanchard, 1847.

Woman, and her diseases. By Dr. DIXON, New York.

Julius Vogel's Pathological Anatomy, translated by Dr. DAY.

SILLIMAN'S *Chemistry*, Philadelphia, 1847.

Education: Its Elementary Principles, founded on the Nature of Man. By J. G. SPURZHEIM, M. D., of the Universities of Vienna and Paris, and Licentiate of the Royal College of Physicians in London. With an Appendix by S. R. Wells, containing a description of the temperaments and a brief analysis of the Phrenological Faculties. Sixth American Edition, improved by the Author from the third London Edition, New York: Fowler and Wells, 1847.

Summary of the Transactions of the College of Physicians of Philadelphia. From April to August, 1846, inclusive.

The Annual Report of the Eastern Asylum, in the City of Williamsburg, Virginia, for 1846.

DR. STEVENS' *Valedictory Address to the Graduates of the College of Physicians and Surgeons of New York*, delivered March 11th, 1847.

Lecture on Obstetrics and the Diseases of Women and Children. By GUNNING S. BEDFORD, M. D., Prof. of Obstetrics, &c., in the University of New York. Introductory, delivered Session 1836–7.

Introductory, Lecture to the Course on the Theory and Practice of Medicine, in the Medical Department of Pennsylvania College, Session 1846–7. By W. DARRACH, M. D., Philadelphia.

Address to the Graduating Class of the Indiana Medical College, at the Public Commencement, February, 1847. By M. L. KNAPP, M. D., Prof. of Materia Medica, &c., Chicago.

Catalogues of Pennsylvania University, University of Louisville, Transylvania University, Jefferson Medical College and Western Reserve College.

We have received our usual American exchanges, and in addition, the American Phrenological Journal, of New York.

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AMERICAN MEDICAL INTELLIGENCE.

EDITORIAL.

is of paramount importance in the study of Ætiology, the writer will, at a proper time, endeavour to point out those distinctive features that are observable in the terrestrial formations of the respective regions into which the State is divided.

In tracing the diseases of Alabama through all the changes they have undergone, we are insensibly led into a wide field of speculation; but after collecting all the facts pertaining to the specific character of each, the time, place and circumstances under which they are found, and their relative bearing to each other, we discover here as every where else, such a chain of facts as would lead to the conclusion, that nature, in all her ways, has adopted an uniform course of progressive action. As we see, that, by the operation of her invariable laws, the tender plant germinates and expands under the influence of heat and moisture, and passes through all its successive stages until it arrives at maturity, so in a similar order of sequences, following in the decay of that same organic formation, and under the same general influences, a noxious miasm is produced which becomes a powerful morbific agent for man's destruction.

That climate exerts a powerful influence in marking the character of endemic disease, is a fact so apparent that the careful observer cannot fail to perceive the regular gradation of all febrile affections, as he advances from a cold region of country to the burning influence of a tropical sun. The luxuriant growth of vegetation also tends to produce a great intensity in the miasma, that constitutes in part, at least, the cause of disease in southern latitudes.

When the early pioneer first settled in this country, disease, as a general rule, was uniform in its character. But as an improved cultivation of the earth goes hand in hand with the march of civilization, and as this, in its turn, exerts a sensible influence upon the atmosphere, by exposing the virgin soil to a tropical heat, we can readily trace a corresponding change in the character of disease.

Sensibly impressed with the difference of atmospheric temperature, in the woodlands, as contrasted with that of cultivated regions, we have endeavored to ascertain by experiment the extent of influence which this has upon the soil; and we have found that a thermometer inserted ten inches into cultivated earth, fairly exposed, will show an increased temperature of at least 8^{0} over that of uncleared soil, in summer; whilst in winter the difference is equally great, excepting that it is reversed.—As a consequence of these changes of atmospheric and terrestrial temperature, the rapid increase and decay of vegetable and animal matter, together with physical causes not enumerated, such a change in the gaseous emanations is doubtless produced as tends to designate the character of disease at various periods of time.

Connected with this matter there is at the same time an altered condition of the constitution and temperament as man advances from a state of native simplicity to the refined and luxurious habits that wealth can command. In that primitive state of existence the mind was unfettered by corroding cares, the articles of food simple in their nature, and the oaken couch was as soft to the wearied husbandman as a pillow of down—then the robust frame enjoyed an almost uninterrupted condition of health, and great longevity marked man's pilgrimage on earth.

But when refinement slowly and stealthily creeps into his habitation; when the rude cabin obscured in the tangled vines of the wildwood, is exchanged for the more modern mansion of the exposed and cultivated plain; when the unlicensed freedom of sensual indulgence with the pampered appetite and excess of libation reign supreme—then is forcibly marked the cause of disease, tending to reduce the once stalwart man to a state of decrepitude; and the prattling urchin, instead of the glow of health that once sat upon his cheek, now presents the aspect of refined infirmity laying the foundation of an early grave.

To these various causes can be traced a gradual change in the character of disease from the first occupancy of our country to the present day—a change so forcibly exhibited that he who notes cannot fail in his diagnosis.

After a careful review and study of the regular gradation of disease' we will so far anticipate inevitable conclusions as to divide the proposed history into three distinct epochs, viz: the Ataxique, Phlegmonic, and Adynamic. After tracing disease from its mild incipient action of early days, through the high toned phlegmasia of later times, until we arrive at that low stage of typhoid affection that marks the character of all disorders at the present day, it will be discovered that the above division, so far from being the result of any hypothesis, is natural and imperiously demanded.

The epoch to be first considered comprises that period in the history of the State extending from the time the hardy emigrant first took up his abode among the children of the forest, down to about the year 1819.—Immigration to the State was not very great until about 1812, when the great fertility of soil, advantages of navigation, and beauty of early scenery, began to be so well known and appreciated abroad as to cause a rapid influx of strangers.

The early settlers came mostly from Georgia, Carolina, Virginia, and Tennessee, and generally pitched their homes at convenient distances from each other. Owing to the fact that these immigrants brought but few laborers with them, they chose the uplands and hammocks rather than encounter, with a feeble force, the rich and heavy timbered alluvion of the river swamps. This in some measure accounts for the health enjoyed by the early inhabitant, still this health was not uninterrupted: coming from places, where the seed of infection had been exhausted, to a new country whose increased heat and the occasional morass and stagnant pool gave rise to noxious vapours, it cannot be supposed that the system could withstand altogether the frequent invasion of this poison. The effect, however, of these exhalations was confined to the immediate vicinity whence they were generated.

The character of fever prevailing in these years consisted of the various types of intermittent, with now and then, in mid-summer, the occurrence of bilious remittent: quotidians in the Spring and the first of Summer, remittents towards the close of Summer, with tertians and quartans in Autumn, and frequently continuing through the winter.

The Remittent fevers of this day were exceedingly mild, yielding readily to the operation of one emetic, and not unfrequently to the spontaneous discharges of bile and copious sweats that usually occurred in the first paroxysm. In the most serious and obstinate cases, the intermissions or remissions were irregular and incomplete ; the hot and sweating stages rapidly succeeding each other, and the chill or cold sensation becoming less distinct with every revolution.

If in that neglect, so often attending the treatment of non-malignant diseases, these fevers were suffered to continue, they not unfrequently glided into a continued irritative type, attended with great *nervousness* twitching of the muscles, general emaciation and debility, constituting, what in those days were termed "nervous fever." After remaining in this situation for weeks, they (the patients) either slowly recovered, or critical sweats and involuntary discharges ensuing, a fatal termination was the result. These remittents, however, rarely assumed this form ; as a general rule the disease gave way after two or three paroxysms, to be replaced late in autumn by the stubborn and unyielding quartan of winter.

As a general rule, the intermittents or agues of those times were not dangerous in their primary stages, but when the patient became the subject of oft repeated and long continued attacks, the constitution was frequently placed in great peril ; the young and athletic seldom failed in the end to triumph over this hated companion ; but the aged, and those of infirm constitution or intemperate habits, had to run the hazard of diarrhœa, anasarca, or severe organic disease.

The cold stage of these Intermittents continued for many hours, attended with *gaping*, *stretching* and incessant *shaking*. It has been stated that in these fevers there was no danger in their primary stages,—inquiry has, however, brought to light a few exceptions to which attention is here invited. After several hours continuance of "a hard ague" the rigors and shaking would suddenly cease and instead of the usual signs of reaction and fever, the physician would be surprized to find the body bathed in a copious perspiration, slow compressible pulse, *cool* blueish tongue, sunken careless expression of countenance, with a cessation of all pain and anxiety. In a few hours the tendency of this condition was made manifest by a deepening of the foregoing symptoms, together with insensibility, swelling of the abdomen, and involuntary discharges. My informants, the late Drs. Shackelford and Crews, formerly of Conneculı, say that without a timely and well directed application of stimulants, both internally and externally, death soon closed the scene ; but if the system rallied, a healthy action of the viscera slowly returned without the recurrence of another paroxysm.

Some of the older physicians refer to these cases as resembling the congestive fever of the present day ; but a little reflection and examination should convince them that there is not the slighest analogy. Independent of the difference in symptoms, character, and mode of termination, we find in these cases the mischief to consist in the shock given to the system, by the violence of the ague, when in an atonic condition —whilst the other is but the sure and inevitable consequence of the invading poison.

Dysentery and diarrhœa in the spring and summer, tertian fevers in the autumn, and catarrhs and ill-defined pleuresies during the winter, were the disorders most prevalent among negroes : from the fact that very few of this class of persons died of disease during this period, these maladies must have been exceedingly mild.

Having noticed such cases of fever as assumed a grave character, with the details our meagre data afford, and endeavored to impress you with their various modes of termination, it is deemed unnecessary to dwell at further length upon the simple affections caused by the imperfectly formed malaria of those days.

This thinly settled region at that remote period was inadequte to the support of scientific physicians ; the few that were here having to unite some other occupation with their time-serving profession. Under such circumstances it was impossible that the Therapeutics of that day should have been dignified into anything like a rational system. Every planter and physician had his favorite remedy which he never failed to apply in the true spirit of empiricism. Tartar Emetic given to emesis was extensively used—as was also, especially in remittents, the *Boneset* or Eupatorium Perfm. But calomel and jalap to "*pass off* the *bile*," and Peruvian Bark to prevent the recurrence of the paroxysm, was the most uniform and successful system of practice. After the regular quartan ague became firmly established, the patient abandoned all attempts at cure ; preferring *the luxury* of the mid-day sun together with the issue that accident or circumstances might produce, to the use of nauseous remedies in which he had lost all confidence.

Before entering upon the consideration of the diseases of the second epoch, it may be well to mention that towards the close of the first, intermittents in some localities were not unfrequently distinguished by phenomena of a character not yet mentioned.

These cases occurred about St. Stephens, Claiborne, and probably other villages, in the years 1816 and 1817. During the paroxysms there was great pain in the back, furred tongue, bounding pulse, anxiety and active delirium ; the intermissions marked by loathing of food, swimming in the head, and swelling in the region of the stomach and spleen. After three or four paroxysms of this kind, the fever became continued, attended with dry harsh skin, quick tense pulse, torpor of the bowels, and slight delirium in the evening. Diarrhœa, inflammation and suppuration of the parotids, or chronic swelling of the abdominal viscera were the usual sequelæ of this form of fever.

The remittents of this epoch were only the sequelæ of an intermittent type immediately preceding. Upon the debility and abnormal condition of the liver and spleen caused by these remittents, disease of a similar but more obstinate character again ensued. The only plausible reason that can be advanced for this unity of disease, arises from the fact that malaria in its generative process from the simple elements that were then in existence was identical, and of course more uniform in its effects.

SECOND EPOCH.

The second stage of progressive action extends through a period of 14 or 15 years, from 1818 to 1833. This is the era the diseases of which present those marked and peculiar symptoms that cannot fail to be recognized as belonging to the active phlegmasiæ.

At this particular period the early settler had become an old citizen ;

the rude log-cabin had in many instances given place to the stately mansion, and the girdled tree that stood in skeleton form around his habitation had crumbled under the pressure of tempest and decay. The evergreen of the hammock and the shady foliage of the undulating upland had now given place to the luxuriant cotton plant. Then too, the sluggish river and the dark lagoon began to yield up their cumbrous veils of foliage to the common mass of decomposing matter. In addition to these supposed causes, consequent upon extensive inroads into nature's forest, we may mention the exposure of a large extent of the earth's surface, the upturning of a soil abounding in organic remains, and the numerous springs of mineral matter, once hid beneath the surface of the earth, but now brought to light by the hand of man, all united in throwing forth their poisonous gases. Then too under the operation of natural laws, sprung into existence those numerous hordes of animated nature, the incessant reproduction and decay of which, doubtless contributed largely to those noxious exhalations, so active in producing disease and death.

And again, we should not forget that there is a principle well established in the progress of society, that as wealth accumulates, so luxury creeps into the favoured domicil, and under the imperious sway of sensual enjoyment, the wealthy inmate is tortured with a protean disease, unknown to the tenant of an humble mansion. From the causes thus briefly alluded to, we may distinctly trace on rational conclusions, that marked change in the character of disease so fully exhibited during this important epoch.

Between the years 1812 and 1818 the town of St. Stephens, situated on the lower part of the Bigby, had a large accession of population attended with a corresponding increase of houses and agricultural improvement in the vicinity. Fever of a severe and malignant type prevailed in this place during the summers of 1818, '19, '20, and '21.—During the two latter years, however, the town began to decline, and sank with a rapidity even greater than that which marked its rise.

Fort Claiborne, situated on the most elevated bluff of the Alabama River, was an unimportant place until about the year 1818, at which time it began to improve, and in 1822 the population had increased to 3,000. This high and elevated plain has a gradual declivity as it recedes from the bank of the river until reaching a few low marshy ponds. In 1819 fever prevailed in this town with great severity—making its annual visitation, with every summer an increased mortality, until the population began to decline about 1825 or '26.

About the years 1817–18 and '19, the town of Cahawba increased very rapidly in population, and at the same time a vast extent of that fertile region of country was brought into cultivation. In 1821, a year memorable in Alabama for a late inundation of the rivers, destruction of the crops, and wide spread disease, the citizens of Cahawba experienced their first epidemic visitation. Dr. Heustis remarks that the great overflow was in July, and that "in the month of August, the putrefactive process was so great that he was assailed in both town and country with a disagreeable odour resembling that of a room in which many sick of the fever were confined."

The newspapers published at that time do not disclose the extent of mortality in their respective cities, but published very liberally of the reports which were brought them by their neighbours. There seems to have been a studied effort on the part of the inhabitants of these towns to conceal from those at a distance knowledge of the great fatality of the fevers of 1821 and '22. The people of Cahawba were so tenacious of the population of their town (the then seat of Government) that they held a public meeting in the midst of the epidemic, and resolved that the fears of those who had retired into the country were not well founded, and forced the public officers to return to the city and resume their various duties. This mortality, not less than twelve per cent. of the entire population, far exceeded that of the country settlements. The population of these towns was composed in part of emigrants from the Northern States and Europe—this class of persons were invariably attacked with more virulence than those who were accustomed to malarious regions. In those days, (and in this all the earlier observers agree,) a climatic attack of fever, generally of a continued type, was incident to the first year's residence, even in the absence of general disease.

In no section of the State which presented during this period a cultivated condition, was there immunity from disease. One year subsequent to that in which the enterprizing immigrant invaded the forest, fever began making its annual visits, until time with her incessant changes had destroyed the material whence it came.

The Spring months of 1819–20–21–22–23 were exceedingly wet, the rains frequently continuing until the first of August. It was during these years that the settlements and towns throughout the entire State were so generally and severely afflicted, as not only to create great alarm at home, but to give the country an unenviable reputation abroad. The inhabitants in the vicinity of the sandy and hammock lands, after 1825 became healthy, whilst those in the neighborhood of the yellow argillaceous soils, continued to suffer even with increased violence.

It is wholly impracticable at this distant day to attempt any thing like an account of the various epidemics of different localities, or to enter into a minute description of individual cases of disease; still it is believed that a sufficient array of facts can be collected for a true and correct representation of disease during those by-gone days.

Intermittent fever and dysentery, from their frequency in the Spring may be said to have constituted the prevailing forms of disease of that season—in many in stances they ran into each other, becoming complicated and very violent. These irregular forms of disease, with the vast number of attending mild intermittents, had scarcely begun to attract attention when remittents, those connecting links in the chain of morbid action in the South, made their appearance.

The remittent fever was ushered in by a feeling of coldness of the extremities extending along the back alternately with flashes of heat and frequently accompanied with vomiting; these symptoms were soon succeeded by violent pain and throbbing in the head, flushed face, hot skin, and bounding pulse. The tongue was coated in the onset with white fur, which after copious secretions and discharge of bile assumed a yellowish caste. In a few hours a copious sweat ensued, and all these symptoms abated until a remission occurred. With some degree of variableness in the recurrence of the exacerbations and remissions, the disease proceeded for four or five days, and yielded under the influence of copious sweats and discharges of dark bilious matter. This mild character of fever prevailed but a few weeks in the first part of summer, before giving place to those of a more virulent character and to which particular attention is invited.

As a general rule the fevers of Alabama have ever been and still are of a remittent form, yet under those circumstances and in those situations in which they prevail with any degree of violence or malignity, there are cases of a continued type, or those in which a remission is but a deceptive veil, amounting only to a momentary calm, which is succeeded by a long and violent storm, constantly threatening the unfortunate sufferer with annihilation. These are the descriptions of cases which at that day demanded attention and to which the watchful eye of the physician was ever turned. They were the cases upon which danger alone waited, presenting that condition of the system in which is revealed the character of the attending and milder cases of the same type. And as such, they are properly considered the standard by which we are to judge and measure the character of disease during that epoch.

And first of the remittents. They were ushered in with a sensation of coldness, slight in degree, but often long continued,—with restlessness, thirst, and burning of the muscles. This condition soon passed into one distinguished by intense heat and dryness of skin, excruciating pain in the head and back, white furred tongue, frequent *tense* pulse, restlessness, and irritability of temper. In 18, 24, or 48 hours, during which time the lancet and antimony were freely used, a moderate perspiration with a decrease of heat and moderate pulse ensues. This abatement of distressing symptoms called a remission, rendered the patient so comfortable, when compared to his previous suffering, as often to flatter him that a happy crisis had approached—but in a few hours there was, without any sensation of coldness, a renewal of the febrile symptoms even more intense than before. After the second or third paroxysms which were irregular in duration, the disease gave way under the influence of copious evacuations of thick black matter and a general perspiration. In some cases, however, after one imperfect remission or a momentary calm, the symptoms became indicative of the most aggravated character of disease, the pulse became strong and corded, delirium of the most extravagant character attending. These cases continued many days, yielding in the end to the heroic practice of the times, spontaneous active hemorrhages, or unfortunately resulting in organic disease, or after the *departure of the fever* leaving the patient in a fatal collapse.

In other cases again, after the second or third paroxysms, the tongue become fiery red, pulse small, frequent and wiery, extremities cool, constant thirst, retching to vomit, great restlessness and delirium. In this description of cases (mis-named typhoid) there was evidently gastric and cerebral inflammation of the most active kind, generally proving fatal to the patient.

The observing men who practiced in those fevers state that the *pulse never lost its tenseness short of convalescence.* Dr. Heustis remarks, "*little abatement of the tension* took place even during the remissions."

But the most common form of obstinate and dangerous fevers throughout this period was the continued; they took place usually without chill or sensation of coldness, the pulse slow at first, gradually became corded and bounding, the skin increased in temperature until it became burning hot, eyes injected and suffused, pain in the head, back, and joints, restlessness, thirst, and sometimes nausea took place; towards the evening, after the first day, delirium made its appearance, with hypochondriasis during the slight abatement of mornings. The duration of these cases depended very much upon the treatment; continuing four, five, and often nine days. These fevers were usually called "bilious inflammatory," and owing to the violence of the vascular excitement, local inflammation often occurred. Spontaneous hemorrhage from the nose, and black grumous discharges from the bowels were frequent, and always attended with happy consequences; a slight yellow tinge of the eye was sometimes observable from an early stage, but no yellowness of skin was visible except in the stage of imperfect and protracted convalescence.

During the violent epidemics of Claiborne and Cahawba, there were a few cases in which there was not such a high degree of vascular excitement or intense suffering. The morbid action set up in these patients was manifested by a slow and less corded pulse, slight stupor and gloomy physiognomy, the skin was slightly yellow and of moderate temperature, the stools passed in small patches, very much the appearance and consistence of putty, urine scanty and of dark brown color. In two of this description of cases, observed by Huestis, there were indications of "rapid putrefaction" and in one the blood ejected from the stomach was so dissolved and blackened as to give it the appearance of black vomit: In fact, in this case it was identical, and had description stopped here, the conclusion that many of those cases were yellow fever, would be inevitable. But the Doctor speaking of the discharges from the bowels, says:—"I took it to be the same as what is called black vomit, though it was discharged in immense quantities,"—he further says "this matter was inodorous, not perfectly black and stained the linen of a dark green color." Hence it is to be inferred, that this discharge instead of being "black vomit" was but one of the various characters of vitiated excretions that occur in the disorders of a highly malarious region. Still it cannot be denied that such of those cases (few in number) as were of low action, gloomy physiognomy, dusky yellowness of skin and eyes, tending to putrescency and collapse, simulated yellow-fever very closely.

There are other cases again (exceptions) that might be alluded to, but pretending simply to portray the general character of disease, attention is once more called to the remittent and continued fevers of high vascular excitement. In these diseases, Dr. Heustis remarks "the *blood was cohesive and free from that attenuated* and *dissolved condition which authors describe* as *characteristic of putridity in malignant diseases.* Not unfrequently the crassamentum *was covered with the buffy coat* as in *cases* of *local inflammation.*" In this statement, the Doctor is sustained by all of the medical men who practised in his day. In the examination of bodies all agree that the liver was distended with blood, the spleen engorged and sometimes softened, and the membranes of the brain and stomach giving evidence of preexisting inflammation. In fact, if the statements of those who made examinations are to be relied upon, these evidences of inflammation are abundantly adequate to the explanation of the fatal termination.

The treatment pursued was bold and prompt, corresponding perfectly with the violent undisguised character of disease which the physician had to combat. Blood-letting, emetics, cathartics, calomel and jalap, with a constant *stand-bye* of the pulvis antimonialis were then the Sampsons of the art. If any patient passed through the stages of fever with an *unscared* arm, it was deemed a hazardous and unfortunate omission. The practitioners of that day now to be met with, tell us that this bleeding was repeated frequently in the same patient, and such was the acknowledged utility of the practice, that the lancet was placed in the hands of all overseers and heads of families. Heustis and Casey tell us that they bled universally, sometimes taking away 70 ounces of blood from one patient in the course of the fever ; at the same time remarking that "the mildest cases required the lancet."

Peruvian Bark, which had been in high repute in the treatment of the diseases of the preceding epoch, was given during the remission of these fevers with unsatisfactory results. The large mass of physicians found it not only disagreeable to the patients, but annually producing increased violence of the succeeding paroxysm ; and not unfrequently, when given in an early stage of convalesence, inducing reaction of the disease ; hence they abandoned its use entirely. Dr. Heustis is the only advocate for its use, and he speaks of it with faint praise. Various remedies were used, still the lancet and antimony were chiefly relied upon. Calomel was used in every case, but it was not until about the close of this epoch that it became the chief ingredient of modern empiricism.

During the epidemics of the towns, children of all ages were frequently attacked with the prevailing fever. Negroes were not so generally or violently attacked as the whites ; the diseases most partial to them, were dysenteries in the spring, agues during the cold nights and mornings of Autumn, and pleurisies throughout the winter.

During this period, the old fashioned intermittent, attended with an ague of sufficient intensity (to use the language of many a sufferer) "to shake down the bed," had given place to a mere creeping sensation, and the febrile excitement that in the former state was very imperfect, became developed in the latter to great excess.

The experience and observation of former times, seemed every where to attest the fact, that the more violent the paroxysm of the cold stage, the less intense was the degree of febrile excitement; and that when a higher tone of vascularity was set up in the system the reverse of this effect was induced ; the chill was imperfect, amounting to nothing more than a rigor and the inflammatory action was increased in a threefold proportion. And these rules will hold good when referring to the exanthematious and remittent fevers of those extinct periods, for in both forms of disease there was invariably noted a depression of the nervous system, at the onset of attack, and according to its violence or minor influence the practitioner could generally arrive at a correct conclusion relative to the progress of the second stage. This principle so completely prevailed in the prognosis of disease incident to the second epoch, that medical men tell us, that in all violent and dangerous febrile affections, the rigor was either wanting or so indistinct as scarcely to be noticed.

Disease at that time did not run into putrescency ; typhoid and typhus fever were not known in the bounds of the State, and scarlatina had not developed that malignant form so fatal during these latter days.

To all of these rules there were exceptions, and so long as man is subjected to the same influences that do now and ever have existed, those exceptions will never cease. Yet amidst the endless variety of phenomena that each era may produce, there are prominent leading features that stamp them with characters too legible to be mistaken. And if we apply the laws of pathology to the increased vascularity of the tissue, bounding corded pulse, burning on the surface, fiery red tongue, constant tendency to local inflammation and buffy coat of the blood incident to the diseases of this period, no doubt can exist of their true character, and the propriety of placing them in that rank which we claim for them.

THIRD EPOCH.

Although the diseases of the State maintained up to 1834 the character described as belonging to those of the preceding epoch, yet after 1830, they began sensibly to change, especially those maladies that belong to the winter months. During the intervening years of 1830 and 1834 the inflammatory affections of winter assumed rather a low typhoid type, whilst the fevers of summer displayed less of that high toned vascularity, than had been their wont, and were attended with little fatality. In 1834 the change in type and character as contrasted with 1828 was complete and striking. During the summer and autumn of this year the red and scarlet livery of past years had disappeared, and disease henceforth robed itself in darker and more gloomy colors. The cold stage of fever had heretofore attracted no other attention than as ushering in the stage of preternatural excitement, the intensity and violence of which alone marked the degree of danger in the case. To local inflammation, or that sinking and collapse which ensued upon the breaking up of such high unnatural excitement in the system, was directed the attention of the physician for the issue of these evils. Disease then was bold in its approach, open and undisguised in its conflict with the constitution. But in 1834 we find its approach was insidious and unobserved, giving no serious warning of its proximity, until the unconscious victim was secure in its grasp. The patient first complained of depression, *heat and burning*, when to the touch the surface was icey cold. That cold, that first stage, is now the stage of disease and of peril; and that reaction which in past days, was looked to with fear and trembling, would now be hailed as the messenger of returning health and vigor.

Causes of a prominent nature have been assigned for the change which took place in the character and temper of disease about 1818.—Whether those causes were adequate to the production of these new pathological features cannot be asserted with absolute certainty, yet such a conclusion is plausible and sustained in part by events in the history of other and older States. But for the change which we are now contemplating, the mind cannot fix upon any tangible or supposed cause with certainty or satisfaction. In many parts of Europe as well as America, it was noticed, about this time, that disease assumed an adynamic type. This change, though not so marked elsewhere as in Ala., was still sufficiently prominent to attract general attention. Watson and other observers seem to hold the opinion that it was in some way influenced by the epidemic cholera which immediately preceded. Owing to the fact that many cases of congestive fever, (then a new disease in Ala.,) resembled the cholera in some of its symptoms, the same conjecture has been made here.* But when it is known that the congestive fever of Ala., is identical with a disease, designated by the names "cold plague," "congestive typhus" and "cold sickness," that has occasionally shewed itself in certain localities along the Mississippi and Apalachicola low lands since 1820, it becomes obvious that this particular form of disease is essentially indigenous to the country, and that its development in Ala. was owing to local causes rather than any (*inexplicable*) foreign atmospheric agency. But this particular form of fever and all speculation on the subject aside, and it becomes our duty, so far at least as unerring testimony can make it, to state, that diseases of every form, season and locality in the bounds of the State became at this particular time characterized by a low enfeebled state of the circulation, opposite that which previously existed ; and that even the enlightened and observing surgeon, in cases of injury and violence, had frequently to stimulate and nourish, where but a short time before the most active depletion would have been required.

In relation to disease as it is presented during this epoch, (from 1834 to 1847,) it has already been intimated that there was a display of certain specific differences in various sections and localities of the state which could not be reconciled in any other way than by supposing that they were owing to some peculiarity attaching to the organic nature of each locality and region.

That the chemical character of the soil varies with the geological systems or formations, and that the morbific agents are as varied as these, is so evident as to require no argument. In fact, it is upon this hypothesis that the old and well established doctrine of malaria alone can rest with security :—for if disease were uniformly the same in regions distinguished by separate and peculiar physical characters, even in the same latitude, then that theory which is based on the supposition that these different formations would give out emanations that are identical, would not be entitled to serious consideration. But we find in Alabama that, although the diseases may belong to the same family, yet they are distinguished by certain differences as apparent as are the physical characteristics peculiar to the regions where they respectively prevail.—Finding that this connection and dependence every where exist, and the influence which physical geography exerts, independent of latitude, is not less apparent, the writer is forced to the conclusion that there will be no further advance in the science of ætiology without the aid of geology and chemistry ; hence the propriety of a sketch of the geological structure of the state, together with such brief notices of the character of the soil of its different regions, as an imperfect knowledge of the requisite sciences, want of personal observation and his limited time will allow the writer to make.†

* The Cholera did not prevail to any extent in Ala.; only a few cases occurred in Mobile and Montgomery.

† For the facts in relation to the Geological features that prevail in the State, we are indebted to a communication from and frequent conversations held with

The middle portion of the state, from its fertility, wealth, population and severity of disease, is the first to demand attention. This region, known as the Prairies, runs through the State from East to West, embracing the following counties ; Russsell and a part of Barbour, Macon and Tallapoosa, with a part of Pike ; Montgomery, Lowndes, Dallas, Wilcox, Autauga, Perry, Marengo, Sumpter, Greene, with a portion of Tuscaloosa ; Pickens, Bibb and Shelby. The strata of this section are of the cretaceous formation ; the boundaries of which are easily traced and defined. The lower stratum of the series consists principally of silicious sand, with various interstratifecations of green sand, clay and limestone ; above these is a bed of soft, inpervious, argillaceous limestone. This bed, commonly known as rotten limestone, is, in some localities, from two to three hundred feet thick ; while in others, it is found exceedingly thin, but never disappearing. Resting upon this stratum is a deposit of yellowish, pulverable limestone, which in a few instances is replaced by a pure white carbonate of lime.

This deposite, though occasionally extending quite to the surface, may be regarded as sub-soil. It is the repository of numerous fossil remains, and is well calculated to furnish a rich supply of nourishing material for the exhausted soil spread upon the surface.

The most perfect type of the cretaceous or prairie formation is between the Alabama and Bigby rivers ; most of the other portions being overlaid by hills, ridges, and slight elevations composed of sterile sand, gravel, or ferruginous clay. The prairies are somewhat undulating, and consist of varieties known as the *black*, *bald*, *post-oak*, *slough*, and *canebrake.* The bald prairies are destitute of trees ; sometimes, when they are elevated, barely covered with grass ; the extensive prairies, however, are abundantly supplied with this kind of verdure, and the soil consists of a deep, black, argillaceous mould resting on a sub-soil already described. The post-oak prairie differs from these very slightly. The slough prairies are generally lower than the others, being the valleys, which are traversed by serpentine creeks ; the soil is a dark grey. The canebrake soil differs but little from that of the slough prairie ; the color is usually lighter, it is more glutinous, and of much greater humidity.

The result of analysis by Mr. Ruffin, editor of the Farmers' Register, shows that the soil of the perfectly bald prairie contains from 50 to 70 per cent. of carbonate of lime, and that this ingredient grows less as the soil is covered with verdure, or trees ; while the slough or cane-brake soil is found to contain only from 3 to 15 per cent. of carbonate of lime, and frequently none at all.

At the instance of Col. James S. Deas, and others, an analysis of the prairie soil was made by Drs. Cooper, and Gibbs, of Columbia, South Carolina, which furnished the following results. The rich black soil (bald prairie) contained 25 per cent. carbonate of lime, and 28 per cent. of vegetable matter ; slough prairie from the same plantation, 15 per cent. carbonate of lime, and 25 of vegetable matter; the other ingredients, aluminous earth, with a small quantity of silex and iron. In proportion as the earth was penetrated, there was an increase of vegetable matter.

Mr. C. S. Hale of Mobile—who, impelled alone by an enthusiastic interest in the geology of a State exhibiting such varied and wonderful structure, has at an immense personal sacrifice made himself acquainted with most of its sections.

In this analysis there was no attempt made to separate the animal from the vegetable matters. There can be no doubt, however, that the animal matter greatly preponderates in these soils, especially as we descend;—the immense mass of fossils on the surface, increasing in the sub-soil below, and the absence of evidence that vegetable matter has ever existed in any abundance since this soil emerged from the ocean, would seem to be conclusive on this head.

The manner in which a deposite, similar to that of the prairie, is formed, can be seen in any of the innumerable shell banks about the Mobile Bay, which are exposed to the air. Small patches or striæ of a deep black or dark greyish mould, evidently the product of the epidermis of shells, and the remains of small marine animals, can be traced throughout those that have been long exposed. These shell banks in the course of a few years are covered with a coarse grass, and as vegetable matter accumulates from the floods in the river, vegetation of a higher order springs into existence.

But from this example of the formation of prairie soils, it must not be inferred that those of the prairies were formed under precisely the same circumstances. The fossil remains found in them are those which are peculiar to deeper waters, and such as are never found in bays or estuaries. Under these circumstances, the question naturally arises, whence came the vegetable matter necessary to the forest growth, which here and there exists.

For its solution, the opinion of an intelligent agriculturist is here given. In a communication to the Farmers' Register, he remarks that "immediately after the elevation of these lands, they were unfit for the higher forms of vegetable life; but that under the influence of heat and moisture, the growth and decay of the inferior grasses, for a long period of time, accumulated the requisite amount of vegetable constituents.—The annual decay of these grasses gave an accession of vegetable matter to the earth, which, by repeated rains, has been washed down the hills; and this increase of vegetable matter has given growth to trees, which, in turn, by the falling of their leaves, have continued their growth, and increased the fertility of the soil." [Col. J. S. Deas.]

Within the boundaries of the prairie region, in the vicinity of the rivers, though unconnected with them, there is an occasional low swamp of the largest forest growth; the soil is a deep black mud, resting on a subsoil of white clay which contains fossil remains in the greatest abundance; the beds of these swamps are generally as low, or even lower than the bed of the river, and though very fertile, were not brought into cultivation until very recently.

The winding streams of the prairies usually dry up in summer, leaving numerous holes, or natural sesspools filled with water. In these, as well as in the waters of the cisterns excavated in the prairie limestone, there is, after every rain or accession of new material, a green scum thrown to the surface; this fermentation ceases in 10 or 15 days, after which the waters become pure.

As a general rule, the prairie soil having any elevation, from the fact that it is very retentive of water, holds such as it has in summer with too great tenacity for evaporation to go on rapidly; hence, after the first two or three years, the superior healthfulness of the elevated prairies ; while the low lands along the creeks known as the slough prairie, the swamps already mentioned and the reed marshes have proved to be more certain and prolific sources of disease than any other formations in the State, the low alluvion of the river bottoms not excepted. In fact, some of these localities, as the reed marshes in Greene, though very fertile, have been abandoned.

For want of proper information as to their fertility, the farmers did not begin to cultivate the prairies until about 1830. From that time until 1837, the labor of the state increased very rapidly, and the prairies and swamps, together with many of the connecting sloughs and morasses were brought into cultivation. But at the same time that the abundance and richness of the products of this region are such as to constitute them the cradle of agricultural wealth, the great prevalence of a new and malignant form of disease has designated them as the bed of those enervating poisons, which have so often struck down the youth and vigor of the country and added new terrors to southern disease.

That portion of the state lying north of the cretaceous formation, presents very different features. Mr. Hale says "it sustains the character of a sedimentary, fossiliferous formation ; and consists of two groups, the oldest of which belongs to the upper Silurian, and extends over the greater portion of this part of the State. The other overlying deposit consists of several detached beds of bituminous coal." These beds extend over portions or the whole of Tuscaloosa, Walke, Blount, Bibb, Jefferson, Shelby and St. Clair counties, and are seperated by a chain of sterile hills from that bed of coal in North-Alabama which seems to be the "continuation of the great Apalachian coal field."

The North-Eastern counties, say, Coosa, Talladega, Benton &c. belong to the granitic formation. These two sections of the State present a broken and hilly appearance ; the valleys are traversed by beautiful, rapid creeks, frequently very wide and affording space for extensive farms. The soil is a yellow, argillaceous lime-stone intermixed with sand and gravel, abounding in organic remains and is occasionaly very productive. The counties of Talladega and Benton are the richest in this section of the State and have come into cultivation since 1830. During those summers when the waters are low and the condition of the atmosphere is such as to favor evaporation from the moist valleys of these two regions, (coal and granitic,) disease is very prevalent, but of a less malignant character than that of the prairies.

"Passing the southern limits of the cretaceous group, we meet with another of the ascending series, which comprise the strata of the lower tetiary formation, usually termed the Eocene." Corresponding with the lime-stone bed of the cretaceous and immediately joining it, is here presented a remarkable bed of lignite, with an underlying bed of clay. These beds are from 30 to 50 miles in width, extending quite across the State and marking in its course the southern limits of the cretaceous formation. This bed of lignite abounds in radiated iron pyrites, which on exposure to the atmosphere decomposes and forms a sulphate of iron. "This part of the Eocene is remarkable for being the seat of numerous sulphur springs ; as those of Clarke ; the Bladon springs ; those of Munroe and many others."

Some portions of the Eocene group may be resolved into three subdivisions. The lower comprises the principle bed of the Claiborne group, (shells.) The middle consists of an uneven, but not very elevated deposit of yellowish limestone, and is noted more especially as being the locality of monster skeletons of the Zeuglodon. The upper consists of what is usually termed the white or nummulite limestone. But in many places, the beds of sand, marl, limestone and clay are so variously interchanged and intermingled in the different deposits as to render it impossible, in many instances, to identify the strata by their mineral characteristics or to give them any systematic classification.

The white limestone group is found situated, very generally, in detached and elevated masses; it consists principally of carbonate of lime, and varies in its complexion from pale yellow to a pure white; it is devoid of argillaceous qualities.

Embraced in this region are the counties of Munroe, Washington, Clarke, Conecuh, Baldwin and Mobile, of South-Alabama. Throughout this region is presented a sandy soil, poor in productive material and freer from causes of disease. The exceptions are to be found along the creeks and branches that here and there struggle through a loose soil, or along the river bottoms. In this region, the Alabama and the Bigby rivers come together making sluggish streams or semilagoons, known as the Mobile, Tensas and Spanish rivers.

From a short distance above the junction of the Bigby and Alabama rivers, extending to the city of Mobile, are extensive bodies of low, swampy alluvions, of mixed, variable and doubtful character; such as low, open, muddy morasses filled with green coarse moss and stubble cane; shell banks of varied elevation extending for miles on a line parallel with the river and an occasional island, studded with a dense forest. The soil throughout this low bottom region is of recent formation and devoid of argillaceous qualities.

There is an occasional farm on the islands of this region; the malaria arising from which is so active and abundant as to defy the attempts of the white man to make a permanent settlement.

(*To be continued.*)

II.—*On Dysenteric Diarrhœa—A peculiar form of bowel affection, which has occurred in Memphis and its vicinity in the last three or four years, with a report of seven cases—their treatment, &c.* By Lewis Shanks, M. D.

Bowel affections may truly be regarded as the scourge of the Mississippi Valley. In that portion bordering the Mississippi river, from St. Louis down, no season of the year is exempt from these affections in some one, or more of their various forms.

Diarrhœa, in its various grades of violence, and stages of continuance, is the form most frequently presented, and indeed, may be said to exist in a greater or less degree, in all seasons of the year, as the endemic of the valley.

The characteristic symptoms of the numerous cases of Diarrhœa occurring along the Mississippi river, as indicated by those which present themselves here for treatment are nearly the same. Though somewhat influenced by climate and different localities, the difference is mainly dependent on the constitution of the patient, the causes that co-operate in producing the disease, the neglect of, or improper treatment, the stage of its continuance, &c.

A great business thoroughfare like the Mississippi river, is sufficiently thronged at all seasons of the year with persons unacclimated and unused to the water, to keep the disease in constant existence; but it is not the unacclimated alone who suffer from it. At this point, (Memphis,) the oldest citizens occasionally suffer with bowel affections; and if I am not incorrectly informed, it is the case generally along the river. While this is true, however, those unacclimated, and unaccustomed to the water, are much the most numerously attacked, and by far the greatest sufferers.

It is not our present purpose to trace out the causes, symptoms, and treatment of the Diarrhœa of the Mississippi Valley; but to describe the pathognomonic symptoms of a peculiar form of bowel affection, which has occurred in Memphis and its vicinity, in the last three of four years; and to show its dissimilarity to diarrhœa, or dysentery.

This form of disease has presented itself in sporadic cases at all seasons of the year; and though it has been introduced by, or supervened upon symptoms of diarrhœa in its first stage, it has been more like dysentery in its progress and results; and for the want of a more appropriate and expressive name, I shall call it dysenteric diarrhœa.

It has been unlike either, however, here in its progress, and especially in its results, for such has been its malignity, that most of the cases have proved fatal, under the various modes of treatment adopted by different physicians.

Most of the cases have occurred in persons enjoying the comforts of life; and some of them in persons of good constitution. The privilege and advantage of post-mortem examinations have not been allowed, to determine the anatomical lesions that have been produced by the disease in its different stages.

Without this indispensable means of learning the true nature and the effects upon the viscera involved, of a disease peculiar in its symptoms and extraordinarily fatal in its results, it is not surprising that diversity of opinion should exist among the physicians who have treated it, both as to their pathological views, and their course of practice.

In the absence of such knowledge of the condition of the viscera chiefly affected, which could only be obtained by dissection, I can only determine the character of the disease, or make an approximation to its true pathology, by presenting my own clinical observations and such as have been furnished me by others; and by such deductions from them as they may warrant.

This I shall endeavor to do by reporting a few cases, in such a way as to show the symptoms of the disease as it has progressed through its different stages and give an outline of the treatment adopted in each case, for the purpose of shewing the influence of the remedies used upon the disease, in modifying the symptoms and determining the results.

In the cases given, the date of their occurrence will not be regarded so much as their character, for the purpose of presenting in the first cases the striking pathognomonic symptoms of the disease; and in others, such modifications as occurred in its milder, and less striking form.

Case 1*st.* J. A., aged about 18 years, of good constitution, had complained two or three weeks of disordered bowels, attended with some griping pains, or rather uneasiness, and increased frequency of the dejections, which were laxative in their character.

July 7th, 1846, I saw him first. Five days previously, discharges of blood and lymph supervened upon his previous diarrhœa, which, according to his own, and his father's description, continued to increase in quantity up to the time I saw him, without being either mixed, or alternated with fœcal matter.

These discharges were attended with considerable pain in his bowels, and occurred eight to twelve times in twenty-four hours. They were described as consisting of nothing but blood and jelly. Some thirst, and a moderate degree of fever had also been complained of.

On the fifth day after the bloody discharges commenced, when I first saw him at 5 o'clock, P. M., his dejections were dark, putrid serum and blood; like the first washings of bloody, putrid flesh; from a gill to half a pint in quantity. His stomach that morning had become irritable.—His tongue was furred, but broad, flaccid and moist. His pulse was so small, feeble, and frequent, as not to be counted accurately. His extremities were cold, shrivelled, and sodden in appearance; the entire surface relaxed and bathed in cold perspiration. His features were contracted and sharp, and his countenance cadaverous, presenting altogether an array of sypmtoms, which indicated an almost hopeless condition, as the general aspect of the case had evidently been produced by a state of the bowels either absolutely gangrenous or bordering on it.

The treatment adopted was morphine, from half to a grain, combined with eight grains of acetate of lead, given *p. r. n.*: a large blistering plaster over the abdomen, covered by frequent re-lays of flannel wrung out of a decoction of hops, as hot as could be borne: the extremities rubbed with dry mustard, and large sinapisms applied to them. Port wine and brandy toddy as drink; and as much ice allowed to be eaten in lumps as he desired. Iced injections, of half a gill of starch, acetate of lead, gr. xx., and tinc. opii. ʒ i—to be given after every discharge.

In a few hours, finding these appliances unavailing in producing reaction, ice was rubbed over his extremeties and body—the surface immediately dried and well chaffed with coarse towels.

These applications of ice followed by friction, were continued so long as the shrivelled, relaxed and clammy state of the skin existed, and were renewed for two or three days, whenever this appearance of the surface recurred, in any degree.

The ice thus applied externally, soon produced increase of plumpness, redness and partial reaction, and finally restored the capillary circulation, the heat of the surface, &c., and doubtless was mainly instrumental in relieving the almost gangrenous condition of the bowels, and restoring curative and healthy action.

10th. Reaction established. The discharges on the 8th and 9th were diminished in quantity and reduced in number to four, or six in the twenty-four hours; and gradually, as reaction took place, changed from their dark and putrid character to the appearance of fresh blood, largely mixed with lymph. The thirst diminished—pulse 110 to 120—but slight tenderness upon pressure over the abdomen had been complained of.

Treatment.—Morphine and Acetate of Lead continued; ice allowed to be eaten freely; Infusion of green Persimmons ℥ iv. with Tinct. opii. ʒ i., as an Enema to be given after each dejection—Emollient poultices to the abdomen, and mucilaginous drinks.

11th. Symptoms improving—slight exacerbations of fever in the evening. Dejections smaller, more fibrinous, and less bloody in appearance. Treatment continued.

13th. General symptoms better. Dark bilious discharges alternated with thick healthy looking pus, tinged with blood, have occured in the last two days. One discharge of pus, striated with blood, was at least a gill—others less in quantity. His thirst diminished. Exacerbations of fever at night continue.

Treatment continued.—Rice water for drink and nourishment.

16th. Convalescent. Discharges of pus much diminished. Bowels still irritable and the dejections too frequent; but improving in appearance. Tongue cleaned off—some appetite—still some fever at night.

Under the occasional use of opiates, and opiate astringent injections, and the Muriated Tinct. of Iron as a tonic, with a rigid diet, consisting of rice water, gruel &c., he gradually recovered.

His much emaciated frame was slowly replenished again, as his bowels continued for weeks in an irritable state, and consequently his small allowance of food passed off too rapidly to furnish much nutriment to the system.

Case 2nd. Mrs. I, aged about 30 years, of delicate constitution, from previous disease, and the use of much drastic medicine for several years, had been complaining for some weeks of disordered bowels.

On the 2nd of July, 1846, after more than usual fatigue, she ate, at a late dinner hour, with other food, boiled corn; that night she complained of pain in her bowels, and had towards morning muco-sanguinolent discharges. Calomel and opium were given in the morning, and paragoric in the evening.

July 14th. She was visited by Dr. Frayser. Her pulse was soft and about 120 in the minute. Tongue slightly furred, with a thick white coat, thirst considerable—surface natural in temperature. There had been considerable pain in the bowels the two previous days, but it had nearly subsided. Tenderness upon pressure over the abdomen, inconsiderable. She had three discharges in twenty-four hours—in quantity about half a pint each, consisting of blood and lymph, with a preponderance of lymph, a portion of which exhibited the transparent appearance of animal jelly.

Treatment.—Hydrarg. cum Creta with Dov. powd.; ice-cold, mucilaginous drinks; and the same, as injections with Tinct. of opium. Hot fomentations, with decoction of hops, frequently applied to the abdomen.

July 7th. On the 5th and 6th the discharges occurred about every three or four hours—in quantity from a gill to half a pint. They were dark and sero-sanguinolent, with flakes of opaque lymph, putrid in appearance and smell on the seventh. The pulse 130 to 140 and feeble; abdomen swollen and tympanitic; no tenderness on pressure; extremities and face cool, and presenting a purplish hue.

Treatment.—On the 6th ʒ i. of Creosote to ℥ xij. starch was administered as an enema, which produced no pain or irritation and was retained about six hours. After the enema the discharges occurred at intervals of about six hours. They were darker and more consistent—still however resembling more than any thing else, the first washings of blood, putrid flesh. The creosote injections were twice more repeated with seeming good effect, other remedies continued.

July 8th. All her symptoms worse, although on the previous day she seemed to have no apprehension of a fatal result, not feeling herself dangerously ill; the bowels were approximating a gangrenous state, if it did not really exist, as the discharges became more and more putrid, the pulse more feeble and frequent, and approaching dissolation manifest.

July 9th. Though every means that promised any benefit were employed to sustain her, she died this morning.

This case succumbed on the morning of the ninth day from the attack, evidently from mortification of the bowels.

Case 3d. R. I. S., aged four years, of delicate constitution, with a tendency to a laxative state of the bowels for several months, was attacked with Hooping-cough about the 15th of August. Six or eight days after the symptoms of Pertussis commenced, the discharges from her bowels, though consistent and healthy in appearance, became too frequent, and were slightly tinged with blood.

July 24th. She had considerable fever—tongue furred and white, was a good deal distressed by her cough, her discharges became more frequent, consisting of blood, mucus and lymph, of a tenaceous pasty consistence, attended with considerable uneasiness and griping pain in the bowels, which were relieved for a considerable time after each dejection.

29th. Her discharges in the last three days numbered 6 to 8 in 24 hours. They were in quantity from ℥ j. to ℥ iv., and consisted of blood and lymph pretty well mixed, presenting a plastic appearance. In some of them the lymph so much predominated, as not to be entirely mixed and colored with blood, and presented the appearance of thin, transparent, tenaceous jelly. These discharges were alternated once or twice in the 24 hours, generally in the morning, by bilious fœcal discharges of favorable appearance. The feverish heat diminished, pulse still frequent, thirst considerable, pain only complained of a short time before and during the discharges; tenderness upon pressure inconsiderable, cough distressing, but without pain in the chest.

30th. The past two days the feverish heat was less—perspiration considerable, except three or four hours in the night, when the exacerbation of fever produced increased heat and dryness of the surface; pulse frequent; thirst less ardent, except during the exacerbation at night.

The sanguino-lymphy discharges gradually changed to an appearance like tolerably thick mucilage, with a considerable quantity of brownish coloured, shreddy flakes of disorganized membranous structure, so soft and fragile, as to be easily broken to pieces.

But three of these discharges in the last twenty-four hours, and they like all the previous ones but slightly offensive.

31st. 4 P. M. No discharge from the bowels for the past sixteen hours until now. It is thin, sanious and very offensive ; very much in appearance and smell like the pent-up discharge from a stump, not disposed to heal, at the first dressing after amputation. The general symptoms indicate a giving way of the system.

August 2nd. This morning she died. In the last thirty-six hours she had but three evacuations, all of the sanious putrid character of the one before described. The feeble pulse, prostration, shrunken features, all denoted the fatal lesion of the bowels, which the discharges indicated.

This case terminated the night of the ninth day from the attack. It was much modified, the distress greatly increased and the fatal result the more certainly produced, by its complication with hooping-cough, in its inflammatory stage.

The treatment consisted of a small amount of mercurials, acetate of lead, and diaphoretics in combination with opiates. Ice allowed to be eaten at pleasure, and cold gum water as drink and nourishment ; iced mucilage with laudanum as injections, fomentations to the abdomen followed by a blister ; quinine was also given to mitigate the nocturnal paroxysms, and finally stimulants &c., to sustain the system.

Case 4th. D. H., aged about 50 years, of active temperate habits, and vigorous, healthy constitution, was attacked with Colic, produced by imprudence in eating, on the 10th of August 1845. The pain and distress were relieved in a day or two and he resumed his business. He continued however to have occasional uneasiness in his bowels and several dejections every day, though he disregarded these tokens of insidious and serious disease still preying upon him, and continued his usual habits of eating and attention to business, not heeding the slight uneasiness in his bowels, or the frequency of the discharges, and not even noticing their appearance or character.

Finally, from an increase of complaint, and an evident appearance of serious disease, he was induced by his family to retire from business to his dwelling, and on the 21st of October, forty days after the disease had its origin, Dr. Frayser was called to see him. He was then considerably reduced, his skin dry and scurfy—his pulse normal, tongue slightly coated, but moist, and but little thirst—no complaint of tenderness over the abdomen of consequence.

His dejections were four to six in twenty-four hours, and he discribed them as having been free and preceded by uneasiness in his bowels, which was relieved by each evacuation, for a time. He had never observed their appearance. His appetite had continued pretty good.

Hydrarg. cum Creta with Dov. powder, were prescribed for him.—Mucilaginuos drink and a simple bland diet, and his discharges desired to be preserved for inspection.

The succeeding forty-eight hours the discharges occurred at intervals of four to eight hours, with the same feeling he had previously described. In quantity they were from a half to a pint, and consisted of blood and lymph, tolerably consistent and plastic in appearance—inodorous and without any admixture of fœcal matter.

The next morning he had dark bilious fœcal discharges, as favorable in consistence and appearance as could be desired, and discharges of this character occasionally occurred, until a few days before his death; but throughout the continuance of the disease, the evacuations were mainly the product of the extensive lesion of his bowels, in its different stages, and generally united with bilious or fœcal matter.

The general discription of this interesting case is now brought up to the third day of medical treatment when the local cause of these alarming discharges had still but slightly involved the general system. The pulse, appetite, appearance of the tongue, heat of the surface, secretions of the liver, kidneys &c, still were seemingly but little influenced by the disease.

From this time more appearances of inflammatory action began to develope themselves. The discharges lymph and blood—the lymph so predominating in portions of them as to present the appearance of tolerably thick animal jelly. Evening exacerbations of fever, with restlessness and increase of thirst, set in, during which the proportion of blood, of a more florid color, was increased, and the lymph, which gave consistence to the evacuation, assumed a thicker, and more opaque fibrinous appearance.

These general symptoms, without any notable complaint of pain or soreness upon pressure over the abdomen, gradually, in the course of a few days, run into another stage of the disease, in which the fever was more from irritation, and the discharges became puro-sanguinolent. The pus striated with blood and containing an admixture of fibrinous matter, partially disorganized, or dissolved—then gradually assuming, as the disease progressed, a dirty, brownish or dark purulent and tenaceous appearance and consistence. During this change in the character of the evacuations the evening exacerbations became less and less distinct and the appearance of fibrine and florid blood, produced by increased excitement during the evening exacerbation, gradually disappeared in the stools. The discharges became more and more offensive, and of a dark, dirty, thin sanious appearance, still presenting the kind of tenaceous, flowing consistence which would be produced by a large admixture of pus. These changes in the evacuations were accompanied by corresponding changes in the general symptoms; until finally, without much suffering, and without seeming to realize his danger, though an intelligent man, his physical and mental energies both gave way, and the closing scene occured on the 14th of November, after 14 days of medical attendance, and near two months after the attack of Colic from which the commencement of his disease must be dated.

The discription of this disease has been hastily run through, without giving the treatment, for the purpose of reporting connectedly the symptoms of the different stages of the disease; and particularly the character and changes, in the different stages, of the evacuations from the bowels.

The treatment consisted of venesection, cupping, a moderate use of mercurials, acetate of lead, opiates, mucilages, formentations, opiate injections, which were well received and retained; and towards the close, tonics and astringents; and finally stimulants.

Case 5th. Mr. B, aged about 25 years, was of regular and temperate habits both in living and in business; but of spare delicate frame, though he had generally enjoyed pretty good health. He had frequent discharges from his bowels for eight or ten days without much pain; but being pretty large and free, he was much hurried when the desire to stool came on. About the 21st of August, 1844, he came under medical treatment. His discharges then were 6 to 8 in twenty-four hours, and from a half to a pint in quantity, and consisted of lymph and blood.

This case, in the character of the discharges, the symptoms of the disease, its progress and termination in death and in its general treatment, was so much like the last, that it is unnecessary to go into details in its report.

It presented, however, more uneasiness, amounting to slight pain in the bowels—more fever and evidence of inflammatory action.

Death occured on the 3d of September; about the 14th day from the commencement of medical treatment.

Case 6th. M. R. H., aged 20 years, of good habits, but delicate constitution and subject to irregularities of the bowels, was attacked about the 10th of August, 1846, with Diarrhœ.a The increased frequency of the discharges, attended with some pain, continued to the 20th, when the pain increased and the evacuations became bloody. On the 22nd the disease was clearly recognized as the fatal form of dysenteric diarrhœa. The discharges were pretty large, and consisted entirely of lymph and blood, attended with considerable pain, and the three previous days with more pain than usual in this form of disease. On the 24th but little pain or soreness was complained of, though nothing but the free and large stools of blood and lymph, unmixed with bilious or fœcal matter, had passed through the bowels.

Dr. Frayser, who was the attending physician, having taken as much blood from the arm and by cups from the abdomen as seemed necessary, in consultation with him and Dr. Ashbel Smith of Texas, we decided to carry out fully in this case the Mercurial and Opium practice. Accordingly, calomel grs. xx., with one to two of opium were prescribed every four hours, until the desired effect was produced—fomentations &c. to be continued.

The next day (25th) he had dark bilious stools, consistent and as favorable in appearance as could have been expected. So soon, however, as the bilious secretion and fœcal matter from the upper bowels passed through, the sanguino-lymphy evacuations occurred again, without any improvement in their appearance.

Two to three doses of the calomel and opium were given in the twenty-four hours for three days, with the view to produce ptyalism—and some signs of mercurial influence upon the gums and salivary glands were produced; but no permanent improvement in the symptoms of the disease resulted from it, and so much irritation, of a nervous and feverish character, seemed to be produced by it, that the further use of mercurials was abandoned.

28th. The discharges were less bloody and contained more opaque lymph of an appearance resembling somewhat dense fibrine. No pain or sorness in the abdomen.

31st. The past three days the discharges have been more puro-fibrosanguinolent, occasionlly alternated with bilious fœcal matter. Considerable fever, with marked evening exacerbations.

Sept. 4th. The irritative fever continued, with the sanguino-lymphy evacuations—the blood, the last two days, largely predominating in quantity, but rendered solid and tenaceous by viscid lymph mixed with it. The last 24 hours preceding his death, which occurred this morning, the evacuations indicâted passive hemorrhage, as large quantities of dark blood passed off in coagulated lumps, rendered tenaceous by the lymphy admixture.

This case continued about 14 days after the bloody discharges commenced. After the use of mercurials were discontinued, the treatment consisted of opiates, acet. lead, mucilages, fomentations, blistering, anodyne, mucilaginous and creosote injections, &c. As a laxative, an equal mixture in bulk of lac. sulphur, Cr. tartar, and calc. magnes. was occasionally given.

More pain was complained of during the first three or four days, and more feverish excitement ran through this case than any of the others reported.

Case 7th. S., a negro boy, aged about ten years, was attacked on 2d of October, 1846, with pain in the abdomen and bloody discharges from the bowels.

Oct. 5 I saw him first. His discharges were described as consisting of blood and mucus during the past three days, and attended with slight pain. His evacuations through the day had been preserved and amounted at 3 o'clock P. M. to about a pint—and were chiefly blood and lymph. He complained of considerable pain before and during the dejections. His pulse was rather frequent, but natural otherwise—tongue thinly coated with a white fur—the surface natural in temperature.

Treatment.—V. S. ℥ xvj—Emplast. Canthar. nearly large enough to cover the abdomen. Frequent relays of flannels wrung out of a hot decoction of hops. to be applied over the blistering plaster. Opium gr.j. calomel, gr. j. acet. lead, gr. viij. to be given after each evacuation until they were arrested—mucilage for drink and nutriment and ten grains of quinine to be given next morning.

6th. Discharges diminished in frequency and quantity, and contain less blood. Pain relieved. Treatment continued, with infusion of persimmon and oak bark, and laudanum as an enema after each discharge. Ten grains of quinine to be repeated next morning.

8th. Discharges more frequent and decidedly purulent, though of thick and healthy appearance, and striated with blood. Slight irritative fever. Opium and the opiate injections continued to control the discharges.—Muriated tinc. of iron as a tonic. Diet rigid—consisting of nothing but gruel and rice water.

10th. Improving slowly. Less pus, and some dark feculent matter has been discharged.

Under the continued use of the tonic and the occasional use of opiates, and the most rigidly restricted diet, consisting of farinaceous drinks, he gradually recovered.

These cases are reported mainly from personal observation. All of them, except the 3rd and 7th were seen and some of them were attended by Dr. Frayser who has aided in making out the reports of them.

Of the numerous cases of bowel affections that have presented themselves in a wide field for practical observations, of near twenty-five years, here, and where I formerly practised in Virginia, I can call to mind but few others like these. The late Dr. W. Christian, who for long and extensive experience, and for correctness, and discrimination in his observations of diseases, had no superior in this country, has frequently remarked to me that he had never seen such cases until a few years past, and that he had seen no satisfactory description of them; and a short time before his death, he assured me that he had never succeeded in curing but two cases of this form of disease. Other physicians of long and large experience have expressed the same opinion, and acknowledged an equal want of success in its treatment.

The descriptions given by the best writers of the different grades of violence in epidemic dysentery, and the difference in the symptoms and results of sporadic, from epidemic dysentery, and in their descriptions of other affections of the bowels attended with discharges of blood, furnish no satisfactory and perfect account that we have met with, of the commencement, progress, and termination of a similar form of bowel complaint.

We find well described, and I have often seen dysentery, both epidemic and sporadic, ushered in by the usual premonitory symptoms of fever, produced by various causes, and developod as the effect of the fever, or the influence of the cause which produced it, concentrated upon the bowels, constituting what was not inappropriately termed by Sydenham and other old writers, as fever turned in upon the bowels.

We find well described, and have often seen dysentery resulting from irritating injesta, acting directly upon the bowels and producing primarily or secondarily a dysenteric affection; but in the former, the dysenteric affection of the bowels was evidently secondary to the general disturbance of the system; and in the latter, the general and local symptoms were different from the cases I have briefly described. The general symptoms indicated more inflammatory action, and the local disease was attended with more pain and soreness in the parts involved. The discharges were small, frequent and muco-sanguinolent, attended with tormina, tenesmus, and the almost constant disposition to go to stool, from a feeling like something was in the lower bowels that must be passed off. The discharges mucus and blood, and produced by inflammation and congestion of the vessels of the mucous follicles of the colon and rectum, giving rise to exalted secretion of mucous and also exhalation of blood.

In this peculiar form of disease, slight uneasiness in the bowels, followed by free discharges of blood and lymph, mark its first stage: or if the discharges be small and frequent, and mixed with mucus at first, they become free and sanguino-albuminous, or lymphy, without being attended with tormina, or tenesmus, or a constant disposition to go to stool; and the free evacuations afford almost entire relief for several hours from the uneasiness, or pain in the bowels, if any exist; and also the desire to go to stool. The rectum is but little involved in most of the cases, as injections produce but little uneasiness and are retained for hours, an then only expelled, when the accumulation in the colon and rectum produce a desire to go to stool, which could be deferred in some of the cases, for hours, at the will of the patient: the pulse but little influenced—thirst inconsiderable, and the appetite but little impaired for several days.

These local symptoms, together with but slight general febrile disturbance and the subsidence of pain and soreness in those cases in which they exist, in two or three days, characterize this peculiar form of disease, in its first stage.

Its second stage was characterized in case 1st, by early symptoms of extensive gangrene of the bowels, indicated by the dark offensive sero-sanguinolent discharges, with shreddy flakes of lymph floating in them, accompanied by a frequent thready pulse, and a general collapsed state of the system.

In the second case, the second stage of gangrene indicated by the symptoms evidently commenced on the 5th day, and terminated in death on the 7th, with all the symptoms of extensive mortification of the bowels.

In the 3d case, the second stage was indicated on the 5th day, by disorganized membranous structure, floating in a tenaceous fluid, (probably the gum water which was the only drink used,) passing through the bowels; followed on the 7th by a very offensive ichorous discharge, and resulting in death on the ninth.

In the first, and in the other cases in which pus was discharged, it did not occur until from the ninth to the twelfth day of the disease, and they were consequently more protracted.

The second of the seven cases terminated in death on the 7th day, from gangrene. The third terminated in death on the 9th day, probably from extensive sloughing of the mucous coat of the colon, followed by an offensive ichorous discharge, and an irritative fever which exhausted the system.

The fourth, fifth and sixth cases ran into an irritative fever, produced from ulceration, or a morbid condition of the mucous surface of the colon, giving rise to copious discharges of puro-sanguinolent matter, by which the vital energies of the system were exhausted, and death produced.

In the first and seventh cases, the pus was consistent and healthy in appearance, and not mixed with dark, but striated with florid blood.

After a tedious convalescence, under a most rigid course of dieting, they recovered, though in the first case the thick healthy looking pus amounted to at least four ounces at a time, and continued in diminishing quantities for days.

I have briefly presented the characteristic symptoms of these cases, as a peculiar form of bowel affection; I have pronounced the morbid condition which produced death, without having had the privilege of making a post-mortem examination in a single case.

These conclusions have been based upon the uniformity of the condition assumed, as found by morbid anatomists, to be the cause of the character of such discharges, the con-comitant general symptoms and the results.

While there is but little room for diversity of opinion, as to death being produced in the second case by gangrene of the bowels, the extensive researches made in morbid anatomy do not enable us, without having inspected the parts diseased, to determine either the exact condition which gave rise to the large discharges of lymph and blood which occurred in the first stage, or that which produced the copious discharges of purulent matter in the last stage of the disease.

The post-mortem examinations made and reported by all the most prominent writers on dysentery are unsatisfactory as to the morbid condition of the bowels which would produce so large an exhalation of blood and lymph ; and notwithstanding all the labour bestowed on this subject, we are left to base a conjecture of the pathological state indicated by such discharges more upon the general effects of congestion and inflammation upon the mucous membrane, than upon any precise information furnished, as to the morbid condition in this disease.

Deducing an opinion from the condition found in the different grades and stages of dysentery, this peculiar discharge would not likely result from inflammation and engorgement of the mucous follicles alone ; but from that condition of a large portion of the mucous and sub-mucous structure of the colon, producing lymph as a result of inflammation, and blood by exhalation from the highly engorged vessels ; and a small amount of mucus from the inflamed follicles, and too much engorged to prevent secretion. These products, partially mingled, might produce the quantity and character of the discharges in these cases ; and this condition of the mucous and sub-mucous structure might result, either in extensive gangrene or in sloughing, to be followed either by unhealthy ichorous discharges or healthy pus, dependant on the condition of the vital energies of the general system.

This extensive engorgement and inflammation of the mucous and submucous tissues might be so relieved by the copious exudation of lymph and blood, as to prevent sloughing, and result in that condition, which has been found to produce a large quantity of pus without a broken ulcerated surface.

The probable correctness of these conjectures is sustained by the post-mortem appearances found by Cheyne, Stokes, Watson, Twyning, Carswell, Andral, Gendrin, Horner, Bell, Gross, &c. The observations of these and other pathological Anatomists shew, as a cause or result of Dysentery—extensive inflammation—congestion—gangrene—thickening and ulceration of the mucous coat—exudation of lymph and blood, &c. As a morbid product Carswell found in the West Indies "that from the Ilio-cecal valve to the rectum, the whole mucous membrane of the colon often presenting one vast ulcer of a cherry red, or reddish black, which looked like the advanced stage of Osteo-sarcoma." He also examined cases in which no ulceration had been found, and yet large quantities of pus was occasionally contained in the colon.

The probability that the large quantity of pus discharged in case 1st, did not result from a broken and ulcerated surface, is sustained by the observations often made, that a few ulcers of limited extent in the colon are sufficient by their irritation to produce death ; and also by the concurrent opinion of pathological Anatomists, that the reparation of ulcerated breaches in the mucous coat of the colon is tardy and uncertain in its accomplishment, and indeed it is contended by some that it rarely if ever takes place to any great extent.

In reference to the cause of this peculiar form of disease which has only presented itself here in sporadic cases in the last three or four years, I shall only remark that during a residence of ten years in Memphis, my observations have led me to the conclusion that diseases of all kinds have gradually become more and more inflammatory, as the clearing up of the country and the growth and improvement of the city have advanced.

Ten years since, diseases here were almost exclusively functional and congestive in their character; and especially in persons who had resided here long enough to undergo the relaxing influence of the climate.

Bowel affections, always very common here, were formerly almost exclusively Diarrhœa, attended with their albuminous discharges without accute symptoms of inflammatory action; latterly dysenteric and enteritic affections have been much more common.

In the progressive change here, from the functional and congestive diseases of the mucous and sub-mucous tissues of the alimentary canal to the more inflammatory forms of disease, may not such a condition of these parts occasionally have occurred, as to produce the complicated results of a congested and inflammatory condition of the inner membrane and mucous follicles of the large bowels.

On the treatment of this disease I have nothing that is new and but little of interest to offer. Its onset and progress have been so insidious and attended with so little pain and fever, that persons unacquainted with its fatal tendency have not taken the alarm and called in a physician early enough to arrest the disease in its incipient or early stage; and indeed without a careful inspection of the evacuations, all the other symptoms are calculated to deceive and mislead the physician. The consequence has been, that the disease has either progressed without remedies, or under the use of feeble treatment, until irreparable and fatal lesions have been produced by it. Upon this cause, and not its necessarily fatal tendency, has the almost universal want of success in its treatment depended and the opinion been so generally based, that it is a disease, when strongly marked, of incurable malignity.

The primary affection seems clearly to be congestion and inflammation of the mucous and sub-mucous tissues of the colon. The derangement of the stomach, liver and the general system which occurs in its course, being secondary and a consequence of the primary lesion. In the early stage of the disease, the immediate relief of the local congestion and inflammation is the most important indication, and it is more important in this form of disease than in ordinary dysentery, either sporadic or epidemic; where the local disease is more limited to the mucus follicles, and concomitant with it, other organs are involved, and consequently less tendency exists, on account of less concentration of disease, to rapid, extensive and fatal lesions of the inner surface of the bowels.

To effect this object, general and free bleeding, followed by local depletion—fomentations—stimulating foot baths, and finally counter irritation by a large blister, dressed with emollient poultices, promise most benefit. Auxiliary to these remedies opium, ipecac and acetate of lead, separately or in combination as the symptoms may require, should be used, to relieve local irritation, equalize the circulation, and produce perspiration. If local irritation of the rectum exists, producing tenesmus or disposition frequently to go to stool, it should be allayed by anodine starch injections, to which acetate of lead may be added with advantage.

Mercurial cathartics have not seemed beneficial, for though their desired impression has been made upon the liver, and favorable bilious and fecal avacuations produced, they have been followed by no diminution, if they did not produce an increase of the sanguino-lymphy discharges ; occasioned doubtless by the acrid bilious matter passing the diseased surface of the colon and rectum.

Unlike mercurial and other cathartics however have been the effects, of a combination of lac. sulphur, crm. tartar, and calc. magnesia in equal bulk. Its operation has seemed soothing and alterative, in changing the morbid exhalations to healthy secretions, and it is not incompatible, and need not interfere with the use of other necessary remedies, *pro re nata*, to give this mixture, in sufficient quantity, at intervals of from two to four hours, to pass freely through the bowels.

This is an outline of the treatment of the first stage of the disease which my experience would sanction ; allowing for the attainment of the object indicated, by the use of other remedies promising similar results, which the experience and preference of others may induce them to use.

In the use of depletion in the commencement of this disease, its necessity and the extent to which it should be carried, is not indicated either by the frequency or tension of the pulse so much as by the character of the evacuations, and the constitutional ability to bear the loss of blood. Fatal ravages are probably made by the local disease, before the circulation displays general phlegmasial action, and then the real amount of disease is masked by its internal concentration, causing the pulse to be oppressed and the general surface and the extremeties to be even preternaturally cool. The preponderance of transparent lymph, its increased consistency, or tenaceous, fibrinous appearance, the increased floridness of the blood, together with increase of uneasiness or pain in the bowels, are accumulatiug signs of the necessity of depletion, though the pulse may not fully respond to them.

The arrest of the disease and the final recovery in case 7th I attribute mainly to the large bleeding, seventy hours after the commencement of the bloody discharges, when neither the appearance of the tongue, the heat of the surface, nor the almost normal state of the pulse seemed to demand it.

In laying so much stress upon the importance of bleeding in the commencement of this disease, I am urging nothing new ; for in the treatment of dysentery from Sydenham down to the present time, the highest authorities, with few if any exceptions, are in favor of the lancet, except in camp dysentery attended with typhoid prostration, when it is only allowed to be used cautiously at the outset of the disease.

In the second stage the effect of remedies and the result of the disease are more problematical. If a tendency to prostration and collapse, accompanied by dark, fetid, sero-sanguinolent discharges, portending gangrene, occur, the system must be sustained and roused to healthy action if possible.

A more hopeless condition of this kind I never witnessed to survive, than that of case 1st ; and as the treatment briefly given succeeded in restoring reaction, and conducting the patient through a tedious and critical convalescence, from the evidently extensive ravages of the disease, to health, I should more confidently rely upon it than upon any other course of practice, and therefore need neither recapitulate nor enlarge upon it.

The influence of the ice eaten at pleasure, in allaying the thirst, and relieving the irritability of the stomach, and especially its influence, rubbed over the surface every twenty or thirty minutes, followed by drying and chafing, in relieving the blue, shrivelled and sudden relaxation of the skin, and the pouring out of cold sweat—in restoring plumpness and color, and warmth, and circulation to the surface, and life and healthy action to the diseased structure, deserves to be recorded as additional evidence of its curative powers in this condition of the system, hopeless of relief, and beyond the reach of a cure, by any other remedial instrumentality.

In the cold clammy sweat—and the feeble indistinct pulse and the great internal heat and oppression of collapse occuring in this form of disease, or from cholera morbus, and indeed from any other cause, no remedy in my hands has been so safe, and at the same time so potent, as ice used in this way.

Memphis, January 14th, 1847.

III.—*Report of the Board of Army and Navy Medical Officers, ordered by the Departments of War and Navy to investigate the causes and character of the disease which prevailed at Pensacola, Fla., in* 1844 *and* 1846.

United States Naval Hospital,
Pensacola, Fla., Nov. 13th, 1846.

To the Honorable Secretary of the Navy.

Sir:—The Board of Army and Navy Medical Officers, ordered by the Departments of War and Navy to investigate the causes and character of the disease which has prevailed at this place and its vicinity during the present autumn and that of 1844; and "to devise means of restoring the locality to its former healthy condition," has the honor to report,

That it entered upon the discharge of the duties assigned it, fully impressed with a sense of their great importance, and determined, if possible, that the result of its labors should equal the expectation of the Departments.

Early in the prosecution of its enquiry, after hearing a history of the disease from Surgeon J. Hulse, U. S. Navy, President, and examining, by his invitaion, a number of cases in the wards of the Hospital, the Board came to the conclusion that the disease in question is the autumnal fever of hot climates, differing in type according to the predisposition of the subject of it, and the degree of exposure to the *causa causans*; that it is of miasmatic origin; that during two several years at least, it has appeared under the forms of intermittent, remittent, continued and congestive; and that in many cases it has been of a character so aggravated, as justly to entitle it to the appellation of malignant or pernicious.

The next object was to endeavor to ascertain the source of the malarious emanations, which were supposed to be the primary or exciting cause of the endemic. With this view, it made an extensive and thorough examination, during two successive days, of all the grounds around and about the Naval Hospital, the Navy Yard, Cantonment Barrancas, Fort Barrancas and the Redoubt, which examination revealed to the Board the object of its search, viz: many marshes and fresh water ponds, having dirty, slimy and miry surfaces exposed to the action of a hot sun, and most of them so contigous to the three first of the above mentioned places, and so located in relation to them, that the effluvia arising from their beds and margins must be wafted by the prevailing winds, in a very concentrated form, to all of these points.

By reference to the accompanying map it will be perceived, that a chain of ponds, in some instances having connecting branches, running South of and within one hundred yards of Cantonment Barrancas and the Naval Hospital, extends Eastward till it nearly reaches the West wall of the Navy Yard.

Another chain commences near Cantonment Barrancas, passes in front of the Navy Hospital, and during wet seasons, by overflow, has an outlet a little to the Eastward of the Hospital. This very important chain happily affords natural facilities for drainage.

A third chain commences about half way between the Hospital and Navy Yard, following the direction of the table land, and at the foot of it makes a curve to the Northward within a quarter of a mile of the Navy Yard wall; and near this curve is a pond almost dry, having a muddy surface exposed and apparently abounding in materials of a most deleterious character. This chain has its outlet to the North, by Jackson's Bridge, into the Big Bayou, and can be easily drained, and its margin covered with sand.

Between parallel sand ridges other ponds run nearly East, West, and North of the Navy Yard and their natural outlet is into the Bay North of the Yard, to which their surplus waters can easily be conducted.

As an evidence that the ponds North of the Navy Yard are the chief source of the febrific agent which produced the disease within the Yard, it may be mentioned that the occupants of the range of buildings in the north part of the Yard suffered first and more severely than the residents of other houses.

The ponds near the West side of the Navy Yard are better protected from the sun's rays by the trees that surround and overhang them, which not only prevent the evaporation of the water and thereby the elimination of paludal effluvia, but confine to its source whatever malaria may be generated. This supposition obtains support by the fact that in the village of Warrington no more than two cases of fever have been known to exist this season, and these so light that had no enquiry been instituted, they would have passed unnoticed.

The Board has been placed in possesion of another fact, proving the correctness of its opinion as to the origin of the disease under consideration, which is, that the inhabitants of the surrounding country, and of the city of Pensacola, have enjoyed an entire immunity, the disease being confined to a space of not more than two square miles.

The Board is happy to state its belief that for so great an evil there is a remedy, which, if applied, will prove effectual and complete.

In the execution of this part of its duty the Board reccommends most earnestly the grading and drainage of the grounds which have been described. By filling the ponds with earth to the depth of several feet, and leaving a canal six feet wide at the top, walled up with bricks laid in cement, the possibility of standing pools in front of Barrancas and the Naval Hospital will be entirely precluded.

The chain which extends from the Hospital to near the West wall of the Navy Yard, has its debouche through the village of Warrington, and here again, grading and drainage are recommended. The parallel chains North of the Navy Yard, curving towards the North and East, may be made to empty into one canal, through which the water can be led into the Bay.

The Board respectfully suggests the propriety of placing the work herein recommended upon the Navy grounds, under the direction of the Commandant of the Navy Yard, in connection with the senior Medical Officer of the Hospital; and that on the army ground, under that of the commanding officer of the military works in the harbour, in connection with the senior Medical Officer on duty at the port.

Hereto appended is a history of the disease, as it appeared at the Naval Hospital in 1844 and 1846, accompanied by meteorological tables for the months of July, August and September in 1844, 1845 and 1846. (Vide Appendix A.)

Also, a history of it, as it occured at Cantonment Barrancas, under the observation of Ass't. Surgeon E. H. Abadie during the present season (see Appendix B.)

Very respectfully,

ISAAC HULSE, *Surg'n U. S. Navy, President.*
GEORGE TERRILL, *Surg'n, U. S. Navy.*
For H. A. STINNECKE, *Surg'n, U. S. Army.*
H. H. STEINER, *U. S. Army.*
JOHN C. SPENCER, *Surg'n, U. S. Navy.*
E. H. ABADIE, *Ass't Surg'n, U. S. Army.*
H. H. STEINER, *Ass't Surg'n, U. S. Army.*

Appendix A.
History, &c. of the Disease.

It appears, from all the information that can be obtained, that the locality to which the attention of the Board has been directed was regarded by the first settlers and by their successors, as remarkably healthy. It was never suspected of containing a focus of infection until some years after the establishment of the Navy Yard and Naval Hospital. In the progress of improvement, clearings were made about the ponds, and their borders were divested of the evergreen trees and shrubbery with which they were clothed, the foliage of which had previously presented an impervious barrier to the rays of the sun.

The first authentic account of any autumnal fever occurring here, dates back no farther than 1840, when it commenced at the village of Barrancas, and, before the end of the autumn, reached the Naval Hospital. From that time up to the summer of 1844 no notice is found of any unusual sickness.

In 1844 a bilious intermittent and remittent fever prevailed at the military cantonment at Barrancas. The precise date of its commencement is not ascertained, but it is known to be earlier than that of its appearance at the Naval Hospital, which was about the 1st of July. During that year it did not extend to the Navy Yard, as it appears from the accompanying extract from the Hospital reports.

In 1846, a similar fever appeared at Barrancas about the latter part of July, at the Naval Hospital about the 1st of August, and at the Navy Yard about the middle of August. During the present year it was marked with greater severity at Barrancas, the seat of the military cantonment and the Navy Yard, than at the Naval Hospital; and in the course of inquiries into the remote cause, it has been discovered that near these two points where it appeared in the most aggravated form, materials were exposed in a manner the most calculated to generate malaria in a concentrated degree.

For the history of the disease in 1844, the following extract is submitted from the report of the surgeon of the Naval Hospital.

"Early in July, a fever of an intermitting character appeared among the servants at the surgeon's house, and in the course of a month, or a little more, every individual of the family, consisting of 15 persons (nearly an equal number of whites and blacks,) fell under the influence of the malady.

"A short time subsequently to its appearance at the surgeon's house the disease extended to the hospital, and from the 4th of July to the 14th of November were enumerated among the inmates subject to its attack, patients 39, attendants 10, medical officer 1, which comprised all the persons in any way connected with the hospital, with only two exceptions.

"*Symptoms.*—rigors, chills, pain and burning sensation in the head, the pain extending along the spine, through the thighs and legs, and to the soles of the feet. Delirium in some cases—tongue swollen, with the impression of the teeth upon its edges—thirst great—vomiting of bilious matter—sometimes cholera—pains in the bowels; urine high colored, scanty, and irritating the mucous passages. The phases of the disease were almost as numerous as they are in yellow fever.

"It was treated with mercurial cathartics, cups to nucha and other seats of pain, sudorifics &c., and on remission or intermission, with quinine in large quantities. In general, it was easily arrested by these means, but relapses were frequent and the course of treatment had to be repeatedly renewed.

"The shipping in the port, the Navy Yard and villages near it, enjoyed a complete exemption from this complaint, while every person sent here from on board ship experienced an attack of it in three or four days after his admission.

"The remarkable characteristic features of this disease are the great prostration and nervous irritability which it causes even after a duration of only a few hours."

"*Case.*"—Chambers Pope, O. Seaman, admitted for ophthalmia, attacked with fever August 31st, 1844. Treated in the usual way, became much exhausted; had hiccups, tenderness of abdomen Sept. 7th and 8th,—convalesced, but was afterwards attacked with dysentery and died Nov. 18th.

Autopsy, by Assistant Surgeon Thornley.

"*Thorax.*—Effusion into both pleural sacs; old pleural adhesions of upper lobe of right lung, lungs engorged with serum—heart normal.

"*Abdomen.*—Liver much enlarged, indurated and of a dark fleshy colour; gall-bladder moderately distended with bile of natural colour; mucous coat of great curvature of the stomach of a blueish black colour and softened, tougher than natural in other parts; some adhesions between the concave surface of the liver and the intestines; slight effusion into the peritoneal cavity; pancreas hard; spleen natural, colon contracted—foul ulcer on mucous coat of the rectum. *Brain*, not examined."

The following description of the disease is given as it appeared at the Naval Hospital in 1846, during the months of August, September and October.

Symptoms—pain in head, back and extremeties, in light cases:—to those, in the severe ones, are added pains in the globes of the eyes; intolerance of light, nausea, vomiting of bilious matter either of a greenish, or of a yellowish colour; pain in the hypochondriac regions and in the abdomen, with intolerance of pressure upon the same. Cramps in the legs; loss of sensation in one or more of the limbs, which resolves itself into partial paralysis, generally, however of short duration. Delirium occasional, but not frequent, and when it occurs, it is not violent.—Tongue swollen, with the impression of the teeth upon it, but coated only lightly, sometimes not at all; occasionally it becomes, after a day or two, in the worst cases, dry and rough, and in this state there is nausea, vomiting and inability to take either nourishment or drinks. In the latter stages, in fatal cases, the tongue becomes tremulous. Thirst, in most cases not great; in some few, insatiate; and in these, there is intolerance of the stomach, so that only minute quantities of fluid are admissible. There is no great heat of skin at any period; in the worst cases the skin becomes cold and the capillary circulation is nearly destroyed at an early period. This takes place when remedies fail to act early, and the great viscera are engorged;—in these cases, all efforts to save are nugatory, death ensues speedily.

A child in the surgeon's house, ætat 7, was slightly ill on Monday. On Tuesday medicine could not be retained and evacuations could not be had from the bowels. On Wednesday, she was comatose and insensible, yet she swallowed stimulants and nutriment, and on Thursday was the fatal termination.

In the worst cases the stools at first were of a very dark colour; afterwards, with some exceptions where remedies acted well, they assumed a yellowish hue and good consistency. In the lightest cases, no bad looking stools were observed. The urine was in larger quantity than usual; although in some, at first, there was a paucity of it and that high coloured, and as the disease yielded, it was secreted in profusion, having a turbid appearance.

In a few instances there was great tendency to hemorrhage and the blood was observed to be remarkably thin, and not readily coagulable. Hemorrhages occurred in one, from the mouth, nose and intestines.—This was fatal in 7 days. In two, it occurred from the mouth. In one of these it was fatal; the other subject of it is still in a critical condition. The hemorrhage had not a duration of more than two days.

The peculiar characteristics of the disease are, great nervous prostration and protracted debility, even where the fever continues but a few hours.

The disease was intermittent, remittent and continued, and in the several forms there were patients who were subject to from one to six attacks of it during the season.

The condition of the atmosphere during its prevalence was such as to prove unfavorable to ordinary cases sent to Hospital from the ships of the squadron, with the exception of a few hereafter to be named.

There was an approximation to black vomit in the case of Sweet, who for 24 hours threw up dark greyish coloured flocculi mixed with mucus, and lay eructating it from the stomach to a distance, without effort and without apparent suffering. His skin became yellow before death, and livid about the neck and chest a few hours afterwards.

The attack was rarely preceded by premonitory symptoms; frequently the subject of it was unconscious of any indisposition one half, or even one quarter of an hour before he was found writhing with the agony of extreme pain.

It occurred not often, although it did sometimes, that there were three distinct stages; more frequently the patient had sensations of heat and chilliness at the same moment, or alternate chills and flushes in quick succession. Sometimes the intermission from this state took place after a sweat, and oftener after but slight perspiration.

The most usual course has been fever, pretty violent for eight to twelve hours; then a remission or alleviation of the symptoms for three or four; then an exacerbation of fever less aggravated than the first;—this was followed by a longer remission, or perhaps by an intermission for several hours, after which there was a slight exacerbation, which was most frequently the last before the establishment of convalescene.

The pulse was frequent but compressible and of moderate volume.—In no case originating in the hospital has venesection been practiced.—Two cases from the Navy Yard were bled previous to admission at the hospital, one of which terminated fatally in seven days, the other is not expected to recover.

Quinine has been relied upon as the chief remedy, to make an impression of its own upon the nervous system and brain more powerful than that made by the poison causing the disease; and the use of it to saturation has been attended with such results as to entitle it to be regarded almost as an antidote or specific. It has been given in all the stages, and in all of them, in large doses, it has proved a sedative.

The ordinary prescription after an attack has been proto-chloride of mercury, sulphat quiniæ àà gr. x. m.; followed by ol. Ricini ℥ i., or Enemata of Decoct, Sem. Lini. and ol. Ricini, three or four hours afterwards. Evacuations were ordinairly procured in due time, and these means, with the aid of cups, leeches, sinapisms, frictions with dry mustard &c., procured an alleviation of the most distressing symptoms.—The largest quantity of S. quinine given in one day and successfully, was 45 grains. The use of ice proved peculiarly grateful during the hot stage, both in the mouth, and at intervals applied to the head. If fever continued after the use of these means, Spts. Ether. nitrici, or this combined with ammoniæ acetat., or occasionally G. Camphor added, brought relief to the cephalalgic symptoms and procured moisture upon the skin. On the second day, it was common to prescribe S. quinine gr. x. to be taken early, and again at 11 A. M. If the point of saturation was found to be easily attained, (which was tested by the evidence of partial deafness and ringing in the ears,) one half or one fourth of the above quantity was ordered. Five grains twice a day were usually given for a few days after the fever had entirely subsided, and then, not unfrequently, to accelerate convalescence, Infus. Cinchon. and Serpentaria, or Porter. It is proper here to state that in the exhaustion which frequently took place soon after the attack, brandy-toddy was given in moderate quantity for a time.

To allay excessive vomiting and hemorrhage, various means were resorted to, such as the administering of ol. Terebinth, kreosote, lime water and morphia, lime water and milk, old opium, neutral and effervescing mixtures, &c.

Epispastics, frictions with hot spirits and capsicum, pediluvia with mustard, &c.

Among the most constant sequelæ of this disease were Diarrhœa and Dysentery, and as these exhibited an intermittent character, quinine figured again among the remedies. It was given in combination with opium and ipecac, with opium, ipecac and calomel, with opium alone, or with G. camphor, according to circumstances. Small enemata containing opium and acet. plumbi were occasionally resorted to. When there was slight constipation alternating with diarrhœa, quinine was given with pulv. Rhei. in proportion of gr. x. of the former to gr. xv. of the latter and it was thought, in that formula, to promote the cathartic effect of the Rhubarb. Tannin was frequently employed with good effect, either alone, or with opium.

Under an attack of this disease absorption of fluid has taken place to a considerable degree in a case of dropsy of the knee joint. In a case of chronic orchitis, the testicle was reduced to one fourth its former size, and in a case of node of the tibia, the pain and swelling subsided entirely. Relief was also experienced in two cases of neuralgic rheumatism, where there was disability of the deltoid muscle.

The following statistical table is given by the surgeon of the Naval Hospital, showing the number of cases of this fever that have occurred at the hospital from the 1st of August to the 31st of October 1846.—In this statement he has counted every attack or relapse.

The deaths were :

Emmett,	attacked	Oct. 2nd,	died	Oct.	5th,
Sweet,	"	Oct. 10th,	"	"	15th,
* Bampton,	"	Sep. 18th,	"	"	12th,
† Cantoni,	"	Oct. 4th,	"	"	15th,
† Mr. Bright,	"	Oct. 10th,	"	"	17th,
† Python,	"	Oct. 24th,	"	"	31st,
Total, 6.					

* Light attack, followed by Dysentery, which was fatal.

† Attacked at the Navy Yard, and removed to the hospital in from one to seven days afterwards.

Whole number of patients in hospital during August and September, 195, medical officers and attendants 22, total 217, among whom were attacks, ... 175

Whole number of patients in hospital during October, 207, medical officers and attendants, 23, total 230, among whom were attacks, ... 105

Total number of cases and relapses, ... 280

At the Surgeon's house where the family consisted of 11 persons, all were attacked and there were two deaths.

The following tables have been compiled from the meteorological journal of the Naval Hospital, and they are designed to show the parallel or contrasts between the seasons during July, August and September, of the years 1844, 1845, and 1846. It will be recollected that 1845 was a healthy season.

	MEAN OF THERMOMETER.		MEAN OF BAROMETER.	PREVAILING WIND.
1844	July, . .	84.55	30.67	S. W.
	August, .	81.51	30.63	S. W. to S. E. as 31 to 28
	September,	77.03	30.69	N. and E.
1845	July, . .	83.66	30.57	S. W. to S. E. as 35 to 19
	August, .	81.72	30.64	S. E. 31, N. W. and S. W. each 17
	September,	77.59	30.60	S. E. to N. E. as 15 to 19
1846	July, . .	80.92	30.16	S. E.
	August, .	81.26	30.16	N. E. and S. E. about equal.
	September,	81.10	30.21	S. E. to N. E. as 19 to 12

RAIN.

	1844.	1845.	1846.
January,	$7\frac{1}{4}$ inches.	$1\frac{1}{8}$ inches.	$11\frac{1}{8}$ inches.
February,	$\frac{3}{4}$ "	$\frac{3}{8}$ "	$8\frac{3}{8}$ "
March,	5 "	$1\frac{15}{16}$ "	$8\frac{15}{16}$ "
April,	$2\frac{1}{2}$ "	$\frac{9}{16}$ "	$8\frac{1}{2}$ "
May,	$5\frac{1}{2}$ "	$3\frac{1}{4}$ "	4 "
June,	$5\frac{1}{4}$ "	$\frac{3}{4}$ "	$13\frac{3}{8}$ "
July,	$9\frac{3}{4}$ "	2 "	$9\frac{5}{8}$ "
August,	$12\frac{1}{2}$ "	$\frac{1}{2}$ "	$10\frac{5}{16}$ "
September,	$1\frac{3}{4}$ "	$9\frac{1}{16}$ "	$3\frac{5}{16}$ "
October,	$\frac{5}{8}$ "	$11\frac{1}{16}$ "	$\frac{1}{2}$ "
November,	$10\frac{3}{4}$ "	$2\frac{3}{8}$ "	
December,	1 "	$3\frac{3}{8}$ "	
	$62\frac{5}{8}$ inches.	37 inches.	$78\frac{1}{16}$ inches.

CASES TAKEN FROM THE BOOKS OF THE NAVAL HOSPITAL.

Case 1st.—James A. Pithon, Sergeant of Marines, admitted from the Navy Yard, October 26th, with the following account of his case by Surgeon Spotswood, U. S. N.

"Pithon was taken with fever on the night of the 24th inst. I was sent for at 7 o'clock, A. M., on the 25th. Had great heat of skin, pulse quick and hard, pains in head, back, and loins, insufferably severe.—Ordered V. S. ℥ xvj. Pulse stood bleeding wonderfully well—slept for three hours after and perspired; gave at 10 o'clock quinine gr. xxv., cal. gr. xij. Fever continued all day, though skin moist. At 6 P. M., gave ol. ricini. ℥ jss., quinine gr. viij. Medicine operated very freely; saw him at 9 A. M., to-day (26th), skin moist, but pulse full and quick, tongue brown—sent to hospital."

When admitted, (26th,) complained of no pain, skin hot but moist, pulse quick—intellect good—great jactitation, tongue coated, stomach so irritable that a mouthful of water would produce vomiting. Ordered effervescing draught in small quantities, every two hours. Continued same treatment through the night.

27th. Has fever and nausea, bowels have not been open since the 25th; continue effervescing draught; quinine gr. x., enema of ol. ricin. ℥ ij, water ℥ xvj. Evening, still has fever and headache, very restless, vomiting continues—ordered lime-water ℥ ss. with milk—also spts. ether. nitros ʒ ss., solut. morph. ʒ j., aquæ. ℥ ss—the bowels have been imperfectly acted on by the enema of this morning—repeat sinapism to nucha.

28th. Has no fever to-day; no headache, or other pain; stomach still irritable—bowels torpid—ordered blister to epigastrium; effervescing mixture, enema of ol. oliv. ℥ ij., water ℥ viij. Evening, vomiting continues—lime-water ℥ ss. tr. opii. gtt. xxv. three times.

29th. No pain; skin cool, tinged with yellow, pulse rather feeble, stomach extremely irritable. Ordered pill of old opium gr. j., brandy toddy—rub legs and arms with hot brandy and capsicum. Evening—continue brandy; stomach quiet during the day.

30th. Has vomited this morning, a blueish fluid without sediment, in large quantities. Ordered spts. terebinth. gtt. xxv., mucilage ℥ ss. three times. Re-apply blister to epigastrium, apply one to calf of each leg—continue friction with hot brandy and capsicum—brandy toddy. Evening, blue mass. gr. vj. in two pills. Had to-day a white stool.

31st. Passive hemorrhage from nose, mouth, stomach, and bowels.—Stomach still irritable, though less so than yesterday. Strength much reduced—skin very yellow. Ol. terebinth. gtt. xxv., mucilage ℥ ss. three times a day—blue mass. gr. vj. At 11 A. M., tannin gr. ij, water ℥ ss. for a mouth wash; likewise, tannin gr. ij., mucilage q. s. for an enema. Tinct. ferri. muriat gtt. x. with water—ice during the day.—The above medicines were used with the view of arresting hemorrhage, which was very profuse. Evening—hemorrhage continues—had convulsions this afternoon at four o'clock, from which he soon recovered, and took about ℥ ij. of weak brandy toddy which was near. Took at night acet. plumbi, gr. j. opii. gr. ss. About half past 9 o'clock, a medical officer was called, and it was reported that the patient had again been attacked with convulsions—was at this time much prostrated, respiration laboured and hurried, and to all appearance moribund—ordered carb. ammon gr. v—sinapisms to chest, arms, and soles of feet; from this time sank rapidly and expired at 10¼ o'clock.

Case 2nd.—Jacob Scholls, Sergeant of Marines. Admited from the Navy Yard, October 20th, at 3 P. M. Was attacked on the night of the 19th with a severe chill followed by high fever; took no medicine before he entered the hospital. At the time of admission was suffering with severe chill, which was followed in half an hour by high fever—bowels have not been open to-day—stomach very irritable; severe pain in head and back—as soon as febrile action came on, ordered cal. and quinine aa gr. xv. Cups to nucha and along the spine, freely. Vomited the medicine soon after taking it. Ordered immediately calomel and quinine àà gr. x. in pills, which was ejected—ordered acet. ammon. liquor ℥ ss., spts. nit. dulc. ʒ ss., morphia sol. ʒ j. every third hour, also enema of ol. oliv. ℥ ij., aq. fervid. ℥ vj., which was followed by several copious stools of a dark colour.

21st. Rather better—has not much fever—still some head-ache and pain in back—tongue coated—stomach quiet—bowels open—ordered quiniæ sulph. gr. xv. three times in the day—barley water for drink—arrow root for diet.

Evening—no pain—nothing required.

22nd. Free from fever, tongue looks better, bowels open—has some appetite—continue quinine gr. x. three times during the day—chicken soup for dinner.

23rd. Has pain in the head and back—no fever, no appetite—bowels open, stools dark—ordered quinine gr. x. 3 times, drink tea—no nourishment was taken.

Evening. Slight fever—ordered spts. ether. nitric. ʒ ss., water ℥ ss.

24th. No improvement. Pain in the head and back continues—stomach irritable—ordered effervescing draught, sinapism to back, arrow root with wine. Evening—bowels have not been open to-day. Enema of warm water ℥ xvj.—spts. ether nitric. ʒ ss. to be taken in water ℥ ss.

25th. Much better this morning—free from fever, no pain—bowels open—stools natural, tongue cleaning—appetite returned—broiled chicken for dinner.

From this date, no further treatment. Discharged cured Oct. 21st.

All of which is respectfully submitted by

ISAAC HULSE.
Surgeon U. S. Navy.

Appendix B.

This disease, a Pernicious Intermittent and Remittent Fever, commenced at this place about the latter part of July, every individual living here having experienced an attack with the exception of two.

The disease attacked generally without premonition; the patient, apparently in perfect health, was seized with pain in the limbs, back and head, general lassitude, great weakness and prostration; numbness in the right arm and shoulder in some cases, in others the pain in the extremities was very severe, that in the back most violent. The pain in the head is at times violent, referred to the temples, sometimes to the back of the head; in cases but little pain is complained of in the head, there being an uneasy sensation at times amounting to pain in the eyeballs.

In the cold stage which is seldom more than slight rigors or horripilations, and which is generally observed only in the first paroxysm, whether the attack is of a quotidian, tertian or remittent type, vomiting is present and continues in cases during the stage of reaction, which it sometimes appears to develope, attended with burning pain at the epigastrium, sometimes very acute, distention, tenderness increased by whatever is taken into the stomach, pressure at the pit of the stomach producing pain. The countenance sallow, lurid, sometimes flushed of a duskey red hue, expressing anxiety. In many cases the countenance is natural, no pain is complained of in any part of the abdomen upon pressure, complaint being made only of pain in the back.

In children this stage was attended in three cases out of five with convulsions, effusion on the brain, one half of the body being paralysed, the other half being thrown into spasmodic contraction. In the case of a Creole 30 years of age, the disease was ushered in by violent spasms succeeded by coma and paralysis of the left side, which disappeared when the paroxysm terminated twelve hours afterwards, the man regaining his perfect consciousness, which he retained until the next paroxysm of fever. The case terminated fatally on the 5th day. The mother was subject to Epilepsy, he had never had an attack before.

In the stage of reaction which is seldom high, there is an aggravation of the symptoms first mentioned, there being present but little increased heat of skin which is moist in cases, the heat being sometimes dry, seldom pungent. In some the heat alternates with chills during the whole period. The eruption of urticaria tuberosa has been observed in this stage. The pulse is quick, oppressed, sometimes soft, never hard and full. This stage terminates in cases with gentle diaphoresis extending only to the head and breast, at other times with profuse sweats cold and clammy, the pulse becoming very frequent and feeble ; sudamina are frequently observed on the breast.

According as the different portions of the alimentary canal are more or less implicated, if the stomach, the tongue is pointed, red at the tips and edges, covered with a white fur, sometimes brown towards the base, the papillæ rising through it, becoming clear red and glazed as the disease progresses, attended with incessant nausea, vomiting, burning pain at the epigastrium, tenderness and hiccough, nothing being retained on the stomach, pain on pressure at the epigastrium and intense thirst ; if the liver and duodenum, the tongue is broad, clean and moist, or covered with a thin yellowish or white fur, mostly swollen, bearing the impression of the teeth. Intense thirst is sometimes present, at others it is null. In cases no nausea or vomiting exists, in others there is vomiting of a colorless glairy fluid ; sometimes greenish or brown flocculi are suspended in it. Diarrhœa or dysentery are often present, precede the attack or are sequelæ of it at times. The dejections from the bowels are fluid, watery and greenish ; sometimes of a dirty black colour, very fœtid, the more consistent portion falling to the bottom of the vessel in a pulverulent form ; sometimes the discharges consist of the mucus of the bowels with more or less blood, (dissolved,) there being no appearance of the biliary secretion in them. The urine is sometimes scanty, of a dark brown-red colour, at other times, citron coloured ; nearly natural in quantity ; sometimes limpid and in excess.

The blood when drawn appears changed in its characters, dissolved, coagulating imperfectly, presenting the appearance of the dregs of claret shaken up in water in some cases.

The brain does not appear to suffer much in most cases of the disease.

Delirium is slight when present, persisting in some through the hot and sweating stage. Coma, with involuntary discharge of both the urine and fæces, was present in one instance on the second day of the attack.

The intermissions and remissions were perfect in many cases according to the type and of the usual duration, whilst in others the fever was continued for the first two or three days of the attack, then remitting or intermitting as the case might be.

The disease, whatever may be its type, is marked by the great irregularity of the occurrence of the paroxysms, its imperfectly developed stages, all three however being perceptible, its tendency to fatal collapse as the paroxysm decreases, the great debility the patient suffers from, after recovery from it, however short the attack may have been; as well as the great tendency to relapse on the 15th and 21st days.

Another feature characterizing it is the insidious manner of its attack, the patient complaining only of a slight evening exacerbation of fever for a few days, when suddenly the powers of life appear to be exhausted, the blood abandons the external surface of the body, throws itself upon the internal organs; the skin becomes cold and clammy, of a lurid hue, the pulse is weak and fluttering, 150 in a minute; sometimes it cannot be felt, the intellect is clear, no pain is complained of, the patient dying in from 6 to 30 hours if reaction is not brought on by the most powerful counter-irritation to the surface combined with stimulants internally.

The *Treatment*, modified by circumstances, consisted generally of mercurial purges, followed in a few hours by oil and spts. of turpentine laxatives. If the stomach was irritable, calomel and opium was given in small portions frequently repeated; the liquor ammon. acetat. with mistur. camphoræ et spts. nitri dulc. during the fever. Cold applications to the head, sinapised pediluviæ, cups to the epigastrium, hippochondrium, back of the head and down the spine, as the case required, followed by blisters, those on the epigastrium and hippochondrium being dressed with ungt. hydrarg. fort. et ungt. resinos. Laxative and terebinthinate enemata. Dover's powder, carbonate of ammonia, camphor julep and stimulants had to be used very freely in cases from the commencement. For the relief of collapse, friction with boiling spts. of turpentine, followed by the application of sinapisms to the extremities, inside of the legs and thighs, and down the course of the spine, the impression being kept up by vesicatories to the calves of the legs, and stimulants internally with quinine.

The quinine was the specific, administered either alone in doses of ten grains, commencing at the decrease of the paroxysm, giving it in intervals of from 3 to 5 hours, or in combination with calomel in from 3 to 10 grain doses until the secretions were restored; (they being suspended generally in all attacks,) the quinine appearing to assist the action of the calomel in exciting the secretory action. In cases where the stomach from its congested state appeared to have lost the power of acting on the remedies taken, the quinine was given in gr. xx. doses in enema of starch, with great benefit. Acidulated drinks, ice, the effervescing draught with anodynes and camphor were given to relieve the nausea and vomiting present, the relief of this symptom being promoted by laxative enemata and counter-irritation to the spine and epigastrium, mild bitter infusions with quinine in from gr. ii. to gr. v. doses two or three times a day were given for some time after convalescence had commenced, to which wine was added in most cases with great benefit.

Since the commencement of the disease to this time 38 cases of it have been under treatment, (inclusive of relapses,) occurring among the men of the command, most of whom had been on duty at this Post at different times.

Of this number three terminated fatally, one from the intermittent, and two from the remittent form of the disease. Two of the cases had had diarrhœa previous to the fatal attack; one had returned about the middle of August, from the Rio Grande. This was the first service south, of all the 3 men, one of whom had not been in the country two years.

There were 73 cases of the disease treated since the latter part of July to this date, occurring in the families of the officers and camp followers, including all relapses. Of this number 6 died; one an adult female, the remainder children from 2 to 10 years of age.

RECAPITULATION.

Number of cases in the command, . .	38	
Number of deaths, do . .		3
Cases occurring in the families, . .	73	
Number of deaths, do . .		6
	111 cases—	9 deaths.

All of which is respectfully submitted,

E. H. ABADIE,
Asst. Surg. U. S. A.

Hospital, Camp Barrancas,
November 12th, 1846.

IV.—*Remarks on Remittent Fever complicated with symptoms of Tetanus.* By W. M. BOLING, M. D., of Montgomery, Ala.

During the last five or six years, I have met with a few cases of remittent fever, complicated with symptoms of a tetanic character, and as I do not remember to have ever read any thing in which the combination, or seeming combination is mentioned, suppose that a few remarks relative thereto, may not be entirely devoid of interest.*

Whatever the true nature of the disease, or combination of diseases to which I have reference, the cases were all sufficiently distinctive and peculiar, and could be recognised at once, as presenting phenomena altogether different from remittent fever attended with simple convulsions. In the time stated I have met in all with about ten cases, and they are, no doubt, exceedingly rare; for, I have conversed with but one physician of our place, who recollects to have had such under treatment. During the past fall two cases occurred in the practice of Dr. Baldwin.

* Dr. Caldwell, in his edition of Cullen, mentions a case of intermittent tetanus which occurred under his observation, but does not speak of any febrile complication.

Having never seen a case of uncomplicated tetanus in the adult, I am incapable of judging from my own observation, how far, or in what respect, the spasms in the disease or form of disease under consideration, differ from those in the former, but so far as an opinion can be formed from the descriptions in books, to me they appear very nearly identical. I have always, however, been somewhat at a loss under what name to enter the cases in my note book—and they there appear with the different mixed titles of "remittent fever with tetanus," "febrile tetanus," and others equally indicative of a want of certainty as to the true character of the disease, or as to which of its elements was predominant.

Nearly all the cases which I have observed, occurred early in the fall, soon after the first frost—and the remaining few later in the fall or winter, soon after a frost which had been preceded by a week, at least, of warm and pleasant weather.

The patients, with one exception, were all negroes.

Generally the spasms were preceded several days by a well marked remittent fever, but in two or three instances, of so mild a character, that the patients did not take to bed till these occurred. In such cases a continued feeling of chilliness was complained of, even during the febrile exacerbations. In one instance, the fever was of the tertian type.

After a few exacerbations of fever, suddenly and unexpectedly, at the period of an approached paroxysm, spasms resembling those of tetanus supervene, and present themselves so prominently, that without some enquiries in relation to the early characters of the case, these alone would be noticed, or claim at a late period almost exclusive attention; the febrile phenomena being overlooked, or viewed rather as accidental concomitants. During the further progress of the case, however, the febrile symptoms are as regularly remittent as if no complication whatever existed, and of quite an aggravated character.

The muscles of the abdomen, of the neck and jaws, and of the superior extremities, after the commencement of the spasms, remain generally in a firm and rigidly contracted state. Those of the back were in two instances affected, as the spine was bent and the head drawn permanently back as in opisthotonos. Occasionally the rigidity extends to the inferior extremities. Besides this permanent rigidity, there are occasional aggravations of the spasmodic action, in which motions of a more decidedly convulsive character take place. In these the arms and fingers are firmly flexed and rigidly drawn against the chest in front, and often the radial edges of the forearms are turned so much under and outward, as to bring the hands into a state of supination. The head is generally turned to one side, causing the face to look over one or the other shoulder, and both eyes and the corner of the mouth are often drawn in the same direction. During these paroxysms, the pupils,—at other times but moderately dilated,—become excessively so. The muscular rigidity, so far as I have been able to ascertain, does not generally come on gradually, but is mostly ushered in by one of the above paroxysms.

The jaws are not firmly closed, but remain rigidly fixed from a quarter of an inch to a half an inch apart; the patient being apparently almost as incapable of bringing them closer together, as of separating them further. Sometimes in the intervals between the paroxysms, by considerable force exerted upon the chin, the mouth may be somewhat more widely extended. In one case, during the paroxysms, the mouth was thrown widely open, with great apparent suffering, and a tremulous motion of the lower jaw, as if from a contest, as it were, between the temporal and masseter muscles which elevate it, and the steno-hyoidei, sterno-thyroidei, genii-hyoidei, and other muscles, whose conjoined action serves to depress it. In some instances the rigidity and convulsive action are more manifest on one side of the body than on the other.

The paroxysms of spasm differ in frequency and severity, not only as regards the period of the disease itself, but also as regards the period or stage of the febrile paroxysm. During the remissions of fever an abatement also takes place in the violence and frequency of the convulsive paroxysms. In the exacerbations they are more frequent and severe. In the advanced remissions, or in the more aggravated cases, the violence and frequency of the paroxysms diminish only; while in the milder cases, or in the earlier remissions of more severe cases even, these may cease entirely for the time; muscular rigidity alone remaining. With each succeeding exacerbation,—as with the symptoms of uncomplicated remittent fever,—in the onward progress of a case,—the frequency and violence of the paroxysms increase; a certain relative proportion seeming to exist between the tetanic and febrile phenomena; *pari passu* advancing and diminishing. In one or two of my cases, however, this relation was not exactly maintained, but little abatement taking place in the paroxysms of spasm, during very decided remissions in the fever.

From the low condition of the patients in most instances when first visited, I have not been able to learn whether or not the precursory febrile symptoms were attended with the most common precursor of open and declared tetanus,—rigidity of the muscles of the neck and jaw,—but in one instance, which was distinctly intermittent, both in regard to the febrile and the tetanic phenomena, this feeling preceded for a short time, each paroxysm.

In one instance I observed frothing at the mouth during the paroxysms.

In two cases, no manifestations of intellect or consciousness were exhibited from the first appearance of severe symptoms,—complete coma or stupor being also present,—and in some others this was the case only during the febrile exacerbations. In a majority of the cases, however, the patients seemed to comprehend what was said to them, would direct their eyes with seeming intelligence when spoken to, and make efforts, in most instances ineffectual, however, to protrude the tongue when requested to do so.

While conciousness continues, the expression of the countenance gives indications of alarm, anxiety, and great physical suffering, and that the latter exists in a high degree, is still further impressed upon the observer by a moaning sound which the patient makes during the paroxysms.

Articulation is somewhat imperfect even in the milder cases or in the earlier exacerbations; and in some instances utterly impossible, even when the patients are conscious of what is said to them, and while the countenance and actions give indications of considerable remaining intelligence.

There is in all cases more or less difficulty of deglutition, and in some the act seems impossible. One patient, evidently retaining his mental faculties in perfection, and most anxious to do any thing which he was told would give him relief, retained in his mouth a most disagreeable dose of medicine, for half an hour at least, making repeated efforts to swallow during the time, but without success. In some few instances, the effort seems to be followed immediately with increased muscular rigidity and perhaps convulsive movements. Substances possesing a certain degree of consistency, as for instance, medicines suspended in syrup, are generally more readily swallowed, than those of more tenacity.

The pulse, undergoing the usual changes in regard to frequency during the exacerbations and remissions, is generally quick and frequent, varying from 120 to 160 or 170, though soft; rarely it is small and corded. The action of the heart is at the same time loud and strong. In one case, a mere coincidence in all probability, a loud bellows murmur was heard, synchronous with the first sound.

The respiration is difficult, the inspirations seemingly impeded, and cut short, as it were, by muscular spasms, preventing the expansion of the chest, and the free admission of air. During the paroxysms it is laborious.

The appearance of the tongue is in most instances peculiar, being swollen, broad and relaxed, protruded frequently more or less between the teeth, and covered with a dirty, yellowish, opaque, viscid mucus. Towards the termination of fatal cases, it occasionally becomes pointed, and covered with dried laminæ of the peculiar mucus just mentioned.

The bowels are generally operated on with difficulty after the commencement of the spasms, though these supervened in two cases, during the rather free action of purgative medicine.

The temperature of the surface generally is considerably above the natural standard, and the head in some instances is quite hot—the temporals beating with force, and presenting prominently under the integuments—though the extremities, if not attended to, soon become cool.

The disease in no instance which I have observed extended beyond the fifth day after the spasms supervened, and in most cases a fatal termination took place before that time.

From genuine and uncomplicated tetanus, judging from the discription of that disease in books, the spasmodic action in the character of disease under consideration differs principally in this; that in the latter it extends more frequently, and at an earlier period to the hands and fingers, and seems, as far as the superior extremities are concerned, to predominate decidedly in the flexor muscles. As regards the muscles moving the lower jaw, it is more violent in those which serve to depress it, than in those intended for its elevation; for the action of the powerful pterygoid, temporal and masseter muscles, is so counteracted by the sterno-hyoid, sterno-thyroid, mylo-hyoid and genio-hyoid muscles, that instead of being closed, the jaws are held a short distance most rigidly separate. Where recovery occurs, too, the cessation of spasmodic action is not so gradual, but takes place in a short time after the febrile symptoms are removed.

In the New Orleans Medical and Surgical Journal for Nov. 1846, in an article on Tetanus, by Dr. Hester, we find malaria noted among the probable predisposing causes of this disease: and the cases upon which the present remarks are based, seem, though a different interpretation may be admitted, confirmatory of this opinion. We have, for example, patients in a number of instances powerfully under the influence of malaria, as evinced by the presence of phenomena which are almost peculiar to its action, seized also with symptoms of a tetanic character, the two diseases present at once, each apparently exerting a modifying influence upon the phenomena of the other. Under these circumstances, are we to suppose that the two seemingly different diseases with which the patient is attacked, are owing to the same predisposing cause, malaria; or have they each a different predisposing cause; the fever proving the exciting cause of the tetanic symptoms, through the disorder of the system which its presence occasions? Under either supposition, as regards the predesposing cause of the tetanic symptoms, without the development of the fever, the predisposition might pass perhaps away, with an altered or improved condition of the system, or rest in abeyance for the application of some other exciting cause. As bearing an interesting analogy, I will mention *en passant*, that in a patient who was subject to epilepsy, whom I once treated for remittent fever, each exacerbation of the latter disease was ushered in by a paroxysm of the former. Judging from my own experience alone—never having met with an uncomplicated case of tetanus, and with a number of the mongrel character of which I have been speaking, it would appear that the tetanic irritation is much more readily produced, or developed under a much slighter predisposition, during or by the presence of fever than when this does not exist; and this idea is sustained by the less violent character of the spasms, during the remissions of fever, and, as was the fact in one patient who recovered, their entire cessation, almost immediately on the removal of the fever.

It is possible that the tetanic symptoms are in some manner connected with sudden engorgement or inflammation of the meninges of the medulla spinalis, supervening during the progress of the fever, and extending in the cases in which loss of consciousness is also present to those of the brain; but having made no post mortem examinations, I do not know that there are any morbid appearances to give evidence of the existence of one of the above named conditions, either as the cause, or as a consequence of the spasmodic action. Even admitting, however, the existence of either of these pathological states, it is presumable that this alone would not be sufficient—that some peculiar irritation must also be present; a primary link perhaps in the mordid catenation, as in uncomplicated tetanus; seeing that the cases are peculiar or nearly so, to a certain season of the year, and in some manner connected with particular changes in the weather. Were it otherwise, we might expect the same symptoms to supervene proportionately as often, during the progress of fever, at any other season of the year. Nor would the supposition that either of these conditions was the main and primary cause of the spasms, be at all favored by the effects of remedies. In only one instance of several in which bloodletting was resorted to, did the opperation fail to be followed by an aggravation of all the tetanic symptoms; and in two instances, these, as already observed, first supervened during the active operation of cathartic medicine, which had been administered in the treatment of the previously existing fever.

In the cases which have fallen under my observation, in only three instances were there external points of irritation discoverable, from which morbid action may have been conveyed, or excited in the nervous centres. In one of these patients, a negro girl 16 years old, there existed a wound in one leg, produced by the entrance of a large and ragged splinter of wood, and there were also several cuts on her body, caused by a flogging from the overseer. They were all of about two weeks standing, and had an irritable, inflamed aspect, and without any appearance of purulent secretion. In another case the patient, a little girl eleven years old,—the end of one of the index fingers was swollen, red and painful,—but without the formation of pus,—from a small splinter which a week before had penetrated beneath the nail. In the third case,—the patient a strong negro man,—there existed a small indurated tumour, immediately under or rather in the integuments;—painful, especially when pressed upon,—in the site of a cicatrix produced by venesection. The operation had been performed about ten days prior to the occurrence of the spasms. The orifice had at first inflamed, and there took place from it a discharge of pus, but afterwards, in closing up, the tumour spoken of was formed.

The result in most instances in my practice has been unfavorable; the remedies applied,—mercurials, venesection, opium, blisters, quinine &c.,—in various proportions and combinations, in different cases,—exerting generally no favorable influence, and some of them at times indeed seeming even to have an injurious effect. The latter article, until very recently, in consequence of the mixed nature of the cases, and some supposed contraindicating symptoms, I used less freely, and with less confidence than perhaps I should have done, viewing at the same time the disease, as I did, as a form of pernicious remittent fever. In the fall of 1845, I met with a case, very much resembling those which I have described, in which both the spasmodic and the febrile phenomena were distinctly intermittent,—the only one of the kind which has fallen under my observation,—and in which the disease was arrested, after the third accession, by the free use of Quinine.* Prior to this my cases all proved

* In connection with the mention of this medicine, I cannot refrain from alluding to an article in the late number of the Amer. Jour. of Med. Sciences on its poisonous action by Dr. Wm. O. Baldwin. It is to be hoped that this excellent paper will have the effect of restraining within more moderate bounds, the administration of quinine. The experiments of Dr. Baldwin were conducted with all possible care; and his opinions in regard to the effects of the article under consideration are entitled to no little weight; for there are few physicians in this part of the country, who use it more extensively in practice, or in more decided doses. After witnessing the unpleasant results, produced by it now and then, in some constitutions and under particular circumstances, in the comparatively moderate doses in which it is generally exhibited in this section; one can scarcely, without a feeling of horror, learn of its administration elsewhere, in the quantities here given, multiplied ten or twenty-fold.

From the time of the introduction first, of the Peruvian bark into practice, to within a recent period, the most exaggerated ideas were entertained, by, it may be said, a large mass of the profession, of its deleterious effects upon the human system, (as also of its chief medicinal ingredient, quinine, since its discovery,) and many who ventured to use it, did so in fear and trepidation at first, on the constant "*qui vive*," for some terrible and mysterious explosion of its poison-fatal. The only one that I have since treated, terminated favorably under the use of the same remedy. I must observe, however, that neither in the intermittent case, nor in the one last spoken of, were the symptoms in all respects of so grave a character at the commencement of treatment, as in most of the fatal cases.

I append brief notes of the last case.

Sally Wood, a negro girl 17 years old, had a chill October 23d, 1846, about noon, followed by considerable fever; a remission occurred on the morning of the 24th, but about noon again an exacerbation of fever came on, without a chill, in which there was a tendency to stupor, and some difficulty in swallowing. There was again a remission on the morning of the 25th, during which she took a little quinine and a dose of purgative medicine. I visited her first on the evening of the 25th, and found her with the following symptoms, the exacerbation having come on about 10 o'clock A. M.: nearly complete stupor, abdominal muscles

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ous action. But, with these fears of our forefathers staring us in the face even, the opposite extreme is run into at present; and by a few at least it would seem the opinion is entertained, that there is that anomaly in the materia medica, an article, potent for good, but incapable of mischief under any circumstances or in any quantities; an article possessed of powerful medicinal properties, and not poisonous.

But while condemning the administration of such excessive quantities of quinine, I would be far from inculcating the doctrine, that in all cases, as soon as any of the symptoms enumerated as those of its poisonous action supervene, the article should be discontinued; as in so doing, the usefulness of the remedy, and the advantages derivable from it, would be circumscribed within very narrow limits.

It may be said, indeed, that manifestations of what have been considered the poisonous effects of quinine, supervene almost invariably, simultaneously with its curative action. It is seldom that we observe any beneficial results to follow its administration, even in the mildest and simplest cases, without some degree of deafness and *tinnitus aurium.* Carried to the extent only of producing but the very slightest dullness of hearing, where the disease for which it is administered is of a mild character, may suffice; but where the morbid action to be overcome is more violent or obstinate, larger quantities of the remedy are necessary; and with the administration of these, and in proportion to the extent to which they are carried generally, in many cases, as its curative effects are manifested, its poisonous symptoms are also increased in number, and augmented in degree. The tinnitus and deafness become extreme, the respiration slow and perhaps irregular, and tremulous motions of the hands, dilatation of the pupils, and in some instances dimness of vision, in succession supervene. Now, under such circumstances, these unpleasant attendants of the curative action of quinine, remaining within the limits of safety, in cases of high morbid action, where the disease itself seems to be yielding, more especially in cases in which smaller doses had failed, or ceased to have a beneficial influence, I would not advise the discontinuance of the remedy, but on the contrary a perseverance with it, in the smallest quantities, of course, capable of affecting the desired end, and this notwithstanding the persistence of the symptoms above enumerated under its action.

It sometimes occurs that the remedy in a given dose for a time will seem to be controlling the disease,—the pulse becoming slower, and (as in pneumonia for instance,) the dullness on percussion, &c., less extensive; the only one of its unpleasant attendants present being slight deafness perhaps. After a while, however, its controlling influence, in the dose given, ceases, and the disease remains stationary, or again advances. Under these circumstances I have frerigidly contracted, as also those of the superior extremities, by which the arms are flexed and held firmly against the chest. Mouth stands open about half an inch, but the lower jaw held so that it can neither be elevated nor depressed. Pupils dilated. Tongue protruded slightly between the teeth, broaerelaxed, and covered with a white slimy mucus. Pulse 140, soft and moderately full. Skin hot—seems to swallow with much difficulty. As often as about every half hour, there are paroxysms, —each lasting about two or three minutes, perhaps,—in which the spasmodic action is augmented. The fore-arms are then drawn more rigidly against the chest, and the hands brought nearly to a supine position, the radial edge being turned under and outward, as if the motion were produced by an exaggerated action of the pronators. The head is drawn backwards—the pupils excessively dilated—the eyes rolled up—respiration quickened and noisy—profuse perspiration about the head; and she makes a moaning sound, as if from severe bodily pain.

Directed four grains of quinine to be given every 2nd hour—and a blister to be applied to the back of her neck and between her shoulders.

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quently observed an increase in the quantity of the medicine, almost immediately to bring the patient into an improving condition, so far as concerned the morbid state for which it was administered; as again indicated by moisture of the skin and tongue, less hardness and frequency of the pulse, and (in pneumonia) diminished dullness, &c.;—but with an augmentation, at the same time, as already remarked, in number and degree, of the unpleasant symptoms referred to; yet I have not deemed it proper, on this account solely, to withdraw the remedy. Of course, this persistence in the use of an article, in quantities capable of producing such unpleasant results, could only properly be advised, in cases in which, from his past experience, or otherwise, the physician was led to believe, that the use of other medicines instead, would prove entirely unavailing. It would be desirable, most certainly, to subdue the morbid action, without pushing the remedy sufficiently far to produce any of these, still less to cause their presence in a high degree; but, when under the circumstances enumerated they do supervene,—if not in any excessive degree,—I repeat, I do not think they should be considered as imperatively demanding its suspension.

Were the physician actuated by a selfish regard for his own interest, rather than a desire to preserve the life of his patient, the *policy* of such a proceeding would be very questionable; for, the fact is undeniable, that in a vast majority of instances, the occurrence of any unpleasant symptom during the treatment of a case, as an evident or acknowledged effect of any remedy used; though for the preservation of the life of the patient, it was necessary to administer the remedy in quantities sufficient to produce such effect; will injure the reputation of the practitioner more even than the death of the patient. Should then any of the distressing symptoms enumerated, as resulting from the action of quinine, come on in a marked degree during its administration; and more especially, should they not immediately subside on the recovery of the patient; the physician must expect to suffer in reputation, and to meet with wrathful upbraidings, proportionate to his own disinterested doing; even though as observed, the full quantity of the medicine productive of the effect complained of, was necessary for the preservation of the patients life.

How seldom, when the physician is moderately attentive, are hard thoughts entertained, or censorious remarks indulged in, by friends or relatives against him, in consequence of the death of a patient, but how invariably the reverse, where any unpleasant symptoms result from the action of his remedies; the most trifling inconvenience, of a temporary character even, being remembered, and severely commented on. In the former case, "the patients time had come," "our lives are not at our own disposal"—or "the hand of fate was apparent in

26th. The rigidity of the muscles of the superior extremities, abdomen and jaws continues, but the more severe fits of spasmodic action have ceased almost entirely. Articulation imperfect, but from the expression of her countenance she seems to comprehend all that is said to her. Pupils natural, or nearly so. Pulse 100. Moans occasionally, as if from bodily suffering.

To take at 10 o'clock, eight grains of quinine and the third of a grain of morphine—and at noon four grains more of quinine.

Evening. She is perspiring freely; is somewhat more restless than she was in the morning, but in other respects remains much the same.

℞ quin. sulph. grs. xxxvj. div. in partes sex æqual—one to be taken every 4th hour. Early in the morning, the third of a grain of morphine.

27th. Talks distinctly and without difficulty. The muscular rigidity is subsiding. She is quite thirsty, but otherwise there is but little evidence of the presence of fever.

This patient took no more medicine and soon recovered.

'*his*' *taking off*." In the latter the hand of the physician is manifest. His duty, however, is still the same.

I am induced to continue this note.

In the leading article, in the Southern Medical and Surgical Journal for January, 1847, Professor L. A. Dugas, of the Medical College of Georgia, recommends the sulphate of quinine in intermittent and remittent fevers, even with local determinations; *and ventures also, so far*, as to advise its administration in cases of pneumonia. For this the Professor observes, "I cannot but apprehend, that the charge of ultraism will be preferred against me, by those who are still unacquainted with its properties," but "will be amply compensated for the temporary *odium*, if this article will induce any who have been backward in the use of quinine, to give it a fair trial, under the circumstances here recommended." The self-sacrificing devotedness of the Professor certainly cannot be too much lauded and admired; but at the present day, the charge of ultraism it is probable will be urged by few Southern practitioners against the views which he has advanced; nor is it likely that the odium thereby incurred will be entirely unendurable. Of the physicians of Alabama, Mississippi, and Louisiana, at least of the Southern States, few can be found, I imagine, under forty years of age, who would not be willing to endorse, in almost every particular, the views advocated by Professor Dugas, on quinine; and I had nearly said, that within the circle named they are almost universally received. However, views of a somewhat similar character, published in the American Journal of Medical Science, for July, 1844, by the writer of the present article, have been denounced from the rostrum by a Southern teacher of medicine—and the success attendant on the practice, in inflammatory diseases, explained by the assumption, of its having been used only in *fevers* of a low character, in constitutions broken down by long residence in a malarious locality. In this, however, the Professor is mistaken; for here, quinine is by no means reserved exclusively for patients with broken down constitutions, but is used most successfully also in genuine inflammatory affectionss, occurring in subjects, young, robust, muscular, plethoric, and in whole and vigorous health. In *genuine* inflammatory affections too I repeat. (else none such ever occur among us,) or at least, in cases in which all the symptoms, nay, all the physical phenomena of inflammation are present; and present not during febrile exacerbations only, but during the remissions; after periodicity is entirely removed or where this has not even existed; and gradually, day by day may be noted, subsiding under its influence,—or increasing on its suspension, where this is permitted at too early a period.

Part Second.

REVIEWS AND NOTICES OF NEW WORKS.

I.—1. *Physiologie Pathologique ou Recherches Cliniques, experimentales et microscopiques, sur l'inflammation, la tuberculisation, les tumeurs, la formation du cal, etc.* Par H. Lebert, Docteur en medecine et chirurgie, etc., etc., etc. p.p. 1050. Accompagné d'un atlas de vingt-deux planches gravies. A. Paris, 1845.

2.—*The Pathological Anatomy of the Human Body.* By Julius Vogel, M. D. Professor of Clinical Medicine at the University of Giessen. Translated from the German, by Geo. E. Day, M. D., and L. M. Cantab, Member of many learned Medical Societies. The whole illustrated by one hundred engravings. Philadelphia: Lea & Blanchard, 1847, pp. 534.

The two works at the head of this article, the first French, and the second German, may be regarded as fair exponents of the science of human pathology, existing at the present time, not only in the respective countries from which they emanated, but throughout the medical world.

If the science of therapeutics could but keep pace with the progress of pathology, our cemeteries would be less crowded with the remains of the youth and manhood of the land.

Indeed it is to be regretted that so much of the talent of our profession has been directed to pathological researches, to the almost entire neglect of its sister branch of the profession—therapeutics, the truly useful science. The rich and abundant materials with which our works on physiological pathology are illustrated, is indeed a sad commentary upon the impotency of medicinal agents and our ignorance of the true action of remedies. In spite of our great works on morbid anatomy and pathology, disease still does its work, as if "*macies et nova febrium terris incubuit cohors,*" and this has failed to prolong life, or to realise the just expectations of those who look upon our science as the noblest of human pursuits.

The ultimate object of every science should be to understand and expound the laws by which nature carries on her operations, whether they relate to the modifications effected in the system by a physiological or a pathological change. Disease is under the control of laws no less fixed and positive than those which preside over the growth and development of the various organs and tissues of the economy. To understand these laws is but to learn to interpret those pathological changes which constitute a departure from the normal—the healthy state. In slight affections, creating unimportant disturbances in the system, it becomes a matter of no trifling difficulty to determine the precise point, at which the normal gives place to, or is superceded by, the abnormal state. In other words, it is almost impossible to say when or where healthy action ceases, and the disease commences. It is fair to suppose that this varies with the temperament and other constitutional peculiarities of individuals. Thus much on the nature of the subjects discussed in the pages before us.

In order to pursue the plan adopted by M. Lebert in his "Physiologie Pathologique," we shall like him begin with that fruitful theme among writers,—inflammation. To examine the subject with that care which, as a pathological change, it demands, we must first advert to the anatomy and the peculiar arrangement of that system of vessels, usually called capillaries,—the seat of almost all inflammatory action.

The capillary system of vessels serves as channels through which the nutritive elements are conveyed to the organs and tissues of the body; and from their extreme tenuity, they are constantly liable to obstruction and other morbid changes. They seem to be placed between the arteries and the veins, connecting the two together. If colouring matter be injected into both veins and arteries at the same time, it will be found arrested in the capillaries, thus showing that they are connected on the one si dewith the venous, and on the other side with the arterial, system of vessels. They are supposed to exist in every part of the system, though more abundant in some structures than in others.

We have alluded to the universal existence of capillaries throughout the human body; and now we shall enter into details; and first of the caliber of the capillary vessels. The diameter of these vessels, in a physiological state, is sufficiently large to allow the passage of blood-corpuscles, one after another. In man the diameter of these vessels is not much less than 0.003 of a line; this is Webber's estimate; whereas Henle places them at less than 0.004, as examined on the Schneiderian membrane. They seem to differ in different membranes. Thus Valentin sets down the smallest capillaries of the gastric mucous membrane at 0.0057, and in the small intestines at 0.0048. Valentin has constructed a table, taking the finest capillary vessels of the cerebral structures as a unit. He gives the following as the result: Lungs, 0.97; median nerve, 2.3; biceps muscle, 3.3; dermoid structure, 3.6; intestinal villosities, 4.4; small intestine, 4.9; stomach, 5.4; kidnies, 5.5; bodies of malpighi, 7.09; M. Lebert gives a similar table, with different results, but his examinations were confined to the capillaries of other mucous and serous tissues. So much in regard to the speculations on the size of the capillary system of vessels.

We shall now say something of the changes produced in these vessels by diseased action, and chiefly of an inflammatory character.—When the capillary vessels become overcharged with blood, the circulation is arrested, and in many cases a rupture is the consequence.—Hence, in pneumonia, dysentery, and the like, the excretions are tinged with blood, the result of this rupture and the consequent escape of the blood-corpuscles.

It was for a long time supposed that the colouring matter of the blood having exuded through the coats of the vessels, imparted this peculiar reddish tinge to the secretions and excretions; but microscopic analysis has proven beyond a doubt that this colour is produced by the presence of the globules of the blood,—the result of a rupture. Globules of blood are never found in the pure secretions; they cannot be elaborated by any secretary process.

This fact may be of considerable importance in the diagnosis of disease. We are now approaching the confines of that field in pathological science, over which the microscope is yet destined to shed a flood of light. The globules of blood undergo, in ordinary frank inflammation, but little material change; sometimes they are separate or grouped together, with some trifling modification as to their form. In animals whose systems may be poisoned by purulent infection, the globules of blood experience a series of changes, and in some instances, according to Lebert's experiments, are completely destroyed. The different changes through which they pass, prior to their complete annihilation, may be observed with the microscope. The colouring matter grows fainter, the globules become irregular in form, and more diffuse, and finally they are reduced to the state of the small segments of their primitive globular forms, not inaptly compared to the moon when waning from her full circle down to smaller segments, and ultimately disappearing. Such would seem to be the state of the globules in certain forms of low fevers, such as the petechial, typhus, and yellow fevers, attended with hemorrhages and other exhausting discharges.

It is the received doctrine of many of the pathologists of the present day, that in the reparation of solutions of continuity, a new class of capillary vessels, independent of the general circulation, is created. To this supposition M. Lebert is opposed, and in proof of this connection of the newly formed capillaries with the general circulation, lays down the following proposition. "This new formation always takes place in a centrifugal manner, proceeding from the vessels of the general circulation; it takes place either by the dilatation of vessels existing in a normal state, but too fine to allow the passage of the blood globules, or by new capillary vascular arches which are created by the augmented impulsion of the blood through existing vessels."

In examining false membranes, we find some evidence of an independent set of vessels, but the microscope has dispelled this illusion, and instead of well formed vessels carrying blood globules, we discover neither walls nor globules, but a group of reddish spots destitute of any organization. Both by dissection and artificial injection, M. Lebert has been enabled to trace the connection between the old and new set of vessels. He therefore thinks he has refuted the doctrine of the independent existence of a capillary system of vessels.

We now come to the pathological changes which take place in the capillary system of vessels and their contents during inflammation. When the blood accumulates in these vessels and becomes stationary for a considerable length of time, a transudation—a kind of leakage takes place of certain parts of the circulating fluid. The blood-globules never exude through the coats of the minute vessels; this effect is always the result of a rupture. One of the most important and interesting products of this exudation is a slightly turbid and reddish fluid, in which we find a particular species of *granular globules.* These granular globules, says M. Lebert, are ordinarily round and spherical, sometimes slightly oval and in a few cases pointed at both extremeties with a tendency to the formation of cellular fascicles. The enveloping membrane of these globules is delicate, transparent, tolerably solid, and neither soluble in water nor acetic acid. The number of granular globules contained in a drop of this exsudation varies from 10 to 30, sometimes more.

Mr. Lebert gives an interesting table of the different measurements of these granular globules, as the product of inflammation in the various tissues of the body; we have not room to copy it. They are found to vary with the character of the inflamed tissue.

These granular globules constitute the element for red hepatization of the lungs in pneumonia; they are found, says Mr. Lebert, in the inflamed brain and spinal marrow—in an apoplectic clot—in accidental serous membranes—in sanguineous effusions, and in phyogenic membranes which isolate certain abscesses in the splancnic cavities. They are found abundantly in different forms of pneumonia, both acute and chronic; and in Bright's disease of the kidney they constitute the chief element. Mr. Gluge, who was the first to detect and describe these peculiar globules, believes that they are formed by the nucleus of the blood-globules changed by capillary stases.

Mr. Lebert does not coincide with Gluge, but says they are formed simply by the grouping together and the agglomeration of a certain number of granules, which he supposes to be rather of a fatty nature than fibro-albuminous, and which are enveloped in a membrane.

The products of inflammatory exudation have been examined with the microscope, and the following is the result of Mr. Lebert's observations.

"In the early stages of inflammatory exudation producing false membranes, we can only see them under the form of small, delicate, greyish, semi-transparent flocculi, disseminated along the course of the vessels. The microscope reveals in it an irregular web of fibre, forming a net-work and a finely hyaline granula substance. This substance often predominates, and then we have in it rather a fibroid stratification than true fibres with well marked outlines. This fact will convince us that it is not pure fibrine as obtained by whipping blood, but it is rather a mixture with other elements, among which we discover in the small flocculi already mentioned, pyoid or purulent globules.

Exudations analogous to that already described is formed in the healing of wounds by the first intention.

A fibro-albuminous matter is effused between the lips of the wound, and by its viscosity and coagulability, it glues together for a time the separated parts, and the union is afterwards perfected by the new anastomosing vessels which shoot through the new lymph. Thus in wounds a fibrinous effusion takes place through the entire length of the wound; vessels are formed, the fibrin contracts, and cicatrization is completed by means of a thin ligamentous band. The same process is effected in the ligature of arteries, by which the clot is rendered sufficiently solid to block up the mouth of the vessel,"

Our author makes some interesting remarks upon the *cartilaginous* *transformation.* He says that the microscope teaches us that such transformations are deceptive—that the true nature of cartilage is entirely wanting, and that we are led into error by its appearance. Sometimes again the effused matter is transformed into a gypseous, even into an amorphous mineral substance, arranged in layers, resembling very much accidental osseous production. This is usually described as an osseous transformation of false membranes.

M. Lebert denies that these so called mineral deposits possess any of the characters of an osseous deposit. He describes another form of inflammatory effusion, as a gelatinous matter, of a yellowish, semi-transparent color, containing irregular nuclei of a cartilaginous or osseous appearance. He next proceeds to prove that this gelatiniform substance is the same in all the different accidental transformations, being only a complication and not a particular species either of cancer or any other abnormal product. Chemical analysis has shown that it is not pure gelatine, but probably altered fibrin. A transformation different from this, coming from inflammatory exudation not purulent is that of ossific plaques, the chief element of which is found to be cholesteric crystals. M. Lebert cites an interesting case of pericarditis in which he found the above crystals deposited in the substance itself of the heart.

The next product of inflammation examined by Lebert is pus,—its description—the theory of its formation and its importance in pathology.

He states that pus is composed of serum and globules ; the first may be obtained by allowing pus to stand for some time,—the globules sink to the bottom and the serum floats on the top. When this serum is injected into the veins of a rabbit it produces death no less certainly than pure pus, except that death is not so speedily produced in the first as in the second case ; the effects are the same however in both instances ; (viz :) bloody effusions—both in the lungs, and other organs of the body. The proportions which these two products bear to each other is variable ; the globules are about a fifth to three fourths of the whole mass. But the most interesting element in pus, is that which has been long known under the name of the *globule.* The pus-globule averages 0.01 in diameter. Monsieur Lebert then enters into a long and a minute description of these globules, being perhaps more elaborate and accurate than any preceeding author ; but we cannot follow him.

Pus-globules are much larger than blood-globules ; the latter have no nuclei,—are of a reddish yellow, and more transparent in the centre than on the circumference. Blood-globules, besides the above distinctions have a more flattened and discoid form than pus-globules. The white globules of the blood must not be confounded with either of the two globules already mentioned. The microscope will correct the error, and prevent any confusion on this subject. Fibro-plastic globules may be confounded with pus-globules ; the covering membrane of the former is much paler and more dense than that of the latter kind of globule.

The globules of tubercle are much smaller than those of pus ; their borders are irregular and angular ; they contain molecular granules in their substance, but no nuclei. Our author describes another kind of globule which he calls pyoid, a variety of pus-globule, of a spherical form and made up of two elements ; of these two elements he gives us a description ; but we do not deem them of sufficient importance to arrest attention.

Fatty elements may be often detected in pus, and as a general rule the more fœtid and corrupt the pus, the more abundant the fat; this fat presents itself in the shape of molecular granules, containing both elain and stearin. In addition to the above elements entering into pus, M. Lebert has found in it crystals of cholesterin and other kinds of crystals "which may be reduced to the form of an elongated prism with from three to six sides truncated or pointed at their extremity."

In this connection let us not overlook our author's observations upon a particular element, in the shape of small animals, (insects, &c.,) called infusoria, always found in pus. They belong to the species, *vibrio rugular,—lineola et bacillus.* They are very minute, and always in motion either progressive or giratory. Mr. Lebert, however, expresses some doubts as to their animal nature. He has detected these vibrions in the pus from an abscess—in that from cancerous ulceration—in bronchial sputa—in the stools from dysentery—in purulent urine—in an hepatic abscess produced by hydatids in a living subject.

The following microscopic observations, in the language of this elegant and learned pathologist will show that such investigations may add immensly to our means of diagnosis. "When we open a glandular abscess, we sometimes discover small, soft, floculent masses, rather of a caseous nature. Well! if the abscess has its seat in the substance of a tubercular gland, we find tubercular corpuscles not to be mistaken. If, on the contrary, these particles of matter contain only fibroid coagulations or cellular fibres, we may be certain that it is a case of simple phlegmonous inflammation.

In abscesses of the mammary gland, the microscope alone can enable us to say whether there is a mixture of the elements of pus and those of milk in it, the globules of which, in similar cases, may be easily recognised. In very rare cases, we may see this mixture with the naked eye. "We have often found pus in the urine," continues Mr. Lebert, "and according to the appearance we are enabled to say whether the pus is furnished by the kidnies or the bladder; if from the former organ the epithelium is much smaller than that from the latter." The urine sometimes presents a purulent appearance, caused by a saline sediment of the urate of ammonia, yet the microscope can detect no pus-globules.

This fact is important in a diagnostic point of view, and once enabled Mr. Lebert to speak positively as to the nature of a case.

How are we to distinguish what are called cold abscesses, which sometimes point in the groin, from those called lumbar, in the same region, the result of caries of the vertebra? With the naked eye we can rarely decide, but arm this organ with a microscope and the particles of bone thus seen will determine its true nature and seat.

In dysentery, pus-globules are mixed with blood-globules and granular globules; and when we find in the evacuations the elements of bile, we may announce an approaching convalescence. This discovery can be made much earlier with the microscope than with the naked eye; hence the utility of these researches in morbid secretions.

It is to be regretted that we have no means of distinguishing gonorrhœal matter from either syphilitic or ordinary pus ; this M. Lebert thinks desirable, but he has not been able as yet to point out any difference ; perhaps they are identical as to origin and nature, yet different in the mode of their production.

M. Lebert next examines the chemical habitudes of pus. He says it is usually neutral, but readily becoming acid when exposed to the action of air, owing to the development, says Nasse, of lactic and acetic acids. Pus is very rich in protein combinations and fatty substances ; it has a density between 1.027 and 1.0409. Pus contains from 85 to 90 parts of water in 100, the fatty elements vary from 9 to 24 parts in the 100—besides it contains pyin and cholesterin in variable quantities.

Bibra, who has analysed eighteen different kinds of pus, found that the protein combinations of pus greatly surpassed the fatty substances ; the first varied from 7.10 in the 100, to 18.60 ; the latter, or the fatty, between 0.7 in the 100 to 4.63.

The average of the elements of protein is 10.74 ; and that of the fats, is 10.84 in the 100 parts. The salts of pus differ from those of the blood. Pus contains more water than normal blood ; the quantity of fibrin and albumen is likewise greater. During inflammation the cholesterine of the blood is increased, but the ashes are sometimes more and sometimes less. Pus contains more fibro-albuminous parts than the serum of the blood, a proof this that the globules,—the solid part of the blood, is usually modified in the formation of pus, thus furnishing a certain proportion of its constituent element. According to Lebert, pus is formed by the exudation of the liquid part of the blood changed by capillary stasis, mixed probably with some of the elements of the more solid parts of the blood, seeing that the proportion of fibro-albuminous substances is greater in pus than in the serum of the blood alone. All these elements, says our author, escape from the circulation in a perfect state of solution in the form of a liquid, which constitutes a true pyoblastema, in which globules of pus are formed of every kind by a peculiar transformation of the protein bodies and especially of the different shades of fibrine.

The next question examined by Lebert is whether pus can be formed without inflammation ! He contends that a hyperemia and a capillary stasis always precedes the formation of pus ; it may be slow or rapid. If we recur to the chemical characters of pus, we found it less richly endowed with those elements which in the blood are said to impart vitality to the tissues, consequently more simple in its nature ; hence we may conclude that to produce pus, it is only necessary for the blood to give up some of its elements, and the tissues be reduced to a fluid state—as the cellular and some other tissues. If that chemical affinity which preserves the blood and the animal tissues in a state of integrity —in a state of normality, be impaired or weakened, either by an inflammatory or other process, it is easy to conceive that pus may be abundantly produced, without necessarily appealing to a state of phlegmasial capillary stasis to account for such a product.

We pass over M. Lebert's interesting observations upon the mucus of expectoration, also several other chapters respectively upon granulation, cicatrization, and other effects of inflammation. We regret that we cannot follow him regularly through both volumes, but, had we space and time to do all this, still the book of plates, the most elegant of the kind, illustrating his microscopical researches, could not be made available to the reader in a short review. The reader must not suppose from what has been said of M. Lebert's writings that they contain nothing practical; quite the contrary; his apparently theoretical speculations are illustrated by clinical observations, many of which are well chosen and admirably described. We shall confine the few remaining remarks to be made to a brief analysis of his "*Resumé Général*" at the close of his first volume.

In order to arrive at a correct knowledge of the mechanism of inflammation, our author believes that experiments upon living animals are absolutely necessary, yet the value, although real, of these experiments are secondary when we come to deal with the same in man.

He places the first effects of inflammation in the capillary circulation. "It is at first accelerated, then the caliber of the capillary vessels is diminished, soon, however, to become dilated again. After these alternations of contraction and dilatation, the circulation is arrested, the globules of blood accumulate in the vessels, and the serum which surrounds them coagulates."

The first change which the blood undergoes in cases of capillary stasis, is the exudation of serum which holds the colouring matter of the blood in suspension. The general redness which accompanies inflammation arises from the effusion and transudation of the serum.

One of the most common and frequent effects of inflammation is a rupture of some of the capillary system; hence the hemorrhages and the escape of globules of blood in the parenchyma, or in inflamed surfaces.

Another striking effect of inflammation is the formation of new vessels, which takes place as already stated in a centrifugal manner; that is they shoot out and inosculate with those already formed. Another effect of certain inflammations is the obliteration of some of the capillary vessels, and if a collateral circulation is not established, the organ becomes atrophied. The pyogenic membranes which line ulcers and fistulas, or which form closed cysts, are nothing but vascular membranes, expansions of coagulated fibrine; subsequently we have developed here fibrous tissue by the transformation of plastic-fibrin.

We shall now notice briefly in M. Lebert's *resumé* some of the effects of inflammation upon particular organs. "In pnuemonia, red hepatization must take place either in the inter-vesicular, or in the pulmonary vessels, the result of an inflitration of granular globules—of reddish serum and blood—globules, the effect of a rupture of capillary vessels. Gray hepatization is caused by a general purulent infiltration, in which in some cases the pulmonary tissue is infiltrated with pyoid globules, fibrinous elements and fibro-plastic matter."

Carnification of the lung is only a chronic red hepatization, in which the more fluid parts of the inflammatory effusion has been re-absorbed. But we are transcending our limits, yet the subjects introduced are too interesting to be passed over without regret. We shall notice some of the concluding remarks of our author upon "*Tuberculization.*" He thus enumerates the constituent elements of tubercle: "Mollecular granules—an hyaline inter-globular substance, and the corpuscles or globules proper to tubercles. The diameter of the latter is from 0.005

to 0.01, their form is irregular, angular, with rounded angles; their centres are generally very smooth; they contain in their interior, which is yellowish and a little opaline, a certain number of molecular granules, but no nucleus. They are not acted upon or dissolved either by ether, water, or feeble acids; but the concentrated acids, as liquid ammonia and potash, dissolve them."

Tubercular globules are cellules very imperfectly developed. When tubercle softens, the interlobular substance liquefies, the corpuscles are disintegrated, become rounded, and may by the absorption of a liquid, appear larger; this is the beginning of decomposition. "The pus which surrounds a tubercle never is derived from the tubercle itself, but always from the surrounding parts. Softened tubrecle may always be distinguished from pus by the microscope. "The second stage of tubercle consists in their disintegration, their swelling,—this is a softening.—The third is that of their deliquesence; the molecules of the globules separate, these last loose their individuality and form a semi-fluid granular mass." "Tubercles in a cretaceous state reveal, under the microscope, amorphous mineral granules, with which we find mixed crystals of cholesterin and elements of pigment."

He enumerates the following elements as sometimes found in tubercle: "Melanosis which frequently complicates tuberculization of the lungs—bronchial ganglions, and peritonitis; fat;—fibres; large globules of a brownish green;—crystals which have the form of the ammoniaco-magnesian phosphates." According to Lebert, tubercle is always located in the inter-vesicular cellular tissue in the lungs; occasionally they may be found in the pulmonary vesicles or in the smaller bronchi.

The tissue which surrounds tubercle in the lungs is ordinarily congested or inflamed. "The most internal and the most fluid part of the contents of a tubercular cavity, contains :—*a*) tubercular matter, when globules are rarely intact, but swelled or on the point of liquefaction; *b*) globules of pus, sometimes in small quantities; *c*) mucus, or muco-pus; *f*) globules of blood; *g*) pulmonary fibres; *h*) black pigment; *i*) epithelium; *k*) crystals; and *l*) globules of fat. Theformation of the false pyogenic membrane which lines the cavities in the lungs, demonstrates the curative efforts of nature,—and effort to isolate the diseased pulmonary tissue from the contact of the air, and thus to favor the cicatrization of the ulcer."

Turn we now, after a hasty and an imperfect analysis of M. Lebert's great work on *Physiologie Pathologique*, to one no less valuable, though less minute on the subject of pathological science; we allude to the *Pathological Anatomy* of Julius Vogel,—a man, though young in the profession, still he has added many valuable facts to the science.

Vogel's work, which has already attracted and fixed the attention of the medical profession, embraces all the recent discoveries made in morbid anatomy. To give a general idea of the character of the work, we shall enumerate the different subjects which he has introduced and examined under separate chapters, and then sketch some of the leading practical views which the book contains.

The first chapter treats of the *abnormal development of gaseous matters—pneumatoses.* The second, of *abnormal collections of aqueous fluids—dropsies;* the third, of the *pathological relations of the blood;*

the fourth, of the general *relations of pathological epigeneses;* fifth of the *special relations of pathological epigeneses;* the sixth, of the *pathological changes in the physical properties of the tissues and organs of the body;* the seventh, of the *combinations of morbid elementary changes;* the eighth, of the *independent organisms in the human body—parasites;* the ninth, of the general *modifications of the human body—malformations*; and lastly, chapter the tenth, treats of the *changes occuring in the body after death—post-mortem changes.*

The arrangement of the work is sufficiently declared in the above enumeration of each chapter. On the subject of the first chapter, we shall say nothing, as it contains little or nothing new; and on that of the second, or dropsies, we may state before proceeding farther, that Vogel recognises two kinds of dropsical effusions; the one *serous*, the other *fibrinous.* The first is the most frequent and constitutes true dropsy—as that of ascites, hydrothorax, hydrocele, anasarca and œdema; it is derived chiefly from the serum of the blood, as its name implies. Whereas the second, or *fibrinous* dropsy, is that species distinguished by an effusion containing dissolved fibrin; it may occupy the same cavities as the first, and in addition fill the interstices of the parenchymatous structures. Chemically, it is identical with the plasma of the blood, though usually containing rather more water.

In his chapter on the *pathological relations of the blood*, Vogel has enumerated in a few words all that is known at present on the subject, and given in some instances new and interesting explanations of many of the phenomena connected with hemorrhages, extravasations of blood &c. With Lebert, he asserts that no extravasation can possibly occur without a rupture of the capillary vessels; in some cases, when the hæmatin dissolved in the serum escapes by exosmosis through the attenuated walls of the vessels, we may, by mistaking the dissolved hæmatin for pure blood, imagine a real hemorrhage has taken place.

We are the more likely to fall into such an error after death, as the hæmatin is not so readily soluble in the living body.

The deep discoloration with which we often find the tissues tinged after death, as the lining membrane of the great veins and arteries, the inside of the heart and many of the tissues of the body, is due to the hæmatin which has been thus acted upon and dissolved, by ammonia and other products, the result of decomposition. In a dead body examined several hours after death, all the tissues of the body, and especially those in contact with blood, presented a deep, dark, mahogany color—the effects of the dissolved hæmatin acting upon and tinging the tissues. It is important that this fact should be remembered, as the young pathologist might mistake such a discoloration for the effects of an acute inflammation.

Having glanced at a few of the points embraced in the second and third chapters, we hasten now to give a rapid sketch of some of the views of M. Vogel, as arranged under the head of "*Pathological Epigenesis.*" The term "*epigenesis,*" as used in pathology, is intended to signify the new formations which take place in the histological tissues of the organs. The laws which govern their formation are "closely allied to those which direct the development and formation of tissues in the normal state;" sometimes they seem indeed to be precisely the same.

Vogel divides pathological epigeneses into two groups—the organized and the unorganized. After explaining what he calls a morphologic and a genetic difference, or the difference between the organized and the unorganized, the writer enters at once into the merits of the question; he says—"like every thing else in nature, pathological growths require a material for their formation—a matter from which they may be produced. To this, which may be either fluid or solid, and may vary extremely in its chemical composition, we apply the general terms—*plasma*, or *formative matter*." This elementary or formative matter, or plasma, must be neither crystaline, nor organized, but must be amorphous. This plasma may be the starting point either for organized or unorganized products. Vogel calls the plasma, for the development of unorganized formation, such as crystals and the like, *mother liquid*; that for the production and formation of the organized, which takes place by cells, he calls *cytoblastema*, or *blastema*—meaning the cell formation—the cell growth—terms now so common in works on physiological pathology. Lastly, he designates the plasma from which both organized and unorganized products are developed, as mixed plasma. Thus much in explanation of the terms introduced in the chapter before us.

Any of the fluids of the body may, if a solid precipitate forms from it, serve as a *mother liquid* for the formation of organized or unorganized products. If a fluid saturated with insoluble compounds comes in contact with a membrane, on the other side of which a fluid deficient in water is placed, by endosmosis, the liquid will traverse the intervening membrane and leave the solid deposits, as the nuclous for unorganized formations. Again, in certain secretions of the body, as the urine, we have in a normal state an excess of acid; this excess of acid will dissolve all the solid products, thrown off from the blood, and thus prevent any unorganized formation: but if, on the contrary, from disease or otherwise, the alkaline predominates over the acid, the earthy phosphates being no longer held in solution, are precipitated as insoluble salts. As many of the fluids of the body contain the phosphate of magnesia—quite a soluble salt, when this salt meets in the system with ammonia, that insoluble compound—the ammoniaco-magnesian phosphate is instantly formed. Hence another source, a fruitful one too—of unorganized products in the system.

In order that the cytoblastema whether solid or fluid, may become the seat or centre of a new organized formation, it is absolutely necessary that fibrin should form a constituent part of this plasma; neither any of the salts or extractive matters can act as cytoblastema, although other protein-compounds, besides coagulated fibrin, may, under favorable circumstances, act as such, in determining the productions of new formations. Hence, says Vogel, "there remain, as the actual and potential constituents of the blastema, only the protein-compounds; although these are never found alone in the body, being always associated with the above named substances." None of the protein-compounds ever act as cytoblastema, however abundant, without the presence of fibrin; since in dropsical effusions, albumen, although notoriously abundant in these cases, we seldom, or never witnessed any thing like a characteristic morbid organization. The same remark holds good of casein, another protein-compound. Hence, from what has already been stated in regard to the formation of morbid products, a certain amount of fibrin is actualy necessary, in all cases, to give rise to new pathological formations. This fact is so universal and well established, that it may be laid down as a law in morbid anatomy. This law, however does not hold good in regard to the development of normal structures, since, according to our author, the presence of albumen is alone necessary in the egg, to cause the evolution of the chicken.

From the preceeding observations, ("establishing the principles that the blastema, for morbid products must always be amorphous, the old idea—that a normal tissue may be directly converted into a pathological formation, is overthrown.")

Let us pursue this subject yet a little farther. Now, if it be admited, with our author, that the basis of the blastema-fibrin is derived from the circulating fluid, and chiefly from the blood, whence is it that this blastema becomes the seat or rather the origin of different morbid products? Do wee not have cancer in one place, encepholoid in another, tubercle in a third, and so on throughout the long list of morbid products? Is it rational to suppose, therefore, that the surrounding tissues, in the midst of which this plasma or blastema is deposited, should so modify its future formation as to determine the growth of those different morbid products, according to the histological character of each particular tissue?

These are perhaps ultimate facts, which the human mind, with its present limited means, is unable to comprehend. We must now close our disjointed remarks on this interesting volume; perhaps we may continue them in the next number.

A. H.

II.—*Remarks on the Pathology and Treatment of Fever.* By James C. Harris, M. D., of Wetumpka, Ala.

This is a well written paper, (in the Western Journal of Medicine and Surgery for Feb. 1847,) and evinces considerable reading and research on the part of the writer. But though the author's style is good, and the article altogether creditable to his industry; there are passages from which we would infer, that, as a practitioner, he is somewhat inexperienced, or at least, that the present remarks are not based upon, or the result of his own personal observation. If we are mistaken in this opinion, we cannot be in another; to wit, that Dr. Harris was governed in the composition of his paper somewhat by pre-conceived opinions and has yet the prejudices of education to overcome: The following is one of the passages upon which our belief is predicated; though first we may mention that by Dr. Harris quinine is set down as "always a stimulant."

"This remedy (quinine) in the hands of those who entertain different views concerning its operation, and who are in the habit of prescribing it, at the time of high febrile excitement, to the almost entire exclusion of the lancet, the cold bath, and mercurial cathartics, are, in our opinon, *doing more injury to the constitutions of their patients* than ever resulted from the employment of any other vegetable remedy." Perhaps the Dr. means by this—" what becomes of the bile ?"*

Dr. Harris further remarks : " Why, in the treatment of fever, where a furred tongue, nausea and torpor of the liver and bowels, with yellowness of the skin exist to a considerable extent, any good results should be expected from the use of a remedy which does not evacuate in any sensible manner, and which according to some is a powerful narcotic, is to us passing strange." And is it not equally strange too,—for there are many strange things in medicine,—that where the symptoms here enumerated exist, benefit should be expected from a medicine which does evacuate in a sensible manner? The beneficial action of one it would seem is equally as inexplicable as the other. The materia medica would be circumscribed within narrow limits indeed, were we reduced to the necessity of using those articles only, the manner of whose curative action is perfectly understood ; in whose influence upon the human system there is not something more than "passing strange." But facts are not reversed by our inability to explain them—or by their seeming strangeness.

Is it certain however, that quinine "does not evacuate in any sensible manner?"—undoubtedly its diaphoretic action is most decided, and though not *directly* purgative and diuretic ; by, in some *strange* manner, allaying diseased action, establishing a healthy condition of the different organs,—permitting them consequently to assume their healthy functions, thence the natural secretions, and therefore the natural discharges to take place, it often proves indirectly so. We have seen purgative after purgative given in fever,—mercurial as well as others,—without any other effect than that of rendering the tongue still more dry and furred, —the " nausea and torpor of the liver and bowels, with yellowness of the skin" greater,—irritating the bowels, I would say, to too high a degree to permit their secretive action—("locking up the secretions" I believe is the word) till after the administration of quinine, when evacuations soon followed. Blood letting, (and there are other remedies also) under such circumstances, will allay the irritation, and have in many cases a similar effect. It is less generally safe however in its action than the quinine.

But it is really so rare a thing that it should be considered "passing strange," for an article which "does not evacuate in any sensible manner" to prove beneficial in the treatment of diseases, in which a "furred tongue, nausea and torpor of the liver—(as much so at least as takes place in our remittent fevers) and bowels, with yellowness of the skin exist ?" Are not these symptoms present, to as great an extent in pneumonia, as it prevails in the south, as in any of our febrile diseases ; and yet does not tartar emetic have a beneficial effect in its treatment ; and is it not a universally admitted fact, that it is only where it "does not evacuate in any sensible manner,"—where "a tolerance" of the remedy is established, that its virtues are manifested to the fullest extent?

The *strangeness* of the circumstance, that any good should be expect-

* See Southern Journal of Medicine and Surgery, for January 1847, Page 6.

ed from quinine under the conditions mentioned, is augmented too, it would seem, in the opinion of Dr. Harris, because it, according to some, is considered a powerful narcotic." But are there indeed no circumstances, with the most of the symptoms, enumerated by the Doctor, as contraindicating the use of quinine present,—to wit: furred tongue, nausea &c., in which opium itself even may be productive of good results? Do we not meet with such cases indeed frequently,—though less so now than formerly;—when Cook's pills and scruple doses of calomel, and proportionate quantities of other cathartics were more the fashion than at present,—in which the condition referred to, if not actually originally produced by medicines which do "evacuate in a sensible manner," is at least aggravated by them; and in which opium has a decidedly beneficial action?

III.—*Hand Book of Human Anatomy, General, Special, and Topographical. Translated from the original German of Dr. Alfred Von Behr, and adapted to the use of the English Student*, by John Birkett, Fellow of the Royal College of Surgeons of England, and Demonstrator of Anatomy at Guy's Hospital. Philadelphia: Lindsay & Blakiston, 1847, pp. 487.

This is a neat little work, into which the author has condensed a great deal of valuable information. He says in this preface "it may serve either as an introduction to the study of anatomy, or for refreshing the memory, more especially of those preparing for examinations. With this view, the relation which anatomy bears to physiology, pathology, and surgery, have been considered in an especial manner, either tacitly intimated, or expressly mentioned; but critical and useless observations have been as far as appeared practicable, disregarded." We think the work gives a "well-defined outline of the subject," and may answer a valuable purpose to those who have neither the time nor opportunity to examine large works or visit the dissecting room. It may be had of S. Woodall, 49 Camp street, New Orleans.

IV.—*Woman and her Diseases, from the cradle to the grave. The diseases of critical periods.* By Edward H. Dixon, M. D. Author of the diseases of the sexual system, etc. New York, 1846, p.p. 309.

We have yet to be convinced that such works as the above can accomplish any useful purpose. This work is not even good of its kind; it teaches the female absolutely little or nothing that would aid her in passing through the critical periods of life. It were better to leave the entire matter in the hands of nature, than trust to the advice of a matron who had read Dr. Dixon's book with its neatly turned periods and occasional poetic digressions. If Dr. D. has aimed to impart sound and useful instruction to the female touching her sexual diseases, he has said too little to do much good; but if his object has been to teach her the art of preserving her health and beauty, his book is worse than useless;

for it says but little on those important subjects not already familiar to every intelligent mother.

In his 17th chapter he discourses quite learnedly to the "dear young ladies and matrons, of *inflammation and hardening of the uterus and cancer*," interesting subjects no doubt to such men as himself and Mr. Lisfranc. In our humble estimation, it is an act of egregious folly, not to call it quackery, to talk to females about "chronic inflammation and cancer of the uterus"—diseases for the diagnosis and treatment of which the most skilful and careful physicians are taxed to their utmost.

The 18th chapter introduces to the attentive female reader the "*diseases of the ovaria*," a subject with which Dr. Dixon will make his patients perfectly familiar, although neither the learned author himself, or any of his professional brethren can diagnose with certainty diseases of the ovaria. In a work written expressly for the benefit of females, we think it in bad taste to speak of those obscure organic diseases which are not only of rare occurrence, but very difficult to detect.

It would have been the wisest plan to have pointed out the injurious effects of certain fashionable habits prevalent among the sex—dwelt upon the evils of over-much bandaging—urged the necessity of regular exercise in the open air—told them that "dipping" hurts digestion, clouds the mind, weakens the nerves, and produces hysteria: such seems to us are some of the subjects, with others of a kindred nature, which should be served up for the daughter and the mother. But such subjects are scarcely alluded to, too much being said of prolapsus of the uterus, chlorosis, leucorrhœa, fistula, and other surgical diseases. In a word, we cannot recommend Dr. Dixon's work to females, for whom it was written, nor to the profession, for it is not worth the least attention from the latter.

A. H.

V.—*First Principles of Chemistry, for the use of Colleges and Schools.* By Benjamin Silliman, Jr., M. A. Professor in Yale College, of Sciences as applied to the Arts. With two hundred Illustrations. Philadelphia, 1847.

Chemistry is at once a useful and a beautiful science, and in some respects as certain as the science of figures. The necessity of a familiar acquaintance with chemistry is now universally acknowledged, hence the number of works that are now issuing from the press on this science, both in this and other countries. The demand is equal to the supply, a good evidence that this branch of medicine is yet to shed additional light upon various other departments of our science.

Benjamin Silliman is the son of the distinguished professor of the same name, and although quite a young man, yet he has given proofs not only in the work before us, but elswhere, of great application and no ordinary share of learning. As an elementary work on chemistry—as a text book for colleges and academies, this work is equal to any of the persent day, and superior to many of much higher pretensions.—The illustrations are fine and apposite; the style of the auther neat, simple, and well adapted to instruct the uninitiated. His descriptions are very lucid—always giving the rationale of chemical action and reaction. His spelling differs slightly, in a few instances from the majority of chemists, but this is more a matter of taste than of utility. At the bottom of each page, appropriate questions are placed—another important feature in the work, which makes it doubly valuable to the young student.

The book is divided into four parts ;—in the first he treats of physics ; in the second, of chemical philosophy ; in the third, of inorganic chemistry ; and in the fourth, of organic chemistry.

Thus the student gradually advances from the simple to the compound, beginning as it were with the alphabet of chemistry, and finally the new world of organic chemistry opens to his view.

From a hasty examination of this book, we have spoken perhaps too warmly in its favour ; but if we are not mistaken it will, as soon as known, be adopted as a text book in many of our colleges and other institutions of learning. We recommend it to teachers and students throughout our country, as a neat compilation from other and more elaborate works on the science of chemistry.

A. H.

VI.—*Fourth Annual Report of the Managers of the New York State Lunatic Asylum. Made to the Legislature Feb. 2nd,* 1847. By Amariah Brigham, M. D., *Physician to the Asylum.*

2. *Twenty-Sixth Annual Report of the Bloomingdale Asylum for the Insane.* By Pliny Earle, M. D., Physician to the Asylum, New York, 1847.

The Annual Reports of Drs. Brigham and Earle are always of interest. We have noticed those of the last two or three years so fully, that we will not now enter into the general subject of these laudable institutions, but confine ourselves more especially to the reports before us.

According to Dr. Brigham, the number of patients in the State Lunatic Asylum at the beginning of the

year 1846, was - - - - -	285—Men	143—Women	142		
Admitted during the year, - -	337	"	163	"	174
Total, in the course of the year,	622	"	306	"	316
Of this number there have been					
Discharged, recovered, - -	133—Men	65—Women	68		
" improved, - -	60	"	26	"	34
" unimproved, - -	33	"	15	"	18
" died, - -	22	"	13	"	9
Total discharges during the year,	248	"	119	"	129
Remaining on the 20th Nov. 1846,	374	"	187	"	187

"In addition to the recoveries of patients, mentioned in the foregoing table, there are now above thirty in the Asylum who are well."

We agree with Dr. Brigham that, "the number of deaths (22) is small, considering the number of patients (622) that have been under our care the past year. Two died of inflammatory affections of the lungs, seven of marasmus, five of consumption, two of convulsions, two of diseases of the heart, one by effusion on the brain, one by suicide, one by paralysis, and one by apoplexy."

According to Dr. Earle, the number of patients in the Bloomingdale Asylum, Jan. 1st 1846, was Males 60—Females 57—Total 117

Number of cases admitted during the year, - - -	"	75	"	58	"	133
Whole number in the Asylum,	"	135	"	115	"	250
No. of cases discharged, -	"	50	"	56	"	106
Died, - - - -	"	10	"	3	"	13
Remaining Dec. 31st 1846,	"	75	"	56	"	131
Of cases disch'd, there were						
Cured, - - - -	"	29	"	25	"	54
Much improved, - - -	"	7	"	8	"	15
Improved, - - - -	"	13	"	8	"	21
Unimproved, - - -	"	6	"	10	"	16
Daily average number for the year, - - - -						124.

These results also are certainly very satisfactory. Dr. Earle reiterates the complaint that patients believed to be curable are removed prematurely from his institution. On the other hand Dr. Brigham says he has not had occasion to make this complaint, so common amongst Institutions of the kind. This he attributes to a law of the State which provides that "no patient shall be admitted for a shorter period than six months." However Dr. B. says they are discharged at any time when it is deemed good and safe to do so. He gives some curious statistics in regard to the age, sex, occupation, height, weight, size and shape of head, frequency of pulse, &c, &c. The following extracts relating to the moral and intellectual exercises of the lunatics will be read with interest.

RELIGIOUS WORSHIP.

"Religious services have been continued throughout the year. All the patients who wish to attend, and can conduct with propriety, assemble every Sunday in the Chapel, together with the officers, attendants and assistants that can be spared from other duties. The services last about one hour. Rarely any disturbance occurs. The patients are generally attentive, and several assist in singing. We are of the opinion that much good results to our patients from thus religiously observing the Sabbath. The Rev. Chauncey E. Goodrich, who resides in the immediate vicinity of the Asylum, officiates acceptably as Chaplain.

" Our monthly concerts or monthly meetings of the Chaplain with the officers, attendants and assistants have been continued with advantage. In addition to religious services, the occasion is often embraced to instruct those in the employ of the Institution respecting the nature of insanity, and in their duties towards the insane.

" Recently we have added to our Chapel a new and valuable organ, it has been purchased with a portion of the avails of our last annual fair and those anticipated from the next. It is usually played by a patient, and we consider it a valuable addition to our music.

SCHOOLS AND AMUSEMENTS.

" Schools have been continued as heretofore, during the winter season, and our confidence in their usefulness in Institutions for the insane, increases with every years' experience. Schools with occasional exhibitions, the acting of original plays, and other literary exercises, together with labor, constitute our best, and in fact nearly our only amusements. Card playing and other games in which there is no exercise of the body, we are inclined to discourage. We think cards are often objectionable. They become too engrossing, and we fear may sometimes have a bad influence upon young persons after their recovery.

" But as yet we have discovered no objection to schools, on the contrary, we daily see their advantages, and occasionally some striking cures apparently effected by the mental exertion they induce. It is the voluntary mental effort of the patient himself, that in numerous cases is most essential to recovery.—Talking *to* patients and lecturing, reading and preaching *to* them, are very well, no doubt, and often serviceable, but in our opinion less effective as a means of restoring the disordered mind. Patients who come to us in an irritable or melancholy state of mind, brooding over their suffering and delusions, not unfrequently have their attention arrested and awakened by our schools and exhibitions, and soon feel disposed to take part themselves. They then become more contented and cheerful, and thus the way is prepared for their recovery.

" Recently we have had at the Asylum several theatrical performances, embracing tableaux, the acting of short original plays, declamation, music, &c., which has not only interested the patients themselves, but elicited the applause of visitors and strangers who were present.

" In addition to the good influence of schools in promoting the recovery and contributing to the enjoyment of curable cases, we witness with great satisfaction their good effects in improving the incurable and the demented, and keeping them from sinking into a more hopeless or idiotic condition. They also improve the minds of all, for there are few persons, however well educated, that are not benefitted by renewing, as they here have an oportunity in our schools, their knowledge of history, geography, &c. Among our patients are *seven* who are graduates of colleges. Several of these take an active part in our schools and exhibitions, and some assist as instructors.

" Among the men, a debating society has existed for several winters. Their meetings are in the evenings, once a week, and their discussions are conducted with decorum and ability, and to the enjoyment and edification of those who do not take part in speaking. Patients, whose minds become thus aroused by their own voluntary efforts, soon acquire a taste for reading, and they then read with attention and for a valuable purpose, that of acquiring information *for use.* We have a good library, and in addition, as will be seen by our list of periodical literature at the end of this report, we are supplied with the most valuable reviews and magazines in the country, and about sixty different newspapers, some daily, and some weekly, amounting in all to above one hundred a week.

" Under the head of amusements, we ought, perhaps, to mention a variety of animals that are kept at the Asylum, to interest and amuse the patients, among which are deer, two large warrens of rabbits, tame raccoons, canary birds, peacocks, &c. &c. Also a green-house containing above one thousand plants."

Similar exercises and amusements are practised in the Bloomingdale Asylum. These exhibitions must present novel and curious spectacles.

The following remarks are worthy of serious attention ;

HEREDITARY PREDISPOSITION TO INSANITY.

" There is nothing in connection with the study of insanity more deserving of attention than the tendency of this disease to be transmitted from parents to their offspring. The fact is most unquestionable, and we are of opinion that it has more influence in producing that disease, than all other causes combined. It does not of itself excite the disease, but when it strongly exists a trivial cause will develope it. Thus most of the supposed exciting causes in the foregoing table would, of themselves, be inoperative, if there was not an inherited constitutional tendency to insanity.

" Sometimes the children of an insane parent are however exempt from the disease, while it appears in the grand-children. Contrary to the opinion of many, we have found this inherited form of insanity as curable as any other, though the subjects of it are very liable to relapse, and from slight and various causes. Sometimes a little sickness, a slight fever, or severe cold, and at others a little mental disturbance, such as the loss of relatives, or property, or religious anxiety, excite it. We have known individuals, thus predisposed to insanity, have repeated attacks, and each time from a different exciting cause.

"Of 1181 patients who have been at this Asylum, viz: 594 men, and 587 women, 315 were known to have insane relatives. That many of the others were thus predisposed we do not doubt. but we were not able to learn anything respecting their relatives. 175 were known to have insane parents, viz: 79 men, and 96 women.

"It would appear from our inquiries, and they have been very carefully conducted, that insanity is a little more likely to be transmitted by the mother than by the father, and that mothers are considerably more likely to transmit it to daughters than to sons; while the fathers most frequently transmit it to sons. Thus, out of 79 men, 42 had insane fathers and 35 insane mothers, and in two instances both parents were deranged; while of 96 women, 37 had insane fathers and 56 insane mothers, and three inherited a predisposition to insanity from both parents.

"Sometimes the children of an insane parent do not inherit any tendency to insanity. In such instances the exemption appears to be in consequence of inheriting the constitution and temperament of the parent not insane. When however the children resemble in personal appearance the insane parent, and manifest the same peculiarities of feelings and temper, there is reason to apprehend they will be more or less disposed to the disorders of the parent they resemble.

"These facts cannot fail to arrest the attention, not only of those who have relatives and friends that are insane, but of every philanthropist; and be taken into consideration in forming matrimonial alliances, and be duly regarded in the physical and moral education of those thus liable by inheritance to insanity.

"The early education of all such requires much attention. Great pains should be taken to form a character not subject to strong emotions, to passion and caprice. Among the most frequent causes of insanity in those not predisposed to it, is the over-indulgence of the appetites and passions in early life; and to those who inherit a tendency to this disease, such a course is highly pernicious.

"The utmost attention should be given to secure a good bodily constitution. Such children should be confined but little at school; they should be encouraged to run about the fields and to take much exercise in the open air, and thus ensure the equal and proper development of all the organs of the body. They should not have the intellect unduly tasked. Very early cultivation of the mind, and the excitement of the feelings by the strife for the praise and the honor awarded to great efforts of mind and memory, is injurious to all children, and to those who inherit a tendency to nervous diseases or insanity, most pernicious."

Dr. Brigham's remarks on the different forms of insanity, *suicidal* and *homicidal*, the latter illustrated by interesting cases, are very interesting.

He says of the former:

"Usually there are a considerable number belonging to this class in every large asylum. Thus of 1,181 patients received into this asylum, 146, viz: 63 men and 93 women, were disposed to suicide. It is, however, a consoling fact, that this alarming variety of insanity is quite often a curable one. Among the most complete and permanent recoveries from insanity we have ever known, are a considerable number, who, for several months were very strongly inclined to self-destruction."

In regard to homicidal insanity, he says:

"According to our observation, the *homicidal insane* may be arranged with propriety in six classes; those belonging to one class appear to be actuated by motives or feelings different from the others, or the circumstsnces accompanying the act indicate a difference in their mental condition.

"I. Those who take life in a paroxysm of insane passion or fury.

"II. Those who commit a homicide from delusion, who are deceived and misled by their hallucinations, illusions, or disordered imaginations.

"III. Those who kill indiscriminately and apparently from a love of taking life, from a diseased propensity and conscious desire to destroy others, against which act neither reason or conscience remonstrates.

"IV. Those who kill without any apparent motive, from a sudden impulse, but of which they are not conscious, and who retain no recollection of anything that prompted them to the act.

"V. Those disposed to commit the same crime, and without motive, from an irresistable impulse of which, however, they are conscious, and against which reason often remonstrates.

"VI. Those who kill from imitation, or from an insane love of notoriety.

"Instances of each variety above mentioned we have seen and had under our care, and there may be other forms of this diseased propensity that we have not seen, though all we now recollect in books, may, we think, be classed as above.

"I. Those belonging to the *first class*, who are disposed to assault and kill others from a sudden or violent burst of insane passion or fury, are to be met with in every Lunatic Asylum. Insanity frequently causes extreme irritability. Many of the insane cannot bear opposition or denial, and having lost the power of self-control, they give way to their excited feelings, and suddenly strike, when offended, with any instrument they find, and thus sometimes destroy life.

"One insane person nearly killed another because the latter unconsciously stood before him so as to keep the sun from shining upon him. Without requesting the other to move he violently assaulted him. Another threatened and attempted to take the life of one who had spilled some water upon his coat. A lunatic on Blackwell's Island killed another with a shovel because the latter "bothered him by asking for tobacco."

"To this class belong most of those cases we occasionally see in the newspapers, of insane persons who are at large, killing their neighbors or relatives in a moment of fury. One, for instance, killed a neighbor, severing the head entirely from the body with an axe, because he did not immediately give him some cider when requested. In such cases the existence of insanity is generally known previous to the homicide, and the offender is not considered a subject for punishment. Occasionally, however, some very doubtful cases arise, in which it is difficult to determine whether the act is the result of insanity which has deprived the person of self-control, or whether it is the mere result of depravity and violent passion.

"Usually, however, in these somewhat doubtful cases, it will be found that those who ought not to be considered responsible for their actions, by reason of insanity, have suffered from some severe illness or from some great mental disturbance, since which time their temper and disposition have undergone a marked change. When no such facts are found to exist, nor any other evidence of mental disorder, we think the act itself, committed during violent passion, is not proof of insanity, though it may have been committed under such aggravated provocation as to render it in public opinion justifiable homicide.

"II. In the *second class* we have placed those insane persons who take life from delusion, deceived and misled by their hallucinations, illusions or disordered imaginations. Probably more homicides are committed by insane persons laboring under this form of insanity than any other.

"Thus the mother of four children became melancholy and wished to die; but considering suicide to be a heinous crime, for which she could not be pardoned, concluded to kill one of her own children, aged sixteen months, who being innocent would be happier in another world, and she would then be hung for the crime, and thus accomplish the purpose she had at heart.

" She killed the child by cutting its throat with a razor, and was soon after placed under our care. She remained very gloomy for six months, apparently much bewildered in mind, and wondering why she was not arrested, tried, and executed. She finally recovered, and has been well for two years, and had the charge of her family. This excellent lady was naturally of an amiable disposition, and at the time of killing her child had a full knowledge that it was considered a heinous crime, for which death was the punishment.

" A well educated and highly esteemed single lady, aged 42, who lived with, and had the care of her aged parents, was noticed to be rather gloomy and excentric, and at times a little deranged, in the spring of 1845. A few weeks after this she requested her mother to walk to one of the neighbors, and urged her to do so for her health. Soon after her mother was gone, she killed her father, aged 76, with an axe, as he was sitting shaving himself. She cut his head open with two blows of the axe, then the back of his neck with the same instrument, and finally his throat with his razor. She then put the room in order, and went and called her mother home, exhibited to her what she had done, saying, "I have killed father, and will now kill you, and then myself, as it is time we were in heaven." She then seized her mother, whose screams attracted the attention of persons passing by, who rushed in and rescued her.

" Soon after this the daughter was brought to this Asylum, which then being too full to admit her, she was taken to Bloomingdale Asylum, where she remained about three months, when a vacancy occurring here, she was brought back to this place in July.

" She was quiet and industrious and, though extremely melancholy, was much of the time rational. Sometimes, however, she had insane delusions, and was at times manifestly deranged in mind, and strongly disposed to suicide. She was much inclined to talk about the tragic scene we have just related, and said that "at first she thought only of killing herself, but reflecting that her parents, now aged and dependent on her for supervision and care, would be uncomfortable without her, she concluded that it would be best to kill them first, and as they were all pious persons, they would go to heaven together."

" She remained with us until Dec. 30th, 1845, when, relying more than we ought on her expressed conviction that she should be able to overcome her occasional disposition to self-destruction, we permitted her to lodge in a room by herself, where, after a few weeks, she hung herself during the night. This is the case of suicide we have already mentioned as having occurred this year.—She left a letter expressive of thanks for the kindness with which she had been treated, and making known her wishes in case, as she expressed it, she "should again be overcome by her evil temptations."

" As remarked, she was a considerable part of the time entirely rational, and there seemed to be nothing to prevent her complete recovery, except her sad and distressing thoughts. She often said, that when she awaked in the morning she felt as well as ever in her life, until thoughts of the past came over her. Her mind seemed constantly to dwell upon the act we have mentioned, and for which she could assign no cause except the "instigations of the evil one." In fact this was her decided opinion when she appeared sane.

" In this deplorable case of homicide there was no malice ; on the contrary, we have no doubt this most unfortunate woman was actuated by kind and benevolent motives.

" Cases of a similar character, in which under the influence of delusions the insane commit homicide, are numerous in books, but we think it best to refer for the most part to those that have fallen under our own observation, and although we have seen several others, the foregoing will be sufficient for our purpose.

" III. In the *third class* we have placed those who destroy others indiscriminately, and apparently from a love of taking life, from a diseased propensity, an intense and conscious desire to kill, and against which act neither reason or conscience remonstrate.

" Some insane persons appear to be almost constantly desirous of injuring or killing others. There is a patient now at this Asylum who often says to us " give me a knife—let us go and kill somebody—you know you promised me we should go and kill folks, &c." Once having obtained a knife, he endeavored to cut the throat of a demented patient near him. He appears to be almost constantly desirous of killing, and expresses a wish to do so, and has attempted it whenever he has had a good opportunity. Yet he is almost uniformly pleasant and smiling, and passes several hours of each day on his knees in prayer, and appears not to have any feelings of malice towards any one, and does not manifest any passion.

" This man committed a homicide before he came to the Asylum, and his history is briefly as follows :

When a young man his character was considered good, but about the year 1835, when he was 25 years of age, he became deranged, in consequence, it was said, of disappointment in love, and subsequent ill health. For several years he was permitted to be at large most of the time, though known to be dangerous, as he would often arm himself with a scythe or pitchfork, and was several times put in jail for breach of the peace. Early in 1843, while living with his brother-in-law, he was left by him in the barn with a young man about fifteen years of age. Towards evening his brother-in-law returned, and enquired of him where the young man was. He replied he did not know ; and when more earnestly inquired of, he said, "that he expected he had stolen some money and run off." Search was made, and the body of the young man was found burried very carefully under the barn, pierced through more than twenty times with the pitchfork, besides several deep wounds made with a knife. The insane man was charged with having done this, but denied it then, (and has ever since,) and soon after took a horse and rode off, and was not overtaken until he had rode thirty miles, and then not without a severe struggle, in which one of those who attempted to arrest him was severely wounded.

" Considerable design and cunning was shown by him in concealing the body and making the barn look as if nothing had occurred, by conveying the bloody straw to a swamp and concealing it, and wiping the shovel and floor very clean. In this case there might have been some trivial motive for the act ; but as he had heretofore manifested a disposition to injure others, and ever since to a very marked extent, we believe we have very properly classified him with those who have a diseased propensity,—an intense and conscious desire to kill.

" Another instance of this same propensity is as follows :

S., a young man, after having been in college three years, became deranged from excessive study, as it was supposed, and was soon after committed to our care. His mind seemed at first much shattered, talking constantly in a rambling and incoherent manner, chiefly in relation to his past studies—Latin, Greek, History, &c. After a few months he suddenly exhibited a propensity and expressed a strong desire to fight, and would attack any one that came near him, though he did not appear to have any malice, and was habitually pleasant. One day when he was sitting at the dinner table he took a knife and attempted to cut the throat of the person that was near him ; and for several months after this, expressed the most urgent desire to kill some one, he did not care who.—When locked into his room, he would stand for hours watching with a most anxious countenance for the door to open, that he might have an opportunity to attack any one that opened it. Yet he was ever pleasant, and exhibited neither malice or passion, and apparently acted without any motive.

" We might refer to many other cases of a like kind, but we believe the foregoing will suffice to fully establish the fact that some insane persons have a propensity and desire, of which they are conscious, to kill others, without malice, motive, or passion.

" IV. In the *fourth class* belong those who kill without any apparent motive, from a sudden impulse, but of which they are not conscious, and who retain no recollection of any thing that prompted them to the act.

"Those belonging to the third class, have an intense desire to kill, of which they are conscious, and are usually evidently insane in other respects but those belonging to the fourth class, kill from a sudden impulse, without any desire or conscious feeling relating to the act, and not unfrequently the act itself *is the first noticeable evidence of their mental darangement.*

"Mrs.—— an amiable lady, and the affectionate mother of three children was noticed by her husband to be more low spirited than usual; but her manner was not such as to attract the attention of other members of the family who ate at the same table with her. Suddenly she killed one of her children by repeated blows with a hatchet. Soon after the act, she attempted to kill herself, and was placed under our care, a wretched maniac. For several weeks she remained without much change, rather stupid as if she had no recollection of the past. She however recovered after various changes in her mental condition, and has now been well for several years, and had the care of her family as usual. She has often informed us since her recovery that she could recollect no motive whatever that induced her to commit the act, and is confident she never thought of it until she saw the hatchet and the child together, and then she instantly accomplished it, without any feeling of which she was conscious.

"An accomplished and well educated young lady at this Asylum, who most of the time appears entirely rational, is occasionally seized with a sudden impulse to attack others and to destroy things. On one occasion, about the dawn of day, when entirely alone, she suddenly broke the mirror in her room, the toilet table and twenty-three panes of glass. When interrogated on the subject, either at the time of her paroxysms, or subsequently when perfectly sane, she can give no reason for her conduct, nor assign any cause for what she has done. Sometimes she threatens the lives of others, and suddenly attacks them without any provocation.

"V. In the *fifth class* we have placed those who are disposed to commit the same crime without motive, from an irresistable impulse, of which, however, they are conscious, and against which reason often remonstrates.

"We have been twice consulted by different individuals who appear to be entirely sane, but who felt at times an almost irresistable propensity to kill others, especially their dearest friends and relatives.

"Mr.—— an intelligent and respectable gentleman in an adjoining county, has repeatedly asked our advice in his own case. At times he experiences a sudden and intense desire to kill his wife and children to whom he is very strongly attached. He is also at times disposed to suicide. He can assign no cause whatever for this dreadful propensity. He is a man of property, of good reputation, and knows of nothing to disturb his mind. On one occasion his intelligent lady accompanied him, and confirmed all he said of the agony of his mind, in consequence of the sudden, dangerous and unaccountable feelings which we have described. By change of occupation, which keeps him much of the time travelling, he is of late less tormented with this unnatural propensity.

"A young lady in this Institution, who much of the time is rational and industrious, once in a few weeks experiences a strong desire to tear her clothes and to break things, or to injure herself or others. When these attacks are coming on she begs to be confined. Of the origin or cause of such feelings she has no knowledge, and none are apparent to others.

"Many cases of a similar character have been published, but we deem it unnecessary to quote them. They may be found in works on insanity.

"VI. To the *sixth class* belong those who kill from imitation or from an insane love of notoriety.

"Some belonging to this class are partially idiotic, or from faulty organization of the brain appear to have no moral sense or feeling. Thus a young man whose head is unnaturally small, from early childhood manifested a total indifference to the sufferings of others. When a child, he would strike and wound other children without any obvious motive or passion. When be became older and larger, he would assault and sometimes kill the domestic animals that came near him, and on one occasion killed a cow by cutting into her with an axe, with the apparent indifference that he would cut a stick of wood. He was under our care for some time, and was often amiable and obliging, and had a good memory, but as we have said, he seemed destitute of moral sense or feeling.

Another young man, of good education, set on fire at different times, several buildings, and once poisoned with arsenic a friend to whom he was much attached. In each instance he immediately gave notice of what he had done.—He was arrested and adjudged insane by a jury, and sent to our care, and from much attention to his case, we believe him mentally deranged, and that he is properly classed with those, who, partly from an insane love of notoriety, and partly from imitation, commit crimes and confess them. He says that hearing and reading of large fires, where several people were burned to death, made him first think of burning buildings himself; and that an intense love of excitement and being talked about, together with an indiscribable confusion of mind, lead him on to commit the crimes we have mentioned. He is an amiable and intelligent young man, and at times exhibits strong religious feelings.

Dr. Brigham's illustrations and authorities on the difficulties involved in the *medical jurisprudence of insanity* are excedingly interesting, but our limits will not allow us to extend our quotations any farther. In concluding our notice of these interesting reports, we will reiterate the hope that they may be extensively circulated, as well amongst the people as the lawyers and law-makers of the land.

F.

VII.—*Encyclopœdia Americana. Supplementary Volume. A Popular Dictionary of Arts, Sciences, Literature, History, Politics, and Biography, vol. XIV.* Edited by Henry Vethake, L. L. D. Vice Provost and Professor of Mathematics in the University of Pennsylvania, &c., &c. Philadelphia : Lea & Blanchard, 1847.

Those who have this work should by all means supply themselves with the supplementary volume, which contains important additions to all its departments. The Encyclopædia Americana has already passed through eight editions, and a ninth is now in course of publication. It is an immense magazine of useful knowledge, and should be in the library of every American gentleman. The American editor of the supplementary volume is a man of high standing, and has performed the task with credit to himself. This is all we deem it necessary to say concerning a work so generally and so favourable known as the Encyclopædia Americana.

VIII.—*Report of the Surgeon General.*

This report is very brief, and we are sorry to say defective in many points of information respecting the medical department of the army of invasion, but we suppose this has been unavoidable. We give below all of it except the portion that relates to the fiscal transactions of the bureau. It is much to be hoped that efficient steps will be taken to procure full reports on the health of the army, as well the volunteer as the regular service. Dr. Lawson's remarks on the volunteer service are worthy of special attention: they are pretty severe, though founded in truth. In regard to the qualifications of applicants for the place of surgeon or assistant surgeon, it is a severe reflection upon the general standard of professional merit to see how small a number come up to the standard of requirement.

"Ample supplies of medical and hospital stores generally have been regularly forwarded to the troops in the field, both volunteers and regulars, and it is believed that every article of supply in the way of remedial agents, compatible with the nature of the service, has been provided for the sick and wounded of the army.

"Perfect accuracy in the reports of the sick of the army in the field, under a constant change of position, with the alternate states of dispersion and concentration of the companies of the different corps, cannot be expected. The following statement, however, approximates sufficiently near to the truth to constitute the basis of a statistical investigation, with a prospect of fair results.

"From the tabular report of the sick and wounded of the regular army, hereto appended, it will be perceived that the number of cases of indisposition, including wounds in battle, which came under medical treatment during the year just past, was 27,399; 26,685 of which occurred within the last twelve months, 714 being cases that remained of the preceding year.

"Of the whole number of cases treated, 25,667 are reported to have been cured; while of the men who continued to make up the number of cases of indisposition (some of them having been on the sick list twice, thrice, or oftener, in the year) 226 have been discharged the service, 15 have deserted, and 246 have died, leaving 641 still on the sick report.

"As the mean strength of the army for the last twelve months was 9,083, and the number of cases of sickness during the same period was 26,685, with a loss by death of 246, it follows that the proportion of cases of disease to the number of officers and men in the service was 2.93 to 1, or that on an average each man was sick 2.93 times during the year; that the ratio of deaths to the number of men was as 1 to 35.16, or 2.84 per cent.; and the proportion of deaths to the number of cases under treatment, as 1 to 108.47, or 0.92 per cent.

"In relation to the sickness which has prevailed among the volunteer troops, I have not sufficient data upon which to found a report leading to any useful results.

"The surgeons generally of the volunteer corps have made no regular return or other statement of the sick to this office; and no information on the subject, derived from other sources, is sufficiently accurate or explicit to be adopted as the basis of an official report. All that I can say understandingly on the subject is, that, whether stationary or on a march, in camp or in the field, the volunteers have been exceedingly sickly.

"The Tennessee and Kentucky regiments of cavalry left a great many of their men sick in hospital at Memphis, Tennessee, and again at Little Rock, Arkansas. How many men were left on the road afterwards I do not know; but it is understood that the Kentucky regiment of cavalry continued to be sickly, having, while at Port La Vaca, three hundred on the sick report.

"The same proportion of sickness, from all accounts, seems to have prevailed among the volunteers located on the Rio Grande, one-half being from time to time, as is understood, on the sick report.

"From the best information which has been received at this office, it is believed that the extent of sickness among the volunteers on the Rio Grande has been fourfold to that among the soldiers of the regular army, with a corresponding excess of mortality in the ranks of the former."

"This state of things, it is apprehended, will ever exist with volunteer troops, or undisciplined men employed on distant service and in a foreign clime; more particularly with volunteer corps, gotten up under the impulses of the moment.

"Old men forget their age—young men think not of their physical disabilities. Impelled by a feeling of patriotism, a thirst after military fame, or the spirit of adventure, many of them recklessly enter the ranks and undertake to perform the duties of a soldier, the toils, the privations, nor the self-restraint attendant on which are they in a frame of mind or of body to endure.

"It is not until they have embarked in the enterprise, have journeyed several hundred miles at great expense to the government, and much to their own discomfort, that they find out there is something more required to constitute an efficient soldier than patriotism, chivalry, and valor. Then, for the first time, they understand that the labor and exposure, the watching and fasting, the self-denial and self-restraint, they have to undergo, and for which neither nature, nor education, nor habit has fitted them, are beyond passive endurance.

"In this vexed state of mind they readily take sick, then become melancholy and despondent, with a corresponding aggravation of the disease; so that, should they not sink under the accumulated weight of mental and physical infirmities both, they seldom, after being once stricken down, return to the duties of the field.

"By the time that they have been restored to their feet again, the battle has been fought and the laurels already borne off; and then, though it has not been their good fortune to attain the object of their high aspirations, (a triumphant conflict with the enemy,) they have exhibited, at the sacrifice of their health, their zeal in their country's cause, and are anxious to return home.

"The correctness of these remarks will, it is believed, be admitted by the volunteers themselves, many of whom enrolled their names with the prospect of wearing a commission; but, having failed in their competition for the station of commissioned officer, are obliged to serve in the ranks as a private soldier.

"It is proper to state here that one-third and more of all the men who offer to enlist in the regular army, are rejected; and it is reasonable to suppose that very many of those who are enrolled for the volunteer service would, if critically examined, be pronounced physically incapacitated for the arduous duties of a soldier.

"As far as I understand the matter, the government has, under the present state of things, virtually to pay a hundred men, while they realize the services of but fifty.

"What with the extraordinary expenses attending the concentration of the individuals at a point, their organization into companies and corps, then their outfit and transportation to the theatre of war, together with the expenses of their return home before the expiration of their term of service, on a sick ticket, or on a certificate of discharge, the volunteers have cost the government 100 per cent. more per man than the men of the regular army.

"But this is not all—the presence of a numerous body of invalids seriously embarrasses the service; for, besides consuming the subsistence and other stores required for the efficient men, they must have an additional number of surgeons and men to take care of them, and a guard to protect them, which necessarily lessens the disposable force, the available force for active operations in the field.

"From the foregoing statement of facts, it may readily be conceived that measures ought to be taken to prevent the introduction into the volunteer corps, the same as in the regular army, of men who, from disease or original constitutional defectibility, are disqualified to perform the active duties of a soldiers.

"As the gentlemen of the medical profession participated in the general enthusiasm, and desire for employment in the military service, the applications for appointment to the medical department of the army were very numerous.

"Believing, at the time, that Congress would make an addition of twelve or fifteen members to the present number of the medical staff, and calculating somewhat on more frequent vacancies from the casualties of the service during a state of war, sixty-three applicants were invited to present themselves for examination before the army medical board, which convened for that purpose on the 1st of July last.

"Of the sixty-three who were invited, forty-three only presented themselves for examination; the others having declined, or having been prevented by business from appearing before the board.

"Among those who reported to the board, three were deemed physically disqualified, and of course not admitted to an examination of their professional attainments; fifteen retired without any examination at all; five withdrew after a partial examination; and twenty were thoroughly examined for appointment.

"And of the twenty who passed through a thorough examination, eight came up to the standard of professional merit required, and were recommended for appointment to the medical staff of the army.

"The officers of the medical staff serving with the several army corps employed against the enemy have participated largely in the toils, the privations, and the dangers of the field, with their associates-in-arms of the line of the army.

"The services of those, with Medical Director Craig at their head, attached to the army of occupation, have heen more conspicuously brought to our notice; and it is but justice to say that they have been found present wherever their honor and their duty called them, nobly fulfilling in every particular their obligations to the country.

"Those gallant spirits led on by Major General Taylor, always in the presence of the enemy, and frequently in conflict with him, have necessarily afforded ample scope for the exercise of skill and judgment in practical surgery; and the ability which the medical officers have displayed, and the unremitting attention they have bestowed on the sick and wounded soldiers, (the enemy included,) have called forth a willing tribute of respect, and the grateful acknowledgements of all who have experienced or witnessed the results of their humane efforts and practical skill.

"Of the twenty full surgeons of the army, thirteen are serving with the army corps operating against the Mexicans, two en route for the theatre of war, and five at other stations.

"Of the fifty assistant surgeons, one is sick, twenty-nine are employed with armies in Mexico and California, and eight at extreme frontier posts; leaving the the remaining twelve on duty at various stations through the country, but available, or most of them, for service in the field.

"To enable us to furnish medical officers to the several general hospital establishments, and to the different depots and other military post on a long line of operations, in addition to the surgeons employed immediately with the corps in the field, I have again respectfully to suggest the propriety of making an accession of from twelve to fifteen members to the present strength of the medical staff of the army.

"All of which is respectfully submitted.

"TH. LAWSON,
"*Surgeon General.*

"Hon. W. L. Marcy,
"*Secretary of War.*"

IX.—*The Half-Yearly Abstract of the Medical Sciences: Being a Practical and Analytical Digest of the contents of the principal British and Continental medical works published in the preceding six months. Together with a series of critical reports on the progress of Medicine and the Collateral Sciences during the same period.* Edited by W. H. RANKING, M. D., Cantab. Vol. III, January—June, 1846.—Vol. IV, July—December, 1846. Philadelphia: Lindsay & Blakiston, 1847.

We cannot too strongly urge upon our readers the importance of taking this cheap and valuable work. For the trifling sum of *one dollar and a half*, upwards of 700 hundred pages, comprising the most valuable extracts from the medical journals of the day, may be obtained. *Cheap enough* in all conscience; and a striking and rather humiliating instance of the advantages we derive from the labours of trans-atlantic authors, without returning a just *quid pro quo*. This arises from the absence of an international copy-right. Such being the case, however, American readers are altogether inexcusable for neglecting to avail themselves of the advantage presented.

Part Third.

EXCERPTA.

1.—*On the Mutual Relations existing between Physiology and Pathology, Chemistry and Physics, and the Methods of Research pursued in these sciences.* By Baron Liebig.

(*Continued from our March No.*)

EXAMINATIONS IN ILLUSTRATION OF THE FOREGOING GENERAL REMARKS; THEORIES OF CONTAGION; PARASITES; CHEMICAL THEORY OF THE PUTREFACTIVE AND FERMENTATIVE PROCESSES.

The power of the Sunbeams to attract Water.

In certain mines, during the summer months, the flow of water into the galleries and shafts has rendered the continued working of them impracticable.—This the naturalist explained by ascribing to the sunbeams the faculty of attracting water, the heat of the sun drying the soil, and thus creating a vacuum, which is filled up from depths beneath by capillary attraction. Certainly a connexion exists between the heat of the sun and the water in the mines, but it is simply that the streams which work the pumps employed to keep the mines dry, cease in summer to flow, and the mines consequently fill.

In the same way, probably, the drinking of brandy may be connected with self-combustion, since drunken people are most commonly those who fall into the fire.

Boerhaave's notion respecting the origin of Alkali in the Ashes of Plants.

The false distinction of living and dead, or vital and material forces, which at this moment separates physiology and chemistry like an impassable gulf, arises only from want of correct, and the prevalence of erroneous observations. The views entertained so late as the eighteenth century respecting the existence of alkalies in plants are precisely similar to those held at the present day by pathologists on the growth of crystals, and the nutrition and increase of organic beings. Boerhaave taught, that the alkali found in the ashes of a plant was derived neither from the sap nor any part of the plant, but was simply a production of combustion. He demonstrated to his pupils, that decayed wood yields no alkali, and that the alkali produced in the combustion of plants no more forms a constituent of the plant, than the glass which many plants yield upon incineration.

False Comparison between the organic force and the force of cohesion operating in Crystallization.

"There is," says Henle, (Rational Pathology, vol. i. p. 101,) "a limit to the growth of crystals as to that of cells, even under the most favorable conditions, although that growth is less circumscribed in the former, than in the latter. Crystals conjoin, like cells, into bodies, which, by their arborescent appearance, remind us of the elementary parts of plants. Both inanimate and living bodies oppose a certain determinate resistance to external influences; under certain circumstances they adjust themselves to such influences, or altogether lose their form. But the most remarkable point of resemblance between crystals and organic beings is to be found in the deportment they manifest after being mutilated by external influences. Crystals, like organic bodies, possess the property of regenerating lost parts, more or less completely In both, the power which formed the body continues to operate independent of the matter, the loss of which it survives and replaces. If a crystal, an angle of which has been broken off, is placed in a fluid whence it can derive homogeneous matter, it grows, indeed, on all sides, but principally, and more rapidly, in the direction of the mutilated parts, so as, in the first place, to recover its regular shape; in precisely the same manner as the food partaken of by a mutilated animal is employed to reproduce the lost parts, so far as the typical laws will permit.

Now although it is perfectly true that the increase of an organic body is effected by a force of attraction, yet there is not the slightest similarity even in the external appearance between the increase of a crystal and the growth of an animal organism. The shape of the reproduced membrane is in no way dependent upon the physical form of the gelantine atoms. A crystal of alum, however large, is a mere aggregation of smaller molecules having precisely the same form. An organic cell is a perfect and distinct whole in itself, not an aggregation of smaller cells.

Explanation.

There is no limit to the growth of a crystal as there is to that of a cell. The enlargement of a crystal is not like the growth of an organized body, effected by a cause operating from within, but simply by the attraction of surfaces. This force of attraction operates at all points of the surface of the crystal; the particles beneath the surface take no part in its increase, and they may be removed without diminishing the power of increase possessed by the surface. The new faces produced by truncating the angles of a crystal, exert no stronger attraction upon the molecules of the surrounding medium than the other surfaces, and their increase proceeds *pari passu* with that of the latter. By striking off an angle from an octahedron, we obtain a cubic plane of the crystal limited by four convergent octahedral faces. In the crystallizing fluid the crystal increases in three dimensions, the four planes grow longer and broader, and it is simply and exclusively in consequence of their elongation and convergency that the original angle is restored. Nay, this restoration will take place even if the cubic surface be encrusted with a varnish, and thus altogether precluded from participating in the increase of the other planes. But a crystal of alum, from which one side had been struck off, does not increase in a greater ratio in the direction of the removed, than that of any of the other sides, consequently the original form is not restored, precisely because the force of attraction exerted by a portion of one of the faces is not greater than that of an equal portion of any of the other five faces.

Crystals increase in a saturated solution, in a preponderating proportion, only in one direction—namely, that towards the bottom of the vessel, that face being always in contact with that portion of the fluid which has the greatest specific gravity, and is most highly charged with crystallizing matter. There are certain conditions in which whilst the crystal is increasing its lowest surface, it is losing matter from the upper, as, for instance, when there is a difference in the temperature of the upper and lower portion of the mother liquor.

Theory of Parasitic Plants and Animals compared with the Chemical Theory of Contagion, Miasm and Putrefaction.

Some of the most serious errors and mistakes in medicine arise from the habit of pathologists of considering two things which occur frequently in conjunction, as standing in the mutual relation of cause and effect. Thus with respect to correct conceptions of the nature of diseases, and the judicious choice of remedies for their treatment, there is nothing more deficient of a scientific basis, or more mischievous, than the hypothesis which regards miasms and contagions as animated beings, parasitic fungi, or infusorial animalculæ, developing, propagating, and multiplying themselves in the body, thus creating diseases, and causing death.

A glance at the principles upon which the parasite and chemical theories respectively rest, will suffice to exhibit the relative merits of the two.

In endeavouring to establish, by the following series of facts, a connexion between certain processes which are observed in the living organism and the phenomena of inorganic matter, I wish it to be understood that I do so much less from a desire to advance any new view respecting the nature of contagions and miasms, fermentation and putrefaction, than for the purpose of drawing attention of the natural philosopher to an universal cause, hitherto little regarded, which coöperates in all changes of form and condition of matter, and all processes of composition and decomposition. And if it can be demonstrated that this cause exercises a very decided and perceptible influence upon the manifestation and direction of the forces of cohesion and affinity, it will be the less questionable that it also bears a part in the operation and effects of the vital force, since the latter belongs to the same class as chemical forces, inasmuch as both act only upon direct contact, or, at least, only at inappreciable distances.

Influence of Mechanical Motion upon Crystallization.

It is a fact universally known, that water becomes solid, or crystallizes, at all temperatures below 32° Fahr., and that during the act of freezing, the temperature remains stationary at 32°. Yet, nevertheless, water may be cooled down to 5° Fahr., without congealing, if it be maintained during the process perfectly at rest. When cooled below 32°, the slightest agitation or disturbance of the water suffices to produce instant coagulation.

Influence of Mechanical Motion upon the Crystallization of Salts from Solutions.

Many hot, saturated, saline solutions comport themselves in a manner precisely analogous to this. When permitted to cool in a state of perfect rest, they deposit no crystals; no separation of the water from the dissolved salt takes place. But the slighest agitation, even a particle of dust, or a grain of sand, thrown into the fluid in such cases, instantly induces crystallization, and when once commenced, it proceeds throughout the whole mass.

Influence of Mechanical Motion upon Sulphuret of Mercury, Iodide of Mercury, and on Iron.

Continued agitation or friction transforms the black amorphous sulphuret of mercury into the red crystalline sulphuret, (cinnabar.) The simple process of hammering imparts a crystalline structure to the irregularly arranged molecules of crude iron. The lemon-coloured iodide of mercury acquires a new form of crystallization and a scarlet colour when subjected to friction.

From these facts it follows that mechanical motion exercises a decided influence upon the action and manifestation of the force which governs the state and condition of the bodies that this motion once imparted, is propagated to the ultimate molecules of bodies. In order to form crystals, these molecules must turn towards the direction in which the force of attraction is strongest; and it is perfectly clear, that in fluids, as well as in solids, the atoms may be set in motion by a stroke, or by friction; in one word, by mechanical forces in general.

But it is not upon the force of cohesion only that mechanical motion exerts an influence, it is also seen to influence chemical affinity.

Influence of Mechanical Motion on Chemical Affinity.

Tartaric acid fails to produce a precipitate in dilute solutions of chloride of potassium; but simply agitating the vessel, or rubbing its sides with a glass rod, causes the immediate deposition of crystals of bitartrate of potass. The fulminates of silver and mercury, the fulminating silver of BERTHOLLET, pecoinitrate of lead, and many other compounds, explode with violence when struck or subjected to friction. It is obvious that in these cases, the blow, the friction, or in other words the mechanical motion, is communicated to the atoms of these compounds, changing the direction of their mutual attraction—thus giving rise to the formation of new products. Fulminate of silver contains the elements of cyanic acid. A blow or friction applied to this compound induces a new mode of arrangement of its elements; part of its carbon is evolved, in combination with the whole of its oxygen, as carbonic acid, and, at the same time, nitrogen is set free; and this sudden transition to the gaseous state causes the explosion.

By mere mechanical motion, the colourless fluid, styrol, becomes solid and hard.—(SULLIVAN.)

The Action of Heat similar to that of Mechanical Force.

Many substances are decomposed by heat, which acts in these cases precisely the same as a mechanical force. These effects of heat are similar to those of a wedge driven in between the atoms. If the chemical force holding the atoms together be less than the force tending to sever them, decomposition ensues. Thus oxide of mercury is resolved by heat into oxygen gas and metallic mercury.

Heat acts in precisely the same manner on substances containing more than two elements. The fulminating bodies above mentioned explode when exposed to a certain degree of heat. The effect of the heat is to disturb the original mode of arrangement of their atoms, and consequently the equilibrium of their mutual attraction. They now assume new relative positions in the direction of the altered attraction. The formation of the new produdts depends upon the establishment of a new state of equilibrium. The new products at the same temperature undergo no further alteration, but, in many cases, by raising again the temperature still higher, a new disturbance occurs, and consequently a new mode of molecular arrangement ensues. Thus a faint-red heat resolves acetic acid into carbonic acid and acetone; the former containing two-thirds of the oxygen, and the latter all the hydrogen, of the acetic acid. Acetone, exposed to a higher temperature, is resolved into a compound of carbon, containing all the oxygen of the acetone, and into an oily carburet of hydrogen.

Styrol, at a temperature of 392°, loses its fluidity, and acquires solidity and hardness, being from a limpid liquid substance converted into one resembling the most beautiful crystal glass.

Influence of the State of Chemical Action of one substance upon the power of another to enter into Combination.

Platinum does not decompose nitric acid, nor does this acid alone either oxidize or dissolve platinum; but an alloy of platinum, with silver, dissolves readily in nitric acid. Upon boiling copper in dilute sulphuric acid, no decomposition of the water occurs; but certain alloys of zinc, copper, and nickel, dissolve readily in dilute sulphuric acid, with evolution of hydrogen. There are, on the other hand, also, alloys of these three metals, in certain proportions, which dilute sulphuric acid fails to dissolve; but if, in these cases, the slightest trace of nitric acid be mixed with the sulphuric acid, oxidation will commence and afterwards continue without the further coöperation of the nitric acid.

The solution of the platinum in the former, and that of the copper in the latter example, is contrary to the supposed laws of electricity governed affinity.—Neither heat or any other cause tending to increase the affinity between the metal and oxygen, or the oxide and acid, have any share in the process of combination and solution.

Influence of the State of Chemical Action of one substance upon the susceptibility to Decomposition of another.

Peroxide of lead, or oxide of silver, brought into contact with peroxide of hydrogen, effects, like many other solid bodies, a speedy decomposition of the latter, which is resolved into water and oxygen gas. The action of these oxides upon peroxide of hyhrogen is most energetic, and the process is attended with lively effervescence. But what is very remarkably in these cases is, that the two oxides also suffer a similar decomposition, the oxide of silver being resolved into oxygen gas and metalic silver, the peroxide of lead into oxygen and oxide of lead. Both these oxides, under these circumstances, comport themselves as they would under the influence of a faint-red heat.

From these facts, it follows incontrovertibly, that the state of combination, or decomposition of a body, the state of motion, or change of place and position of its molecules, exercises a positive influence upon the contigious molecules of other compounds, causing them to pass into an analogous state, and imparts to them the power of entering into combinations, which, in themselves, and apart from such an influence, they do not possess. The decomposition of a substance thus caused by another progress of decomposition, presupposes, of course, that the resistance of the force maintaining its atoms in combination is less than the force operating to severe them.

The same Influence in Organic Bodies.

This property of bodies, during the progress of decomposition or combination, to induce, in other substances (whether similar or dissimilar) in contact with them, the same change of form, condition, and properties, belongs to organic matter in a still higher degree than to inorganic. Thus, for example, decaying wood, in contact with fresh wood, gradually imparts to the latter, under the same conditions, its own state of decay.

Deportment of Urea and Hippuric Acid in Urine.

If fresh urine be kept perfectly protected from the access of oxygen; the urea and hippuric acid contained in it undergo no alteration; but upon exposure to the air, another substance present in the urine absorbs oxygen, and consequently suffers a change of form and properties, which is subsequently transmitted to the urea, hippuric acid. The urea is resolved into carbonic acid and ammonia; the hippuric acid disappears, and is replaced by benzoic acid.

Influence of the Decay of Wood upon the Oxidation of Hydrogen Gas.

Decaying wood absorbs oxygen from the air, and yields to the latter an equal volume of carbonic acid. If hydrogen be added to the air, in contact with the decaying wood, this hydrogen acquires the property of combining with the oxygen of the air at the common temperature. Under the same circumstances, alcoholic vapour absorbs oxygen, and is converted into acetic acid.

The Fibrin of Blood, and Yeast, comport themselves alike, with Peroxide of Hydrogen.

Fresh blood fibrin manifests the same deportment in the air as damp wood —viz., it passes into a state of decomposition. If, in this condition, it is brought into contact with peroxide of hydrogen, it resolves the latter instantaneously into oxygen and hydrogen, the process being attended with effervescence. But if the blood fibrin is previously heated with water to the boiling point, it loses this property of accelerating the decomposition of peroxide of hydrogen. Beer yeast, when brought into contact with peroxide of hydrogen, resolves the latter instantaneously into its constituent elements: if previously heated to ebullition, it loses this property. (Schlossberger.)

Deportment of Complex Organic Atoms.

The complex organic atoms possess these properties in a peculiarly high degree; in fact, the greater the number of individual elements and atoms that have combined into a group of atoms, having definite properties, and the more manifold and varied the directions of their mutual attraction, the less, in the same proportion, must be the force that maintains the connexion between every two or three of the ultimate molecules of the group. They oppose to the causes tending to alter their form, condition, and properties, such as heat or chemical affinities, a far slighter resistance, and undergo modifications and decomposition far more readily than substances of more simple composition.

Putrefaction.

The sulphurous and nitrogenous constitutuents of plants and animals belong to the most complex of organic atoms. The instant that these are separated from the organism, and brought into contact with the air, decomposition commences, and continues, although the air be subsequently excluded. The colourless sections of a potatoe, a turnip, or an apple, acquire speedily a brown tint in the air. In all these substances, the presence of a certain amount of water, through which mobility is imparted to their minutest molecule, forms an indispensable condition to the change of form and properties, and resolution into new products, which the most transient contact with the air induces, and which continue incessantly until the original body has altogether disappeared. This process has been designated by the term " putrefaction."

Affinity is not the Cause of Putrefaction.

Experience has abundantly proved that many substances alter their properties immediately when brought into contact with such putrefying, sulphurous, and nitrogenous matters. They undergo the same decomposition with those matters, and their liberated elements combine to form new products, into the composition of which, in most cases, none of the elements of the putrefying matter enters. These facts, taken in connexion prove clearly that the decomposition of the substance thus acted upon by the putrefying matter, is not the consequence of a manifestation of affinity, precisely because the idea of affinity is inseparable from that of combination.

Decomposition of Amygdalin, and Asparagin, through Putrefying Substances.

In contact with the nitrogenous constituent of germinating barley, asparagin is resolved into succinic acid and ammonia. Amygdalin, in contact with the nitrogenous constituent of sweet almonds, is resolved into hydrocyanic acid, oil of bitter almonds, and sugar ; the bitter salicin into saligenin and sugar.

Conversion of Starch into Sugar.

Potatoes and the flour of the cereals contain no sugar; but the simple contact with water suffices to effect the conversion of the starch into sugar, in consequence of the change which it induces in the sulphurous and nitrogenous constituent of these vegetables.

Conversion of Milk and Grape-sugar into Lactic Acid by Animal Membrane.

Animal membrane, moistered with water, effects the transformation of milk and grape-sugar into lactic acid; the gluten of the cereal, animal cheese, and extract of malt, possess the same property.

Fermentation and Fermentability.

The property of organic bodies to pass when brought into contact with putrefying substances, into the same state of decomposition, or dissolution, as the latter, is termed " fermentability." The process of this dissolution which organic bodies then undergo, is termed " fermentation," or, more correctly, " putrefactive fermentation ;" the putrefying substance which induces this process of dissolution is designated by the term " ferment," or " fermentative agent."

Influence of the various stages of Putrefaction, or Dissolution, through which the ferment may successively pass, upon the fermentation of the organic body in contact with it.

Now if it be true that the changes of form, condition, and properties, which the fermenting substance has to undergo, are determined by the alterations and modifications occurring in the ferment; if the new order of arrangement of the atoms of the fermenting body be dependent upon the mode and direction in which the dissevered molecules of the ferment re-arrange themselves; in short, if the fermenting body comport itself like part and parcel of the ferment—then it is obvious that the mode of decomposition of the one must alter with that of the other, and that the products yielded by the fermenting substance must vary with the different stages of dissolution through which the ferment passes.

The correctness of the conclusion is demonstrated by innumerable facts.

Milk of Almonds and Sugar.

For instance, if milk of almonds, which, in its fresh state, exercises no action upon sugar, be kept for some time, it will altogether lose the property of acting upon amygdalin, and acquire, instead, a fermentative action upon sugar: this latter substance, when added to it in this state, will commence to ferment, and resolve itself into alcohol and carbonic acid. Milk of almonds that has been kept for a still longer period of time, effects the conversion of sugar into lactic acid. Infusion of malt presents very similar properties: fresh-prepared, it converts starch into sugar, but in about eight days it loses this action, and effects now the fermentation of sugar.

Casein in Sugar.

In the first stage of putrefaction, animal casein effects the conversion of milk and grape-sugar into lactic acid; and at a higher temperature, the transmutation of grape sugar into alcohol and carbonic acid. In the last stage of dissolution, and the formation of free acid being prevented by addition of an alkaline base, casein resolves the sugar atom into carbonic acid, butyric acid, and hydrogen gas.

Animal Membrane and Sugar.

Animal membrane comports itself in precisely a similar manner; at first, it effects the conversion of starch into sugar, subsequently, the transmutation of sugar into lactic acid; and finally the decomposition of sugar into carbonic acid and alcohol.

Influence of Temperature upon Fermentation.

The same sugar which, in beet-root juice fermenting at the common temperature, is resolved into alcohol and carbonic acid, yields, upon elevation of the temperature, (no addition whatever being made to the fermenting juice,) mannite, lactic acid, gum, carbonic acid and hydrogen gas.

Production of Fousel Oil from Sugar.

The same sugar under altered conditions of fermentation, yields butyric acid, and is, in the fermenting molasses of beet-root sugar, resolved into water, carbonic acid, and hydrated oxide of amyl, (Fousel oil.)

The resolution of Sugar into Alcohol and Carbonic Acid, similar to that of Acetic Acid into the same products, effected by the action of Heat.

Milk-sugar and grape-sugar contain the same elements, and in the same relative proportion as lactic acid. The products which grape-sugar yields upon fermentation contain exactly the elements of the sugar atom. The decomposition of this latter consists in a simple disseverance, or transposition of its constituent molecular elements, exactly as is the case with the decomposition of acetic acid under the influence of a high temperature; the carbonic acid containing two-thirds of the oxygen, and the alcohol the whole of the hydrogen, of the sugar atom.

All highly Complex Organic Atoms possess the property of causing Fermentation.

The fact that the property to induce putrefaction and fermentation belongs alike to substances of the most varied and different composition, and that blood, flesh, (muscular fibre,) cheese, (casein,) membranes, cells, saliva, extract of malt, milk of almonds, &c., acquire this property the instant that, through the chemical action of oxygen, a disturbance of the state of equilibrium in the mutual attraction of their elements is created, seems to remove all doubt with respect to the true cause of all their phenomena.

Cause of change of form, and properties, of substance.

Changes of place or position of the molecules of a great many (organic) compounds, and their resolution or transposition into new products, may be occasioned by chemical action, by heat, or by electricity ; but they may likewise be induced by the transmission of a state of motion, or, in other words, by contact with substances of which the molecules are in a state of actual transplacement or transposition.

Propagation of Decomposition from one Molecule to another.

When, from an external cause,—contact with oxygen for instance,—the state of equilibrium in the mutual attraction of the elements of one of these compound organic atoms is disturbed, a new state of equilibrium is established.—The motion imparted to the first molecule is transmitted to the second, third, &c., molecule of the same element, to all the molecules of the other elements ; in short, to all other organic substances in contact with it, whenever the power that maintains their elements in the original form and condition is less than that operating upon them with an opposite tendency. Want of power to maintain the original state is want of power to resist external influences. Every substance capable of increasing this power of resistance impedes or prevents putrefaction and fermentation; in most instances by entering into chemical combination with the body susceptible of either of these conditions. Every new accession of attractive forces strengthens the power of maintaining the original mode of molecular arrangement. To the force which strives to maintain the original form and condition of the one body, is added, in the second body, with which this combines, a new force of attraction, that must be overcome before the elements of the former can be made to change their place or position.

Antiseptic substances.

Amongst the substances which counteract putrefaction and fermentation, we have to enumerate, in the first place, sulphurous acid and arsenious acid, and besides these, mineral acids, many of the metallic salts, empyreumatic substances, volatile oils, alcohol, and common salt. The action which these substances exercise upon putrefying bodies is of very dissimilar nature. Alcohol or common salt in certain proportions, for instance, stay all processes of fermentation and putrefaction simply by withdrawing from the putrefying or fermenting substance, a certain amount of water, and thus one of the principal and most essential conditions of its decomposition ; whilst sulphurous acid, which is capable of entering into combination with organic matter in general, and consequently, likewise, with all matters susceptible of putrefaction, prevents this and other fermenting processes by combining with the putrefying or fermenting substance.

Deportment of Arsenious Acid with Animal Membranes.

Arsenious acid does not exert the slightest influence upon the fermentation of sugar in vegetable juices, nor upon the action of yeast upon sugar, (Schlossberger ;) nor does it arrest the putrefaction of blood ; but its action upon membranes and membraneous tissues is unquestionable. Whilst a piece of bladder or membrane, in contact with water, undergoes complete decomposition and dissolution in the course of about six weeks, generating products of a most offensive nature, another piece of bladder, or membrane, in contact with water, containing an admixture of arsenious acid, remains perfectly unaltered and inodorous. The reason of their preservation is this: that the gelatinous tissues enter into chemical combination with the arsenious acid, forming a compound possesed of properties similar to those of the compound which skin forms with tannic acid. The knowledge of the cause of the origin and propagation of putrefaction in organic atoms renders the problem of the nature of many forms of contagion and miasm capable of a simple solution. This problem may be reduced to the following terms;—

Propagation of the process of Putrefaction, or Fermentation, in the living Animal Body.

Are there facts on record, demonstrating that the state of decomposition or putrefaction of a substance may be transmitted equally to parts or constituents of the living animal organism, and that contact with putrescent substances may induce in these parts or constituents of the living organism a state of decomposition, analogous to that of the putrescent body? This question must be answered unhesitatingly in the affirmative.

Facts.

It is a fact that bodies, in anatomical theatres, frequently pass into a state of dissolution, which may be transmitted to the blood in the living body; the slightest puncture with a lancet or knife that has been used in the dissection of such bodies is sufficient to induce a dangerous state of disease, which in many instances, terminates fatally.

Magendie observed, that "the application of putrescent blood, cerebral substance, bile, pus, &c., to fresh wounds, occasions vomiting, lassitude, and finally, after a longer or shorter period of time, death." These observations of Magendie remain uncontradicted.

It is a fact, likewise, that the use of many articles of food, such as meat, ham, sausages, in certain states of decomposition, occasion the most dangerous diseases, and in many instances even death.

Definition of the term "Products of Disease."

These facts demonstrate, that an animal substance in course of decomposition may occasion disease in the body of a healthy individual, and that its own state of dissolution may be transmitted to parts or constituents of the living organism. Now, as the term "products of disease" means simply parts or constituents of the living body, in a state of change of form, condition, and properties, it is obvious, that as long as this state of alteration is proceeding, the disease may be transmitted, through these products, to a second or third individual.

Antiseptic substances impede, or prevent the propagation of Contagion and Miasms.

If we take, moreover, into consideration, that all those substances or causes which tend to destroy the transmissibility of contagion and miasms impede or arrest likewise all processes of putrefaction and fermentation; that, as every day's experience shows, the process of disease in malignant suppurating wounds is totally modified under the influence of empyreumatic substances, which counteract putrefaction most energetically, such as wood, vinegar, (pyroligneous acid,) for instance; and finally, that in a great many contageous diseases, and more particularly in typhus, ammonia, the almost constant product of putrefactive processes, is found, either in the free state, or in combination with other substances, in the atmosphere surrounding the patient, in the urine, and in the excrement, (as phosphate of magnesia and ammonia)—it seems impossible to entertain the slightest doubt concerning the cause of the origin and propagation of a great many contageous diseases.

Putrefactive Processes as causes of Contagious Diseases.

Finally, universal experience demonstrates that "the origin of epidemic disease is frequently traceable to the putrefaction of large quantities of animal and vegetable matter, and that miasmatic diseases are epidemic in localities where decomposition of organic matter is constantly proceeding—in damp and marshy districts, for instance, and likewise after inundations; also in places where a large number of people are crowded together without sufficient ventilation or change of air—in ships, for instance, and in jails and besieged towns." (*Henle, Pathological Investigations*, page 52.) And at page 57 of the same work: "But the invasion of epidemic diseases may never be predicted with more positive certainty than when a marshy, flat locality has been dried up by a protracted heat, or when extensive inundations are succeeded by intense heat."

Conclusion from the preceding Facts and Observations.

The preceding facts and observations fully justify the conclusion, that in all cases where the invasion of a disease is preceded by a putrefactive process, or where the disease may be transmitted and propagated by solid, liquid, or gaseous products, eliminated from the body of the patient, and where no more immediate cause is assignable, the substance or matter in course and progress of decomposition is to be looked upon as the immediate cause of the disease in question.

Conditions of Infections.

The transmission of a disease from one individual to another requires that the body of the second individual contain matter which opposes to the cause of alteration of form and condition operating upon it, no resistance, either in itself or through the vital force of the organism. If this matter forms an essential constituent of the body, the disease is transmissible to all individuals, but if it form only an accidental constituent, only such organisms will be liable to infection as contain this matter in the requisite quantity and condition. The disease itself, and its symptoms, consist in the endeavour of the organism to destroy and remove this matter, and to establish a state of equilibrium between the inherent forces that sway and regulate its normal functions, and an extranuous force which tends to alter them.

Invitation to Physiologists, and Pathologists, to examine the Chemical Theory of Contagion, and Miasm.

Practical medicine will soon decide whether this view is correct or not; it will be seen whether there exist actual relations between the deportment which arsenious acid exhibits to animal membranes severed from the organism, and the operation of this acid in certain fevers—between the deportment exhibited by mercurial preparations to dead animal matter, and the operation of such preparations in contagious diseases. If this so-called chemical view, deduced from a careful study of the processes of putrefaction of both single and mixed substances, and of all the maturer causes tending to modify, to impede or arrest, or to accelerate, such processes, and from comparison of these with analogous processes occuring in the organism, fails to enlarge and enrich the experience of the physician, to advance and correct his insight into the nature of diseases, and to establish the choice and application of remedial agents upon a more solid and scientific basis than is the case at present—then it is, indeed, not worth while to defend and maintain it. The great obstacle to the universal admission of this theory, seems to be its simplicity. Whilst every physician or physiologist admits bad food, deficient supply of fresh air, the continued use of salted food, &c., as causes of the most striking modifications of the vital processes, and unhesitatingly regards slight, and in many instances hardly appreciable, changes of temperature as the cause of inflammation, fever and death, one of the most active and powerful causes of modifications of form and condition is denied all share and co-operation in the organic process of life; and a theory, based upon a firmly interlinked chain of numerous and most evident facts, is even refused examination, although nothing, save its own simplicity and facility of comprehension, can be advanced against it. It is precisely in this that the true characteristic of the physical method of investigation consists. Although every pathologist and physiologist is fully convinced that no organic process can be explained without the co-operation of chemical and physical forces, yet, up to the present time, every explanation that admits the chemical and physical forces to a share of co-operation in the processes of life has been rejected by physicians.

Comparison of the Chemical Theory with the Parasite Theory.

If we examine the basis of the parasite theory of diseases, and compare the evidence adduced for its support with the foundation of the chemical theory, it will seem scarcely credible that intelligent men, shrewed observers, moreover, can adopt and defend opinions which are refuted by every day's experience. The foundation of the parasite theory may be referred to two principal facts—1st, the nature and mode of propagation of the itch; 2ndly, a disease to which the silk worm is subject, which is designated by the term *muscardine.*

The Itch.

"The itch is a cutaneous inflammation occasioned by irritation of a certain species of the genus mite, (acarus scabiei, sarcoptes humanus,) which lives upon the skin, or, more correctly speaking, burrows within it. The transmission of the itch requires continuous and protracted vicinity or contact, more particularly during the night, since the itch-mite belongs to the class of nocturnal predatory insects. That the itch-mite forms really and actually the contagion of the itch is demonstrated by the following facts:—*a* Inoculation with the pus of scabious pustules does not engender the itch, nor is this disease engendered by placing the scales of scabious pustules upon the bare arm.—*b* The itch is cured by the abrasion of the mites with brickdust, and is transmissible only by impregnated female mites. But by this kind of propagation the itch may become a universal or epidemic disease, which may spread *ad infinitum.* The itch is a chronic disease, and does not disappear spontaneously."—Henle.

The Itch propagated by Animalculæ.

The contagion of the itch is, therefore, an animal provided with an apparatus for feeding, and it lays eggs; it is termed "a fixed contagion," because it is unable to fly, and its eggs are not carried off and disseminated by the atmosphere. If it be once demonstrated that the itch is propagated by animalculæ, then it requires neither the chemical nor any other theory to explain the communication and dissemination of this disease, and it is perfectly self-evident that all those distempers which are of a similar nature with the itch, belong to the same class with the latter—in all casses where observation reveals analogous or similar causes of transmission, and propagation.

Contagious diseases not propagated by Animalculæ.

Now if we require for the results of the examinations into the causes of other contagious diseases, we find that the most attentive and conscientious observers have failed to discover animalculæ, or any other kind of organized beings, to which the power of propagation might be ascribed in the contagion of the small-pox, plague, syphilis, scarlatina, measles, typhus, yellow fever, dysentery, milzbrand, (a disease of the spleen occurring in cattle) hydrophobia.

Parasites in the body of Animals of the higher order.

On the other hand, it has been observed that a great many parasitic animalculæ are generated and propagated only in the body, or under the skin, of animals of the higher order, occasioning, in many instances, disease, and even death. It is perfectly obvious that the itch belongs to this class of disorders, since the greater or lesser size of the animal can make no difference.

There are, therefore, diseases that are occasioned by parasitic animalculæ, developed within the body of other animals, and thriving at the cost of the constituents of the latter. These diseases cannot be confounded with others, however much they may resemble them in external appearance and symptoms. Further examination and research may demonstrate that some other contagious disorders besides the itch belong to the class of diseases generated by parasites; however until this fact shall be fully proved and established, the strict rules of natural investigation demand their exclusion from this class, and it is the province and task of science to discover and ascertain the special and peculiar causes that produce them; the simple inquiry for these causes leads the way to their discovery.

It was principally upon the position of like effects implying like causes, that pathologists endeavoured to establish the theory that infection in contagious diseases is occasioned by organized beings, and that the itch is to be regarded as the type of contagious diseases, (Henle, Zeitschrift, vol. ii. p. 305.) This is the very principle and mode of reasoning which has for centuries impeded the progress of the natural sciences, and leads, even at the present day, to so many errors.

The origin and mode of propagation of the purely miasmatic diseases have hitherto remained inaccessible to investigation, and accordingly neither the parasite (?) nor the chemical theory has been applied to explain their nature.

The parasite theory has designated muscardine as the type of those mixed miasmatic and contagious (miasmatico-contagious) diseases which are generated both by matter admixed with, and disseminated by, the air, and by matter derived from, or transmitted by, the diseased body.

Muscardine.

Muscardine is a distemper of the silkworm, occasioned by a species of fungus. The germs of this fungus introduced into the body of the caterpillar, grow inward, at the expense of the latter. After the death of the caterpillar they penetrate the integuments, when the surface soon appears covered with a forest of fungi, which gradually dry up and become reduced to powder. This powder detaches itself subsequently, upon the slightest motion, from the body upon which it rests, and ascends into the air, by which it is then disseminated. Good food, and perfect health and vigor, increase the liability to infection; and then, in a colony of silkworms, the diseased grubs are the best; (and thus, in a colony af silkworms, the muscardine affects the finest and largest grubs most.)

Parasites in Animals and Plants.

Similar parasites have been observed in diseased fish, in the eggs of birds, and in a great number of vegetable infusions. These observations, taken in connexiou, clearly establish a series of facts with respect to the animal organism, and the same are very frequently observed in the vegetable kingdom—viz., disease and death by parasites which live exclusively at the cost of the constituents of other plants. But between these facts and the generation and propagation of miasmatico-contagious diseases there exists no connexion whatever; and if it be allowable to designate a fungus or its pores by the term contagion, it is obvious that, as the size of the fungus can make no difference in this respect, there exists contagions from six to eight inches in length, this being the size reached by the fungus, Sphæria Robertii. which is developed in the body of a grub found in New Zeland, occasioning the death of the insect.

The Parasite Theory based upon an erronious notion of Putrefaction.

A totally erronious notion of the cause of fermentation and putrefaction has hitherto formed the main support of the parasite theory of contagion. The advocates of this theory regard putrefaction as a decomposition of organic beings, induced by infusoria and fungi, and look upon every putrescent substance as a kind of hot-bed for the breed of infusoria, or a plantation of fungi; so that, where putrefying organic substances cover a considerable extent of ground, the whole surrounding atmosphere must be filled with germs of these organic beings, which germs, according to the advocates of the parasite theory, are the miasm or the causes of contagious diseases.

Fungi and Infusoria are not the Cause of Putrefaction.

The preceding paragraph shows that the intimate connexion existing between putrefaction, contagions, and miasms, has not escaped the advocates of the parasite theory; it is simply in their mode of considering and apprehending the nature of their connexion and the relation of mutual dependency existing between these phenomena, that the suggestions of the parasite theory differ from those of the chemical theory. The connexion of cause and effect which, according to the former, exist between infusoria or fungi, and processes of fermentation and putrefaction, and miasm and contagious diseases, would indeed be fully established if it were clearly demonstrated that infusoria or fungi actually occasion putrefaction or fermentation, and that, by means of their nutritive and respiratory processes, sugar is resolved into equal volumes of carbonic acid gas and vapour as alcohol; that urea is thereby converted into carbonate of ammonia, salicin into sugar and saligenin, protosulphate of iron into crystallized sulphuret of iron, gypsum into sulphuret of calcium, sulphate of soda into sulphuret of sodium, blue indigo into white indigo, starch into sugar, sugar into lactic acid, mannite and gum into fousel oil, acetic acid, butyric acid, and hydrogen gas, amygdaline into hydrocyanic acid, bitter almond oil, and sugar.

The following reflections will suffice to expose the utter inadmissibility of this view:—

Antagonism of the Vital Process and Putrefaction.

The constituents of vegetable and animal structures have been formed under the influence of a cause of change of form and condition operating within vegetable and animal organisms. The *vital force* determines the direction of the mutual attraction of their component atoms, and opposes and annihilates the influence of the force of cohesion, of heat, of electricity,—in short, of all those causes which, out of the organism, counteract and prevent the association of the atoms into compounds of a higher order. In compounds of so complex a nature as organic atoms, these forces induce changes of form and condition after death, when the vital force no longer opposes their operation. The same leaf, and the same grape, which possesses the property of yielding pure oxygen gas to the atmosphere, succumbs to the chemical action of the oxygen from the very instant that it is severed from the organism, and comes into contact with the oxygen of the atmosphere. No organism, no part of a plant or animal, is, after the extinction of the vital force, able to resist the chemical action which air and moisture exercise upon it; its component elements become subject to the absolute and unlimited sway of the chemical force. Fermentation and putrefaction are the first stages of resolution into compounds of a less complex order; finally, the elements of organic beings, incessantly acted upon by the inorganic forces, reassume their original and most simple forms, in which they may again serve as sustenance to new generations.

Fungi and Infusoria subject to Putrefaction, Fermentation, and Dissolution.

Fungi and infusoria are organic beings; their constituents are of as complex a nature as those of the higher order of plants and animals; we observe in their bodies after death the same phenomena which attended the decay and annihilation of all organic beings—viz., putrefaction, fermentation, and dissolution. How, then, is it possible to regard fungi and infusoria as the causes of these processes, when we see these pretended causes themselves fall a prey to prutrefaction, fermentation, and dissolution, leaving nothing behind save their inorganic skeleton!

Fungi and Infusoria ATTEND *the Processes of Putrefaction but* DO NOT OCCASION *them.*

It is an indisputable fact, that fungi and infusoria are observed in a great many putrefying and decaying substances; but surely the frequency of their attendance upon the processes of putrefaction and decay is no reason that they should be regarded as the *causes* of these processes. Nature has ordained that fungi and infusoria should feed and thrive upon organic atoms which have ceased to be constituents of living organisms. They appear, in most cases, only after the actual putrefaction has commenced, or has even merged already into the process of final dissolution. It is unquestionable, that all putrefactive processes and their products are modified by the presence of these beings, since, through the operation of their nutritive and respiratory processes, they accelerate the final dissolution, and limit, thus, to a far shorter period of time than would otherwise be the case, its baneful influence upon surrounding objects.

Fungi and Infusoria accelerate the Process of Putrefaction and Dissolution.

The conversion of the elements of organic beings into carbonic acid and carbonate of ammonia, terminates and accomplishes all putrefactive processes; it is perfectly obvious, therefore, that the time required for this consumation must be most materially shortened, if the putrescent body becomes inhabited by a colony of infusoria, and millions of these animalcules busily engage resolving its constituent elements into their ultimate products, through the operation of their digestive and respiratory processes.

The accelerating influence which Infusoria and Fungi exercise upon Putrefactive Processes render them Checks and Antagonists to such Processes.

The important part which Nature has thus assigned to the infusoria—viz., antagonism to every form of contagion and miasm, can no longer be doubted or mistaken, since the most incontrovertible facts have demonstrated that the green and red species of these beings are, during the processes of their existence and propagation, sources of the purest oxygen.

Fungi perform a similar part in putrefactive processes. By consuming the real excitors of putrefaction in vegetables—viz., the sulphurous and nitrogenous constituents of the latter, they check putrefaction, and promote the gradual conversion of the elements of the decaying vegetable into the ultimate products of dissolution.

Nature of Yeast.

The views which the advocates of parasitic theory have formed respecting the causes of putrefaction are based mainly upon observations made on the fermentation of yeast in wine and beer. But the investigation into the nature of yeast is not yet complete. It is by no means impossible but that the microscopic observations may be strengthened by future discoveries, and that all doubt as to the vegetable nature of yeast may be removed. Even assuming this to be accomplished, the resolution of sugar into alcohol and carbonic acid admits of no other explanation—it can be expressed in no other way than that given in accordance with the chemical theory.

Deportment of Yeast in Solution of Sugar, Grape-juice and Beer-wort.

It has been clearly and fully demonstrated that in the vinous fermentation the elements of grape-sugar are found in the form of carbonic acid and alcohol without the smallest diminution of weight, whilst the elements of cane-sugar have acquired an addition of weight. The employment of the molecules of sugar in the digestive and respiratory processes of an organized being is therefore, according to commonly received notions, altogether out of the question. In the fermentation of grape-juice and beer-wort, the weight of the yeast increases; whilst in the same process, in a solution of cane sugar in pure water, its weight actually diminishes. If we repeatedly employ the same yeast to fresh solutions of sugar, it undergoes a continued diminution of weight, and finally loses altogether the power of exciting fermentation. According to the theory which makes the parasites the cause of fermentation, we must here ascribe one and the same effect to two diametrically opposite causes ; the one, their increase of numbers—the other their decrease. Assuming the nutritive and respiratory processes of the fungus to be dependent upon the presence of a sulphurous and nitrogenous substance containing the elements of the fungus, and the fermentation of the sugar to be an accidental phenomenon attendant upon the formative process of an organized being, it is perfectly iuconceivable how it should happen that the fungus placed in a fluid containing this chief condition of its growth and propagation, should nevertheless, not reproduce, whereas it commences to propagate from the instant that sugar, the presumed accidental attendant upon its vital processes, is added to the fluid. When, for instance, the decomposition of the sugar in grape-juice is once accomplished, the remainder of the sulphurous and nitrogenous substance dissolved in the juice will keep for years without suffering the slightest alteration, provided the access of air be precluded. Addition of a fresh amount of sugar to the juice will cause the fermentation to recommence, and yeast will again be deposited; this deposition of yeast will cease the instant that the newly-added sugar is completely decomposed, and will recommence upon further addition of a fresh amount of sugar, and so forth, until the fluid begins to contain an excess of sugar.

The formation of Yeast, Alcohol and Carbonic Acid, are mutually dependent.

These facts demonstrate evidently a mutual relation of dependency, such as the chemical theory demands, between the form and properties of the sulphurous and nitrogenous substance which becomes yeast, and the new forms and properties acquired by the sugar atom ; and it is obvious that the peculiar mode of resolution which the latter undergoes in this process is occasioned and governed by the state and condition in which the elements of the former exist during their association or combination with yeast and resolution into other products. In no other form of decomposition to which sugar is liable—such as its conversion into lactic acid, by animal membranes, for instance, or its conversion into mannite, gum, butyric acid, acetic acid, &c., &c.—has observation detected organic beings similar to fungi, or animalculæ of any discription whatever; nor have organized beings, which, recurring invariably in the same forms, determine the nature of the products ever been observed in any other process of putrefaction or fermentation.

Presence of Vibriones in Urine.

The presence of vibriones in putrescent urine has been observed in many instances ; but in innumerable other instances, it has been impossible to detect their presence, or that of any organized being whatever ; in putrefying urine and if the absence of vegetable or animal organisms be positively demonstrated in but one single instance where putrefaction has been induced, in fresh urine, by the white deposit formed in putrid urine, this one isolated fact is quite sufficient to remove all doubt as to the real cause of the putrefaction of the urine.

Fungi contain Sugar.

If finally we take into consideration, that analysis of fungi has hitherto invariably revealed a certain amount of sugar, which during the vital processes of these beings, is not resolved into alcohol and carbonic acid, but that spirituous fermentation commences immediately after their death, from the very instant that a change in their color and appearance becomes perceptible, the conclusion is inevitable, that there exists no analogy whatever to justify our regarding the vital process of these plants as the cause of fermentation. It is to the reverse of the vital process that fermentation is to be ascribed (Schlossberger, Annals of Pharmacy, vol. liii. p. 117.)

Modification of Putrefactive Process in Heated Air.

It has been proved by a series of the most admirable experiments, that the putrefactive process of meat and many other animal substances assumes a totally different form, when such substances are kept in vessels containing air which has been subjected to a red heat, and when, consequently, the co-operation of infusoria is altogether precluded; but these animal substances do not, by any means, under such circumstances, retain their original condition; their colour and state of cohesion change; and if, for instance, the water necessary for the total dissolution of the meat be present, this will, after a shorter or longer period, dissolve into a most offensive mass.* We need only recall to mind the deportment of fresh urine, to see at once that the putrefaction of many of these animal substances require an incessantly renewed afflux of oxygen; that the exclusion of this element prevents the transformation of urea into carbonate of ammonia; that these animal substances, enclosed in a vessel, convert the oxygen contained in the latter into carbonic acid; and that the removal of the oxygen puts a stop to the process, or at all events exercises a modifying action upon it.

The advocates of the parasite theory assume that the transient contact of the grape-juice with the air, without which the fermentative process would not begin, procures access for the germs of the yeast fungus which are presumed to be present everywhere in the air, and that these germs, having thus found an appropriate soil for their development, begin then to thrive most luxuriantly. But these gentlemen do not explain how it happens that the brewer is obliged to add yeast to put his beer-wort into fermentation; and that, consequently, these same germs (were they really contained in the air, as pretended) do not thrive in this soil, although all the conditions of their life and propagation are here found united. They forget altogether, that the fermentation of grape-juice commences with a chemical action; that a measureable amount of oxygen is absorbed from the air; that the juice becomes turbid and colored; and that the fermentation commences only with the formation of a precipitate. They do not take into consideration that the fermentation decreases, instead of increases in proportion to the amount of oxygen absorbed, and that the fermentation of the juice ceases when the proportion of this element has reached a certain height, and the matter capable of absorbing oxygen has become insoluble.†

Until all these phenomena and facts shall have been most thoroughly investigated, it must be considered contrary to the principles of sound reasoning to regard the vital process of an animal or plant as the cause of any fermentative or putrefactive process, and in all cases where the strictest examination has failed to demonstrate the presence of organic beings in the contagion of a miasm or contagious disease, the hypothesis that such beings have co-operated or do co-operate, in the morbid process, must be rejected as totally void of foundation and support.

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* De Saussure, in his beautiful experiments, observed the remarkable fact, that hydrogen gas, produced by transmitting aqueous vapour over red-hot iron, forms no combination with oxygen, in contact with putrescent or decaying animal matter, whereas pure hydrogen gas, produced at the common temperature, is readily condensed under the same circumstances. This fact may deserve attention in investigations concerning the influence of ignited air upon the putrefactive process. Possibly, the destruction of infusoria and germs of fungi is not the only cause of the modification thus produced in this process.

† Two cubic centimetres of must, three millimetres thick and thirty millimetres in diameter, in contact with twenty centimetres of oxygen, do not ferment; whilst the same layer of must, without addition of oxygen, evolved a considerable amount of carbonic acid. (De Saussure, Annals of Chemistry, lxiv. p. 47—51.)

Two simultaneous Phenomena are frequently considered to stand to one another in the relation of Cause and Effect.

It happens frequently that two phenomena, resulting simultaneously from one and the same cause, are erroneously looked upon as standing to one another in the relation of mutual dependency, or of cause and effect; and that the description of the one of these phenomena is regarded as an explanation or definition of the other. This is the case, for instance, with the explanations that are given of the nature of fevers, of crises, &c. A few examples of similar erroneous associations, which are of daily occurrence, will best serve to illustrate the meaning of the preceding remarks.

EXAMPLES

Storms regarded as the cause of unusual Changes in the state of the Barometer.

Nothing is more common than the opinion that storms occasion a fall of the mercury in the barometer, and, consequently, that they exercise an influence upon the state of this instrument. Storms are effects of a disturbed equilibrium of the pressure of the atmosphere, occasioned by a difference of temperature, or by some other cause. Changes of the pressure of the atmosphere occasion likewise a fall or rise in a mercurial column standing in equilibrium with a column of air of equal diameter. There exists no immediate connexion between the state of the barometer and the storm; the latter exercises no action upon the former. The only connexion existing between them is this, that they are the simultaneous effects of one and the same cause. The same applies to the connexion existing between the fall of the barometer and the occurrence of rain.

The symptoms of Fever not to be regarded as the Causes of the Disease.

Many pathologists entertain similar erroneous views on the cause of fever. Thus Henle, in his "Pathological Investigations," page 240, says,—"I will not undertake to decide the much disputed question, whether there are essential fevers or not; but I think I may venture to throw out a few hints that will, perhaps, enable the two contending parties to arrive at a more just and correct comprehension, both of their own views, and of those supported by their opponents. It has been found that the symptoms of fever manifest themselves in consequence of an alteration of the central organ. This alteration is the proximate cause of the febrile symptoms, and since the fever consists precisely in the symptoms—i. e., in the complication of altered temperature, altered circulation of the blood, thirst, lassitude, &c., this alteration of the central organ is to be regarded as the proximate cause of the fever; in short, as the disease itself.

Now, even setting aside that these three propositions are not connected parts of a logical conclusion, since the second and third are simply variations of the first, the febrile symptoms can, according to the strict rules of natural investigation, only be regarded as indications of an alteration of the spinal marrow, until a connexion, as cause and effect, between this alteration and those symptoms, shall have been fully demonstrated. But until then we must continue to look upon the alteration of the central organ as one of the symptoms of fever, ascertained by scientific investigation, and a fact perceptible to our senses, but surely not as the cause of the disease.

Proper method of investigating the cause of Fever.

Supposing that the alteration of the central organ is found to be constantly and invariably associated with the symptoms of fever, the discovery of its cause, the true explanation and definition of the disease, must embrace the connexion and mutual dependence of the three invariable and characteristic symptoms—namely, the subjective feeling of indisposition, the altered circulatory and respiratory movements, and the altered phenomena of animal heat. The state of the patient's feelings, the heat and cold of the fever, are, in the present state of our knowledge, inexplicable. But laying these aside, we have first to ascertain the connexion which exists between the observed change in the spinal cord, and the accelerated motion of the circulation and respiration, together with the increased temperature. Before we can accomplish this, we must learn precisely the nature of motion, and the source of motory power and animal heat. Now, supposing a certain amount of force serving to propel the blood through the organism to be generated within the heart by the concurrent operation of several, say, of *two* causes, the circulatory motion will be uniform or normal, if the number of pulsations remains in every second the same—in other words, if the propelling force is distributed over equal portions of time.

Various Points of View.

If from a disturbance of the relations between the two causes supposed to have their seat in the heart, the blood-propelling force be increased at one time, and diminished at another, the pulsations of the heart will be corespondingly quicker or slower. The generated force is, in this case, not proportionate to the time of its consumption. It is obvious that (assuming the propelling force to be generated in the heart) the alteration of the spinal cord can exercise no other influence upon the acceleration or retardation of the heart's motion, save in this, that the spinal cord, in consequence of its altered state and condition, opposes somehow or other, to the latter less resistance at one time than at another. But the causes of motory effects do not exist in the heart alone, they exist in every part of the organism, in the spinal cord as well as in every individual muscular fibre.

Connexion of the Spinal Cord with Organic Motion.

But we may assume the primary cause of the motions of the heart, and of every other part of the organism, (the motions of the intestines, the voluntary motions, &c.,) to be seated in the spinal cord; if it be so, it is obvious that an alteration in the state and condition of the latter must cause a change in all the phenomena of motion. And the same must happen, if any part of the nerves connected with the spinal cord, and with the apparatus of circulatory and intestinal motion, suffer any alteration in state and condition: the altered action of the affected nerves must again exercise a certain influence upon the spinal cord and the motory apparatus. The laws of the propagation or transmission of motion are universally the same, from whatever cause the motion may proceed.

The cause of the motion of a mill, the rotatory motion of the stone, the bolting of the flour, &c., is not seated in the water-wheel, this latter itself being simply a part of the mill. It is perfectly true that the correct and uniform working of the mill may be disturbed by the removal of one or several of the buckets of the water-wheel, since the pressure of the water will of course cease upon part of the wheel. But the uniform working of the mill may be equally disturbed by the removal of one or several of the cogs of any one of the other wheels composing the mechanism of the machine; and any hurt or damage of this kind will uniformally manifest itself by the irregular motions of the water-wheel, as well as by the impaired action of every other part of the mill.

Regular and Irregular Motions.

Now, supposing the organism to generate, within a given time, a certain amount of force to be distributed from a central seat, the spinal cord, over the various motory apparatus—the motions will be uniform and regular, if the motory force is distributed equally over the latter, whilst they will be irregular if any one organ receives a larger proportion of force than another. Consequently, any acceleration of the respiratory and circulatory motions will be attended with weakness in the limbs, or disturbance of the digestive process. The force which the heart receives over and above its just and proportionate share, and which causes the acceleration of its motion, is necessarily lost for the other motory apparatus.

Having thus endeavoured to trace and establish the connexion between the spinal cord and the motions of the organism, we have next to examine the relations of these motions to animal heat.

Relation of the Phenomena of Motion to Animal Heat.

Observation shows that irregularities in the phenomena of motion are attended with changes in the phenomena of animal heat. In many cases, the subjective and objective phenomena of heat rise or fall with the acceleration or retardation of the motory phenomena; in other instances, the phenomena of heat do not correspond in degree to the phenomena of motion manifested simultaneously and concurrently with them.

Relation of the Phenomena of Motion to Animal Heat.

Observation has shown that irregularities in the phenomena of motion are attended by corresponding changes in those of the temperature; in many cases, the subjective and objective phenomena of animal heat rise and fall with the acceleration or retardation of the motory phenomena, whilst in other cases the two classes of phenomena are not manifested simultaneously in the same degree. But with the equalization of the temperature, the phenomena of motion become uniform; or when the latter are restored to their normal state the phenomena of temperature correspond. Now if it can be proved that the motion, by its velocity, does not produce heat,—as, for example, that it is not produced by mere friction,—then it must follow that temperature and motion, in the animal organism, stand in no closer relation to each other than the storm and the rise and fall of the mercury in the barometer, that is to say, the two classes of phenomena are simply occasioned by the same cause. If, at the same time, it should be discovered that the amount of heat evolved in a given time, stands in a constant and definite ratio to the number of blood corpuscles which have passed through the capillary vessels in the same time, we must seek for the source of animal heat in the blood, or in some conditions of relation between the blood corpuscles and the vessels.

Relation of the Phenomena of Animal Heat to the Oxygen of the Air.

Now, investigations having proved that it is the property of absorbing oyxgen that renders blood a source of heat, since the amount of oxygen absorbed in a given space of time bears a definite proportion to the number of inspirations in the same space of time, it results that the unequal effects of heat are dependent upon the respiratory motions, the contraction of the heart, and besides these, upon an external cause—viz., the chemical action of the oxygen. Any change in the mutual relations of these three factors must cause a corresponding change in the phenomena of heat, and if, in any part of this organism, the power of combining with oxygen is increased by some superadded cause, a larger amount of heat will be evolved from this than from other parts; consequently, if the circulatory and respiratory motions experience a simultaneous and equal acceleration, the absorption of oxygen, and consequently the number of degrees of heat evolved, will increase; in accordance with the beautiful law established by VIERORDT. Differences in the respective accelerations of the circulatory and respiratory motions are attended with a corresponding modification of the subjective or objective sensation of heat. When all these relations shall have been examined and established, we shall be able, not simply to explain the symptoms of fever, and the nature of this disease, but likewise to refer the whole of the febrile symptoms to one single and ultimate cause, (the *causa morbi.*)

Such is the way in which an inquiry into the cause and nature of fever ought to be conducted.

Erroneous conclusions frequently deduced by singling out, and bringing prominently forward, one of several concurrent causes.

In the examination of natural phenomena dependent upon several concurrent causes, it frequently occurs that a totally false conclusion is arrived at by singling out one of these causes and ascribing to it an efficiency which is not really inherent in it, but imparted to it by the concurrence and coöperation of the other causes. Thus, for instance, SCHLEIDEN (elements of the Botanic Science, 1845, p. 282) will have it that the view that "fermentation and putrefaction are effects of the transmission of a motion," is in part based upon the untenable atomic theory, and in part upon a false conception of mechanic laws. SCHLEIDEN says, "the amount of the motion is measured by the product of the mass into the velocity. Now one part of diastase extends its decomposing power over 1000 parts of starch." (This is a mistake of Schleiden, since, according to GUERRIN VARRY, one part of diastase acting upon 60 parts of starch yielded only 10.3 parts of sugar, and one part of diastase acting upon 16 parts of starch yielded only 14 parts of sugar.) "It would be necessary, accordingly, to assume, in the diastase atom, a velocity a thousand times greater than that required for the decomposition of an equal weight of starch. It is not difficult to see that, to arrive at the desired end, we should here have to raise a gigantic structure of the boldest hypotheses upon the weakest and most untenable basis. On the other hand, the objection that there is no instance of a body at rest setting another body in motion, is likwise borrowed from the atomic theory, and is physically wrong. Gravitation, magnetism, electric attraction, are instances of motion imparted to other bodies by bodies at rest."

Correction of Schleiden's views.

As regards diastase, and its action upon starch, SCHLEIDEN has forgotten altogether to take into account the time required to effect the transmutation of starch into sugar. The view which he contests does not presuppose that the diastase atom should possess a greater velocity, but simply, that the transposition of the elements of a starch molecule has been completed while the motion in the diastase molecule still continues, and the latter has consequently not yet been restored to a state of equilibrium. Transmission of the motion from the diastase atom to the starch atom signifies, simply, that the starch molecules, in contact with the diastase molecules, comport themselves as if they were part and parcel of the latter. The action of diastase within a given space of time is dependent upon the number of starch molecules that may, within this space of time, come into contact with the diastase molecules. The time required for the conversion of the starch into sugar is dependent upon the relative number of diastase molecules: the action of the diastase upon the starch ceases the instant that the transposition of the elements of this fermentative agent is completed; a double or treble amount of diastase shortens the time of the process, or effects the conversion of a larger amount of starch into sugar.

Different ways of imparting Motion to Bodies at rest.

With regard to SCHLEIDEN's assertion, that gravitation, electricity, &c., are instances of motion imparted by bodies at rest, it must be taken into consideration, that a body at rest may be put in motion in two essentially different ways—viz.:

1. By transmission from a body in a state of motion. This motion is transmission by the stroke of the hammer upon the nail, of the water upon the wheel, or of the wind upon the sail.

2. By the operation of an attractive or repulsive force acting between two bodies. In this case, the action is invariably mutual, and the velocities acquired are inversely proportional to the moved masses.

Certain Chemical Processes must be considered Phenomena of Motion belonging to the Second Class.

Since we consider chemical processes in the light of phenomena of motion, it is unquestionable, that all chemical processes which result in the formation of new compounds belong to the new second description of phenomena of motion, since the attractive power of the component parts, or the chemical affinity operating between them, calls forth the resulting change of place or condition (or, in other words, the motion) of the matter. The motion discontinues the instant that the formation of the new compound is completed, in the same manner as the falling stone ceases to move the moment it reaches the ground, or the iron filings, when they touch the pole of the magnet.

Fermentation and Putrefaction, Phenomena of Motion belonging to the First Class.

But if a body in a state of decomposition—i. e., a body of which the component parts are in a state of transposition, or, in othor words, in a state of motion, is found to place another body in a similar state,—and observation has proved that, with the exception of one, none of those causes that are known to produce alterations in bodies, or to occasion transposition of their elements, have the slightest share in the process; and if, finally, it is demonstrated that this one cause (transmission of motion, friction, stroke, or blow, &c.,) takes a definite part in the formation and decomposition of a number of compounds, then this latter should surely be regarded as the efficient cause, if the notions of motion gained in mechanics are at all applicable to chemical action. The determination of this last and only cause is, accordingly, not a mere word, that has been substituted for the term "catalytic force," but is the expression of an idea the very reverse of that of a catalytic body. The proposition announced above, under the head of "Different ways of imparting Motion to Bodies at rest," exposes the fallacy of SCHLEIDEN's conclusion, that gravitation, magnetism, &c., are instances of motion imparted by bodies in a state of rest.

The Force of Gravitation in itself produces no motion.

A clock is kept going by the weights, but it requires to be wound up at certain fixed periods, not possessing the property of accomplishing this requisite by means of its own mechanism, and the heat of the sun has as little share in the motion of a mill-wheel as gravitation. The water that impels the mill-wheel was previously vapour, which latter, again, was previously liquid water. The latter evaporated under the influence of heat, the vapour thus generated became again liquefied by abstraction of heat, and the liquid water is made to fall in obedience to the law of gravitation, and continues to fall, until, as is the case with the clock, resistence arrests its motion.

Want of precision in the enunciation of Facts or Phenomena becomes frequently a source of erroneous conclusion and of mistakes.

To the many sources of errors and fallacies inherent in the method of physiological research, many of our modern physiologists join a certain individual defect, which may be properly characterized as "carelessness in the enunciation of observed facts and phenomena." The defect consists in this, that they are in the habit of representing facts and phenomena which they have perceived by their senses, as logical inferences. This habit finally engenders in them a tendency to demand ocular demonstration of actual conclusions, deductions of an unknown magnitude from two or several known magnitudes, which conclusions and deductions, of course, are not palpable to our sense; and in default of such ocular demonstration, they reject all such conclusions and deductions as fallacies. It is probable owing to this unfortunate tendency, that chemists, notwithstanding a superabundance of the clearest and most evident proofs, are frequently unable to convey the most simple truths to the minds of physicians.

Examples in illustration of the preceding remarks.

Examples in proof of this assertion may be met with in every work on physiology. I will content myself with quoting a few passages from one of the recently published treatises on that science. VALENTIN says, (at page 6 of his "Manual of Physiology," published in 1844, at Brunswick, by Vieweg.)—"we divide the facial nerve on one side, and observe, subsequently, that the muscles of the corresponding side of the face have become inaccessible to the influence of the will. From this *we conclude correctly* (from this 'we *see* or *perceive*' VALENTIN ought to have said) that the nervus facialis is the medium through which the intimations of our will are communicated to the muscles of expression.

"After injuries to the trunk or to the optic branch of the nervus trigeminus, we observe secondary inflammation, suppuration, and still more serious affections of the pupil, and we *infer* (from this 'we thus *perceive*,' VALENTIN ought to have said) that the continuance of the normal state of the eye depends upon the continued integrity of the trigeminal nerve."

And again, at page 3: "If I know, for instance, that the walls of the arteries are elastic, I may at once *infer* that the arteries, when filled with a larger amount of blood than usual, will extend to a certain degree, and, subsequently, when the circulation resumes its customary regularity, return to their original size," (which means really nothing more than that the arteries are elastic.)

Points of Contact and Union between Physiology and Chemistry.

In the preceding paragraphs, I have endeavoured to show in what manner the difference of the respective physiological and chemical methods of viewing and judging facts and phenomena obstructs the way to a clear understanding between physiologists and chemists; I will now proceed to indicate the points where physiology and chemistry ought to meet and unite, in order to bestow mutual assistance and benefit upon one another.

The Laws governing Vital and Chemical Phenomena differ from Mechanical Laws.

If we attempt to apply the notions gathered from the knowledge of what are called mechanical forces, for the investigation of vital or chemical phenomena, we perceive at once that the laws governing the former differ in many respects from those upon which the peculiar properties of chemical or vital compounds are dependent.

Relation of the Properties of the Elements to the Properties of the Compounds.

The chemical combination of two bodies results in the formation of a new body of totally different properties from those of either of the two components. The chemical force of the new compound (the property which it may possess of entering into new combinations, or of affecting decompositions) is not the sum of the chemical forces of its elements, and we are unable to infer the properties of the latter from those of the former. This applies to compounds: we are unable to infer from the properties of organic muscular fibre those of its component elements, carbon, hydrogen, nitrogen, &c.; and yet, notwithstanding, nothing can be more certain than that there exist certain constant relations between the properties of the elements and those of the compound formed by their union.

Cinnabar is a metallic sulphuret possessed of totally different properties from those of sulphuret of lead (galena) and sulphuret of zinc, (zinc blende.) There can be no doubt that this difference in the respective properties of these three sulphurets depends upon the one containing mercury, the other lead, and the third zinc, in combination with the sulphur, and that the respective properties of these three metals exert a definite and determinable influence upon the nature of the respective properties of the three sulphurets. The truth of this is most clearly and fully demonstrated in isomorphous substances. There exists hardly any perceptible difference in outward appearance between sulphuret and seleniuret of lead, between potass alum and ammonia alum, (the double sulphates of alumina and potass and alumina and ammonia,) between sulphate of soda and seleniate of soda. The relations existing between the chemical and physical properties of the elements have, in many of these compounds, remained constant, and in those in which differences in the colour, solubility, &c., are observed, one property—viz., the physical form—has remained constant. The same, or similarly, constant relations, doubtless exist between the properties of all elements and their compounds, and it is obvious that all the efforts of chemistry are directed to the discovery of these constant relations. This is the only way by which chemistry can arrive at the knowledge of natural laws, and it is likewise the only way by which physiology can expect to acquire a scientific basis.

The Chemical Forces of the Elements have a share in the Vital Phenomena.

It is perfectly true, that we are as yet unable to infer the physiological properties of animal or vegetable substances from the laws or properties of the elements composing them ; yet there can be no question that these physiological properties are deducible from laws which begin to operate when the elements have arranged themselves in a certain manner. When the elements have combined to form an animal or vegetable substance, and have thus acquired physiological or vital properties, the chemical forces that imparted to them their original properties are by no means annihilated or abrogated, any more than the cohesive force of the sulphur molecules is annihilated by the fusion of a piece of sulphur. In this latter instance, a new force (heat) has simply acceded to the existing force of cohesion, and this new force abrogates the effect of the latter, (solidity,) so that this effect is no longer perceptible. The new condition of the sulphur (fluidity) is a state of equilibrium between two antagonistic forces; it is an effect in the production of which both bear an equal share.

In vegetable and animal substances the elements continue to obey mechanical and chemical laws, if the effect of these is not abrogated by resisting forces, which are to be considered as the indications of new laws, which govern the organic parts of the organism.

Necessity of ascertaining and examining the Relations existing between the Effects of Chemical Forces and those of the Vital Force.

If, consequently, the joint operation of several causes gives rise to new laws and new phenomena, that bear no resemblance to the separate effects of the individual causes, the effects of the latter stand in a discoverable relation to the effects of the newly-created causes. These relations between the separate and individual effects of the several coöperating causes, and the effects of their joint production, must be ascertained and examined. When we shall have acquired a thorough and accurate knowledge of these relations, we shall be able to deduce from them, without the aid of further observations, a number of unknown facts or phenomena, in the same way as we do now in the case of isomorphous substances.

The Relation between the Weight of the Elements and the Properties of the Compound formed by their Union.

The discovery of the fact that the weight of the elements remains constant in all chemical combinations, or, in other words, that the weight of a chemical compound is invariably equal to the sum total of the respective weights of its component elements, (a truth which seems so self-evident, that one can hardly believe that its establishment should have cost so much time and labour as it really has,) has imparted a purely scientific character to one branch of chemistry.—(Stochiometry.) The knowledge of definite chemical proportions has enabled us to predetermine every combination which a body may possibly form ; but this knowledge did not suffice to enable us to explain the apparent exceptions where bodies were observed to combine, not in constant, but in all imaginable, proportions. The discovery of another property—viz., that of the relation which the outward form of componnds bears to their composition, has not only enabled us to explain these deviations, but has also led to a far clearer idea on the cause of the constancy of combining proportions in general.

Universality of Laws of Mutual Dependency in Natural Phenomena.

It is the conviction that such laws of mutual dependency obtain universally in the properties of bodies, and are discoverable, which leads to progress in all branches of natural philosophy, in the physical sciences, as well as in physiology.

Way to attain to a Knowledge of these Relations of Mutual Dependency.

In the study of Nature, the only method of arriving at a knowledge of the mutual relations existing between the properties of bodies, is to endeavour, in the first place, to acquire a knowledge of these properties, and subsequently, to ascertain and establish the circumstances in which they undergo changes. It is one of the universal laws of Nature, that the deviations, or differences, in one property, should invariably and immutably be attended by corresponding differences in some other property; and it is perfectly obvious, that the knowledge of the laws of these deviations and differences will enable us to infer at once, from the deviation observed in one property, that of the other. This power of inferring the unknown from the known forms our great desideratum.

A few examples, in illustration, will suffice to demonstrate the truth of these propositions.

Instances of Laws of Mutual Dependency—Pressure and Boiling Point.

It is a well-known fact, that the respective degrees of temperature at which any given fluid commences to boil is invariably and constantly the same for the same fluid, provided the essential conditions under which it is exposed to this temperature be likewise constantly the same. The immutable constancy of this property has led us to rank the boiling point of a fluid among its characteristic properties.

External pressure is one of the conditions of the constant temperature at which bubbles of vapour form in the interior of fluids. The boiling point of all fluids alters and varies with this pressure, and this according to a separate law for every individual fluid. The boiling point rises or falls respectively with the increase or diminution of the pressure. Every individual boiling temperature corresponds to a definite degree of pressure, and so, *vice versa*, every individual degree of pressure corresponds to a definite degree of temperature. It is a well-known fact, that the knowledge of the law of the mutual relation of dependency existing between the boiling point of water and the pressure of the atmosphere, has enabled us to decide, by means of the thermometer, at what altitude we may happen to be above the level of the sea, and thus to judge of one property by the variations observed in another.

Boiling Point and Composition.

The relations which the boiling points of fluids bear to their composition are less generally known. Pyroxilic spirit, alcohol, and the fousel oil of potato brandy, are three fluids, of which the respective boiling points differ greatly from each other. Pyroxilic spirit boils at 138.2° F.; alcohol at 172.4° F.; and fousel oil at 275° F. The comparison of these three boiling points shows that the boiling point of alcohol is by 34.2, ($138.2+34.2=172.4$;) and that of fousel oil by four times 34.2 degrees higher than that of pyroxilic spirit, ($138.2+4 \times 34.2=275°$.)

Upon oxidation under like circumstances, each of these three fluids yields an acid—viz., pyroxilic spirit yields formic acid; alcohol, acetic acid; and fousel oil, valerianic acid. Each of these three acids has again a distinct boiling point,—formic acid boils at 210.2° F.; acetic acid at 244.4° F., and valerianic acid at 347° F. A comparison of these three boiling points will at once show that they bear the same relative proportion to each other as the boiling points of the three fluids from which they were formed. The boiling point of acetic acid is 34.2, higher than that of formic acid; that of valerianic acid is four times 34.2 degrees, higher than that of formic acid. Now, if we compare the respective composition of these six bodies, (pyroxilic spirit, alcohol, fousel oil, and formic acid, acetic acid, valerianic acid,) we find that the composition of pyroxilic spirit is expressed by the formula, $C_2\ H_4\ O_2$; that of alcohol, by the formula, $C_4\ H_6\ O_2$; and that of fousel oil, by the formula, $C_{10}\ H_{12}\ O_2$.

Now let us substitute for the formula C H (which means equal equivalents, by weight, of carbon and hydrogen) the letter R, and it will appear at once that the formula of alcohol may be expressed by that of pyroxilic spirit plus 2 R.

$$\underset{\text{Pyroxilic spirit.}}{C_2\ H_4\ O_2} + \underset{(2\ R)}{C_2\ H_2} = \underset{\text{Alcohol.}}{C_4\ H_6\ O_2}$$

The formula of fousel oil may be expressed by that of pyroxilic spirit+8 R.

$$\underset{\text{Pyroxilic spirit.}}{C_2\ H_4\ O_2} + \underset{(8\ R.)}{C_8\ H_8} = \underset{\text{Fousel Oil.}}{C_{12}\ H_{12}\ O_2}$$

The formula of formic acid is $C_2\ H_2\ O_4$; that of acetic acid, $C_4\ H_4\ O_4$; that of valerianic acid, $C_{10}\ H_{10}\ O_4$. Here it will be again readily observed, that the formula of acetic acid may be expressed by that of formic acid+2 R, and the formula of valerianic acid by that of formic acid+8 R. It follows from these data, that a rise of 34.2 degrees in the boiling point corresponds to the accession of every two equivalents of carbon and two equivalents of hydrogen, or, in other words, to that of every 2 R. That the relation between the composition and the boiling point is constant throughout this group of compounds, and that the knowledge of the boiling point of any of the compounds belonging to this group enables us to infer the composition of the compound, are facts capable of demonstration. The boiling point of formiate of oxide of methyle is 96.8° F.; that of formiate of oxide of ethyle is 131° F.; the difference between the two is 34·2. From this we should accordingly be able to conclude that the composition of the latter differs from that of the former by $C_2\ H_2$, or, in other words, by 2 R; and this is indeed the case. The formula of formiate of oxide of methyle is $C_4\ H_4\ O_4$; that of the corresponding compound of ethyle is $C_6\ H_6\ O_4$; the latter contains, consequently, exactly $C_2\ H_2$ more than the former. Thus, butyric acid boils at 312.8°F.; its boiling point is consequently exactly three times 44.2 degrees higher than that of formic acid. A comparison of the respective formulæ of the two acids shows that butyric acid may be regarded as formic acid+6 R. Toluidine and aniline are two organic bases, of which the composition differs, inasmuch as the latter contains $C_2\ H_2$ (or 2 R) more than the former. A comparison of the respective boiling points of these two substances shows that the boiling point of aniline is exactly by 34.2° F. higher than that of toluidine.

(To be continued.)

Part Fourth.

MEDICAL INTELLIGENCE.

FOREIGN.

1.—*Clinical Lecture on Typhus Fever.* By Dr. Corrigan.

[The following extract is taken from the continuation of a lecture, the first part of which appeared in our last volume (vol. III., p. 17.) The lecturer having there stated his views of the pathology of fever, now passes in review some of the functional disturbances which supervene during the course of the disease, and add materially to the difficulty of treatment, as well as to the danger. The lesion of the function of the cerebro-spinal system is one of the most important which can arise during fever, and is manifested by want of natural rest. A patient may die, observes Dr. Corrigan, of this lesion, and may die of this cause alone; if it continue for a few successive nights, delirium and coma are the invariable consequences. Taking this view of the case, the importance of the question—Has the patient slept? becomes apparent.

The treatment of this symptom is thus laid down by the lecturer:]

If we now pass to the question of medical treatment, the first thing to be done will be to have the head shaved; and this of itself is generally sufficient; the application of intense cold is often a serious error; it very often obviates the object you have in view—that of procuring sleep. After this, if you ascertain that the patient has still had no sleep, you will not be doing wrong, I am sure, if you apply four or six leeches to the temples. This is a remedy upon which, for my own part, I place more reliance than almost any other. If, after your patient has passed three or four successive nights without sleep, you apply four leeches to the temples, and you procure for him even half an hour's sleep, you will have done a vast deal towards his recovery. A state of general debility, requiring the exhibition of wine, need by no means prevent your employing this remedy; for while you give wine with a view to the increase of tone of the heart, larger vessels, and capillaries, you are perfectly justified in relieving the distended capillaries of the head by the local abstraction of a small quantity of blood; just as in some forms of ophthalmia, it is often the best practice to employ local bleeding, to relieve the distended capillaries, and tonics, to give them and the general circulation tone. I was myself first led to adopt this practice in reference to procuring sleep, chiefly from having observed that, on the occurrence of epistaxis, sleep supervened, and the patient recovered, although in such cases the general debility had been so great as to have required a free exhibition of wine. You need have no fear of taking this small quantity of blood from the capillaries, for they very soon accommodate themselves to the trifling loss sustained: there is not the risk that might attend even so small an abstraction of blood, taken from a vein. The application of cold to the head is another useful means of obtaining the same object, but it is one which is too often carried to excess. A bladder containing pounded ice is placed on the head, but air is quickly disengaged, the ice lies heavily upon the top or back part of the head, and sometimes causes even sloughing of the integuments. Instead of ice, take a single fold of linen dipped in cold lotion; lay it on the head, and it will generally answer all purposes.

The question might suggest itself here, why, as opium is given in delirium tremens, and in such like diseases—why should it not also be administered here, where obtaining sleep is a matter of such paramount importance? Why not give it for the lesion of the cerebro-spinal function which we are now considering? Because, if opium be given in fever, and if it do not procure sleep, it does mischief of another kind. It will act injuriously upon the nutritive function, including that of secretion and excretion. In analyzing the disease before us, we may derive great assistance from comparing some of its lesions in function with symptoms in other diseases. For instance, in delirium tremens, while the tongue and gums secrete their natural quantity of mucus, and the secretion of urine is abundant, you may continue to give opium for the purpose of procuring sleep; but suppose, in the course of the treatment, the tongue became dry and brown in the centre, the urine high coloured and scanty, and a slight wound or cut, if there happen to be one on any part of the body, becomes dry and everts its edges—in other words, as the nutritive function is passing into a deranged state, then the opium is acting as a poison upon the system, and the continuance of its administration would be fatal. The same considerations are to guide you in the exhibition of opium in fever; if the condition of the skin, of the mucous membrane, and an abundant secretion of urine, indicate a sufficiently active state of the nutritive and secreting function, you may venture on a trial of opium; but if the desired effect be not produced at once, do not attempt to press it; if you do, mischief will surely follow. Hyoscyamus is a remedy which has not an injurious effect upon the functions of nutrition and secretion, but this cannot be said of opium. At the approach of coma you must not, unless there be strong vascular action about the head, persist in leeching, but have recourse to blistering, as a means of rousing the patient from his state of torpor. Here I may observe, that in the ordinary way of applying a cap-blister, as it is called, the blister cannot be made to lie close to the surface: for this reason, I have been in the habit of having the blister cut into strips of an inch in width, laid across the head in a direction from ear to ear, so that no part is left uncovered, and that the strips lie close to the skin. Counter-irritation may also be produced in a few hours, by means of croton oil and ung. hdyr. mixed.

[Dr. Corrigan alludes to hurried respiration in fever in these words:]

There is a state of the breathing preceding the approach of coma which you might confound with that of bronchitis, but in which the danger is very different; it becomes exceedingly rapid and laboured, amounting to 40 or 50 respirations in a minute, but the respiratory murmur is audible in the tubes; there is no mechanical obstruction in them; this is what is called cerebral respiration; while in that of bronchitis or catarrh the tubes are filled with mucus, and a mechanical obstruction is thus offered to respiration, from which very few cases recover. You observe, besides, the characteristic lividity of the countenance, while in the cerebral breathing the countenance is pale, natural, high colored. The cerebral breathings is a breathing of debility, very similar to that you hear in a person after very violent exercise.

[The lecturer concludes with the following summary of his preceding observations.]

In the analysis we have made of fever, as purely a disease of functions, and these lesions of functions, not symptoms but portions of the disease itself, the treatment is to be adapted not to the circumstance of the fifth, sixth, tenth, or twentieth day of fever, but to meet the state of each particular function, day by day. Take up, in their proper order, as you stand by the bedside—first, the circulating function, made up of the heart, larger vessels, and capillaries: examine the state of these functions; next look to that of nutrition and secretion, as declared by the state of the tongue, gums, urine, and the other symptoms, as I have gone through them; then to the cerebro-spinal system, which we have examined to-day. If you understand clearly the rules I have endeavoured to lay down, you will understand why it is that we may with advantage combine the application of leeches with the administration of wine, and along with the latter the exhibition of mercury; the one to meet the lesion of the circulating function—the other for the purpose of relieving the derangement of nutritive functions. You also know that mercury may be combined with aqua acetatis ammoniæ, &c., when febrile excitement is to be subdued; and you can perceive the value of blisters and other stimulants as a means of stimulating the function of circulation, or of rousing a patient from the lethargic condition into which he may fall. Thus, if you follow up the rules which in the analysis of fever I have endeavored to carry out in the short space of three lectures on the subject, you will be able to understand why it is that remedies of an opposite therapeutic tendency may be usefully combined. You can understand, also, that fever is not a disease to be treated by guess-work, but that it has, in fact, on accurate investigation, rules for its treatment as definite as those of any other disease you may have to contend with.—*Dublin Hosp. Gaz.* May 1, 1846.

2.—*On the Abuse of Alkaline Remedies.* By Professor Trousseau.

The object of this paper is to caution practitioners against the abuse of alkalies. These remedies exercise an immense influence on the economy. If by their use the alkaline state of the blood is increased, all the secretions from that fluid are modified. The secretions which are naturally alkaline become more so, those which are neuter will become alkaline, and those which are acid will become either less acid, or more or less alkaline. The presence of acids being one of the conditions for the digestion of food in the stomach, it cannot be a matter of indifference to neutralize the acids which the economy wants—for the transformation of fecula into glucose, for instance. The digestion of amylaceous substances becomes, therefore, incomplete, or extra-natural, if we may use the expression. The presence, also, of alkalies in the blood in due proportion, gives to this liquid the means of burning, to a certain extent, the carbonaceous elements absorbed in the process of digestion. An imperfect combustion gives rise, without doubt, to morbid symptoms; but a too great or too rapid combustion, on the other hand, is not the less attended with inconvenience, as it gives rise to important mutations in the composition of the blood, and consequently in the texture of the organs.

It is therefore, under no circumstance unimportant to administer alkalies.—Taken without any real indication for a few days, they only give rise to momentary disturbance, but, taken in large quantities, they occasion a cachetic condition, followed by a deplorable state of emaciation. The ancients have admirably indicated the influence of alkalies on the composition of the blood: they had remarked that it became more fluid, being paler than in the normal state; and that at last a cachexia became established, characterized by paleness, general puffiness of the tissues, and passive hemorrhage; moreover they had also perceived, that these symptoms were followed by emaciation. Within the last few years, the abuse which has been made of the mineral waters of Vichy and Carlsbad, in the treatment of gout, has proved the above fact. The abuse of alkalies has certainly done more harm than that of iodine.

How is it that physicians do not see that a remedy powerful to cure is also powerful to do evil? Alkaline remedies are daily administered with inconceivable indifference. A physician prescribes to a patient one or two months' use of the waters of Vichy, Carlsbad, or Ems, as he would barley-water to drink. Is it a matter of so little importance to change all the secretions of the economy? Other alterative medicines are wielded with more prudence. Mercury and iodine, for instance, are administered with care and precaution, because the danger which attends their use is known.

In conclusion, Professor Trousseau unhesitatingly states, that the danger of alkalies is greater than that of mercurials, because the danger is less suspected, and that their administration is often only arrested when the health of the patient has been irreparably destroyed. This is not so often the case with mercurials, because the experience of three centuries has told us that mercury could not be taken long with impunity. It behooves us, therefore, to make generally known both the immense utility and the extreme danger of alkaline remedies.—*Journal de Médicine*, March, 1846.

3.—*On the Accidents which result from the Puncture of a Nerve. Cause of Neuralgia.* By M. Aug. Berard, Surgeon to La Pitié.

At the moment when a nerve is pricked the individual experiences an extremely sharp pain at the site of the wound, which radiates through the divisions of the nerve to the parts where it terminates. Sometimes the pain proceeds upwards towards the origin of the nerve. Having continued some days, rendering the motions of the part difficult, or preventing motion, it usually subsides, but is sometimes followed by more or less serious accidents, as excruciating pains, convulsions, tetanus, spasmodic contractions; usually confined to the parts to which the wounded nerve is distributed, and occurring in paroxysms accompanied with more or less suffering, occasionally extending over the whole body; and if the inferior limbs are involved, the patient is unable to walk or to support the motion of a carriage; or obstinate neuralgia may occur a considerable time after the infliction of the injury.

M. Bérard, in his own person, experienced neuralgia from a prick of the frontal nerve. For the purpose of a galvanic experiment a needle was forced into the external branch of the ophthalmic nerve, as it proceeds from the supra-orbital foramen, and an electric current was directed throughout the division of the nerve. At the instant, violent pains were felt over the forehead and crown of the head. The pains ceased when the needle was withdrawn, but some months afterwards a fresh attack of neuralgia was experienced in the division of the wounded nerve. The pain had the quotidian intermittent type, and gave way to the use of sulphate of quinine. Since this period repeated attacks of neuralgia have occurred, with many years' interval, some of which were excessively violent and long-continued, and always seated in the frontal branches, but sometimes extending to the nasal and lachrymal branches of the ophthalmic nerve.

A young person met with a cut with a penknife, in the forearm, above the wrist. A violent pain was felt in the forearm, wrist, and fingers; this was soon followed by spasms, while the voluntary motions of the fingers were either incomplete or impossible. Subsequently the spasms became general, and for two years the individual led a most miserable existence; the case being ultimately cured by repeated applications of the actual cautery. Similar symptoms occurred in a young woman, from a wound above the wrist, inflicted by a piece of glass, probably injuring the median nerve; and the following detail is drawn up from the case of a woman at present in La Pitie, with a similar affection, having been bled at the fold of the arm some time previously.

Diagnosis.—The symptoms of a puncture of a nerve cannot be confounded with those of any other disease. A wound corresponding with the site of a nerve, the pain propagated along the nervous filaments, tremblings, and convulsive movements, which resist all ordinary remedies, leave no doubt of the nature of the affection.

Prognosis.—The accident is in general exempt from danger, the pain ordinarily ceasing after a few hours, or at most a few days, but the disease becomes very serious when spasm, convulsion, and neuralgic pains continue. The patient may become the subject of continual torment for many years, and of sufferings which produce their influence over the whole economy. Happily these results have been observed in a few cases only.

Treatment.—When the wound is recent—rest, antiphlogistics, and opiates, the latter being indicated when the symptoms persist; friction in the course of the nerve, with oil and laudanum, or hyoscyamus, bathing the limb with decoction of poppies or of belladonna, the application of morphine by the endermic method, &c. But the disease, when fully established, most frequently resisting these measures, there remains no other resource but to destroy the affected nerve; for which purpose cauterization or incision may be employed. The first, although most painful, destroys a greater portion of the nerve; one application rarely succeeds. When incision is resorted to it should be done two or three centimetres from the wound, towards the nervous centre. The purpose may, perhaps, be equally well effected by subcutaneous section. In the few cases in which this operation has been resorted to the symptoms have subsided the moment the nerve was divided.

Pathological Anatomy.—The puncture of a nerve produces a circumscribed tumefaction in its substance, with effusion of blood into the cellular tissue between the nervous filaments and within the neurilemma. When the symptoms of acute inflammation have subsided, and the absorption of the effused fluid has taken place, there remains, according to Wolff, Béclard and Descot, either in the whole thickness of the nervous cord, or, if the puncture has been very limited, at one point of its circumference only, a hard, opaque swelling, of a fibrous consistence, which is invariably formed by a thickening of the cellular-fibrous tissue. This tumefaction may furnish a useful indication where accidents of this kind have resisted the ordinary curative means.

(*Condensed from La Nouvelle Encyclographie.*)

4.—*On the Treatment of Wounds of the Arteries.* By S. J. GUTHRIE, Esq., F. R. S.

(*General Conclusions in a Treatise " On Wounds of the Arteries of the Human Body, with the Treatment and Operations required for their Cure." Condensed from the Medico-Chirurgical Review*, Oct., 1846, p. 475.)

1. The Hunterian operation is not applicable to the treatment of a wounded artery, inasmuch as the wound of the artery communicates with the external parts, and nothing intervenes to prevent blood flowing from the wound in its side, or from its cut extremities.

2. When a large artery is divided and bleeds, the wound should be enlarged if necessary, and a ligature placed on both the ends; but if the artery be not divided the ligatures should be applied, one immediately above, the other below the injured part. The artery may or may not be then cut across at pleasure, and the limb or part should be placed in a relaxed position. A bandage should not be applied, but the edges of the wound should be simply brought together by adhesive plasters, which do not extend completely round the limb.

3. No operation is to be performed on any artery unless it bleeds at the moment of its performance, inasmuch as hemorrhage once suppressed may never return.

4. The intervention of muscular fibres, or of whole muscles, is not a sufficient reason for tying the artery at a distant part. They must be divided, if it be possible, to the extent required for a due exposure of the injured artery and its accompanying veins and nerves.

5. If the wound pass indirectly to the principal artery, from the back of the thigh, for instance, to the femoral artery in front, or from the outside of the arm to the humeral artery on the inside, the surgeon may (on satisfying himself of the part likely to be injured by the introduction of a probe) cut down on the vessel opposite the part supposed to be wounded. When the artery is exposed the probe will point out the spot at which the vessel has in all probability been

wounded. Pressure made below this spot on the artery will cause it to be distended and to bleed, if the flow of blood be not prevented from above ; the artery is then to be secured by two ligatures, and the lower one should, if possible, be applied first.

6. The tourniquet should never be used in an operation for aneurism, or for a wounded artery. Compression by the hand in the course of the wounded vessel is allowable.

7. The blood from the upper end of a divided artery, is of an arterial color.

10. The blood from the lower end is of a venous color, when it happens to flow immediately after the division. Subsequently it may assume more of the arterial color, but it rarely does so for several days after the receipt of the injury, and always, or at least until a very late period, flows in a continued stream.

11. This regurgitation of blood from the lower end of a divided artery is a favourable sign ; it shows that the collateral circulation will probably be sufficient to maintain the life of the extremity.

12. The collateral circulation is in almost every instance capable of maintaining the life of the upper extremity when the axillary artery is divided, and the color of the blood which flows from the end of the artery, on its being divided, is not always as dark as in the lower extremity, and it sooner resumes its arterial color.

13. The collateral circulation is not always capable of maintaining the life of the limb when the femoral artery is injured. The best assistance which art can give is to rub the foot and leg, in the gentlest manner, between the hands of one or two strong young women for several hours, or even for the first three or four days, relaxing this process very little even during sleep. When the vein is divided at the same time, or rendered impervious, the limb usually mortifies.

14. The collateral circulation is sufficient to maintain the life of an extremity in almost every case in which an aneurism has existed for eight or ten weeks, although it may be incapable of doing this if the principal artery has been suddenly divided, without any previous disease having existed in the part.

15. The theory and the operation for aneurism are never to be applied in the treatment of a wound which has caused a diffused or circumscribed aneurism, *whilst the external wound communicates with the artery*, unless it be impractible to tie the bleeding vessel.

16. When an artery has been wounded, and the external opening has healed for weeks or months, so as to give rise to a diffused or circumscribed aneurism, this may be treated according to the theory of aneurism occurring from an internal cause, if the case will permit it without danger, although with one difference, that as the artery is sound the operation may be performed close to the tumor. If any doubt exists as to the capability of the collateral circulation after the external iliac has been secured by ligature, to support the life of the lower extremity, the operation should be performed at the injured part by opening the swelling and enlarging the wound, as in the case of a wounded artery.

17. When circumscribed or diffused aneurism has formed after a wound has been opened, whether by accident or design, it is placed in the situation of a wounded artery, and should be treated as such. If the aneurism has arisen from disease of the vessel, and the wound or opening into it cannot be permanently closed, the limb is in a worse state than if the artery had been wounded by accident, because a ligature or ligatures placed on a diseased artery are little likely to be successful. They are liable to all the difficulties and inconveniences attendant on the old operation for aneurism. If a case of the kind should occur in a popliteal or femoral aneurism, situated at or below where the artery passes between the triceps and the bone, amputation, if it can be done low down, will be the best remedy. If the aneurism should have formed higher up, and the opening can be closed with any prospect of its healing, a

ligature may be placed upon the artery above it; but on the recurrence of hemorrhage, which cannot be restrained by moderate pressure, the artery must be tied below, or recourse had to amputation. It is, however, to be observed, that amputation, under these sircumstances, when resorted to as a third operation, rarely succeeds.

18. When an artery is wounded with a simple fracture of a bone, or with a comminuted fracture of smaller bones with an external communicating opening, both ends of the artery should be secured, and the limb treated in the usual manner.

19. When the bone broken is the femur, and the artery divided is the femoral artery, the operation of amputation will generally be advisable. It will always be so if the fracture is a comminuted one, or the shaft of the bone is extensively splintered.

20. When the broken bone iujures the artery and gives rise to an aneurism, the treatment is to be first of the fracture and then of the aneurism, as soon as circumstanes render it advisable or necessary to have recource to the operation for aneurism, which can only be after time has been given for the collateral branches to enlarge, so as to maintain the life of the limb.

21. When mortification takes place in addition to, or as a consequence of, a wounded artery, amputation should be had recource to forthwith.

22. The place of operation should be in almost all cases at the seat of the original wound, but there may be an exception, viz.—

23. When the injury has been merely a cut, just sufficient to divide the artery and veins immediately below Poupart's ligament, and mortification of the foot supervenes, amputation should be performed below the knee, or at the part where the mortification more usually stops for a time.

This rule is founded on the observation that great efforts are made by nature to arrest mortification a little below the knee. Sometimes they succeed: when they fail death is almost inevitable. The advice to amputate at this part is founded on the fact of its being infinitely less dangerous when done there than on the thigh, independently of saving a joint.

24. When mortification has *continued for several days*, and is spreading without having stopped, the constitution of the patient being implicated, as marked by fever, amputation should not be performed until the mortification has been arrested, and the line of separation well formed. In many cases, where there is great weakness or irritability of constitution, it will be advisable to defer the operation to a later period, particularly if there be hope of the patient's becoming stronger and more tranquil.

25. If the mortification has once stopped, and then begins again to spread, it will never again cease to extend, and amputation may give some chance of life.

26. Amputation of the arm should never be had recource to in consequence of a wound of the axillary artery, unless mortification takes place.

27. When mortification takes place after the operation for aneurism, the surgeon must be guided by the state of the patient's constitution, in resorting to, or refraining from, amputation.

28. When hemorrhage occurs from the surface of a stump, the artery should be tied at the part from which the blood comes in the first instance, if it can be easily done. If this should not suffice, the artery must be tied higher up, just at such distance as will afford a fair hope of its not having been affected by the derangement of the stump, which has led to the failure of consolidation in the extremity of the artery, yet not too high to admit of the junction of any large collateral branches. If the bleeding proceeds from several small vessels, and cannot be arrested, the principal trunk should be tied immediately above the diseased part, and the patient removed to a purer atmosphere, without which an operation rarely succeeds.

29. When an aneurismal tumour mortifies it is improper to tie the artery above the tumour, because it will be obliterated if the mortification be arrested by the efforts of nature, which the operation may interfere with, and even prevent, whilst, if the mortification spreads, it will be a matter of supererogation, and only hasten the patient's dissolution. When an aneurism inflames, is opened by ulceration, and bleeds profusely, so as not to be arrested, it is a proper case for amputation, if such an operation can be performed. (p.85.)

5.—*Inhalation of Sulphuric Ether—Estimation of the great American discovery in London and Paris.*

[We find the Foreign Journals full of the accounts of the wonderful effects produced by the inhalation of sulphuric ether. It is impossible for us to notice a tithe of what has been written about it. The following summary extracts will show that it has struck the medical *savans* of Europe with astonishment. It has been applied to other purposes besides producing insensibility with the view to undergoing surgical operations. It has been used beneficially in various neuralgias, hooping-cough, other spasmodic coughs, to mitigate uterine pains whilst turning the child &c. Indeed every body is on the *qui vive* for its wonderful effects, and every day brings to light some startling development. We learn that a student of M. Velpeau of Paris has succeeded in producing insensibility, whilst he still retained *perfect consciousness*, and has actually been seen *dissecting and studying* anatomy upon his own person

Wonder what the wise man of old would say now about "*new things under the sun*," if he could return to the earth ?]

Etherization in Surgical Operations.

"Some hundreds of operations without pain have been performed in America. Capital and minor surgical operations have also been performed in this country, at University College Hospital, King's College Hospital, Guy's Hospital, St. George's Hospital, the Westminster Hospital, Addenbrooke's Hospital, Cambridge, Richmond Hospital, Dublin, the Bristol General Hospital, Birmingham, and Wolverhampton, and by many individuals in private practice, and the operations are increasing in number daily, so that there can be no reasonable doubt of the reality of the new mode of producing temporary insensibility to pain. Medical men have always been seeking for this priceless treasure to sugery—the greatest numbers of our profession, in ancient and modern times, have sought, and hitherto sought in vain, for any practicable and safe method of conquering the horrors of the operating-room. Up to this time there were no means of alleviation, except the administration of narcotics and intoxicating drinks, the compression of the nerves, celerity in operating, and the division of the nerves at the first stroke of the knife. It at once stands confessed, that the inhalation of sulphuric ether vapour for a few minutes, which is found to produce a transient but total suspension of sensation, without any ill results, is vastly superior to all measures previously devised for this purpose.

"As to the possibility of fallacy. There can be no doubt that the most terrible operations are much less painful, in reality, than they are imagined to be; that, in fact, the emotion of fear supplies a great part of the pain suffered under the operating-knife. Hence men and women with strong power of will have often had the most formidable operation performed upon them without a murmur.—In such cases, it is not merely the heroic endurance of pain, but the emotion of fear being absent, less pain is actually felt. With timid people, an assurance, or perfect faith, that no pain would be felt, would have much the same effect; but in the case of the operations upon etherized persons, they have been too numerous, and the effects have been too uniform, to admit of any valid doubt respecting the reality of our acquisition, in the inhalation of sulphuric ether vapour, of a new anodyne of great and remarkable power.

"The respiration of gases and vapours is by no means a new idea. At the end of the last century, it is well known that what was called pneumatic medicine, or the treatment of disease by the inhalation of gases and vapours, attracted great attention. BEDDOES, JAMES WATT, and HUMPHRY DAVY, devoted themselves to the matter with the utmost enthusiasm: it was in the pneumatic institution founded by Dr. BEDDOES, at Bristol, that DAVY laid the foundation of his subsequent fame. Again and again the young chemist risked his life, and on some occasions was barely rescued from death, in his reckless inhalation of gases which we now know to be poisonous. DAVY'S investigation of the gases led to some of the most brilliant discoveries in modern chemistry, and gave the world the safety-lamp as a preservative against the inflammable gases, but it fell short of the present acquisition. Since the time of DAVY and BEDDOES, the inhalation of vapours and gases has been little practised, except by empirics. Thus it has frequently happened in science: when a few eminent men have, with a kind of æstrum, given their attention, at a particular time, to special subjects, they come to be neglected for awhile, from the apparent hopelessness of making out anything fresh, or of finding a harvest in fields newly reaped.

"An eminent philosopher gave it as an axiom—"Always examine what others reject as worthless; and it has certainly been in what seems the most inconsiderable sources, that the greatest discoveries have been made. GALILEO made his great discovery of the uses of the pendulum by watching the swaying of a chandelier; COLUMBUS saw the new world in the floating of a sea-weed; NEWTON drew some of his splendid discoveries in optics from a soap-bubble; BELL'S discovery glanced into his mind from a diagram. But simple as all important discoveries seem in their elements, and when they are known, it is undoubtedly one of the highest qualities of the human mind, to strike out from the observation of common things latent truths never before discerned, and to perceive uses and applications in things previously trifling or hurtful. And though this quality of the mind may be exercised for the meanest or the noblest ends, there being a kind of chance or fortune in the results, the world has always measured the fame and reward of such achievements by their utility to the human race. The discovery of Dr. MORTON, the hitherto unknown dentist of Boston,—more striking to the general than to the scientific mind,—will, undoubtedly, be placed high among the blessings of human knowledge and discovery. Sulphuric ether has long been used as an anti-hysterical remedy of ordinary power; the inhalation of the weak vapour of ether was known as a toy, and sometimes used by chemical youths for mere frolic; and it was known also, in a concentrated state, as one of the narcotic poisons. By a new and happy application and generalization, this drug, or toy, or poison, has been invested with fresh powers, which almost realize the fabled Lethe. From being one of the playthings of knowledge, it has been metamorphosed to one of its greatest triumphs; it has been, at one leap, transferred from the pages of toxicology to the latest, and almost the fairest, page of the healing art. That its discoverer should be an American is a high honor to our Transatlantic brethren; next to the discovery of FRANKLIN, it is the second and greatest contribution of the New World to science, and it is the first great addition to the medical art.

"There can be no doubt of its peculiar applicability to surgery. Some have thought of its extension to medicine, and tetanus and hydrophobia have been mentioned as diseases likely to be benefitted by its use. Any such trials will assuredly end in disappointment; these diseases being diseases of motion, not of sensation, and remaining with an intensity as perfect during insensibility as in complete consciousness. It may possibly be used hereafter in some cases of operative midwifery; but here it would have to be tried with the utmost care and caution, owing to the tendency to convulsions belonging to the puerperal state. Etherization, as far as we yet understand it, simply obviates pain.

In all operations, pain is one, and the principal and perhaps the only, source of *shock* to the system. We say, perhaps, because it is possible that shock may affect the spinal and ganglionic systems after the influence of the cerebrum has been entirely withdrawn. This is a point which must be made out: if it is found that operations produce some amount of *shock*, even when sensation is withdrawn, it will then come to be an important question whether an operation performed with great rapidity produces a lesser or greater shock than one performed with some deliberation. It is by no means impossible but that it may prove that a sudden operation—a sudden shock—performed even during a state of insensibility, produces more injury than a less hasty one. It must be confessed that this question has been in some measure lost sight of, and that celerity has been the prime and great object with operative surgeons. We have said that in surgical operations the great object is to escape the pain; but in some diseases and states of the economy, the presence of pain may produce beneficial results; the existence of physical pain may excite some physiological action beneficial to the patient. We may take an instance of this from parturition. Where there is danger of laceration, the pain excites the patient to cry out, and when the cry opens the glottis and emits the air of the chest, the thorax is no longer a fixed point for the muscular actions, so that a sudden removal of the pressure, threatening laceration, occurs. This instance is sufficient to show that even pain is not in all cases an unmitigated curse.

"The insensibility produced by etherization appears to be of a peculiar kind, and to vary considerably in different individuals. In some cases, there is perfect insensibility to pain, and entire loss of consciousness; in others, the operation is felt, and the patient is aware of the different steps of the proceeding, but the pain is extremely slight, merely a sensation of scratching; while some describe the etherized condition as one of partial consciousness of the most exhilarating and agreeable kind; and a few describe their sensations as almost similar to those produced by the inhalation of the nitrous oxide gas; and there are cases in which it appears to act as a violent stimulant, or else it does not act at all. The form of insensibility will have to be examined hereafter by therapeutists, and the etherous inhalation will take its proper place amongst anodyne or narcotic remedies. Probably, the variations observed in different persons may depend, in some measure, on the amonnt of etherous vapour inhaled, rather than on differences of constitution, just as opium or alcohol may be made to act as exhilarants, or as causes of profound coma. In its effects, it is more comparable to opium or alcohol than ether medicines; as regards the celerity and duration of its effects, it strikingly resembles the laughing gas. Its great feature—that which commends it as an alleviator of pain in operations—is the rapidity with which a great degree of insensibility may be produced, and the celerity with which the coma, or unconsciousness, disappears, leaving no mischievous traces of its presence behind. As a measure of insensibility to pain, we must be entirely guided by the credibility of the patient, and his own subsequent account of the matter; some have argued pain from the presence of twitching of the limbs and features during operations; others, from noises and cries uttered by the patient. It is however, strictly physiological to believe that contortions of the face and limbs may take place during operations, as purely reflex acts, without any volition or emotion of pain. There may even be a cry, apparently of pain, without consciousness, in the same way that epileptics frequently cry out before falling into convulsion, without any subsequent recollection whatever of pain or fear.

"A patent has been taken out for the discovery, the American patent standing in the names of Dr. C. F. Jackson, of Boston, one of the most celebrated physicians in the United States, and Dr. Morton, the discoverer. This question of patent is a stain upon the whole matter. We trust it will speedily be relinquished; and we are assured, that for the patentees, to attempt to maintain it by law would be most preposterous, and impossible. If it should realize all the present anticipations of temperate and calm judging men, the thing is far too noble to be clogged with a mere commercial transaction. Not that Dr. Morton should pass unrewarded; he deserves, if his discovery stand the test of time, the gratitude and reward of every civilized people and government upon the face of the earth; he will have, we should hope, too strong a claim on their spontaneous gratitude, to need a resort to compulsory reward. How much more imperative would his claim be than the demands of those who have merely invented some new engine of destruction!—*London Lancet.*

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(From the Gazette des Hôpitaux.)

"*Inhalation of Sulphuric Ether to prevent pain during Surgical operations.* The following is a digest of the discussions of the learned societies of Paris concerning the properties of this agent, and of hospital reports, contained in the *Gazette des Hôpitaux* for the last month:—

"*Académie de Medécine, Meeting, Jan.* 12*th*, 1847.—M. Malgaigne announced that he had tried upon five patients the method proposed by the Americans, to render surgical operations painless. The first patient was a young man, aged eighteen, who was afflicted with an abscess at the lower part of the leg. He breathed the ether for two minutes, which sufficed to plunge him into a state of complete lethargy. The abscess was opened with a bistoury. In half a minute afterwards, the patient woke up, and affirmed that he had experienced no pain, and up to that time believed he had not undergone any operation, but wished it to be proceeded with.

"The second, an Italian, a little older, who had a tumour in the neck, respired the ether for five minutes. By the time he had revived, the operation was finished. He said he felt that the tumour had been removed, but had not experienced any pain.

The third patient was a young woman, also having a tumour in the neck.—She did not fall into a state of insensibility until she had inhaled the vapour for eighteen minutes. She did not feel the first incision; but woke up immediately afterwards, and suffered during the rest of the operation as though she had not been submitted to its influence.

"The fourth was a man who had had his leg broken by a rail-way truck, and underwent amputation at his own desire. He was submitted to the vapour of ether for seventeen minutes. On his coming-to, he said he had felt the operation, but had not suffered more pain than if he had slightly scratched his leg with the point of a knife.

"The last, a young man, who was operated upon for strabismus, previously respired the ether for ten minutes without effect, and suffered during the operation as other patients would have done. In answer to a question from M. Nacquart, M. Malgaigne explained the process as used by the American surgeons, which he had adopted for the first patient; but for the others he had introduced into one of the nostrils, the other being closed, a tube leading from a vessel, the bottom of which was covered with ether—the patient inspiring by the nose, and expiring by the mouth.

"M. Velpeau questioned whether ether was altogether innocuous to the system. He feared it might produce some injurious effect upon the patient, counterbalancing the advantage derived from the absence of pain. Besides, as the influence only lasted for a short time, its use in operations of a long duration was doubtful.

"M. Guibourt had no fear of a bad result from the employment of ether, his only anxiety was as to the certainty of its operation, he himself having frequently and for a long time inhaled air strongly charged with ether, without experiencing any ill effect; and on this point he was supported by M. Chevallier.

" M. Roux, of the Hôtel Dieu, detailed the particulars of a case of compound fracture of the leg in the *Gazette des Hôpitaux*, January 16th, 1847. The patient who was about forty-five years old, breathed by the mouth the vapour for twenty minutes with great earnestness ; in about ten minutes his eyes closed, but he still answered any questions put to him, and in ten minutes more the operation was finished ; the pain thereof being evidently diminished, as the patient was not aware that the operation had been completed, until he was told such was the case.

" *Hôpital St. Louis.*—A patient of M. Malgaigne—a man about thirty-five years old, of strong constitution, presented at the lower and internal part of the leg, about the level of the malleolus, a phlegmonous abscess—was submitted to the influence of the etherial vapour for two or three minutes, which short space of time sufficed to put him into the state necessary for the commencement of the operation, which can only be compared to drunkenness. M. Malgaigne addressed the patient, asking him whether he felt any particular sensation, or found his sight confused. The man having answered that his vision was imperfect, M. Malgaigne immediately used the bistoury, making an incision in the abscess and in a portion of the skin much supplied with nerve and considerably inflamed. He then pressed the pus from the abscess. On the termination of the operation the patient appeared agitated—his face was red, his features contracted, his eyelids closed, and, in short, his whole muscles, particularly those of the face and superior-extremities, exhibited symptoms of abnormal contraction. He seemed to be under the weight of painful feelings which he wished to shake off. He had undoubtedly lost his reasoning power, for his conduct was outrageous—he closed his eyes, and foamed from the mouth. This state lasted but two or three minutes. He was aroused by the voice of M. Malgaigne, and on recovering his consciousness he declared the pain was slight, not more than a prick, but he directly afterwards complained of the smarting which resulted from the wound.

" A second patient, a man aged forty-five, had necrosis of the bones of the finger, resulting from a whitlow. M. Malgaigne determined to remove the finger, at the articulation, and caused the patient to inhale the ether for about four minutes. The patient declared he was drunk and unable to see. The operation was performed in the usual manner. This patient also compared the pain he felt to a prick, afterwards experiencing the sensation of smarting over the surface of the wound. The pulse was carefully observed during the operation, and found to be eighty-eight during the inhalation of the ether, and ninety-two after the operation.

" A young girl of eighteen, with an affection of the hand, requiring incisions, was submitted to the action of the vapour, and in four minutes she declared her sight to be confused. She compared the operation to a prick. It is remarkable that in this case there was a considerable want of sensibility in the wound for some time after the operation.

" M. Malgaigne administered wine in each of the last cases, to effect a more speedy recovery from the stupor ; and it may be worthy of notice that the air exhaled was impregnated strongly with ether, so that it was impossible to mistake the agent that had been employed.

" At the meeting of the *Académie de Medécine*, on Jan. 19th, a letter was read from M. Menière, respecting the successful employment of vapour of ether in cases of nervous deafness, hemicrania, paralysis of the facial nerve, complaints of the cavity of the cranium, &c.

" M. Honoré mentioned a case of intense neuralgia, which was alleviated by the breathing of the vapour of ether, placed in a vessel with a large mouth, held close under the mouth.

" M. Malgaigne made the following important observations as to the consequence of the use of ether :—In the case of amputation of the leg, he believed that there was less reaction than in ordinary cases; and another point, which he recommended to the attention of psychologists, was, that in most cases it appeared that the seat of sensation for pain was different from the seat of ordinary sensibility. Many patients retained perfect consciousness, understanding what was said to them, answering correctly, but feeling no pain; it really appeared to him that there were two centres of sensation.

"A discussion also took place at the meeting of the *Academy of Sciences*, on January 18th, when M. Velpeau stated that he had failed in obtaining a complete and satisfactory result from the use of the vapour of ether. One patient had proved unmanageable. In another, the sensorial functions were evidently disturbed; but he had suffered pain while being operated on. A third had suffered in a like manner; but declared that he was plunged into such a state of ecstasy, that he was unable to complain. In short, it appeared that it succeeded with certain persons, and failed with others; and that it was not proved to be altogether without danger.

"M. Dunos said that he had been led, from some experiments, to believe that the ether possessed a cataleptic power.

"On the 22d of January, M. Velpeau, at the *Hôpital du Charité*, having used an apparatus constructed by M. Charrière, succeeded perfectly in removing a tumour without pain. The etherial vapour was inhaled by the patient about four minutes, after which time complete insensibility and relaxation of the muscles were manifested.

"M. H. Larrey, who assisted at the operation, suggested the valuable assistance that the agent would render in the reducing of difficult luxations.

"At the *Hôpital du Midi*, a case occurred in which the sensibility seemed to have been exalted by the inhalation.

"The influence of the vapour has also been tested by M. Guersant, at the *Hôpital des Enfants*, on two children. One child, whose finger was amputated, declared that she felt pain, but was totally unable to cry out. The other, on recovering from the state of insensibility into which she had been thrown, declared that she had no recollection whatever of the operation.

"At the *Hôpital du Midi*, M. Ricord, in injecting in a double hydrocele, employed the inhalation with success, though he was obliged to renew its influence twice during the operation. A second patient, afflicted with single hydrocele, after respiring vapour for thirteen minutes, fell into a complete state of insensibility; the limbs were relaxed; the pupils contracted; the conjunctiva was injected; and the pulse not affected. A third patient, who was apparently perfectly under the influence of the ether, suffered the usual amount of pain during an operation for removing a tumour from the rectum.

"In the first two cases, a state of intense exhilaration preceded that of insensibility. In the last, the use of the ether was followed by sickness and fainting.

"At the meeting of the *Societé de Chirurgie de Paris*, Jan. 13, 1847, M. Malgaigne mentioned a case in which the inhalation having been continued for too long a time, caused sinking of the pulse and coldness of the extremeties to such an extent, that fears were entertained for the life of the patient.

"At the meeting of the *Académie de Medécine*, Jan. 26, 1847, M. Landouzy called attention to a case, where hæmorrhage, after removal of a small tumour from the mastoid process, did not come on till half an hour after the performance of the operation; and suggested that surgeons should be on their guard lest accidents might happen from the arteries not being secured.

"M. Honoré stated, that he had succeeded in relieving a patient afflicted with most obstinate neuralgia of the face, by the inhalation of ether vapour for about two minutes.

"*Académie des Sciences*, Meeting, Jan. 25.—M. Gerdy related several experiments with the vapour of ether, the results of which had been satisfactory in the highest degree.

"From the above it will be seen, that the success which our neighbours have met with has been varied: but we think that in most of the cases in which the ether failed to produce its stupifying effect, that fault was clearly in the instrument used for its administration. At first they attempted to use this agent by causing the patient to inspire by the nostrils, and respire by the mouth, and *vice versâ*; but afterwards they found it requisite to close the nostrils while respiration was carried on by the mouth alone.

"The fact announced by Malgaigne, that some of the patients retained their conciousness, but felt no pain while being operated on, is most interesting, and we leave it to be commented on by physiologists; but the statement requires confirmation by other observers.

"The influence, also, over the power of *expression* of pain, is very curious, but seemed to be quite an uncommon result; for we only find that this was the case in two out of the numerous cases quoted.

"The continued lethargy, with failing of the pulse, and coldness of the extremeties, is certainly a most awkward complication to deal with in treating the shock of an operation, and one which should make us cautious in the employment of the vapour of ether. In this country, in more cases than one, this unpleasant effect followed its use.

"If the vapour of ether prove an efficient therapeutic agent in the treatment of neuralgic affections, then, indeed, will its introduction prove a boon to society. And we much regret that M. Honoré did not give a more detailed account of those cases in which he employed this remedy.—*London Lancet.*

AMERICAN MEDICAL INTELLIGENCE.

1.—*The Discoverer of the Effects of Sulphuric Ether.*

[Communicated for the Boston Medical and Surgical Journal.]

We find an advertisement in one of the papers, that Drs. C. T. Jackson and W. T. G. Morton have made an important invention which has been patented. In justice to a fellow townsman, I will give its true history. The first announcement publicly made was by myself, more than a year since, in an article written for the purpose of establishing the doctrine that in disease the vital power is diminished, and suggested that in all probability pain was but a peculiar depressed state of the sensor nerves, and in proof stated that stimulants acting upon this system, and having a certain relation to it, would relieve or prevent suffering; and that the dentists of Hartford were in the habit of administering nitrous oxide gas, which enabled them to extract teeth without the conciousness of the patient. The original discoverer of this was Horace Wells, dentist in this city, and he tried the first experiment upon himself. After the idea suggested itself to him, he debated for some time which to use, the gas or ether, but preferred the former as he thought it less liable to injure the system. Being now satisfied of its powers, he went to Boston for the sole purpose of introducing it to the faculty. He presented it to Dr. Warren, who laid it before his class, but the experiment first attempted partially failing, and no one seeming willing to lend him a helping hand, he ceased making any further personal efforts. He especially made known his discovery to Drs. Jackson and Morton, neither of whom had any idea of it until this moment, and must allow Dr. Wells the whole merit of the thing up to this point. We see by the Journal that Drs. J. and M. call their invention a peculiar compound. I was fully satisfied that sulphuric ether was *the* article, as it was known to be among the ingredients, and being there, nothing else was wanting to produce the desired effect. The claim, as published, sets the matter at rest; ether, and ether alone is used, and the world will easily judge how much right Drs. Jackson and Morton have to patent it. Had they been the first to discover the fact that any gas would produce exemption from pain, and had made it known, they would have deserved commendation. They have not done this, nor justice to the true discoverer. Is there any merit in using ether in place of nitrous oxide gas? Certainly not, for the properties of the two things are so alike in this respect, that one is constantly used for the other, and for months I supposed our dentists were using both; and the idea of allowing any man a patent for the use of the one after the effects of the other were known, is preposterous. Dr. Wells' experiments were numerous and satisfactory. One fact discovered, is extremely interesting. It is that however wild and ungovernable a person may be when taking the gas, simply for experiment, he becomes perfectly tranquil when it is inhaled before an operation: that the mind being prepared, seems to keep control over the body, indisposing to any effort.

Unfortunately it is too true, that mystery, as of a nostrum, is frequently required to induce people, and sometimes the profession, to notice an improvement, and thus far perhaps thanks are due Dr. Morton for compelling attention; yet we must give Dr. Wells the credit he justly deserves of making the discovery, spending time and money in its investigation, and then in nobly presenting it to the world. It is to be hoped every other gas and substance capable of exciting the nervous system may be experimented upon, but we hope no one will think of patenting any if discovered to be similar in its operation.

Hartford, Dec. 9, 1846. P. W. Ellsworth.

[We find no contradiction of the statement here set forth, in any subsequent number of the Boston Journal. If it be true that Drs. Jackson and Morton derived from Dr. Wells the idea of applying the inhalation of sulphuric ether to operative surgery, and afterwards robbed him of the discovery, under the contemptible disguise of *patenting a compound fluid*, they have been guilty of an ungenerous, if not ignominious act, and deserve to be classed with the Chamberlains who perished with the secret of their female forceps, instead of the illustrious Jenner, who blessed the world with the discovery of vaccination. *Suum cuique.*—N. O. Edrs.]

2.—*Dr. J. Mason Warren on the Inhalation of Sulphuric Ether.*

Dr. Warren gives in the April number of the Boston Medical and Surgical Journal, nineteen cases in which surgical operations were performed under the inhalation of ether; the most of which were favourable, but some not. We give the last of his cases which is very remarkable, and the "results" of his observations.

"Case XIX.—On Saturday, March 6th, Dr. J. C. Warren applied the actual cautery at the Hospital to a patient with a paralytic affection of the lower extremities arising from a caries of the dorsal vertebræ. Previous to the application his pulse was 120, increased perhaps by the excitement of being brought up stairs into the amphitheatre. He inhaled ether with difficulty, so that about ten minutes were required to render him insensible. Dr. Warren then applied successively three irons heated nearly to a white heat, slowly over the back, scoring it, as it were, in different directions. As the flesh smoked, and the hot iron hissed on the surface, the spectators could not but be struck with admiration at the entire immunity from pain, and perfect stillness of the patient, while subjected to this apparently most painful and appalling resort of surgery.

On his return to consciousness he was quite ignorant that the operation had been done, said he had been in a delightful dream, and that his sensations were more than they had been for a year.

" *Results.*—We need not dwell upon the mode in which ether produces the peculiar effects recorded, whether immediately through the nerves of the mucous membrane, or by being imbibed into the pulmonary blood. Dr. Charles T. Jackson informs me, that in order for the inhalation to produce the full and desired effect, rectified sulphuric ether in its purest state should be used, entirely free from alcohol. It thus becomes much less irritating during the inhalation, with more decided effects at the time, and none of those subsequent unpleasant symptoms, which would occur from ether in its ordinary state.

" The following conclusions present themselves, which admit of being arranged under three distinct heads.

" A. *As to Age and Temperament.*—1st. Young children and females seem most easily to be brought under its influence.

" 2nd. Females of nervous temperament are not unfrequently brought to a condition closely resembling hysteria.

" 3d. Men of powerful muscular frames sometimes become violently frantic at first, requiring the exertions of several persons to restrain them. This state is succeeded by one of semi-consciousness, but also of insensibility to pain.

" B. *As to the Method to be employed.*—1st. The use of the ordinary inhaling apparatus seemed in many cases to occasion at first irritation and choking.

" 2nd. This irritation either does not exist, or in a less degree, in cases where cloth or sponge has been used, which has been pretty extensively employed at the Hospital and in private practice by Dr. J. C. Warren and myself.

" 3rd. In many cases it is impossible, in consequence of the tender age of the patient, or his refractory nature, to make him comprehend the use of the ordinary apparatus, and here the cloth or sponge will be found of great service.

" 4th.—A quieting effect is produced even when ether is sprinkled upon the bed clothes, or a sponge moistened with it is laid upon the pillow; thus sometimes superseding the use of an opiate.

" C. *As to its Influence and Effect.*—1st. When the effect is perfect, the patient having recovered, generally expresses himself as previously under the influence of a pleasant dream, notwithstanding the severity of the operation.

" 2nd. A partial effect is often produced, in which the patient is entirely free from pain, but yet conscious of the different steps of the operation. Uneasiness, apparently the result of suffering, is in these instances generally referred to a disagreeable dream, or to some cause not immediately connected with the operation.

" 3rd. In some cases asphyxia is produced, requiring the admission of fresh air, with the use of frictions, to the patient, and in the most severe cases the internal administration of stimulants.

" 4th. Repetition increases the susceptibility, so far as observed.

5th. No limit of time can as yet be assigned at which its use becomes unsafe In one instance here recorded the patient was kept under its influence for a half, and in another for three quarters, of an hour.

" 6th. The effect is generally evanescent, and the patient being spared the shock of severe pain upon his system seems to recover more rapidly than in ordinary cases. Out of more than fifty instances, which have fallen under my observation, I am not aware of a single one, in which its use has proved permanently deleterious.

" 7th. Various observations show, that when the patient is to undergo a severe operation under the influence of the ether, its application should be employed repeatedly before the day of the operation, as well to instruct him how to respire it thoroughly, as to ascertain the peculiar manner in which it affects him.

3.—*Prizes Offered by the Lexington Medical Society.*

Prize for the best Thesis.—The Lexington Medical Society resolved to offer a prize of Fifty Dollars, or a Gold Medal or Piece of Plate of that value, at the option of the successful competitor, for the best Thesis submitted for the degree of Doctor of Medicine, in the Medical Department of Transylvania University, for the Session of 1847–8. Those competing for this prize are at liberty to select the subject of the Thesis.

Annual Prize.—The Society also resolve to offer an *Annual* Prize of Fifty Dollars, or a Gold Medal or Piece of Plate of that value, for the best Original Essay, on a subject to be selected by a Committee.

In accordance with the above resolutions, the Commitee propose, for 1847, a Prize

For the Best Account of Continued Fever,

As it prevails in any of the States out of New England.

Remarks.—The Continued Fever of the Eastern States has been very carefully studied and very fully described. It is found to correspond exactly to the *Typhoid Fever* of France; and it is by that name that the disease is now generally called. The same disease is known to prevail extensively in some of the Middle, Western and South-Western States; but throughout these regions it has not yet been very fully or thoroughly studied. This prize is offered as one means of remedying this defect.

Other things being equal, the prize will be awarded to that essay which contains an authentic and complete account and analysis of the largest number of cases of the disease; its leading phenomena during life, with the lesions in fatal cases; its average and extreme duration; the sex, and age, and race, of its subjects; the season of its prevalence; and the prominent points of difference between it and periodical fever. It is very desirable that a careful examination should be made of the mucous membrane of the lower portion of the small intestines. Speculations about the remote and proximate causes of the disease, will not add anything to the value of the essays that may be offered. The description of the disease must be sufficiently full and minute, to enable the committee to judge of the accuracy of the diagnosis.

The committee also propose, for 1848, a prize

For the Best Account of the Several Forms of Periodical or Malarious Fever, in the U. S.

Remarks.—This subject has already attracted so large a share of attention amongst Physicians of the South and West, that some of them may be surprised at its being thus brought forward. But, extensively as malarious fever has been written about, there are many points of its natural history which need farther elucidation. Amongst these may be mentioned, particularly, the following:—the comparative liability of the sexes, of the black and white races, and of different periods of life, to the several forms of the disease; the influence of race upon its severity and danger; the relative proportions in different years and localities, of the three principal forms—intermittent, remittent and congestive; the most common type of the pure intermittent form; and the variations in the general character of the disease, in different seasons.

The Essays must be sent, post paid, to the Corresponding Secretary, Prof. J. M. Bush, on or before the first day of January, 1848, for the first, and 1849 for the second prize.

Each essay must be accompanied with a sealed address of the author. That of the successful competitor only will de opened. The essays will be considered as the property of the Society, to be deposited in their archives, or published in their transactions, as they may deem best.

The Committee may suggest, that a convenient opportunity for forwarding essays from a distance, will be presented by the annual assembling of the pupils of Transylvania University in the fall.

Medical Journals will confer a favor by copying the above.—*Western Lancet.*

4.—*Prize Essay of the Louisiana Medico-Chirurgical Society.*

At the anniversary meeting of this society held on the first Wednesday of April, it was ascertained that two essays had been received for the prize of one hundred dollars offered for the best essay on *strictures of the urethra*, but neither of them had come to hand within the time prescribed. It was therefore resolved to postpone awarding the prize for twelve months, and to continue the offer to the medical profession at large. Communications must be directed to the President of the Society, and be received by the 1st day of February, 1848. Those now on hand will be retained as competitors, unless otherwise ordered.

5.—*National Medical Convention.*

We are pleased to perceive the unanimity which seems to pervade the medical profession throughout the Union in regard to this important assembly. Every respectable medical college and almost every medical society will be represented. Our colleagues, Professors Harrison and Carpenter, will attend on behalf of the Medical College of Louisiana, and the Physico-Medical Society of New Orleans. The Medico-Chirurgical Society of Louisiana, at its anniversary meeting on the 7th April, passed unanimously the following resolution, viz:

"*Resolved*, That this Society highly approves of the great objects of the National Medical Convention which is to meet in Philadelphia on the 1st Wednesday of May next—and that any of its members who may have it in their power, be requested and authorized to represent this Society in said Convention."

We regret to say that none of the members of this society can leave home at this time. We earnestly hope that the utmost unanimity and harmony may characterize the convention, and that its deliberations may redound to the honour of the profession and the benefit of the community at large. Similar conventions abroad have met with approbation and support from government, but in this land of freedom we can expect but little aid from such a source. We must therefore *legislate for ourselves*, and every honourable member of the profession should feel bound to support the laws that are created. Other professions and societies do the like, and thus maintain their standing. And why may not as much be accomplished by the medical profession? Disunion has ever been the *bane* of our prosperity, but we fervently pray it may be so no longer. A weighty responsibility devolves upon every member who attends that convention; and woe be unto the man or set of men who raise any obstacles to the accomplishment of its great objects!—Let there be made an united and harmonious effort to elevate the standing of the medical profession to its proper rank among the vocations of men, and let a plain line of demarcation be drawn between the *honourable* and the *dishonourable* members.

All sectional feeling, sordid motives and local influences should be freely sacrificed for the general welfare of the profession.

6.—*Resection of the Lower Jaw.* By Dr. A. Mercier.

[On the 3d of February we were invited with several others, among whom were Drs. J. C. Cheesman of New York City and T. Rush Spencer of Geneva, N. Y., to witness this interesting operation. It was skilfully performed and has terminated in the most gratifying result, as will be seen from the following brief statement of the case and the operation.]

Antoine Ricardo, a native of Cadiz, Spain, aged 58 years, entered into the marine service at the early age of 8 years, and remained in it until 1832, at which time he took up his residence in New Orleans and pursued an active life to gain a support. He does not remember to have ever been ill. About the first of last October, when entering his house, he was seized with vertigo and fell upon the pavement, in a state of insensibility. The loss of a lower incisor tooth and marks of contusion on the lower part of the face, gave proofs that the chin had struck the floor in the fall.

The next day, the face was greatly swollen, particularly on the right side. The inflammation soon became so severe as to kindle up fever, which was combatted by leeches, cataplasms, &c. Six days after the accident, the pain about the jaw was so violent, that a physician deemed it proper to extract two large inferior molar teeth on the right side. A large abscess, which had formed on a level with the temporo-maxillary articulation, discharged its contents through the opening made by extracting the two teeth. Twelve days after the accident, a second abscess formed on the horizontal branch of the lower jaw on a line with the right commissure of the lips. This was opened by another physician; and from this abscess a spiculum of necrosed bone was discharged the following day. Finally another abscess formed a little to the right of the symphysis of the chin; it opened spontaneously and remained fistulous, as the second of which we have already spoken. From this time, the right cheek remained swelled, and the fistulæ continued to discharge a fœtid and badly digested pus, and the pains became so severe that the patient begged to have an operation performed.

Ricardo is a small, thin subject, of a highly bilious temperament, with greyish hair, and appears much older than he states himself to be. His jaws are so compressed, that he can take nothing but fluids. The whole of the right cheek is swollen, particularly on a line with the angle of the jaw. All the tissues seem blended together and have a lardaceous feel and appearance.

On a line with the second inferior molar and a little to the right of the symphysis of the chin, we find two fistulæ discharging a fœtid and sero-purulent matter.

The stylet, when introduced into these fistulæ, came in contact with the denuded bone, and this enabled Dr. Mercier to detect a necrosis, which extended from the symphysis as far at least as the angle. It could not be ascertained that the lesion extended any higher. The pains were so constant and severe, that the patient was unable sometimes to sleep half an hour. In other respects the patient was in the enjoyment of good health. On the 3rd of February, 1847, Dr. Mercier performed the operation in the following manner:

Seizing the lower lip with the thumb and index finger of the left hand, he separated the lip from the teeth, and introducing a bistoury, held in his right hand, on a level with the frenum, he carried the incision vertically down to the lower extremity of the symphysis, then changed its direction, and carried the knife along the border of the inferior maxilla as far as the lobe of the ear.

Scarcely had the bone been exposed, when the finger, thrust into the wound, enabled Dr. M. to detect the existence of a necrosis, which extended nearly to the tempero-maxillary articulation. The bone was divided with a chain saw, on the left of the symphysis, on a level with the first inferior left incisor. Whilst Dr. Mercier was making efforts to draw forward the right half of the inferior maxilla, the horizontal branch of the bone was separated from the vertical portion on a line with its angle. This part was separated from the soft parts and completely detached. Then seizing with the left hand the vertical portion which remained, Dr. Mercier drew it out and with the bistoury divided all the adhesions of the soft parts, as far as the articulation.

Scarcely a pound of blood was lost during the operation. Only the facial artery was cut, which was immediately tied and a small branch of the internal maxillary which is distributed to the tempero-maxillary articulation; slight pressure with the end of the finger sufficed to arrest the hemorrhage.

The wound after being cleaned, was drawn together by twisted sutures. The night succeeding the operation, the patient slept six hours. On the third day the wound was united by the first intention, throughout its extent, with the exception of the quarter of an inch, opposite the ligature. On the sixth day, the ligature came away; three days after cicatrization was complete, and on the 13th day after the operation the patient was well enough to take a walk on the levee. To-day, the 18th, the cure is complete.

There are two points worthy of special notice in this operation, 1st, the lower lip was left entire, as the penetration was made on a level with its origin; 2nd, the head of the bone was disarticulated without wounding the 7th pair of nerves. Dr. M. thinks there is no necessity of dividing the lip, and found no inconvenience from leaving it whole.—There has been no paralysis, and the contour of the face is scarcely at all altered.

F.

7.—*Retirement of* Dr. John Bell *from the Bulletin of Medical Science and Select Medical Library.*

We have heard with regret that Dr. John Bell has ceased to edit these interesting works, on account of continued ill health. He has been an honorable and indefatigable laborer in the field of medical science, and commands the respect and the sympathy of the profession.—We sincerely hope he may be restored to perfect health and many years yet be added to his useful life.

NEW ORLEANS, MAY 1, 1847.

The present number closes the third volume of our Journal. Our list of subscribers has been constantly increasing, though it is yet far from affording adequate compensation for the editorial labour expended on the work. The expense of publication is very great in this city: therefore delinquent subscribers must not take offence at frequent duns. The work cannot and will not be continued to those who do not pay.—We should like to extend our circulation at the North, where, from some cause, we have not received as much support as we anticipated. We may mention in this place that we observe in the Foreign Journals several extracts from *ours*, credited to our northern cotemporaries.

HEALTH OF THE CITY.

We have nothing extraordinary to note as to the diseases of the day since our last report. We have had about the customary amount of fevers, bowel complaints, pneumonia, bronchitis, measles, hooping cough, and some small pox. We shall hereafter give the List of Interments in the city, instead of the admissions, discharges and deaths at the Charity Hospital. All reports of cases from the different hospitals will be thankfully received.

The weather has continued very variable, and for the most part very bad. The Spring has been one of the most backward ever known in the South.

The river has been remarkably high for the season. It is now pouring in torrents along the gutters leading back to the swamp. We observed the same in the city of Lafayette a week or two ago. The river is said to be fully as high at Vicksburg as it was in 1844, and still rising. This is very extraordinary, as the Missouri and upper Mississippi are still low. The flood comes from the Ohio, Tennessee, Cumberland and Arkansas, and if it should not pass off before the usual spring rise of the Missouri and Mississippi, the consequences must be most disastrous. The levees have already given way in many places and much injury has been done in the upper Parishes. The rise will not reach this place for several days yet.

We learn from the papers that Yellow Fever is prevailing at Kingston, Jamaica, and that the British ship Endymion had been compelled to leave on account of it—the crew being already severely scourged.—We have no authentic account of Yellow Fever at Vera Cruz yet.

HEALTH OF THE COUNTRY.

We return our thanks to our obliging correspondents for the following communications:

MONTGOMERY, ALA., April 15th, 1847.

GENTLEMEN:—I send you a table of cases, embracing those occurring from the 10th of February to the 9th of April inclusive—compiled from the same sources as that for the last number.

Abscess 3, Abortion 2, Anthrax 1, Aptha (infant) 1, Bronchitis (acute) 8, (chronic) 2, Bleparoptosis 1, Cataract 2, Catarrh 1, Cancer 1, Cholera Morbus 7, Colic 7, Calculus (renal) 1, Caries (of mastoid process) 1, Dyspepsia 2, Dysentery 9, Diarrhœa (acute) 14, (chronic)2, Dysmenorrhœa 2, Difficult Dentition 1, Delirium Tremens 3, Engorgement of uterus 2, Engorgement of spleen (chronic) 1, Enteritis 4, Erysipelas 1, Epilepsy 1, Enlargement of prostate gland 1, Eczema (chronic 1, Fever (Intermittent simple) 37, do. (Pernicious) 1, do. (Remittent simple) 20, (Pernicious) 1, do. (Infantile) 7, do. (Scarlet) 4, Foreign body in ear 2, do. in nostril 1, Fracture (of malar bone) 1, Gastritis 3, Gastro-enteritis 2, Gonorrhœa 8, Hysteria 3, Hæmorrhage (uterine) 2, Hernia (inguinal) 2, Hypertrophy of heart 2, Herpes præputialis 1, Insanity 2, Irritable bladder 1, Immobility of inferior maxilla 1, Involuntary seminal emissions 1, Icterus 3, Inflammation of mamma 1, Leucorrhœa (vaginal) 4, Laryngitis (acute) 2, Mania (puerperal) 1, Necrosis 3, Neuralgia 4, Orchitis 2, Ophthalmia (catarrhal) 2, do. (gonorrhœal) 1, do. (scrophulous) 1, Otitis 3, Paronychia 3, Prurigo 1, Prolapsus uteri 1, do. ani 2, Parturition (nat.) 3, do. (tedious) 2, Phthisis pulmonalis 2, Parotitis 6, Pneumonia 10, Pleuropneumonia 1, Pleuritis 1, Retained placenta 1, Rubeola 1, Rheumatism (acute) 1, (chronic) 2, Rupture of Hymen (accidental) 1, Scabies 2, Stricture of urethra 1, Strabismus 1, Syphilis (primary) 7, do. (secondary) 2, Suppression of catamenia 2, Tumour (fatty) 1, Trismus nascent. 2, Tronsilitis 3, Ulcer (chronic of leg) 1, Urticaria 1, Vertigo 1, Wounds (incised) 2, do. punctured 2, do. contused 2, do, lacerated 1. In all 280 cases.

There were 9 deaths : two from Trismus nascentium, two from acute Pneumonia, one from acute Laryngitis, one from acute Gastro-enteritis, one from Diarrhœa, one from Delirium Tremens and one from acute Enteritis. It is needless to observe that there are still a few of the cases under treatment. W. M. B.

Memphis Ten., April 10th, 1847.

Gentlemen :—The plan proposed by Dr. Boling of Montgomery, Ala. and so cordially approved and recommended by you in the last No. of the Medical Journal, for health reports, is so convenient, and satisfactory and at the same time affords such a suitable basis for brief practical remarks, that I determined to adopt it and to furnish for each No. of the Journal such a table of cases, from this point.

The following list of diseases, contains only my own cases, and some of my partner's, (Dr. Frayser) which I saw, during one month, from the 9th of March, to the 10th of April.

Measles has been, and still is more prevalent as an epidemic, than I ever saw it. Many have taken the disease, who had frequently been exposed to it before. The number of cases that have required medical treatment have been greatly increased, by the complications with bowel affections, and Influenza, produced, no doubt, by the unusual amount of rain, and the coldness and dampness of the weather.

I have somewhat extended the proposed plan of health reports, by brief practical remarks—believing, that in this way every thing that is valuable, peculiar, or original in the practice of each physician making reports, may be recorded in direct connection with his cases, and in a concentrated and convenient form.

List of Diseases.—Arachno-Spinitis 1, Anasarca 2, Amputation of the leg 1, Bronchitis 12, Burn 1, Cataract 2, Convulsions (infantile) 1, Cancer (of the Pyloric orifice of the stomach) 1, Dysentery 6, Diarrhœa 10, Dysmenorrhœa 1, Fever (intermittent) 6, (remittent) 26, Gastritis (chronic) 4, (acute) 1, Gastro-Enteritis (acute) 1, Gonorrhœa 2, Hemorrhage (uterine) 1, (nasal) 1, Hemoptysis 1, Hematemesis 1, Influenza 16, Jaundice 2, Leucorrhœa 4, Parotitis 6, Menorrhagia 2, Rubeola 54, Neuralgia 5, Urticaria 1, Ophthalmia (simple) 2, (scrofulous) 1, Orchitis 2, Pneumonia 10, Pleurisy 5, Puerperal Peritonitis 1, Pertussis 1, Phthisis 3, Prolapsus uteri 1, Parturition 6, Rheumatism 7, Syphilis (secondary) 2, wounds (contused) 1, (incised) 1.

Total number of cases 216. Deaths 8 ; viz : 3 of Pneumonia ; one about three weeks, the other two about twenty months old. Three of Bronchitis ; one about 20 years, one about 6 years and the other 20 months old—one of Gastro-enteritis aged 19 years ; and one of convulsions aged 16 months.

Practical remarks.—In all the cases of death from Bronchitis, Measles had supervened upon Influenza, and although the eruption developed itself well, upon the subsidence of the eruption, the Bronchitic symptoms became more prominent—the fever and thirst continued, the expectoration more viscid, and less in quantity, the dyspnea greatly increased, and from the want of the proper change being produced in the blood, the surface generally became purple, and as the disease progressed, a general mahogany colored, or livid hue pervaded the system. They died of apnea, about the 7th day after the eruption came out.

The eruption having developed itself well, the friends were deceived as to the danger, and I was not called in until the dyspnea and general symptoms of asphyxia became alarming—when the cases were beyond the curative influence of remedies.

Two of the three deaths of pneumonia were produced by its complication with measles. In the other case the pneumonia supervened upon purtussis.

Measles, in many cases have been complicated with bowel affections ; but the most troublesome and frequent complication has been bronchitis, from the prevalence of influenza. During, and toward the close of the eruptive stage—the thirst, harrassing cough, stricture in the chest, difficulty and oppression in breathing, and the viscid, suppressed expectoration, were greatly relieved in many cases, by allowing the patient to eat ice freely. Indeed to the use of ice in this way, the recovery of several cases, running rapidly into a state of asphyxia, is mainly to be attributed.

In one of the cases of cataract, the absorbent operation was performed on both eyes. In one with prompt and perfect success. In the other the lens, which was hard, after being partially cut up in the operation, floated forward into the pupil, and produced considerable inflammation. Now, six weeks since the operation, it is nearly absorbed. The unabsorbed portion lies behind and against the iris, on the outside of the pupil, projecting into the pupil, so as to give it a crescentic appearance.—The sight is now partially, in a short time will doubtless be entirely restored.

The progress of this case is given, on account of its bearing upon a point in practice, in which there is a difference of opinion and practice, among the most distinguished occulists ; that is, as to the best condition of the pupil for this kind of operation for cataract, some preferring a fully dilated, and others a natural, or moderately dilated pupil. In this case the operation was performed when the pupil was fully dilated, and hence the undesirable result.

The case of infantile convulsions occurred in a tolerably robust child sixteen months old, and terminated fatally in six hours after the attack. The spasms were continuous.

In a quarter of a century's experience, only four cases of continuous spasms, coming on as a primary disease, are recollected; and they all terminated fatally in from five to eight hours.

In the case of puerperal peritonitis, the inflammatory symptoms were produced by fatigue and exposure to cold, sitting up the seventh day after parturition. The fever was decidedly inflammatory. The pain and soreness, extending over the right hypochondriac region, very distressing. The pain greatly increased during a paroxysm from seven until twelve every morning.

The first two days, bleeding, cupping, fomentations, hip bath, purgatives and opiates, were used as freely as seemed expedient and safe.—The opiate produced great irritability of the stomach and therefore could not be continued. The third day at 10 A. M. the violence of the pain and the soreness were greater than the previous days—pulse frequent, but not tense enough to bear the lancet—cupping did not afford relief—the bowels had been freely moved during the night. In this condition at 10 A. M in the height of the paroxysm, I gave her 35 grains of quinine. At three P. M. considerable deafness, and numbness in the extremities were complained of; but the pain and soreness was greatly relieved. 10 grains quinine was directed at nine at night, and the same quantity at five the next morning. At ten next day she was quite comfortable. Some complaint of deafness and roaring in the ears—skin cool and moist—pulse much improved. During the night she had copious perspiration. At eight in the morning she had slight pain, but it soon passed off. The quinine was discontinued. The next morning, some return of pain. The ten grain dose of quinine were resumed twice a day for two days. The lochial discharge was re-established and she convalesed rapidly.

No other remedy would probably have acted so speedily, safely and certainly, as a sedative, antiphlogistic, and diaphoretic febrifuge, as the quinine in large doses did in this case ; and probably no other remedies would have saved her life.

The case of cancer of the stomach was a negro man about 50 years old. In addition to the ordinary symptoms of that disease, which had gradually increased for 8 to 16 months, there was during two months previous to his death occasional discharges of blood from his bowels ; and evident obstruction in the pyloric orifice, from the frequent vomitings of the contents of the stomach.

The post-mortem examination exhibited all the stomach sound, except the pyloric orifice and about an inch and a half of the upper portion of the duodenum, which were near half an inch thick, of an indurated, indeed schirrous hardness. Just below the pyloric valve there was an open ulcer, with rather indurated edges, near half an inch in diameter. L. S.

CENSUS OF THE POPULATION OF NEW ORLEANS.

The census of the city recently taken must be full of the grossest errors. It gives us a population of about 96,000; whereas in 1840 the U. S. census allowed us 102,000. The idea that New Orleans has retrograded within the last 7 years is perfectly preposterous. No one places any reliance upon the correctness of the late census—indeed several gentlemen have assured us that their own families as well as many of their acquaintances had been erroneously represented. Whilst the 2nd Municipality is reported as having increased during the last 7 years from 21,500 to 42,919, the others are made to decrease in about the same ratio. Now it is not supposed that the 1st and 3d Municipalities have increased in proportion to the 2nd, but they certainly have not diminished. It is much to be regretted that the late census has been taken in to unsatisfactory a manner.

Inhalation of Sulphuric Ether—Surgical Operations under its Influence, at the New Orleans Charity Hospital.—We mentioned in our last number, that the Letheon had just been introduced into this city, and that we had witnessed an amputation of the thigh without pain. We shall now report the case in full, together with three other important operations which we have witnessed at the Charity Hospital.

The ether was given in several other amputations, and during an attempt to reduce a *luxation of the thigh*, of five or six weeks standing, but as we did not witness them we cannot give them. We may state that on account of an asthmatic affection, it could not be inhaled by one of the patients, and that it did no good in the luxation, as we are informed. We administered the ethereal inhalation recently in a violent and obstinate case of neuralgia, with the effect of affording *immediate relief* to the pain. The subject was a delicate lady who had been afflicted with neuralgia of the stomach for four months. The case is still under treatment, and will probably be reported in a future number.

The following are the operations at the Charity Hospital.

Case 1st.—Amputation of the Thigh under the use of the Letheon, or Inhalation of Sulphuric Ether. By Dr. W. Stone, at the Charity Hospital.

On the morning of the 25th Febry. 1847, I witnessed this interesting operation which was performed by Dr. Stone before the medical class and a number of physicians, among whom were Dr. Lewis of Mobile and Dr. Shanks of Memphis. The ether was administered by Mr. Ward in the presence of Dr. Blaisdell, agent of the patentee.

The subject was a man aged about 50 years, of rather slender form, but sound constitution, who three months previous had received a severe injury from the circular saw of a saw mill, which had cut diagonally across his knee, opening the joint and nearly severing the limb. He came to the Charity Hospital on the 22nd and was operated upon the 23d.

The patient was placed upon the table, the tourniquet applied, and every thing ready for cutting, when Mr. Ward applied the inhaler containing the ether to his mouth and directed him how to use it. The patient seemed a good deal agitated and apprehensive—pulse upwards of 100 at the commencement. Began to inhale at 19 minutes to 10 o'clock A. M. Inhaled rather slowly and without shutting his mouth closely, for 2¾ minutes, then stopped, put away the inhaler, talked a while about the effects, seemed alarmed, but being urged he resumed the inhalation after a lapse of 3 minutes. He now inhaled the ether for 2 minutes and 5 seconds, when he seemed to be completely unconscious, and Mr. Ward took away the instrument, at the same time directing the surgeon to cut away. In one minute the limb was off—he then groaned and stretched his arms above his head—pulse 134. In 3½ minutes from the time the operation was commenced, consciousness began to be evident. He was asked "if it hurt him?" Said "he did not feel it." He asked "if it was off?" Being answered in the affirmative, he replied "I am d—d glad of it—" and seemed much pleased when the amputated limb was held up before him—did not believe it until he saw the leg. His pupils were now much dilated. Was asked "if he had been dreaming?" He replied, "no." Was asked "if he had been conscious of what had been going on?" He replied, "no." Said "the last thing he recollected was pushing the inhaler from his mouth." In 25 minutes from the commencement of the operation the dressing was completed—pulse then 120—pupils still dilated—said he felt somewhat confused, but had no pain. He was then put to bed. The operation was entirely satisfactory to all who witnessed it—a thigh had been amputated *without pain*! Three quarters of an hour after the operation, when quiet in bed, pulse 112—did not recollect any pain from cutting—said "his first recollection was when he was asked if it hurt him? and the leg was presented to him."

February 26th.—Found patient very quiet and easy—said he had rested well through the night—has no pain—was hungry before breakfast—no thirst—pulse 110—tongue clean—skin natural—in short he was in a satisfactory condition.

This man suffered considerable constitutional irritation for several days, but recovered in good season, and was discharged from the hospital cured.

Case 2d.—Amputation of the Thigh. By Dr. Wedderburn, *on the 17th April*, 1847.

[Present at the operation, Drs. J. Hampden, Lewis, Slade, Axson, Hester, Harrall, and a number of medical students.]

John Curry, an Irish laborer, aged 36, has been about New Orleans the last twelve years, in the habit of drinking pretty freely, but says he has generally been healthy and robust; entered the Charity Hospital on the 25th February, 1847, for a *compound comminuted fracture of the Tibia*, caused by being run over by a cart-wheel. He now looks rather pale—is somewhat emaciated and has had a cough for the last six or eight weeks. Appears alarmed—countenance pale—pulse 90. Has a large, flabby, indolent ulcer at the place of fracture, with a brown surface—the lower portion of the limb somewhat œdematous.

He was placed on the table twenty-four minutes to 1, P. M. Every thing being ready, the inhalation was commenced. The ether was imperfectly inhaled for $3\frac{3}{4}$ minutes, but as he did not appear to be fully under its influence, the operation was suspended, and he was suffered to return to entire consciousness, which he did in four minutes. He talked incoherently for a while. The inhalation was commenced again, and being determined to give it a fair chance, he was made to inhale it for five minutes in the sitting posture. He was then laid down and the cutting commenced. He did not display the least sensation, although his eyes were open. The pupils were not dilated—pulse a little upwards of 100. The leg was off in $1\frac{1}{2}$ minute. He became sensible again in four minutes, so as to speak—asked to have water on his face—vomited—said he "felt so sleepy he could not wake."

He now became sick at the stomach and vomited; talked at random about work and various things; said "he would soon be ready for the cutting." Was asked if he had not felt any thing, said "no, that the Doctor marked it round, and he heard something drop." Was asked if he had felt any pain? said "none." I now asked him in a serious manner if he was aware that the operation was over and the leg off? He fixed his attention on me and said, "*no.*" I assured him that such was the fact, and casting his eyes upon the open stump, he remarked with astonishment, "*and so it is! I did'nt know it! it looks like a nice pork steak!*" He now returned fully to consciousness, $7\frac{1}{2}$ minutes from the last inhalation. His pulse was not much excited, nor his pupil dilated, though he looked quite pale, like a person just recovering from a fainting fit. Every person around was satisfied that he had suffered no pain from the operation. The patient did well, and the stump healed kindly, notwithstanding the infirm general health of the patient.

Case 3d.—Extirpation of the Parotid Gland, under the Inhalation of Sulphuric Ether. By Dr. A. Mercier, *at the Charity Hospital.*

On the 24th of April, this operation was performed in the *new amphitheatre* of the Hospital, being the *first* ever done there. There were some thirty or forty spectators.

The subject was a German, aged twenty-four years, who had a tumour nearly as large as the fist, hard and firmly set on the side of the parotid gland. The gland was supposed to be completely involved. As our object is only to give an account of the *operation*, we shall say no more about the history or nature of the case. The young man seemed to be pretty robust, and took his position on the table with firmness and composure. His pulse was now one hundred. Every thing being ready, he commenced inhaling the ether under the hands of Mr. Knapp, a little after 1, P. M. He inhaled freely and fairly for two minutes, in the sitting posture, when he *seemed* to be completely under its influence and was laid down. Dr. Mercier commenced the operation by putting a ligature round the primitive carotid artery. When the first incision was made, the patient commenced groaning, but did not flinch; but as the incisions were extended his groans increased, his hands were firmly clenched, and he appeared to suffer great pain; pulse then one hundred and twenty. The application of the ligature occupied $13\frac{1}{2}$ minutes. He was now completely conscious and commenced inhaling the ether again in the sitting posture. In less than two minutes he *seemed* to be fully under the influence; pulse at least one hundred and fifty; countenance very pale, breathing hurried. He was laid back and the cutting on the tumour commenced. He did not seem to feel the first cutaneous incision, but soon commenced groaning, writhing and giving every evidence of intense suffering. This he continued to do during the ten minutes which were occupied in removing the tumour. At the end of the operation the pulse was one hundred, and the patient soon became perfectly quiet. He said he had felt pain during the whole operation, and had been conscious of all that had been done ; yet he appeared to us to get very fully under the influence of the ether.

During the performance of the operation he frequently said something in the German language, which we did not understand. We have since learned from a gentleman who did understand him, that he was asking for *more ether ;* which goes to show that he either *experienced*, or *hoped* for relief from it.

It was the opinion of some of the physicians present, that the pain of this operation was not so great as it would have been without the ether ; but for our own part, we do not think, from the demonstrations given, that sensibility could have been much deadened. Of one thing we are sure, that in a similar operation performed by Dr. Luzenberg, on a colored woman in this city, three years ago, *without the aid of ether, opium, or any thing of the kind*, there was a perfect contrast in the demonstrations of suffering to what was here seen. The woman remained perfectly passive until the tumour was torn from its strong attachments, when she groaned and exclaimed, "glory to God."

It is proper to state that in Dr. Mercier's case, Mr. Knapp used the ordinary ether of the drug stores. On the day previous he had administered it to this man, and when he appeared to be under its influence, stuck a pin into the tumour. The patient said he felt it and compared it to the sensation of *burning*. In conclusion, we are bound to give as our opinion, that *the ether failed to produce insensibility in this instance.* E. D. F.

ELECTION OF PHYSICIANS AND SURGEONS TO THE NEW ORLEANS CHARITY HOSPITAL.

At a meeting of the Board of Administrators of this Institution held on the 12th of April, the following gentlemen were appointed to serve until the first Monday in November next, viz :

Surgeons.—A. Mercier, M. D., and J. B. Slade, M. D.

Physicians.—A. F. Axson, M. D., E. D. Fenner, M. D., T. M. Logan, M. D., B. Stillé, M. D., W. T. Brent, M. D., E. Martin, M. D. Charle McCormick, M. D.,—Wilson M. D. ; the last named resigned and his place has been filled by Dr. Cross.

House Surgeon.—J. C. P. Wedderstrandt, M. D.

We renew our request to the visiting physicians and surgeons of this Hospital, to furnish for publication all interesting cases that may fall under their observation. The new amphitheatre is now ready for service and is a great acquisition to the Hospital.

MONTHLY REPORTS
OF THE NEW ORLEANS CHARITY HOSPITAL.

MAIN BUILDING.

February—Admitted: Males, 451; Females, 107. Total 558.
Discharged: Males, 447; Females, 97. Total 554.
Died: Males, 55; Females, 12. Total 67.
Remaining on the 1st of March, 323.

LUNATIC ASYLUM.

Admitted: Males, 41; Females, 12. Total 53.
Discharged: Males, 36; Females, 10. Total 46.
Died: Males, 3; Females, 0. Total 3.
Remaining on the 1st of March, 86.

MAIN BUILDING.

March—Admitted: Males, 467; Females, 153. Total 620.
Discharged: Males, 419; Females, 105. Total 524.
Died: Males, 70; Females, 12. Total 82.
Remaining on the 1st of April, 337.

LUNATIC ASYLUM.

Admitted: Males, 35; Females, 22. Total 57.
Discharged: Males, 35; Females 18. Total 53.
Died: Males, 1; Females, 0. Total 1.
Remaining on the 1st of April, 90.

N. B. On the 26th of April, there were 96 admissions into this Hospital, 69 of which came from the ship Stephen Baldwin, just arrived from Liverpool. This ship left Liverpool with nearly 300 Irish emigrants—was out 72 days—fell short of water and provisions, and arrived here with the most miserable set of sick and starving wretches almost ever seen. Twenty-six died on the passage, and found in the watery abyss a quietus to all the miseries of life. The principal diseases presented by those admitted into the Charity Hospital, were typhoid fever, diarrhœa, and dysentery. Some were in a simple state of debility and anœmia from want of food. If they recover from their present miserable condition, they will probably never again suffer the horrors of starvation in this land of plenty.

OBITUARY.

Died on the 25th of February, after a long and painful illness, CHARLES F. SNOWDEN, M. D., aged 40 years, one of the most respectable practitioners of this city. At a meeting of the Physico-Medical Society, the following resolutions were adopted, viz:

Resolved, That the Physico-Medical Society has heard with great regret of the death of Dr. C. F. Snowden, one of its most excellent and esteemed members.

Resolved, That, while this Society deplores the untimely fate of a worthy member, it deeply sympathizes with his afflicted family in its bereavement.

Resolved, That a copy of these resolutions be transmitted to his family, and that they be published in the Medical Journal of this city.

WM. P. HORT, M. D.,
President.

A. F. Axson,
Recording Secretary.

MORTALITY IN NEW ORLEANS,

With a list of the diseases, from 12*th Dec.* 1846, *to* 16*th April* 1847, *being four months or seventeen weeks. Furnished by* A. Hester, *Secretary to the Board of Health.*

Acute Bronchitis, 1; Acute Gastritis, 1; Acute Hepatitis, 1; Abortion, 1; Accidental, 2; Amputation of leg, 1; Anœmia, 5; Aneurism, 1; Aneurism of the Aorta, 1; Apoplexy, 21; Ascites, 4; Asthenic Bronchitis, 1; Asthma, 1; Atrophia, 1; Bladder, inflammation of, 1; Bowels, do., 7; Bowels, ulceration of, 1; Brain, compression of, 1; do. concussion of, 1; do. congestion of, 15; do. disease of, 2; do. inflammation of, 5; do. softening of, 1; do. effusion of, 1; Breast, abscess in, 1; do. carcinoma of, 1; Bronchitis, 14; Burn, 3; Cancer, 1; Catarrh, 11; do. Chronic, 3; do. Pulmonary, 5; Cerebral congestion, 3; do. Hemorrhage, 1; Cerebritis, 14; Cholera Infantum, 2; do. Morbus, 2; Chlorosis, 1; Chronic Bronchitis, 1; do. Diarrhœa, 12; do. Disease, 6; do. Dysentery, 36; do. Enteritis, 17; do. Gastritis, 2; do. Gastro-enteritis, 5; do. Hepatitis, 1; do. Meningitis, 1; do. Myelitis, 1; do. Pleuro-pneumonia, 2; do. do. 4; do. Rheumatism, 1; do. Peritonitis, 2; do. Scrofulous affection, 2; Congestion, 2; do. pulmonary, 2; Constitutional disease, 2; Coxalgia, 1; Colic, 1; Colitis, 5; Consumption, 201; Contusion, 1; Convulsions, 53; Cramp, 2; Croup, 14; Cynanche Trachealis, 2; Debility, 51; Delirium Tremens 12; Dentition, 19; Diarrhœa, 11; Dropsy, 21; Drowned, 15; Dysentery, 25; Encephalitis, 4; Enterocephalitis, 1; Entero-colitis, 1; Enteritis, 14; Epilepsy, 3; Erysipelas, 1; Excessive flagellation, 1; Fever, 11; do Adynamic, 1; do Ataxic, 1; do Bilious, 1; do Bilious Remit. 1; do Cerebral, 3; do Congestive, 14; do do Typhoid, 1; do Hectic, 1; do Interm. 7; do do Pernicious, 1; do Nervous, 2; do Pernicious, 2; do Puerperal, 1; do Putrid, 1; do do malignant, 2; do Remit. 3; do Scarlet, 2; do Typhoid, 64; do Yellow, 6; do Inflammatory, 1; Gangrene, 5; Gastritis, 5; Gastro Enteritis, 21; do Hepatitis, 4; Glottis, Œdema of, 2; Head, injury of, 2; Heart, chronic affection of, 1; do disease of, 4; do hypertrophy of, 6; Hemorrhage, 2; do from Lungs, 2; do Puerprea, 1; Hepatic, Abscess, 1; Hepatitis, 3; do Chronic, 1; Hepatic phthisis, 1; Hydrocephalus, 3; Hydropsia, 8; Hypertrophy, 3; Influenza, 1; Inflammatory disease, 3; Injury, caused by Steam Boat explosion, 1; Injury, from a fall, 1; Intemperance, 1; Intestines, affection of, 2; jaundice, 1; Leg, fracture of, 1; Lock-jaw, 1; Lungs, congestion of, 1; do grangrene of, 1; do inflammation of 6; Mania-à-portu, 4; Marasmus, 5; Measles, 4; Metro-Peritonitis, 1; Meningitis, 14; Neck, abscess in, 1; Old age, 9; Palsey, 2; Peritonitis 3; Penetrating wound, 3; Phrenitis, 1; Pleurisy, 8; Pleurodynia, 1; do. pneumonia, 10; Pneumonia, 34; do. ataxic 1; Rheumatism, 2; Scald, 1; Scorbutis, 1; Scrofulous Inflammation, 1; Scrotum, Gangrene of, 1; Senile Catarrh, 1; Skull, fracture of, 1; Small pox, 8; Sore throat, 1; Spinal arachnitis, 1; Spine, fracture of, 2; Spasms, 1; Still-Born, 64; Stomach, Cancer of, 1; Stricture, 1; Tetanus, 20; Tibia, fracture of, 1; Traumatic Tetanus, 1; Trismus Nascentium, 20; Typhoid pneumonia, 1; Uncertain, 135; Urethra, stricture of, 1; Uterus, cancer of, 2; do. disease of, 1; Vertrebæ, injury of, 1; Womb, cancer of, 1. Total 1325; Whites, 946; Colored, 379; Under 10 years of age, 465; Over 10 years of age, 860.

ABSTRACT OF A METEOROLOGICAL JOURNAL FOR 1847.

By D. T. LILLIE, at the City of New Orleans.

Latitude, 29 deg. 57 min.; Longitude, 90 deg. 07 min. west of Greenwich.

WEEKLY. — 1847.	THERMOMETER.			BAROMETER.			COURSE OF WIND.	FORCE OF WIND, Ratio 1 to 10	Rainy Days.	Quantity of Rain. — *Inches.*
	Max.	Min.	Range.	Max.	Min.	Range				
Feb. - 27	73.7	40.5	33.2	30.29	29.72	0.57	N.E.	3	2	0.606
March - 6	66.0	46.5	19.5	30.29	29.86	0.43	S.E.	3	2	2.735
" - 13	80.0	49.0	31.0	30.14	29.75	0.39	S.E.	$2\frac{3}{4}$	2	0.580
" - 20	76.0	40.0	36.0	30.50	29.88	0.62	N.E.	$3\frac{1}{4}$	1	0.256
" - 27	71.5	45.0	26.5	30.44	30.00	0.44	W.	3	1	1.560
April - 3	87.0	51.5	35.5	30.42	30.04	0.38	W.	$2\frac{1}{2}$	0	0.000
" - 10	88.3	66.0	22.5	30.16	30.01	0.15	S.W.	$2\frac{1}{2}$	3	3.035
" - 17	79.0	52.0	27.0	30.25	30.07	0.18	W.	3	1	0.750
" - 24	79.0	57.5	21.5	30.26	30.09	0.17	E.	$2\frac{3}{4}$	3	5.425

Remarks.—The Thermometer used for these observations is not attached to the Barometer, but is a self-registering one, and is placed in a fair exposure. Regular hours of observation, 8 A. M., 2 P. M. and 8 P. M.

The Barometer is located at an elevation of 19 feet above the level of the ocean, and is suspended clear of the wall of the building.

The Rain Guage is graduated to the thousandth part of an inch, and the receiver is elevated 40 feet from the ground.

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FROM JULY 1846, TO MAY 1847.